Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-1	TS	0	0	0	0	This is not an easy document to read - AR4 used helpful bold initial sentences, and I feel, a more user-friendly narrative tone, to get the message across. There seems to be no echoing of the AR4 "unequivocal" conclusion. Should anything be read into this? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Noted. WGI AR5 author team has decided not to use the bolding approach to highlight certain key statements over others. A novel feature of this Technical Summary that should allow easier access to key policy relevant information has been introduce through the nine Thematic Focus Elements. With regard to the "unequivocal conclusion" from AR4, there are a number of similarly important key conclusions in the AR5, many of which are concisely summarized in this TS and in the SPM.
TS-2	TS	0	0			The Thematic Focus Elements are very useful and material is nicely integrated from across different chapters of the assessment. [Government of Canada]	Thank you.
TS-3	TS	0				Today, the optimal approach for sea ice projections is not clear, although one notes that these 18 methods should have a credible underlying physical basis in order to increase their reliability (12.50, line 17). Add: because the models CMIP3 and CMIP5 and RCP models lacks sufficient data on changes in sea ice volume. (I suggest to put this idea at this chapter.) [CELSO COPSTEIN WALDEMAR, BRAZIL]	Noted, see revisions in Projections section TS.5.
TS-4	TS	0				Today, the optimal approach for sea ice projections is not clear, although one notes that these 18 methods should have a credible underlying physical basis in order to increase their reliability (12.50, line 17). Add: because the models CMIP3 and CMIP5 and RCP models lacks sufficient data on changes in sea ice volume. (I suggest to put this idea at this chapter.) [CELSO COPSTEIN WALDEMAR, BRAZIL]	duplicate comment -- see TS.3
TS-5	TS	0				the same from the comment No 18 see:http://www.annualreviews.org/doi/abs/10.1146/annurev-earth-042711-105345 [CELSO COPSTEIN WALDEMAR, BRAZIL]	no action -- unspecific comment
TS-6	TS	0				the same from the comment No 21 [CELSO COPSTEIN WALDEMAR, BRAZIL]	no action -- unspecific comment
TS-7	TS	0				the same from the comment No 24 [CELSO COPSTEIN WALDEMAR, BRAZIL]	no action -- unspecific comment
TS-8	TS	0				the same from the comment No 27 [CELSO COPSTEIN WALDEMAR, BRAZIL]	no action -- unspecific comment
TS-9	TS	0				the same from the comment No 30 [CELSO COPSTEIN WALDEMAR, BRAZIL]	no action -- unspecific comment
TS-10	TS	0				Unfortunately I did not have the time to finish the review due to the upcoming deadline. However, I have seen that the TS is a clear summary of the report. The most important findings are explained more in detail than in the SPM and this makes the text more directed to an audience with (at least) some scientific background. As for the SMP, I suggest to include a table on the various levels of confidence and plrobability (percentage) that I did not see in this TS either. [Luisa Cristini, United States]	Thank you. Suggestion accepted: a box on IPCC AR5 Treatment of Uncertainty has been added to Technical Summary, incl. the table suggested by reviewer (new Box TS.1)
TS-11	TS	0				Overall I think the TS is very much improved since the ZOD. It is well written and comprehensive, and is close to being in final form. The TFEs now generally work well, and most duplication and repetition present in the previous draft has been avoided. [Nathan Gillett, Canada]	Thank you.
TS-12	TS	0				Although most instances of repetation have been removed, there is still some repetition of material. For example the low confidence in observations of tropical cyclone change is described in sections TS.2.7.1, TS.4.7, TFE.9, and TS.6.1. Five sentences on the climate response to stabilised forcing are exactly repeated in TS.5.3.2.7 (pg 46, ln 57 - pg 47, ln 5) and TFE.8 (pg 53, ln 27-32). Three sentences on the effect of nitrogen limitation on land carbon uptake are exactly repeated in TS.5.4 (pg 41, ln 41-46) and TFE.7 (pg 51, ln 40-46).Text on the affect of aerosol emissions on near term warming is repeated exactly on consecutive pages (Pg 37, ln 30-33 and pg 38, ln 19-22). Text on long-term temperature changes is exactly repeated on pg 43 ln 36-47 and pg 52, ln 17-27, and text on warming after 2100 is exactly repeated on pg 46, ln 53-56 and pg 52, ln 29-32. There is no point repeating the same material twice, and this takes up space that could be used for something else. In most instances where material is discussed in a TFE, I think it should be removed from the main text and a reference to the TFE inserted. For the long-term warming text, I think it would be better to leave it in the main text and remove from the TFE. [Nathan Gillett, Canada]	noted; Much effort has been made in the final draft to reduce unnecessary duplication of material. However, with the new feature of Thematic Focus Elements (TFEs), some duplication can not be avoided as TFEs are intended to cover a specific topic comprehensively end-to-end.

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TS-13	TS	0				Glaciers are metioned multiple times in the TS, mainly in the context of their contribution to sea level. It is not always clear whether the glaciers referred to include Antarctic and Greeland glaciers, and/or ice sheets. It would be easiest for the reader if glaciers were used either to mean only mountain glaciers, or mountain glaciers plus glaciers in ice sheets, and used consistently in this way throughout (or if the other definition were used being explicit - 'mountain glaciers' or 'mountain glaciers and glaciers in Greenland and Antarctica').. For example on page 7, ln 38-39 'glaciers' is used to mean mountain glaciers, plus glaciers within ice sheets, In TFE.2 and TS.15, TS.5.5 and pg 28, ln 1-2 I think the meaning is mountain glaciers only. Further to this, I think the formal definition of glaciers includes ice sheets themselves, but in the TS 'glacier' seems not to include ice sheets. This strengthens the case for clearly defining the meaning of glacier as used here on first use, and then sticking to this definition. [Nathan Gillett, Canada]	Response - Understood. Throughout the assessment a consistent definition of "glacier" and "ice sheet" has been attempted. This definition was chosen for clarity and to avoid multiple, subtle distinctions that serve little function to the generalist reader except to confuse (e.g., "mountain glaciers", "valley glaciers" "outlet glaciers" etc). The definition adopted is clearly given in the Glossary and in the introduction to CH4. In addition, Chapter 13 has attempted to clearly indicate when glaciers in Antarctica and Greenland are including by saying words like "including/excluding peripheral" glaciers of Greenland/Antarctica.
TS-14	TS	0				Predictions should be more clearly differentiated from projections. Decadal predictions are a new topic for the report, and sometimes in the text the only clue that initial value predictions were being discussed was the use of the word 'prediction' rather than 'projection'. e.g. Figure TS.11, TS.6.4, ln 41-43. 'Predict' is used many times in the TS where the meaning is 'simulate in response to a given scenario', rather than 'forecast in response to known initial conditions'. I think this is OK, but it does mean that when the text is dealing with an initial value prediction this needs to be flagged explicitly. At the beginning of TS.5.3.1 a sentence should be inserted clearly describing the difference between a climate prediction based on known initial conditions, and a climate projection. Then when 'predictability' is discussed elsewhere e.g. TS6.4, 'predictability' should be replaced with 'initial-value predictability' or similar. [Nathan Gillett, Canada]	Helpful suggestion. The text and figure captions, in particular, have been revised to clarify when initialized prediction vs. uninitialized projections are being used.
TS-15	TS	0				The section on near-term warming stresses the warming influence of reductions in aerosol emissions in the near term. But chapter 8 shows that much of the near-term sulphate-induced warming will be compensated by enhanced nitrate-induced cooling (Figure 8.20). I am not sure if this nitrate-induced cooling is fully-accounted for in the simulations used for near-term projections (I know it isn't in CanESM2, for example). At least in RCP 8.5 this results in only a very small aerosol adjusted forcing change to 2030. Perhaps this should be mentioned. [Nathan Gillett, Canada]	Agreed. Warming hiatus box fully addresses all related issues and the reader is directed to this box throughout the TS.
TS-16	TS	0				Generally Technical Summary is well written and structured. The absence of references and excessive figures makes it easy to read, perhaps more so than the SPM. However in some cases the overuse of commas makes sentences overly fragmented (i.e. In the Southern Hemisphere, very few long records exist; satellite records of snow water equivalent date from 1979, but show no trends - p. 8 line 23)) [Government of Australia]	Noted; copy edit
TS-17	TS	0				Some acronyms are not spelt out on first use - i.e. AOGCM (p.12 line 20) and SLE (p.12 line 45) [Government of Australia]	Noted; copy edit; Note that an List of Acronyms will be part of the Final Report.
TS-18	TS	0				General remarks: All figures need to be consistent in SPM, TS and in the underlying report. Remaining differences need to be explained. CMIP3 and CMIP5 need to be consistently presented across report, TS, SPM, including assessment of quality and uncertainties. [Government of Germany]	Consistency between SPM, TS and underlying report is ensured. In fact, a number of additional Figures have been included in the TS to provide in the TS more direct links from the figures in SPM to the figures in the underlying Chapters. But consistency does, in our view, not mean all the figures need to be identical. A separate box discussing CMIP3 and CMIP5, and SRES and RCPs has been added (see Box TS.6)
TS-19	TS	0				Reasons for the lower increase in the global mean temperature in the last decade must be given consistently in the TS, and throughout the report, information could be taken from Chapter 10. [Government of Germany]	Accept. A new Box has been added to the TS discussing the recent period of slowed down global mean warming (i.e., "hiatus"; see Box TS.3 and Box 9.2). Discussion on the causes of the hiatus is now everywhere deferred to this box, and conclusions on its causes are now consistent throughout the TS and report.
TS-20	TS	0				The figures in the TS and even more in the SPM will be very important for outreach. They should be simple	Noted. We think that most of the TS figures and all of

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						without diluting the scientific content. An informed layperson should understand the basic messages without reading the text. [Government of Germany]	the SPM figures fullfill these requirements.
TS-21	TS	0				Throughout the whole WGI report where possible, reference should be made to preidustrial levels. Make sure the presentation of temperature changes is consistent with the presentation in the other Working groups so that references to impacts and mitigation scenarios can be made. [Government of Germany]	Different reference periods have been included in the Chapter 12 assessment of climate projections (Tables 12.2 and 12.3) and offsets are being referred to in Table TS.1
TS-22	TS	0				The use of scientific jargon should be strictly avoided. All scientific terms should be supported by simple explanations and whereever possible short definitions in the text for non-experts. At least all expressions used in the SPM and TS should be explained in the Glossary. [Government of Germany]	Noted. The Glossary does include many scientific/technical terms used in this Technical Summary. It is however impossible and inpractical to include all technical terms used in the Technical Summar in the Glossary.
TS-23	TS	0				The information on GWP of greenhouse gasses as provided in TS.2 in AR4 or TS.4 in AR3 should also be provided in AR5 (preferably in tabular form). Regarding GWP, GWP used in greenhouse gas inventories reported by Parties to the secretariat under UNFCCC and Kyoto Protocol is based on the information provided by IPCC such as TS.2 in AR4 and TS.4 in AR3. In addition, in the future, there might be discussion to add a new reporting gas or to review the GWP used under the current framework. Therefore, scientifically reliable GWP which is authorized by IPCC is necessary. [Government of Japan]	Noted. This information is available from Chapter 8, Appendix 8.A, Table 8.A.1 ff, and is not being repeated here.
TS-24	TS	0				The conclusion that models often do not adequately reproduce soil moisture conditions and drought effects is important and may warrant being brought forward into the Technical Summary. [Government of United States of America]	Noted. The assessment conclusion that soil moisture projections are uncertain and not robust in many regions is included prominently in the Technical Summary, both in TFE.1, TS.5 and TS.6.4
TS-25	TS	0				The enumeration of the list of key processes often not included in terrestrial models is valuable; so much so, that an abbreviated statement might be appropriate for the Technical Summary. Given the dependence of the conclusions of this Chapter on model results, it is important that the readers appreciate the limitations of the models. [Government of United States of America]	No action, comment seems to be misplaced -- reference to "dependence of the conclusions of this chapter on model results" remains unclear.
TS-26	TS	0				The finding that there are - currently - no known feasible CDR methods is important and should be considered for inclusion in the Technical Summary. [Government of United States of America]	Noted. A new Box TS.7 on Climate Geoengineering Methods has been included in the TS.
TS-27	TS	0				The word 'attribution' is formally defined in the Glossary, yet it (and variations thereof) appears to also be used colloquially at times in this Technical Summary (e.g. TS-10 Line 40; TS-18 Line 2). An alternative word(s) or phrasing should be used in such instances.' [Government of United States of America]	Taken into account. However, TS language also needs to reflect the language used in the underlying chapters. Thus "attributed to" is still used for example in the TS drivers section in line with Chapter 8
TS-28	TS	0				There is an assumption in the TS that those reading the document have knowledge of the abbreviations used which are not in common use in policy groups outside the IPCC process. [Government of United Kingdom of Great Britain & Northern Ireland]	List of Acronyms will be part of publication of the report.
TS-29	TS	0				There is a need with a translation of the groups understanding of uncertainty terms perhaps as an annex so that the reader has a shared understanding [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted. A new Box on the IPCC AR5 Treatment of Uncertainty has been added to Technical Summary, incl. the table suggested by the reviewer (new Box TS.1)
TS-30	TS	0				Chapter should start with drivers of climate change and then response; not response and then drivers. Drivers are much better known, and logically the progression is from drivers to response. [Stephen E Schwartz, United States of America]	The narrative of the Technical Summary follows the narrative of the WGI AR5 (observations, drivers, understanding, projections), for which the outline has been approved by the IPCC Plenary.
TS-31	TS	0				Congratulations on this Second Order Draft with the FOD Technical Summary. The use of the Thematic Focus Elements gives a very good structure to the TS and makes it easy to search for some key elements. Also the boxes are in general clear and give a good overview. Some improvements: I	Thanks, noted. On the uncertainty: the AR5 guidance note clearly states that the characterization of uncertainty using calibrated uncertainty language

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						believe that the uncertainty language is used inconsistently. This language is already difficult to understand and using it differently in different chapter makes it incomprehensible. For example, According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: Assign a likelihood for the event or outcomes, for which confidence should be “high” or “very high” (see Paragraphs 8-10). In this case, the level of confidence need not be explicitly stated. So When a likelyhood statement of 'likely' or even higher, for example 'very likley', is given, it is not necessary to still give a confidence statement when this is high or very high confidence confidence. Also some of the figures / graphs are unclear because the explanation is unsufficient but most of the time becuase there is too much information in one graph (or presented in a incomprehensible way). [Line van Kesteren, the Netherlands]	should be done in a way that conveys the most information to the reader. The wording used in the Guidance Note regarding complementing likelihood statements with a confidence statement if confidence is high or very high is "need not be explictly stated" and thus does not prohibit it's addition. On the "incomprehensible figures" comment: noted, in this Final Draft many figures have been substantially edited and are now hopefully more comprehensible.
TS-32	TS	1	0	1	0	Note that the header of this report indicates that it is a "First Order Draft" yet it is "Second Order Draft" [Government of Kenya]	Correct. It's the First Order Draft of the Technical Summary that is being reviewed with the Second Order Draft of the WGI AR5.
TS-33	TS	1	1	67	1	nearly all figures are too busy. Take out some of the curves that are only discussed in the individual chapters; increase the "stamp" figures [Andrea Flossmann, France]	no action -- unspecific about what to change in any particular figure.
TS-34	TS	1	1	99	18	"Climate Change 2013: The Physical Science Basis: Technical Summary": This is listed as a "First Order Draft." Is this correct? Should there be a Second Order Draft or is this new? [Forrest Mims, United States of America]	Correct. It's the First Order Draft of the Technical Summary that is being reviewed with the Second Order Draft of the WGI AR5.
TS-35	TS	1				Overall the TS is clear and nicely constructed. I particularly liked the thematic focus elements, although it's a shame that a new acronym was introduced for them. If new acronyms can be avoided, they should be, to make the AR5 more accessible to folks who are not soaked in IPCC-speak. [Dian Seidel, United States of America]	Thanks. We decided to keep the term "Thematic Focus Elements" and to also use the acronym TFE for it.
TS-36	TS	3	5	3	6	Three issues are mentioned (understanding the past, document the present, and projecting the future). I think the issue of forming a scientific basis for effectively mitigating climate change is missing and could be mentioned. [Jan Fuglestvedt, Norway]	Reject. Climate Change mitigation is outside the remit of the WGI AR5 and will be dealt with by WGIII.
TS-37	TS	3	8	3	8	Perhaps include the date of the SREX report, also AR$ for clarity. [Jeffrey Obbard, Singapore]	Reject. The date of publication of the SREX is given in the footnote.
TS-38	TS	3	8	3	13	Sentence is very long and meaning is lost. Suggest breaking into three sentences with full stops between 'climate system' and 'they' on line 9 and between 'in forcing' and 'they quantify' on line 11. [Government of Australia]	Reject. We prefer to keep one sentence.
TS-39	TS	3	8	3	13	The chapters cover direct and proxy observations of changes in all components of the climate system, they assess the current knowledge of various processes within, and interactions among, climate system components, which determine the sensitivity and response of the system to changes in forcing, and they quantify the link between the changes in atmospheric constituents, and hence radiative forcing, and the consequent detection and attribution of climate change.' Sentence too long. Suggest to divide in two: The chapter.... in forcing. In addition, they quantify .... climate change. [Line van Kesteren, the Netherlands]	Reject. We prefer to keep one sentence.
TS-40	TS	3	8	3	13	The chapters cover direct and proxy observations of changes in all components of the climate system, they assess the current knowledge of various processes within, and interactions among, climate system components, which determine the sensitivity and response of the system to changes in forcing, and they quantify the link between the changes in atmospheric constituents, and hence radiative forcing, and the consequent detection and attribution of climate change.' Sentence too long. Suggest to divide in two: The chapter.... in forcing. In addition, they quantify .... climate change. [Line van Kesteren, the Netherlands]	duplicate comment -- see TS.40
TS-41	TS	3	12	3	12	I suggest adding "emissions" after "between" since this is the main driver of changes in atmospheric composition. [Jan Fuglestvedt, Norway]	Reject. While technically true, WGI AR5 does not assess emissions and thus the focus on changes in atmospheric composition is warranted. Emissions are assessed by IPCC WGIII.

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TS-42	TS	3	13	3	14	You do not seem to realise that using only ":simulations" as a technique to judge "projections: is not enough. They will only be considered believable if they can successfully forecast future clinate. This is something that none of them have succeeded in doing [Vincent Gray, New Zealand]	Reject. Claim unsupported by evidence. See Chapter 9 of WGI AR5 for the assessment and evaluation of climate models.
TS-43	TS	3	14			model simulations forced by a new set of scenarios > driven by a new set of emission scenarios (if this is meant) [Petra Seibert, Austria]	Reject. The new scenarios used in the AR5 are the representative concentration pathways, RCPs, which are not emission scenarios.
TS-44	TS	3	22			please write, that the references are gien in curly brackets [Barbara Früh, Germany]	Accept. Sentence has been added.
TS-45	TS	3	24	3	24	replace "of" with "to" [Government of United Kingdom of Great Britain & Northern Ireland]	copy editor
TS-46	TS	3	24			Delete "Multiple complementary" (make same change on p.69) [Government of United Kingdom of Great Britain & Northern Ireland]	Reject. No reason given. (comment should be for p-4)
TS-47	TS	3	24			access TO this information [Reto Knutti, Switzerland]	copy editor
TS-48	TS	3	27	3	32	As with SPM, greater explanation of the treatment of uncertainty, including the meaning of specific terms, is needed in the TS. [Government of Canada]	Accepted. A new box on IPCC AR5 Treatment of Uncertainty has been added to Technical Summary, incl. the table with all the terms used in the AR5 to specify the level of uncertainty (new Box TS.1)
TS-49	TS	3	27	3	32	Suggest you put in a box similar to Box TS.1 in the AR4 Technical Summary, explaining how the confidence and likelihood terminologies translate to odds and probabilities. [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted. A new box on IPCC AR5 Treatment of Uncertainty has been added to Technical Summary, incl. the table with all the terms used in the AR5 to specify the level of uncertainty (new Box TS.1)
TS-50	TS	3	27	3	32	this may not be a sufficient explanation of the expression of uncertainty. Specifically, the distinction between degrees of certainty / confidence versus probability is not clear. Lines 30-31 indicate that probability if quantified and yet on the following pages it is expressed as likely, very likely, etc. Where is the table that relates this languages to quantitative probabilities? [David Sauchyn, Canada]	Accepted. A new box on IPCC AR5 Treatment of Uncertainty has been added to Technical Summary, incl. the table with all the terms used in the AR5 to specify the level of uncertainty (new Box TS.1)
TS-51	TS	3	27		32	Excellent explanation [Gabriele Hegerl, United Kingdom]	Thanks. A new box on IPCC AR5 Treatment of Uncertainty has been added to Technical Summary, incl. the table with all the terms used in the AR5 to specify the level of uncertainty (new Box TS.1)
TS-52	TS	3	32	3	32	The terminology used should be explicitly explained. E.g. what does virtually certain mean? Or high confidence? [Dora Marinova, Australia]	Accepted. A new box on IPCC AR5 Treatment of Uncertainty has been added to Technical Summary, incl. the table with all the terms used in the AR5 to specify the level of uncertainty (new Box TS.1)
TS-53	TS	3	41			Briefly say what the 'Thematic Focus Elements' are. [Nathan Gillett, Canada]	This is briefly done in paragaph 2 of the Introduction, but the topics covered have not been listed. Those can most easily be seen from the Table of Contents to the Technical Summary.
TS-54	TS	4	6			What is the difference between availability & acquisition? That the data's there & that it's being used? Not very clear [William Ingram, United Kingdom]	Availability is it is possible to get it. Acquisition it has been acquired
TS-55	TS	4	7	4	7	change 'have occurred' to 'were achieved' [Rolf Müller, Germany]	Noted
TS-56	TS	4	7	4	8	It would be amazing if they found the climate was not changing [Vincent Gray, New Zealand]	Not a scientific comment
TS-57	TS	4	8			This language indicates that there have been identified also aspects that do not show such evidence. For the sake of balance it would be helpful to identify those for the reader. [Klaus Radunsky, Austria]	This information is presented at many places in the main text.
TS-58	TS	4	12			Scetion TS2.2.1. This section deceitfully fails to mention that there has been no statistically significant warming since 1997, just 2 years after the publishing of AR2. It needs to be mentioned or the IPCC will be accused of being a lobbyist organisation that conveniently "forgets" to mention key information that is contrary	A box on the weaker trends during 1998-2012 has been added.

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						to the lobbyists' claims. [John McLean, Australia]	
TS-59	TS	4	14	4	14	"Globally averaged near surface temperature" What nonsnse! Nobody has ever measured such a quantity. It would require simultaneous measurements of thermometers situated randomly over the entire earth's surface, including the oceans. You are surely referring to the so-called "Annual Global Surface Temperature Anomaly" which is not a temperature at all. but a complex multi-average of a large number of unrepresentative non-standard weather station maximum an minimum temperature measurements. This concoction bears only a very slight resemblence to any genuine global mean suface temperature [Vincent Gray, New Zealand]	Rejected: This has been well studied.
TS-60	TS	4	14	4	16	reason for choosing 1972-73 winter as start point is not clear and not referenced [Government of United Kingdom of Great Britain & Northern Ireland]	This year was not chosen as a starting point.
TS-61	TS	4	15			I would recommend the more precise statement : there has been warming from 1915 to 1940 with the same slope as 1975-1998, cooling in between, and a plateau since 1998 (see experimental data in Fig. 1.4 of SOD) ; and all those observations are consistent with the 60-year period oscillatory component, itself related to the motion of the Sun with respect to the barycenter of the solar system as suggested by A. Mazzarella, N. Scafetta, Theor. Appl. Climatol, DOI 10.1007/s00704-011-0499-4, and by C. Loehle and N. Scafetta, Open Atmospheric Science Journal 5 (2011) 74. [François Gervais, France]	Rejected: Too precise and a single reference.
TS-62	TS	4	15			Is 'virtually certain' a downgrade from 'unequivocal'? Could sound like one to readers, although I imagine the intention is just to put this into the standard uncertainty language. [Nathan Gillett, Canada]	Certain is now used.
TS-63	TS	4	17	4	18	The phrasing of this sentence is so similar to one above that it took me three readings to realize this sentece referred to sea surface temperatures. I think it would help to rephrase; indeed, I would think this sentence should be combined with the one above as it is virtually certain that both the land and sea surface temperatures have gone up during the 20th century. [Michael MacCracken, United States of America]	Made Clearer
TS-64	TS	4	17	4	20	It seems strange to pick out SST as being virtually certain without also providing an assessment of other lines of evidence that show that near surface temperatures have increased. If possible it would be useful here to show the relative confidence of other lines of evidence: land surface air temperatures and marine air temperatures. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Many variables are discussed
TS-65	TS	4	18	4	20	I don't think it is very helpful to say that there is "better understanding" without giving some sense of what it is and that it is limited or something--otherwise, it seems to me almost useless info to a decision maker. The sentence needs to be of the form "Better understanding provided by intercomparison studies indicates that ....(and say something insightful)" [Michael MacCracken, United States of America]	Noted
TS-66	TS	4	23			Figure TS.1: Same comment as above [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	noted
TS-67	TS	4	23			Figure TS.1: It might be helpful to note in the caption why there are multiple grey lines in the Land Surface Air Temperature panel. [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	Noted
TS-68	TS	4	23			Figure TS.1: Why is Northern Hemisphere snow cover plotted for March-April only, and similarly why is Arctic sea ice extent only plotted for September? I know that September is routinely the month of minimum extent for the sea ice, but I confess I don't know why March-April is used for snow cover. It would be helpful to justify these choices in the figure description, if only to show that it is not selective bias. [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	These are the seasons when change is most meaningful.
TS-69	TS	4	24			delete "in the climate element" (make same change on p.69) [Government of United Kingdom of Great Britain & Northern Ireland]	Rejected: We are referring to a particular observation.
TS-70	TS	4	25	4	25	The dataset used to calculate these values should be referenced (e.g. HADCRUT4) as other global temperature datasets result in different rates of temperature rise. [Government of United Kingdom of Great Britain & Northern Ireland]	Rejected: Figure 1 includes many data sets other than HadCRUT4
TS-71	TS	4	25			delete "in this latest version" (make same change on p.69) [Government of United Kingdom of Great Britain & Northern Ireland]	Done
TS-72	TS	4	26			This sentence is incomprehensible. Does it mean "Heat island effects are unlikely to have contributed more	Yes that is what it says.

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						than 10% to the observed centennial global land surface air temperature trend"? If yes, say this. [Government of United Kingdom of Great Britain & Northern Ireland]	
TS-73	TS	4	26			“normalized” obscure if this is supposed to be the policy-makers' route into the detail. “are anomalies (differences) from”? [William Ingram, United Kingdom]	Rejected: We think it is clear what is meant.
TS-74	TS	4	30	4	30	This is confusing due to the overlapping and different time periods, but also because the numbers are not presented the same way. The early industrial versus model reference period trend is justified, whilst the other time periods are just given and do not have the error analysis. Suggest adding a sentence on the reasons trends are different depending on the way they are calculated. [Government of United Kingdom of Great Britain & Northern Ireland]	Rejected: The numbers are reported in this way to support other sections of the report.
TS-75	TS	4	30	4	30	"These are not :"observed" or :"global mean": temperatures but a series of multiple averages of a varying number of means of maximum and minimum tempeture from a variety of weather staions and ship measurements with huge inaccuracues [Vincent Gray, New Zealand]	Rejected: The construction of global mean estimates has been heavily studied by multiple groups and the numbers are found to be robust and meaningful.
TS-76	TS	4	30	4	30	It seems strange to me that Figure 1 does not include a combined land/SST temperature plot - wouldnt it be better to replace the marine air T plot by the combined plot? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	The idea here was to show that multiple independent data sets give the same answer: It is warming.
TS-77	TS	4	30	4	31	I cannot find how the 0.8 ºC rise has been estimated in the text of Chapter 2, it is not in section 2.4.3 [Geert Jan van Oldenborgh, Netherlands]	It is from alinear fit to the combined surface temperature record. This number now appears in Chapter 2.
TS-78	TS	4	30	4	33	This description inappropriately erases the ~ 60 year period sinusoidal component for example discussed by A. Mazzarella, N. Scafetta DOI 10.1007/s00704-011-0499-4, by C. Loehle and N. Scafetta (2011) and other papers cited above. [François Gervais, France]	Rejected.
TS-79	TS	4	30	4	33	"The global combined land and ocean temperature data show an increase of about 0.8°C over the period 1901–2010 and about 0.5°C over the period 1979–2010. The warming from 1886–1905 (early-industrial) to 1986–2005 (reference period for the modeling chapters and the Atlas in Annex 1) is 0.66°C ± 0.06°C (5–95% confidence interval)." Please add this para to the SPM, as it relates to the information given in AR4 about the anthropogenic T-increase of ~0.8 C. In addition, it gives absolute T-increase information, not warming rates. Both is important information for policy makers and communication issues. [Government of Germany]	Done
TS-80	TS	4	30	4	33	Section Changes in sea level: Why are paleoclimate records discussed first in this section, but not in other sections e.g. Changes in temperature? Perhaps there should be a consistent format between all sections on observations? [Government of United Kingdom of Great Britain & Northern Ireland]	Noted
TS-81	TS	4	31	4	31	How does the period 1886-1905 (early-industrial) compare to "preindustrial" as used in the UNFCCC (as stated e.g. in the Cancun agreements: "with a view to reducing global greenhouse gas emissions so as to hold the increase in global average temperature below 2°C above pre-industrial levels")? Would it be possible to make a reference to "pre-industrial levels" here? [Government of Germany]	In the AR5 we have chose to use 1750 instead of preindustrial. The instrumental record does not go back that far, however.
TS-82	TS	4	31	4	33	The warming from 1886–1905 (early-industrial) to 1986–2005 (reference period for the modeling chapters and the Atlas in Annex 1) is 0.66°C ± 0.06°C (5–95% confidence interval).' This does not give the reader important information after the first sentence of this paragraph. Especially snce the years (dates) do not match with the ones in the sentence before, there is no possibility of comparing something. Suggest to delete this sentence. [Line van Kesteren, the Netherlands]	See response above to TS-75
TS-83	TS	4	31	4	33	The warming from 1886–1905 (early-industrial) to 1986–2005 (reference period for the modeling chapters and the Atlas in Annex 1) is 0.66°C ± 0.06°C (5–95% confidence interval).' This does not give the reader important information after the first sentence of this paragraph. Especially snce the years (dates) do not match with the ones in the sentence before, there is no possibility of comparing something. Suggest to delete this sentence. [Line van Kesteren, the Netherlands]	See response above to TS-75
TS-84	TS	4	31			A more precise statement would be : 0.5°C over the period 1978-1998 and a plateau afterwards. [François Gervais, France]	The "hiatus" has been included in the discussion

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TS-85	TS	4	35	4	36	What does "CE" mean? [Government of United Kingdom of Great Britain & Northern Ireland]	Common Era, same as AD: this has been removed.
TS-86	TS	4	35	4	36	It is likely that effects of urban heat-islands and land use change have not raised the centennial global land surface air temperature trends by more than 10% of the observed trend.' Add after this sentence: Regionally, in rapidly developing regions, it is likley that these effects have not raised the centennial land surface air temperature trends by more that 25 %. [Line van Kesteren, the Netherlands]	The fact that there are exceptions has been noted
TS-87	TS	4	35	4	38	It might be noted that at least some of the urban heat island effect is a result of the heat of combustion of the fossil fuels. While those of us involved in climate studies found the significance of this term small when models had resolutions of 5 degrees, with finer resolutions and larger megalopolises, the flux additions can be 10-20 W/m2 over reasonably large areas. So, while not cased by trapping the IR, fossil fuels are contributing to some of the effect. [Michael MacCracken, United States of America]	Noted
TS-88	TS	4	35	5	53	These assessments are just the personal opinions of your paid investigators who have a conflict of interest in providing opions acceptable to the authorities [Vincent Gray, New Zealand]	Rejected
TS-89	TS	4	35			Replace 'the centennial global land surface air temperature trends by more than 10% of the observed trend' with 'the observed centennial global land surface air temperature trend by more than 10%'. This is describing the effect of urban heat islands on the observational estimate of the global mean temperature trend, not the effect on the global mean temperature trend itself (this effect would be much smaller). Note that the effect of urbanisation on global mean radiative forcing is discussed in chapter 8, so a reader might think that this is a statement about the effect of urbanisation on the actual global mean temperature. This is an important disctinction. [Nathan Gillett, Canada]	Noted
TS-90	TS	4	36	4	38	“This is an average …2.4.3}.”change to： “Recent studies based on homogenized observations and high-resolution regional modeling suggested that previous works based on either unadjusted observations or rough modeling could have overestimated the effect of urbanization in general.” [Qingxiang Li, China]	Rejected
TS-91	TS	4	36	4	38	This is an average value; in some regions that have rapidly developed urban heat island and land use change impacts on regional trends have been substantially larger.' Substantially larger is vague, suggest to use the number 25 % from the underlying report. [Line van Kesteren, the Netherlands]	Rejected
TS-92	TS	4	37			Insert 'observed' before 'regional trends'. This is a statement about the observational estimates of trends, not the true regional trends, which have presumably been less affected by urbanisation. [Nathan Gillett, Canada]	Noted
TS-93	TS	4	38	4	38	Please check whether the reference here should be to section 2.4.1.3, not 2.4.3. We could not find information about heat island effects and land use changes in 2.4.3, but did find the information in section 2.4.1.3. [Government of Canada]	Done
TS-94	TS	4	40			The comparison of the recent warming with previous warm periods is an important point. Section TS.2.2 presents very well the historical context. It insists on the amplitude of the warming, but not enough on the spatial heterogenity of the previous warm periods (MCA, Roman Period, mid-Holocene, LIG). Locally the recent warming is not necessary exceptional, but the previous warm periods have not a global characteristics [Government of France]	THis is better reflected in the final text
TS-95	TS	4	42	4	42	CE? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Removed
TS-96	TS	4	42	4	43	Why the climb-down from AR4 assertion about last 50 years being unprecedented in last 1300? Sound scientific basis for this? [Government of United Kingdom of Great Britain & Northern Ireland]	The current assessment has two levels of confidence for past 800 and past 1400 years.
TS-97	TS	4	42	4	43	This sentence: "It is very likely that in the Northern Hemisphere, the 1981–2010 CE period was the warmest of the last 800 years, and there is medium confidence that is was the warmest in the last 1300 years {5.3.5}." is generally contradicted by this sentence from Chapter 5, page 23, lines 19-21: "These few indicate that the last decades are relatively warm for the SH and at global scales (Table 19 5.1), but there is only limited evidence and therefore low confidence that the recent warming has exceeded the range of reconstructed temperatures over the last 4 centuries." These sentences appear to have been composed by different teams, which, of course, they were. The literature generally supports the low confidence of the latter sentence, and it is to be hoped that this sentence will be revised or dropped so as not to eliminate or compromise the Medieval	Noted

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						Warming Period. [Forrest Mims, United States of America]	
TS-98	TS	4	42			explain abbreviation "CE" [Barbara Früh, Germany]	removed
TS-99	TS	4	42			Is "mitigated" the best word - is it more usual to say "corrected"? [Government of United Kingdom of Great Britain & Northern Ireland]	changed
TS-100	TS	4	42			It written "1981-2010 CE period". For many people CE is known, but there are people do not untersand it. It is necesary to mention what CE means [José Daniel Pabon-Caicedo, Colombia]	removed
TS-101	TS	4	42			What is CE? [Jan Sedlacek, Switzerland]	removed
TS-102	TS	4	43	4	45	The words "and regions" that end the following sentence are misleading in that (1) many papers describe widespread warming during the Medieval Period and (2) the current warming is an average of globally distributed regions, some of which are warming some of which are stable and some of which are cooling. For example, the published records of the US National Weather Service (NOAA) clearly show the high stability of temperatures recorded across the State of Texas from 1885 to 2011. (A slight cooling trend accompanied by a decline in column water vapor of 2-3 mm/decade appears in my measurements from Central Texas since 1990.) Here is the misleading sentence: "In contrast to the late 20th century there is high confidence that the Medieval Climate Anomaly was not characterized by a pattern of higher temperatures that were consistent across seasons and regions." [Forrest Mims, United States of America]	Noted
TS-103	TS	4	43			The phrasing "warmest/highest/etc. in X years", leaves open the possibility that we know that X+1 years ago, the temperature was warmer, as opposed to the other posisble meaning, viz that the records only go back that far. That is, CO2, "highest in 800,000 years" because that is the length of the ice core, but we're probably pretty confident that it is actually the highest in a few million years. I think it would be valuable to give some idea of that latter timescale... eg, "medium confidence that it is the warmest in 1300 years, but could possibly be the warmest in 4000 years. [Government of United States of America]	In this section we are limited to what has actually be observed or otherwise inferred from natural recording systems.
TS-104	TS	4	43			Substitute "is" by "it". [Klaus Radunsky, Austria]	Noted
TS-105	TS	4	43			that is was' should be 'that it was' [Jan Sedlacek, Switzerland]	Noted
TS-106	TS	4	45		47	This is a rather empty statement. Delete from here, and defer attribution of LIA and MWP changes to TS.3, where it is also discussed. [Nathan Gillett, Canada]	Done
TS-107	TS	4	47	4	47	"very likely" in italics, as well as other uncertainty qualifiers in this para. [Government of Germany]	Noted
TS-108	TS	4	51	4	52	There are no useful data from satellite sensors "since the mid 20th century", only since the last quarter of the century. [Dian Seidel, United States of America]	Noted
TS-109	TS	4	52	4	52	Please use agreed uncertainty language. "at most medium confidence" is not a qualifier specified in the AR5 uncertainty guidance notes. [Government of Germany]	Done
TS-110	TS	4				Section TS.2.2.2. The likelihood statements on the sign of tropospheric and stratospheric trends, and the confidence statements in their rates are not consistent in my view. If there is 'low confidence' in the rates of change, then how can we be 'virtually certain' that they are of one sign or another? I would suggest replacing 'at most medium confidence' with 'considerably uncertainty' and 'only low confidence' with 'considerable uncertainty'. Alternatively (less preferable) replace 'rate of change' with 'magnitude of the warming rate' and 'cooling rate' with 'magnitude of the cooling rate' (since this at least indicates that we do not have low/medium confidence in the sign of the rate). In the chapter, quote a broader range on the rates of temperature change, so that they can be associated with a higher confidence/likelihood level. [Nathan Gillett, Canada]	Rejected: The authors can be relatively certain that warming is occurring and yet be quite uncertain as to the rate of warming if the lowest rate is well above zero and the highest rate is much above zero.
TS-111	TS	4				Figure TS.1 Caption notes that analyses are independent. This is not always the case. For example there are two marine temperature curves by the same group (Ishii et al), but with differing interpolations and 100 versions of the HadSST3 data set. A more careful wording might be used. Chapter 2 has a more appropriate caption in the FAQ. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted
TS-112	TS	4				Figure TS.1 This figure differs from the 'multiple indicators' plot in the SPM (Figure SPM.1). It would make	Noted

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						sense to ensure that the two are consistent. The virtue of the SPM version is that it draws directly on the time series used in the chapters. However, it is less comprehensive which would seem to be a weakness. I would suggest a combined approach such that material from the chapters (as in SPM.1) is combined in a more comprehensive plot (as in TS.1) [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	
TS-113	TS	5	1	5	17	Key uncertainties about sampling below an ocean depth of 700 m should be mentioned here. The layer between 700 and 2000 m is not clearly represented here while it is a crucial part for dealing with heat content. Therefore we suggest to add a sentence indicating the uncertainties between 700 - 2000 m. See for this: TS p 63, line 20 - 24: 'Below an ocean depth of 700 m the sparse sampling in space and time prevents reliable estimates of temperature and ocean heat content change, since the vertical gradients (especially between 700 m and 2000 m depth) are still sufficiently large for transient variations (ocean eddies, internal waves, and internal tides) to alias estimates from sparse data sets. Towards the bottom, vertical gradients are weaker, and estimates are more reliable. {3.2.4}' [Line van Kesteren, the Netherlands]	Noted
TS-114	TS	5	1	5	17	Key uncertainties about sampling below an ocean depth of 700 m should be mentioned here. See for this: TS p 63, line 20 - 24: 'Below an ocean depth of 700 m the sparse sampling in space and time prevents reliable estimates of temperature and ocean heat content change, since the vertical gradients (especially between 700 m and 2000 m depth) are still sufficiently large for transient variations (ocean eddies, internal waves, and internal tides) to alias estimates from sparse data sets. Towards the bottom, vertical gradients are weaker, and estimates are more reliable. {3.2.4}' [Line van Kesteren, the Netherlands]	Noted
TS-115	TS	5	5	5	6	Where's any information about how the ocean warms to 700m? It's certainly not in the various chapters. By my understanding of physics this can only occur by warming beneath, which means subsea volcanoes. [John McLean, Australia]	It's in Chapter 3 and the box on heat content
TS-116	TS	5	5	5	6	On what basis do you claim that the trend of 0.015C/decade is accurate to 3 decimal places? [John McLean, Australia]	See chapter 2
TS-117	TS	5	5	5	6	I think you are in fantasy world if you imagine that in 1971 temperature sampling to 700m was widespread enough to claim that it was a global average. [John McLean, Australia]	see chapter 3
TS-118	TS	5	7	5	7	What does a 4% increase in thermal stratification mean? Is it simply the % increase in T difference or is a contribution to density anomaly calculated? [Government of United Kingdom of Great Britain & Northern Ireland]	See chapter 3
TS-119	TS	5	9	5	9	It is probably better to use a different word than the statistical term "spurious" because of its everyday meaning. What about coincidental? Alternatively, use "statistically spurious". [Dora Marinova, Australia]	Done
TS-120	TS	5	9			It seems strange this sentence has no statement of likelihood or confidence. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted
TS-121	TS	5	12	5	12	Please explain how the heating occurred. There's no mention of any mechanism in any of the chapters and except for warming from beneath, I see no way that such warming could have occurred. [John McLean, Australia]	Heat is mixed or transported down from above.
TS-122	TS	5	12	5	13	As per comments on similar lines in the SPM, it is not clear why a 'likely" statement is given for ocean warming below 3000m depth, but no rate of warming, and then a rate of warming is given for depths below 4000m but no likelihood statement. How are readers to interpret this? On page 3-10 (section 3.2.5 (not referenced for this paragraph in current draft) it is explained better and it is made clear why these are presented as separate results. More clarity here is required and section 3.2.5 should be referenced. [Government of Canada]	This text has been changed
TS-123	TS	5	12	5	13	"first-difference change" - undefined jargon [Government of United Kingdom of Great Britain & Northern Ireland]	Changed
TS-124	TS	5	12	5	13	I doubt that monitoring of temperatures below 4000m was widspread in the early 1990s. You'll need to defend you statement and state explicitly when in the 1990s this "global" data was available. (I also notice that the sentence on lines 16 and 17 of this chapter contradict your claim that the data is "global".) [John McLean, Australia]	Text has been changed

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TS-125	TS	5	12	5	14	"early 1990s" would make the point more strongly than "1990s" [David Parker, United Kingdom of Great Britain & Northern Ireland]	Changed
TS-126	TS	5	12			“global” eventually seems to mean “averaged over the globe” here – contrary to line 2 where it fairly clearly means “varying across the globe”. Clarify [William Ingram, United Kingdom]	Noted
TS-127	TS	5	12			3000-m' should be '3000 m' [Jan Sedlacek, Switzerland]	Noted
TS-128	TS	5	14	5	15	Has the Souther Ocean warmed at the same rate at all depths? [Luisa Cristini, United States]	Text has been clarified
TS-129	TS	5	15	5	16	The following wording is suggested: .. below 1000 m combine to a heating rate of 48 …. [Klaus Radunsky, Austria]	Text has been modifed
TS-130	TS	5	16			Giving a best estimate to 2 significant figures, & then the range in brackets as if unimportant, when the range doesn't match to 1 is absurd! Quote the range first & the best estimate afterwards in brackets if at all – here where it's the middle it could be omitted [William Ingram, United Kingdom]	Done throughout
TS-131	TS	5	20			will the audience for the technical summary be familiar with the TW and “total energy exchange inventory” [David Sauchyn, Canada]	changed
TS-132	TS	5	22	5	22	In describing the radiative imbalance, the text, as written, suggests the imbalance is in solar radiation only. It's not clear that the energy exiting the top of the atmosphere is long wave radiation. This may seem minor, but there is a prevalent misperception that the greenhouse effect is all to do with solar radiation so the IPCC should communicate clearly about these basics. [Government of Canada]	Text has been changed
TS-133	TS	5	22	5	22	"more ... entering than exiting" - I know what the sentence means to say, but as written it is ambiguous. There must always be more energy from the sun entering than exiting, unless the albedo is greater than 1 :-) Perhaps rewrite "more absorbed shortwave energy from the Sun than there is longwave energy emitted to space by the Earth and its atmosphere"? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Rewritten
TS-134	TS	5	22	5	23	You'll have to do better than just make a statement. Where's your evidence? [John McLean, Australia]	Box 3.1
TS-135	TS	5	22	5	33	this paragraph needs revision, it is difficult to understand the sequence of absolute values. In my opinion it would be better to give reltive values for the shorter time period and for the ocean only values. [Barbara Früh, Germany]	Revised
TS-136	TS	5	22	5	33	The use of ZJ here seems unhelpful - why not use W m-2 throughout here, for consistency with how forcings are reported? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Suggestion not adopted Wm-2 estimates are given in the next section.
TS-137	TS	5	24	5	24	What does first-difference change mean? It has a very specific statistical meaning but its actual usage here needs to be made clear. (Here and in Box 3.1). Is this the cumulative sum of the annual first differences or the first difference between 1971 and 2010. [Government of United Kingdom of Great Britain & Northern Ireland]	Changed
TS-138	TS	5	24	5	24	The term "first-difference change" is undefined and probably not necessary in the TS. [Dian Seidel, United States of America]	Changed
TS-139	TS	5	24	5	25	A 40-year span is no help whatsoever. Break it down. Show a graph of annual values. Or would it be a problem for you to show that the increase followwed shortly after the Pacific Climate Shift of 1976? [John McLean, Australia]	Rejected
TS-140	TS	5	24		26	Again, absurd to bracket the range as if only the best guess really matters [William Ingram, United Kingdom]	Estimates are consistently given as best estimate and a range.
TS-141	TS	5	26	5	26	27TW should be more like 257 TW or 270TW. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Changed
TS-142	TS	5	26			Why not present the global energy imbalance in terms of W/m2, like ocean air-sea flux? [Government of United States of America]	A W/m2 estimate is given
TS-143	TS	5	26			Is 27 TW a typo? Should it be 270 TW? That would be more consistent with the 1971-2010 rate, and also the	Changed

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						ocean rates. [Government of United States of America]	
TS-144	TS	5	26			why not say that the flux has increased? [Government of United Kingdom of Great Britain & Northern Ireland]	Noted
TS-145	TS	5	28	5	29	Sentence is confusing. Incorrect hyphens? 'warming of the atmosphere' does not belong under 'melting ice' [Rolf Müller, Germany]	Changed
TS-146	TS	5	29	5	31	Don't understand this sentence. If true how were the figures in the previous sentence derived? [Government of United Kingdom of Great Britain & Northern Ireland]	Changed
TS-147	TS	5	29	5	31	I would leave out the references to W/m**2 through the sea surface. The expert can make the conversion, for everyone else it is unimportant in a summary chapter. [David Webb, United Kingdom]	Conflicts with other recommndations.
TS-148	TS	5	29		30	I can't believe we know these numbers to 3 significant figures! [William Ingram, United Kingdom]	Uncertainties are given. Rules for rounding were not established.
TS-149	TS	5	30	5	31	For the ocean air-sea flux, is that W/m2 of ocean surface area, or W/m2 of global surface area? Many readers will want to compare this number to the estimate of net radiative forcing… [Government of United States of America]	Global
TS-150	TS	5	31	5	31	It is suggested to add the following sentence before the word “Uncertainties”: It is likely that upper ocean heat content shows little increase or even negative trend since 2000, which is partly because of the El Nino variability on decadal time scales, and partly due to a decrease in the strength of Atlantic meridional overturning circulation.” [Government of China]	Suggestion not adopted
TS-151	TS	5	31	5	33	is "offset" the right word or would it be better to say "reversed" [Government of United Kingdom of Great Britain & Northern Ireland]	The trend has not reversed, just declined, so offset seemed better.
TS-152	TS	5	32			Use of language such as "very small" without noting what it is being compared to is not fully informative. Specifically, 0.7 W/m2 may be "very small" compared to local variability, but it is likely "pretty large" in comparison to global fluxes in the last 1000 years. [Government of United States of America]	Language changed
TS-153	TS	5	37			what types of data sets? SSTs? [David Sauchyn, Canada]	The sentence is about atmospheric circulation
TS-154	TS	5	38		41	I think this statement is too strong. It is true that the NAO has been in a negative phase in recent winters. But there is still a large positive trend over the past 50 years. So I disagree that 'confidence is high' that the trend has been 'largely offset by recent changes'. Also I disagree with the format of the sentence since by using the phrasing 'several trends' and then giving examples the message is conveyed that all circulation trends have largely reversed. Of course this is not true for the SAM which is mentioned later. I suggest 'Trends in the North Atlantic Oscillation index and the Pacific Walker Circulation over the past half-century reported in the AR4 have been considerably weakened by more recent changes (high confidence).' [Nathan Gillett, Canada]	Noted
TS-155	TS	5	39	5	41	This sentence is weakly worded ('several' and 'largely') and open to misinterpretation. 'several...(eg..)...trends have been largely offset...' Are the examples the only cases or are there others too? why just these examples? What does 'largely' mean? [Government of United Kingdom of Great Britain & Northern Ireland]	Noted
TS-156	TS	5	39			I found ”from the 1950s or earlier to the 1990s” hard going [William Ingram, United Kingdom]	Noted
TS-157	TS	5	40	5	41	You're putting the cart before the horse (ie. confusing cause and effect). Since 1976 the ENSO has been dominated by conditions on the El Nino side of absolutely neutral (SOI=zero). This means a decrease in Walker Circulation, an increase in Hadley Cell Circulation and it means a warmer world. There's no dispute about the consequences. (refer Trenberth, K.E. (1990), Guilderson, T.P. and Schrag, D.P. (2006), Trenberth, K.E. (1996), Trenberth K.E. and Carron, J.M. (2000) and Trenberth et al (2002) - Evolution of El Nino–Southern Oscillation and global atmospheric surface temperatures.) Ergo there's good reason to blame the shift in ENSO for the changes over time. [John McLean, Australia]	Noted
TS-158	TS	5	41			replace "to" with "with" [Government of United Kingdom of Great Britain & Northern Ireland]	Noted
TS-159	TS	5	43	5	44	Phrase 'in a zonal mean sense' should be re-phrased using plain English [Government of Australia]	changed

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TS-160	TS	5	43	5	45	What you describe as a widening of the tropical belt is consistent with the shift in ENSO. [John McLean, Australia]	We disagree
TS-161	TS	5	44		45	I think the evidence for a poleward shift of the SH storm track is weak or non-existent. Most observational studies have focused on the SAM, which is an SLP index and doesn't directly measure the latitude of the storm track/jet. Swart and Fyfe (2012) examined the position of the maximum in surface wind stress in three reanalyses and found no significant trends in the annual mean. Also, I don't know what the 'contraction of the polar vortex' refers to here. The polar vortex is a feature of the stratospheric circulation. Section 2.7.7 discussed a deepening of the SH polar vortex, but not a contraction of it. There is no polar vortex in the troposphere. Swart, N. C., & Fyfe, J. C. (2012). Observed and simulated changes in the Southern Hemisphere surface westerly wind-stress. Geophysical Research Letters, 39(16), L16711. [Nathan Gillett, Canada]	Noted
TS-162	TS	5	44			I guess “tropical belt” is intended to be easy reading for policy-makers, but I don't think they'll have a clue. “Hadley Cell, the main tropical circulation, with air rising in limited areas of deep convection & descending more generally”? [William Ingram, United Kingdom]	Noted
TS-163	TS	5	47	5	47	"very likely" in italic [Luisa Cristini, United States]	changed
TS-164	TS	5	47			Italicize “very likely” [William Ingram, United Kingdom]	changed
TS-165	TS	5	48	5	48	"as likely as not" in italic [Luisa Cristini, United States]	changed
TS-166	TS	5	49			Omit 1st “the” [William Ingram, United Kingdom]	changed
TS-167	TS	5	50	5	50	Include North Atlantic Oscillation in full [Luisa Cristini, United States]	Noted
TS-168	TS	5	50	5	51	As with comment on SPM p4, 51-55: 'There is no evidence for decadal trends in the transports of the Atlantic Meridional Overturning Circulation (AMOC), and the Antarctic Circumpolar Current' - Is this because reliable data have shown this to be the case, or because of a paucity of obs data? This needs to be made clear. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted
TS-169	TS	5	50	5	53	There is no reference to the time over which this is assessed. Is it that there is no evidence within a set of collected data that should be able to find the trend, or is it that there is no observational evidence. As these measurements are of often only 10-15 years long, am not sure they could find a long-term trend even if there were one. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted
TS-170	TS	5	52			Capitalize “Circumpolar” [William Ingram, United Kingdom]	done
TS-171	TS	5	55	6	5	'likely ' is not italicised twice here... is this deliberate? [Government of United Kingdom of Great Britain & Northern Ireland]	changed
TS-172	TS	5	55			Italicize “likely” [William Ingram, United Kingdom]	changed
TS-173	TS	5	56	6	57	There is no statement in 2.5.2 that year-to-year variability has incfeased. [Geert Jan van Oldenborgh, Netherlands]	Statement revised
TS-174	TS	5	56			This paragraph is muddled. It leaves the reader uncertain whether humidity should be expected to increase with temperature. [Government of United Kingdom of Great Britain & Northern Ireland]	This paragraph is about the ocean.
TS-175	TS	5				Section TS.2.3: This section needs a good edit. i.e. 'from 1971-2010 - an estimated first-difference change ..." Why isn't this "Between 1971 and 2010 the heat gain was 273 ZJ. This corresponds to an average incoming heat flux of 213 TW. Between 1993 and 210 the gain was 163 ZJ corresponding to a heat flux of 270 [not 27!] TW." [David Webb, United Kingdom]	Changed
TS-176	TS	6	5			“ventilation” needs explaining for policymakers [William Ingram, United Kingdom]	Word ventilation removed
TS-177	TS	6	9	6	9	There is a real ambiguity here (particularly given the heading of TS.2.5.2) whether TS.2.5.1 is talking about land only. The first two paragraphs of this section presumably are, but this is only implicit in the second paragraph and perhaps the reader needs to be reminded that the emphasis is on land because of the quality of the observing systems, rather than there being separate conclusions for ocean precipitation? [Keith Shine,	Land is now mentioned three times in this paragraph

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						United Kingdom of Great Britain and Northern Ireland]	
TS-178	TS	6	11	6	13	These lines seem to be saying that "There is low confidence prior to 1950 and medium confidence afterwards that there has been little (no?) change in land based precipitation since 1900". Because of section TS.2.5.3 on runoff, the statement needs to be much clearer. [David Webb, United Kingdom]	That is exactly correct
TS-179	TS	6	11	6	15	While people interested in the issue know that "global warming" should equate to temperatures going up, how should the reader know what to expect about change in precipitation. Even for scientists, the expectation is that it goes up some places and down in others with no real sense of which is going to win. The discussion here makes it seem as if there should be an expectation that the total precipitation averaged over land should go a particular direction, and then makes things seem uncertain by indicating that the results are mixed. It seems to me that there needs to be some sort of clarification at the start, indicating that the climate system has created areas that are wet like the Amazon and dry like the Sahara and then indicate how the pattern is changing (presumably wet areas getting wetter, dry areas drier, with some shift in boundaries) rather than focusing on the average as the difference between two large numbers and meaning little. [Michael MacCracken, United States of America]	At this point we are still trying to maintain a distinction between what has been directly observed and what one might expect. Indeed the expectation about precipitation is complex.
TS-180	TS	6	11	6	15	Precipitation data indicate a smaller increase in the global mean since 1901 than suggested in previous assessments, rather than no increase. This comment follows from comments I have made on Chapter 2. [David Parker, United Kingdom of Great Britain & Northern Ireland]	The current assessment is that the increase is sufficiently small as to provide low confidence in any trend.
TS-181	TS	6	11	6	35	It is better if the periods for temperature and precipitation are the same. [Qingxiang Li, China]	Yes it would be but the time periods over which reliable estimates of change can be made are very differnt.
TS-182	TS	6	12	6	12	Insert 'data' between 'area' and 'is' to make sentence clearer [Government of Australia]	Noted
TS-183	TS	6	13		15	Why the difference? [William Ingram, United Kingdom]	Unclear question.
TS-184	TS	6	13			Insert 'global mean' before 'land-based precipitation'. There have been changes in particular regions as the section goes on to describe. [Nathan Gillett, Canada]	done
TS-185	TS	6	17	6	22	After the the first paragraph regarding this section (line 11 - 15 page 6 TS) it seems more logical to change the order of the sentences of the second paragraph and to change the first sentence about the Northern Hempispere. Now it feels a bit contradictory. Suggest change this paragraph into: Insufficient evidence exists to define a long-term temporal change of precipitation averaged across the mid-latitudes of the Southern Hemisphere. However confidence is low because of much uncertainty in the data records for the early 20th century, the mid-latitudes and higher latitudes of the Northern Hemisphere do show an overall increase in precipitation from 1900–2010. Precipitation in the tropics has likely increased over the last decade, reversing the drying trend that occurred from the mid-1970s to mid-1990s reported in AR4. [Line van Kesteren, the Netherlands]	Noted
TS-186	TS	6	17	6	22	After the the first paragraph regarding this section (line 11 - 15 page 6 TS) it seems more logical to change the order of the sentences of the second paragraph and to change the first sentence about the Northern Hempispere. Now it feels a bit contradictory. Suggest change this paragraph into: Insufficient evidence exists to define a long-term temporal change of precipitation averaged across the mid-latitudes of the Southern Hemisphere. However confidence is low because of much uncertainty in the data records for the early 20th century, the mid-latitudes and higher latitudes of the Northern Hemisphere do show an overall increase in precipitation from 1900–2010. Precipitation in the tropics has likely increased over the last decade, reversing the drying trend that occurred from the mid-1970s to mid-1990s reported in AR4. [Line van Kesteren, the Netherlands]	Noted
TS-187	TS	6	24	6	24	"absolute moistening"="absolute humidity"? [Government of Germany]	yes. Text has been changed to use different language.
TS-188	TS	6	24	6	25	As reported in AR4, there is very high confidence that absolute moistening of the atmosphere near the surface has been widespread across the globe since the 1970s. This sentence is unclear to me, maybe Add increased/decreased? : As reported in AR4, there is very high confidence that absolute moistening	Moistening seems clear. Absolute means g/kg.

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						of the atmosphere near the surface has been increased/decreased widespread across the globe since the 1970s [Line van Kesteren, the Netherlands]	
TS-189	TS	6	24	6	25	As reported in AR4, there is very high confidence that absolute moistening of the atmosphere near the surface has been widespread across the globe since the 1970s. This sentence is unclear to me, maybe Add increased/decreased? : As reported in AR4, there is very high confidence that absolute moistening of the atmosphere near the surface has been increased/decreased widespread across the globe since the 1970s [Line van Kesteren, the Netherlands]	Moistening seems clear. Absolute means g/kg.
TS-190	TS	6	24	6	31	Para is difficult to understand, it is not clear which statement refers to which time period. In addition, reordering would be of advantage (e.g. group statements for each parameter) [Government of Germany]	Has be rewritten
TS-191	TS	6	24	6	31	replace 2"of" with "in" [Government of United Kingdom of Great Britain & Northern Ireland]	rewritten
TS-192	TS	6	24	6	31	I think the phrasing here is quite confusing, with quite a number of terms being used. In saying the near surface troposphere has moistened, that is the same as saying, I think as the specific humidity has increased (I actually prefer the term absolute humiditiy). I think the discussion on change in relative humidity should be separate, and not between the two points being made. Have a paragraph on absolute humidity and note that it is an important greenhouse gas and its rise confirms the occurrence of water vapor feedback, etc. Then, if there is need for discussion of relative humidity, have a paragraph on that, indicating, I assume that land areas are warming faster than the ocean, that Clasius-Clapeyron will lead to lowered RH, and that lower RH means will get greater evaporation over land and give perhaps other implications. But do so separately from discussion of absolute humidity--just too confusing to combine them. [Michael MacCracken, United States of America]	Rewrittent to be more precise.
TS-193	TS	6	24			I don't think policy-makers will know what “absolute” is intended to mean [William Ingram, United Kingdom]	We now use specific humidity. It is necessary to distinguish specific and relative.
TS-194	TS	6	24			What is the basis for the very high confidence? In my reconstruction of the ERA-interim near-surface specific humidity (variable q at model level 60) there is no trend for the global mean nor for the land surface, in contrast with the (merged ERA-40/interim?) curve in Fig.2.30b for land. A curve for globally averaged specific humisity is not shown in Chapter 2 to base this statemnt on. [Geert Jan van Oldenborgh, Netherlands]	See Chapter 2 where a reference is given in which the direct observatoins are used rather than reanalysis.
TS-195	TS	6	25	6	25	Please specify which time is meant by "recently". [Government of Germany]	Recently removed.
TS-196	TS	6	25	6	25	change 'this' to 'this moistening' [Rolf Müller, Germany]	text revised
TS-197	TS	6	25	6	25	"abated" is ambiguous, I feel. Is it meant that the trend has reversed, or the absolute amount has stabilised? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Trend has flattened. Abated means to become smaller.
TS-198	TS	6	25	6	26	However, during recent years this has abated over land, coincident with greater warming over land relative to the oceans {2.5.5}. Why does greater warming over land relative to oceans cause a decrease in the positive trend of absolute moistening of the atmosphere near the surface over land? Does this have to do with decreasing soil moisture? [Line van Kesteren, the Netherlands]	It is being pointed out that more warming over land than ocean might logically reduce relative humidity over land if the source of moisture over the land is transport of absolute humidty from the oceans. This attribution is speculative, though.
TS-199	TS	6	26			“greater warming over land relative to the oceans” versus “ocean warming dominates the total energy exchange inventory” (page 5, lines 26-27) could be interpreted as conflicting statements [David Sauchyn, Canada]	The landand atmosphere have very little capacity to store heat compared to the ocean.
TS-200	TS	6	27	6	27	recently' is a vague time period subject to different interpretations, please provide more specific time period [Government of Australia]	Noted. Recent is used relative to 1970, so it means during the past decade or so prior to 2012.
TS-201	TS	6	28	6	29	"Radiosonde, GPS and satellite observations indicate increases in tropospheric water vapour at continental scales,…": These assertions are unreferenced here and are contradicted by various studies that show a flattening or even reduction of total column water vapor, which is nearly all tropospheric (the stratospheric water vapor is typical only a mm or so.). This is highly significant and should be noted here. The citation to 2.5.5. will be addressed below in this review. [Forrest Mims, United States of America]	Noted. Assessment here is backed up by references to refereed literature in Chapter 2.

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TS-202	TS	6	29	6	31	"It is very likely that tropospheric specific humidity has increased since the 1970s.": This assertion is contradicted by the NVAP-M study, which is addressed at 2.5.5-6 below in this review. [Forrest Mims, United States of America]	Noted. Assessment here is backed up by references to refereed literature in Chapter 2.
TS-203	TS	6	29			"consistent with" makes no sense, given that the expectation of little change in the distribution of RH has not been mentioned. I would start the paragraph with it [William Ingram, United Kingdom]	Noted: It is assumed that people generally understand that warmer air usually has more water in it.
TS-204	TS	6	30			Should "specific humidity" be explained? [William Ingram, United Kingdom]	Glossary
TS-205	TS	6	30			It is suggested to either explain the term "specific humidity", e.g. in a footnote or include the definition in the glossary. [Klaus Radunsky, Austria]	New Glossary entry introduced for "Specific humidity"
TS-206	TS	6	33	6	35	Is there no information about trends in any type of clouds in any region? It would help to say what you can, if anything, but then, it would also be valuable to indicate that there are no signficiant changes, suggesting that there is an upper limit to what cloud feedback is likely to me--and so this will mean that climate sensitivity is not going to go away or be overly large. [Michael MacCracken, United States of America]	Cloud observations trends are just uncertain at the present time.
TS-207	TS	6	33			This sentence is very difficult to follow. Don't know what "respectively" refers to. [Government of United Kingdom of Great Britain & Northern Ireland]	No respectively in this sentence.
TS-208	TS	6	34			What does "global-scale" mean? As I said for SPM-4, line 2, when I asked the experienced colleagues who happened to be in the office with me, they had completely different ideas. Replace with whatever is meant [William Ingram, United Kingdom]	Noted
TS-209	TS	6	39	6	39	Please specify which trends have been observed for cloud cover. [Government of Germany]	The assessment is that these regional trends are not related to global warming.
TS-210	TS	6	39	6	39	Perhaps add to the end of this sentence '… and, to a much lesser extent, horizontal and vertical advection.' [Ian Simmonds, Australia]	Text revised
TS-211	TS	6	39	6	39	In the context here, the phrase "Surface salinities are governed by evaporation and precipitation" is misleading, the word governed implying that there are no other governing features. Two that have been left out are advection and diffusion - see next comment. [David Webb, United Kingdom]	Text revised
TS-212	TS	6	39	6	40	"The mean regional pattern of sea surface salinity has been enhanced". Is enhanced the best word? It is not clear what this means until you read the full sentence following the colon . [Ned Dwyer, Ireland]	Text revised
TS-213	TS	6	39			Too strong – advection generally plays a role, & sometimes the dominant one [William Ingram, United Kingdom]	Text revised
TS-214	TS	6	43	6	44	The statement "These patterns are very likely caused by an intensification of the water cycle ..." is too strong. The land rainfall/runoff observations do not support any significant intensification of the water cycle. So why should it be different at sea? Page 9, lines 16 and 17 states that "precipitation will have a smaller fractional change than the water content of air as the climate warms". So precipitation may never change much. Page 5, lines 6 to 8, states that the near surface stratification has increased by 4% since 1971. As in many areas the surface salinity is a result of a balance between vertical mixing and evaporation/precipitation, the reduction in vertical mixing resulting from the stronger stratification may, on the evidence given here, be enough to explain most, if not all, of the change in surface salinity. Page 9, lines 49 to 51 indicates that precipitation will only increase by 2% per K. Figure TS.1 shows that marine air temperatures have increased by about 0.5 K between 1950 and 2000. Thus precipitation will only have increased by about 1% - a quarter of the change in the stratification. Assuming that the evaporation change is also around 1%, I would expect the stratification change to be the dominant one.	Text revised to state only what has been observed.

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						The main justification for assuming that the observed salinity changes are a result of an increase in evaporation and precipitation comes from the paper by Durack et al (2012) which is primarily an analysis of model results (with an emphasis on predictions). My comments on Chapter 3 include a discussion of possible weaknesses of this paper Without a better argument and more independent support, all the report can really say is "These patterns may be caused by an intensification of the water cycle ...". [David Webb, United Kingdom]	
TS-215	TS	6	46	6	48	"The more recent studies based on expanded data sets and new analysis approaches, have substantially increased the level of confidence in the inferred change in the global water cycle {3.3.2, Figure 3.4, FAQ 3.3}" See previous comment. The new data and methods have increased confidence in the statement that "salinity has increased in evaporative regions and decreased in precipitation regions" but without a better argument you cannot say more. [David Webb, United Kingdom]	Text revised
TS-216	TS	6	47			Does “global” mean “global-mean” or “globally-varying”? [William Ingram, United Kingdom]	It means consistent changes in both ocean basins, or change on a global-scale. Global-average change in P-E is smaller.
TS-217	TS	6	48	6	50	uncertainties ….cannot yet be used…” doesn’t make sense. Should it be “Due to uncertainties in….they cannot….. [Ned Dwyer, Ireland]	Text changed
TS-218	TS	6	48		49	Well, of course not! [William Ingram, United Kingdom]	Noted
TS-219	TS	6	49			What is “regional or global distribution” meant to mean? How could things be distributed within or across regions without being distributed across the globe? [William Ingram, United Kingdom]	It is a question of whether regional changes contribute to a coherent global change, or are merely spatial variability.
TS-220	TS	6	52	6	52	Change title to "River Runoff" [Government of Germany]	This section was eliminated
TS-221	TS	6	52			Section TS.2.5.3 Runoff: I thought that this was a fantastic section. Short, surprising and completely counter-intuitive. [David Webb, United Kingdom]	This section was eliminated from the TS
TS-222	TS	6	54	6	57	In “year to year variability has increased.”the word "increased" is changed to "obvious". [Qingxiang Li, China]	Text revised
TS-223	TS	6	55			Again, what does “global” mean? Check the word throughout [William Ingram, United Kingdom]	Noted
TS-224	TS	6	56			There is no confidence or likelihood statement attached to the statement regarding the change in variability in runoff. I am not an expert on this, but generally it is harder to detect significant changes in variability than changes in the mean. Is this change in variability really significant, and not a measurement artefact or internal variability? [Nathan Gillett, Canada]	Text eliminated.
TS-225	TS	7	2	25		TS.2.5.4 Sea Ice- Add after "There is robust evidence for a decline in perennial and multiyear sea ice coverage and decreases in 11 ice thickness, and in ice volume": The mean annual cycle of arctic sea ice volume over the 1979 -2011 period ranges from 28,700 km3 in April to 12,300 km3 in September. Monthly averaged ice volume for September 2012 was 3,400 km3. This value is 72% lower than the mean over this period, 80% lower than the maximum in 1979. [CELSO COPSTEIN WALDEMAR, BRAZIL]	Text revised
TS-226	TS	7	4	7	5	Close open parenthesis [Luisa Cristini, United States]	Noted
TS-227	TS	7	4	7	6	Closing bracket missing. Also sentence is very long and difficult to read, suggest breaking sentence in two between 'AR4 has continued (Figure TS.1)' and This 'has been accompanied'. [Government of Australia]	Noted
TS-228	TS	7	4	7	16	This is a well done paragraph. However, just as some earlier points included a comparion with results/findings of earlier assessments, it seems to me that it needs to be said that the recent rate of loss is faster than has	This section focuses on what has been observed.

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						been projected to occur in models (in my personal view, perhaps be cause of not adequately treating the increased forcings due to reduced Arctic haze and the higher methane concentrationin high latitudes than the global average). This discrepancy between models and observed sea ice has grown, and trends based on observations (e.g., based on the changing volume) suggest a much earlier loss of all late-summer sea ice than do models. [Michael MacCracken, United States of America]	
TS-229	TS	7	4	7	25	What can we say about ice extent pre-1979 in the Arctic and Antarctic? 4.2.2.2 notes that pre-1979 data does exist for the Arctic. [Government of United States of America]	Data are limited prior to satellite observations.
TS-230	TS	7	4	7	25	Update to 2012! [David Parker, United Kingdom of Great Britain & Northern Ireland]	Done
TS-231	TS	7	5	7	5	Missing closing bracket “)” after 15% [Ned Dwyer, Ireland]	Noted
TS-232	TS	7	7	7	7	Will these be updated to 2012? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Done
TS-233	TS	7	7	7	8	Data from 2012 should be included in this estimate. [Thierry Fichefet, Belgium]	Done
TS-234	TS	7	8	7	8	"The largest changes" The decline in thickness correspodns to 14%/decade, squarely between the two numbers quoted [Geert Jan van Oldenborgh, Netherlands]	Noted
TS-235	TS	7	11	7	12	What do you mean by "overall" here? Be more specific. I guess that this number refers to the central Arctic basin? [Thierry Fichefet, Belgium]	Done
TS-236	TS	7	11			Over what region is the sea ice volume and mean thickness computed? [Ron Lindsay, United States of America]	Arctic Basin
TS-237	TS	7	18	7	19	Such an observation is contrary to the projections of all models retained in AR5 SOD which predict Antarctic warming as illustrated in maps of Annex I. This contradiction between models and measurements should be explicitly stated somewhere in both TS and SPM. [François Gervais, France]	Noted: Just observational facts here.
TS-238	TS	7	18	7	19	What is the difference between 'area' and 'extent' in the following: 'In Antarctica, there was a small but significant increase in total sea ice extent of 1.4% per decade between 1979 and 2011, and a greater increase in sea ice area, due to an increase in concentration.' This needs be made clear - assume it's because extent represents area with at least 15% cover.... [Government of United Kingdom of Great Britain & Northern Ireland]	wording has been changed
TS-239	TS	7	18	7	21	Data from 2012 should be included in this estimate. [Thierry Fichefet, Belgium]	Done
TS-240	TS	7	18	17	19	I'm not sure that this sentence is clear - also is this term 'concentration' appropriate in relation to ice cover - extent, mass? Needs clarification. [Jeffrey Obbard, Singapore]	Need to use terms as defined by satellite data sets.
TS-241	TS	7	18			Does “significant” mean “statistically significant”? If so, this needs to be made clear, & the significance level. If not, whatever it does mean needs to be made clear. I think this applies repeatedly [William Ingram, United Kingdom]	wording changed
TS-242	TS	7	18			Is the sea ice extent trend an annual mean or a winter maximum? [Ron Lindsay, United States of America]	In the modified version the annual rate is given.
TS-243	TS	7	19	7	19	Please mention the strong seasonality of the growth of Antarctic sea ice. [Geert Jan van Oldenborgh, Netherlands]	Noted: but a discussion of seasonality is too detailed here.
TS-244	TS	7	19	7	22	Suggest adding some or all of this text to lines 29-30 of SPM p5: 'Robust evidence shows strong regional differences within this total, with some regions increasing in extent/area and some decreasing. There are also contrasting regions around the Antarctic where, over the period of satellite observations, the ice-free season has lengthened, and others where it has shortened.' [Government of United Kingdom of Great Britain & Northern Ireland]	Text modified to be inline with the spirit of this comment.
TS-245	TS	7	21	7	22	Replace"the Antarctic" by "Antarctica". [Thierry Fichefet, Belgium]	Disagree, suggested usage would be incorrect
TS-246	TS	7	27	8	57	Uncertainty language: Please use language agreed in the AR5 uncertainty guidance notes. "robust evidence in high agreement" is not mentioned there. If there is high agreement and robust evidence, why do you not	Disagree, suggested usage would be incorrect

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						write "high confidence" as the remaining text? [Government of Germany]	
TS-247	TS	7	29	7	54	The “likelihood” terminology is not used here (first three paragraphs). A similar phrase is used in line 56 with the “very likely terminology”. This is somewhat inconsistent. [Ned Dwyer, Ireland]	Uncertainty language is modified to match standard usage
TS-248	TS	7	29	7	57	In the first three paragraphs on these lines, the uncertainty language of "robust evidence in high agreement' is used, but no likelihoods are given. Yet, on lines 56-57, in describing mass loss from the Antarctic ice sheet, the same confidence levels are given (robust evidence, high agreement) but in this case this is associated with a 'very likely' statement (consistent with Figure 1.12 in Ch. 1). Readers will be puzzled by this inconsistency. Is this because the underlying data did not support likelihood estimates for rates of ice loss from glaciers and the GIS? If so, perhaps that should be made clear. However, at least for the Greenland ice sheet this seems not to be the case as in TFE.2 (pg TS-12) a likelihood is given for mass loss from the Greenland ice sheet. [Government of Canada]	Uncertainty language is modified to match standard usage
TS-249	TS	7	29			“volume & mass”? Isn't glacier density constant enough that these are effectively the same? [William Ingram, United Kingdom]	Text modified but distinction remains. Glacer, volume and mass are not the same and crucially are measured with different techniques
TS-250	TS	7	30	7	30	robust evidence AND high agreement [Luisa Cristini, United States]	editorial
TS-251	TS	7	30	7	30	robust evidence in high agreement' According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: • For findings with high agreement and robust evidence, present a level of confidence or a quantified measure of uncertainty. So, a level of confidence or a quantified measure of uncertainty should be added to this line. [Line van Kesteren, the Netherlands]	Noted - text is modified significantly.
TS-252	TS	7	30	7	30	robust evidence in high agreement' According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: • For findings with high agreement and robust evidence, present a level of confidence or a quantified measure of uncertainty. So, a level of confidence or a quantified measure of uncertainty should be added to this line. [Line van Kesteren, the Netherlands]	Noted - text is modified significantly.
TS-253	TS	7	36	7	36	what kind of uncertaintes? [Luisa Cristini, United States]	This text has been changed
TS-254	TS	7	38	7	38	Why is the trend stopping in 2009? Clarification would be helpful, as line 41 refers to "ongoing" and it is unclear if this refers to now or in 2009. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	This text has been changed significantly
TS-255	TS	7	38	7	40	"...due to climate warming" It is surprising that this attribution is made here, but not for many other trends discussed in this section. Is it justified? [Government of United Kingdom of Great Britain & Northern Ireland]	Assume that this applies to page 8 line 7. The text has been modified to remove attribution statement
TS-256	TS	7	38		39	What does 'glacier' include here? Greenland and Antarctic ice sheets? Or just their outflow glaciers? Clearly define, and stick to the defintion throughout the TS (see my general comment). [Nathan Gillett, Canada]	Terms are used correctly, and are defined clearly in the glossary and chapter 4, they are not redefined in TS.
TS-257	TS	7	39			Again, give range 1st! No doubt many other cases [William Ingram, United Kingdom]	Uncertainty language is modified to match standard usage
TS-258	TS	7	40	7	40	robust evidence and high agreement' According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: • For findings with high agreement and robust evidence, present a level of confidence or a quantified measure of uncertainty. So, a level of confidence or a quantified measure of uncertainty should be added to this line. [Line van Kesteren, the Netherlands]	Uncertainty language is modified to match standard usage
TS-259	TS	7	40	7	40	robust evidence and high agreement' According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: • For findings with high agreement and robust evidence, present a level of confidence or a quantified measure of uncertainty. So, a level of confidence or a quantified measure of uncertainty should be added to this line. [Line van Kesteren, the Netherlands]	Uncertainty language is modified to match standard usage
TS-260	TS	7	43	7	44	"Several hundred glaciers globally have completely disappeared in the past 30 years (robust evidence, high agreement)". However, in Chapter 4, On p. 21, lines 55-56, it is stated that more than one hundred glaciers have disappeared in the last 30 years, not several hundred. [Government of United States of America]	Statement removed

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TS-261	TS	7	43			So what does that rather vague word “regionally” mean – bigger than mountain ranges or smaller? [William Ingram, United Kingdom]	Word is no longer used
TS-262	TS	7	44	7	44	(robust evidence, high agreement)' According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: • For findings with high agreement and robust evidence, present a level of confidence or a quantified measure of uncertainty. So, a level of confidence or a quantified measure of uncertainty should be added to this line. [Line van Kesteren, the Netherlands]	Uncertainty language is modified to match standard usage
TS-263	TS	7	44	7	44	(robust evidence, high agreement)' According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: • For findings with high agreement and robust evidence, present a level of confidence or a quantified measure of uncertainty. So, a level of confidence or a quantified measure of uncertainty should be added to this line. [Line van Kesteren, the Netherlands]	Uncertainty language is modified to match standard usage
TS-264	TS	7	44	7	45	Sentence starting with "Estimates from different methods..." is misleading. Please mention that the topic is the RATE of loss and write "increasing ice loss" "a slight decline of the decrease rate". [Government of Germany]	Noted - text is modified significantly.
TS-265	TS	7	46	7	48	Two estimates indicate that in the 1920s to 1940s, ice loss from glaciers in the Arctic, mainly from the Greenland peripheral glaciers was higher than today (medium evidence, medium agreement). Why is this the case? It is warmer right now than between 1920 - 1940... [Line van Kesteren, the Netherlands]	Noted - text is modified significantly.
TS-266	TS	7	46	7	48	Two estimates indicate that in the 1920s to 1940s, ice loss from glaciers in the Arctic, mainly from the Greenland peripheral glaciers was higher than today (medium evidence, medium agreement). In the underlying material that has been referred to (4.3.4 and table 4.5) I could not find the foundation of this stament (about 1920 - 1940). The closest to this the text in the TS is: CH4 4.3.4 p 25 l 31 - 33: 'Model studies indicate strongest mass losses during the first half of the 20th Century from then unmeasured regions in the Arctic, particularly the periphery of Greenland.' [Line van Kesteren, the Netherlands]	Reference to this is removed
TS-267	TS	7	46	7	48	Two estimates indicate that in the 1920s to 1940s, ice loss from glaciers in the Arctic, mainly from the Greenland peripheral glaciers was higher than today (medium evidence, medium agreement). Why is this the case? It is warmer right now than between 1920 - 1940... [Line van Kesteren, the Netherlands]	Reference to this is removed
TS-268	TS	7	46	7	48	Two estimates indicate that in the 1920s to 1940s, ice loss from glaciers in the Arctic, mainly from the Greenland peripheral glaciers was higher than today (medium evidence, medium agreement). In the underlying material that has been referred to (4.3.4 and table 4.5) I could not find the foundation of this stament (about 1920 - 1940). The closest to this the text in the TS is: CH4 4.3.4 p 25 l 31 - 33: 'Model studies indicate strongest mass losses during the first half of the 20th Century from then unmeasured regions in the Arctic, particularly the periphery of Greenland.' [Line van Kesteren, the Netherlands]	Reference to this is removed
TS-269	TS	7	47	7	47	It would be useful to restate "today" as a specific time period, whether decade, year or whatever the case may be. [Government of Canada]	Word no longer appears in this context
TS-270	TS	7	50	7	54	According to Velicogna 2009 "In Greenland, the mass loss increased from 137 Gt/yr in 2002–2003 to 286 Gt/yr in 2007–2009, i.e., an acceleration of −30 ± 11 Gt/yr2 in 2002–2009, whereas according to Rignot et al. 2011 "The average ice loss from Greenland was 123 ± 22 Gt yr–1 over the period 1993–2010, and 228 ± 54 Gt yr–1 in the period 2005–2010". On this basis Hansen and Sato 2012 (Paleoclimate Implications for Human-Made Climate Change, Figure 8) observes doubling of Greenland ice melt over 5 - 10 years period, projecting the trend to suggest many meters sea level rtise by the end of the 21st century. Such a scenario would be rendered more likely once Greenland becomes surrounded by open water following total summer melting of Arctic Sea ice. [Andrew Glikson, Australia]	This type of material is covered in the chapter but is out of scope in the TS. This is the territory of Ch 13.
TS-271	TS	7	50	8	4	Some earlier discussions compared the results presented in this TS with results from earlier assessments. It seems to me this needs to be done here as well. In AR4 and earlier ones it has been projected that mass will accumulate on Antarctica through the entire 21st century (well, that is mean result--uncertainties allowed a loss) as a result of an increase in snowfall. Thus, the two results for Antarctica--shrinking mass and no change in snowfall--would see to be contradicting earlier resuls (well, earlier projections). For Greenland, the rate of acceleration of loss seems to be faster than was projected, and this past summer there was melting covering virtually the entire surface. It seems to me that some indication needs to be made that there are differences between former projections and current trends. [Michael MacCracken, United States of America]	This sections is about the observations and not projections. A discussion of past projections vs. Current of observations is beyond the scope of the TS. (Actually, projections have never been suggested to have skill at sub-decadal level, and so comparison of recent changes to projections (showing disagreement, or agreement would be v. dangerous).

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-272	TS	7	51	7	52	I find these absolute amounts a little meaningless - elsewhere, percentages are used instead or as well, and it would be helpful here too. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Modified to give figure as sea-level equivalents
TS-273	TS	7	56	7	56	It is not clear why '(robust evidence, high agreement)' is stated here - doesn't this equal high confidence? Consistent approaches to describing confidence is need throughout the TS. [Government of Australia]	Uncertainty language is modified to match standard usage
TS-274	TS	7	56	7	56	also very likely currently losing mass (robust evidence, high agreement).' According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: Assign a likelihood for the event or outcomes, for which confidence should be “high” or “very high” (see Paragraphs 8-10). In this case, the level of confidence need not be explicitly stated. Therefore the robust evidence, high agreement does not need to be stated either. [Line van Kesteren, the Netherlands]	Uncertainty language is modified to match standard usage
TS-275	TS	7	56	7	56	also very likely currently losing mass (robust evidence, high agreement).' According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: Assign a likelihood for the event or outcomes, for which confidence should be “high” or “very high” (see Paragraphs 8-10). In this case, the level of confidence need not be explicitly stated. Therefore the robust evidence, high agreement does not need to be stated either. [Line van Kesteren, the Netherlands]	Uncertainty language is modified to match standard usage
TS-276	TS	7		8		Sections 2.5.5 and 2.5.6: As mentioned above, it will be more relevant to mention about Himalayan Glaciers as these control the fate of rural population and the agriculture system through river and ground water in north India. [Umesh Kulshrestha, India]	Himalayan are part of the "Asian" so are mentioned. The comment really applies to WGII Ch24
TS-277	TS	7				Add as (Figure TS.1).Add a figure: the latest Figure from http://psc.apl.washington.edu/wordpress/wp-content/uploads/schweiger/ice_volume/BPIOMASIceVolumeAnomalyCurrentV2_CY.png, also found as Figure 2 at http://psc.apl.washington.edu/wordpress/research/projects/arctic-sea-ice-volume-anomaly/. The title of the figure is "Total Arctic sea ice volume from PIOMAS showing the volume of the mean annual cycle, the current year, 2010 (the year of previous September volume minimum), and 2007 (the year of minimum sea ice extent in September). Shaded areas indicate one and two standard deviations from the mean." [CELSO COPSTEIN WALDEMAR, BRAZIL]	Noted
TS-278	TS	7				Provide error bars [Government of United States of America]	Taken into account -- uncertainty ranges given
TS-279	TS	7				TS.2.5.4: Confusing terminology for sea ice: Just refer to the "perennial sea ice" as "minimum ice extent"; calling it perennial is both inconsistent and misleading. [Government of United States of America]	Noted, but perenial is specific
TS-280	TS	7				It does not seem appropriate to provide a quantitative answer regarding the changes in the thickness of the Arctic sea ice cover (i.e. 1.8 m between 1978 and 2008), given the level of uncertainty in submarine and satellite-derived estimates. Instead:“The average winter thickness of the Arctic Ocean sea ice has thinned.” [Government of United States of America]	The value is replaced by a range and given likely status
TS-281	TS	8	2	8	3	estimate of East Antarctica mass gain? [Luisa Cristini, United States]	Text is modified signifantly and this is no longer appropriate
TS-282	TS	8	6	8	11	Please reorder para and group statements on the different regions together. [Government of Germany]	Text modified
TS-283	TS	8	7	8	7	The term 'climate warming' is used in this sentence but nowhere else in the TS, it seems slightly imprecise compared to the explanations elsewhere. [Government of United Kingdom of Great Britain & Northern Ireland]	Phrase and attribution removed
TS-284	TS	8	7			insert "," after "cycle" [Government of United Kingdom of Great Britain & Northern Ireland]	Editorial
TS-285	TS	8	9	8	10	enhanced oceanic forcing - do you mean higher water temperatures? pls use clear wording instead of technical jargons [Petra Seibert, Austria]	Text modified.
TS-286	TS	8	11	8	11	I don't understand what the phrase "are stable at present" means--just unchanging at present--what about in recent past, and is this expected to continue. I'd not say "stable" but just indicate, if it is the case, that the areas and thicknesses of these ice shelves have not shown changes over the past several decades (so are apparently in dynamic equilibrium, with as much flowing into them as is calving and melting away. [Michael MacCracken, United States of America]	Phrase is removed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-287	TS	8	13	8	40	No seasonal frozen ground was mentioned here, which can be added here to make it more complete. [Jing Ming, China]	Noted - but there is a lack of substantive statements avaliable for FG, and so none survived to the TS.
TS-288	TS	8	15	8	24	Is there any way to indicate if the losses are occurring in broad plains geographies or in mountain regions as snowline rises? Such informaiton would be helpful in thinking about the water resource implications. [Michael MacCracken, United States of America]	Noted - but this is too detailed for the TS.
TS-289	TS	8	15	8	40	Can anything be said here about the methane emissions that would result from permafrost thaw and how these would be a positive feedback, exacerbating climate forcing? [Government of United Kingdom of Great Britain & Northern Ireland]	Statements on methane are made elsewhere, and there are few observations to build a strong statement here.
TS-290	TS	8	17	8	18	Insert 'data' between 'area' and 'is' to make sentence clearer [Government of Australia]	Noted
TS-291	TS	8	19	8	19	not absolutely clear from text whether the fossil fuel emissions of 9.4+/- 0.8 PgC includes or excludes cement manufacture, numbers for which are indicated above. [Government of United Kingdom of Great Britain & Northern Ireland]	Comment misplaced - not appropriate here.
TS-292	TS	8	19	8	19	Given the dangers/difficulties of trends being influenced by choice of particular end-points, I find it a bit strange to focus on one particular winter - 1972/73. presumably this is influenced by the observational system rather than some meteorological reason? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	text modified to make this obsolete
TS-293	TS	8	21	8	24	State the reason for this 'change of snow' phenomenon [Jeffrey Obbard, Singapore]	Noted - but we avoid attribution in the observations sections
TS-294	TS	8	22	8	22	It would be useful to have some indication of how the snow albedo is changing in response to human activities. Is reflectivity increasing or decreasing and why? Also, it would be worth noting that declines in snow cover extent reduce surface albedo, which would link to the statement about albedo changes of the snow itself. [Government of Canada]	Noted, but this is too detailed for the TS.
TS-295	TS	8	23	8	24	At the same time, there are other measures indicating that there are decreases in glacial extent. Are they unrelated? [Michael MacCracken, United States of America]	Noted - the point being made is not entirely clear, but is liikely too complex for the technical summary
TS-296	TS	8	23			"In the Southern Hemisphere, …" missing multi-decadal snow observation by multi-spectral satellites: There are detailed MODIS-derived global, daily maps beginning in February of 2000 (Hall and Riggs, 2007). With respect to older records, there are papers (see Dewey and Heim, 1983; Romanov and Tarpley, 2001 and 2003) that provide longer-term measurements from NOAA satellites of the SH, though there is evidence that some of the older studies (e.g., Dewey and Heim, 1983) may have overestimated the amount of seasonal snow cover in the SH as discussed by Hall and Robinson (in press). [Government of United States of America]	Some of this material is discussed in the chapter, but overall a statement about SH is still hard to make conclusively.
TS-297	TS	8	26	8	26	is there a minimum value of permafrost T increase? [Luisa Cristini, United States]	text modified to make this obsolete
TS-298	TS	8	26	8	26	There is robust evidence with high agreement that permafrost' According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: • For findings with high agreement and robust evidence, present a level of confidence or a quantified measure of uncertainty. So, a level of confidence or a quantified measure of uncertainty should be added to this line. [Line van Kesteren, the Netherlands]	text modified to make this obsolete
TS-299	TS	8	26	8	26	There is robust evidence with high agreement that permafrost' According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: • For findings with high agreement and robust evidence, present a level of confidence or a quantified measure of uncertainty. So, a level of confidence or a quantified measure of uncertainty should be added to this line. [Line van Kesteren, the Netherlands]	text modified to make this obsolete
TS-300	TS	8	26	8	27	The magnitude of temperature change given for permafrost temperatures needs to be reviewed. Comments have been submitted for Chapter 4, section 4.6.2 that suggest there may be errors related to these findings. Some of the rates presented in Table 4.7 are not in agreement with those in the literature, which for the most part have been up to only about 2°C over the last 3 decades in the northern hemisphere (where records are long enough to examine trends over this period). The literature quoted in chapter 4 and also the SWIPA report	Noted - text modified significantly making this obsolete

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						(by AMAP) summarizes change in permafrost temperature as typically between 0.5 and 2°C over the last 3 decades for the northern hemisphere. It is suggested that a similar statement be utilized here. Suggest also that it is important to give the range and explain that the magnitude of change varies spatially. [Government of Canada]	
TS-301	TS	8	26	8	27	There is a problem with the magnitude of change given for permafrost temperatures. This may be partly due to errors in section 4.6.2 as some of the rates presented in Table 4.7 are not in agreement with those in the literature as for the most part the changes have been up to only about 2°C over the last 3 decades in the northern hemisphere (where records are long enough to examine trends over this period). The literature quoted in chapter 4 and also the SWIPA report summarizes change in permafrost temperature as typically between 0.5 and 2°C over the last 3 decades for the northern hemisphere. It is suggested that similar statement be utilized. It is also important to give the range and add a statement that the magnitude of change varies spatially. See comments on Ch 4 for more details [Sharon Smith, Canada]	Noted - text modified significantly making this obsolete
TS-302	TS	8	26	8	40	It would seem useful here to give some indication about what the results are from the thawing of permafrost in terms of release of CO2 and/or methane--or at least mention that this is an important change because of the potential for this. [Michael MacCracken, United States of America]	Noted, but there are few observations published on which to make a strong statement.
TS-303	TS	8	26			"up to 3°C" expresses only maximum possible warming. What is the range of the temperature changes? [Government of United States of America]	Noted - text modified significantly making this obsolete
TS-304	TS	8	29	8	30	define COLDER and WARMER permafrost [Luisa Cristini, United States]	Noted - text modified significantly making this obsolete
TS-305	TS	8	30	8	30	Degradation' should be defined or an alternative word used. Do we mean degradation in terms of physical degradation, or biological degradation - implying loss of trapped CO2 and methane. If the former, perhaps 'disintegration' is a better word to use for the lay person. [Jeffrey Obbard, Singapore]	Noted - text modified significantly making this obsolete
TS-306	TS	8	30	8	31	Significant permafrost degradation has occurred in the Russian European North (robust evidence, high agreement),' According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: • For findings with high agreement and robust evidence, present a level of confidence or a quantified measure of uncertainty. So, a level of confidence or a quantified measure of uncertainty should be added to this line. [Line van Kesteren, the Netherlands]	Noted - text modified significantly making this obsolete
TS-307	TS	8	30	8	31	Significant permafrost degradation has occurred in the Russian European North (robust evidence, high agreement),' According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: • For findings with high agreement and robust evidence, present a level of confidence or a quantified measure of uncertainty. So, a level of confidence or a quantified measure of uncertainty should be added to this line. [Line van Kesteren, the Netherlands]	Noted - text modified significantly making this obsolete
TS-308	TS	8	31	8	31	If there is high agreement and robust evidence, why do you not write "high confidence" as the remaining text? [Government of Germany]	Uncertainty language is modified to match standard usage
TS-309	TS	8	31	8	31	Are there enough independent studies of permafrost losses in European Russia to say there is "high agreement" of this? These numbers (from section 4.6.2) all come from one study. [Government of United States of America]	Noted - text modified significantly making this obsolete
TS-310	TS	8	32	8	33	permafrost with thickness of 10 to 15 m completely thawed in some regions over the period 1975–2005;' Does this apply to some region in the whole world or some regions in the Russian European North sinse the sentence started with a statement about the Russian European North. This is not clear. [Line van Kesteren, the Netherlands]	Noted - text modified significantly making this obsolete
TS-311	TS	8	32	8	33	permafrost with thickness of 10 to 15 m completely thawed in some regions over the period 1975–2005;' Does this apply to some region in the whole world or some regions in the Russian European North sinse the sentence started with a statement about the Russian European North. This is not clear. [Line van Kesteren, the Netherlands]	Noted - text modified significantly making this obsolete
TS-312	TS	8	33	8	34	What is the difference between NH permafrost and continous permafrost? Is the first not continuos? [Government of Germany]	Noted - text modified significantly making this obsolete

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-313	TS	8	33	8	35	Note that in ch 4.6.2 this retreat of permafrost boundaries only refers to Russia not the entire northern hemisphere. [Government of Canada]	Noted - text modified significantly making this obsolete
TS-314	TS	8	33	8	35	Note that in ch 4.6.2 this retreat of permafrost boundaries only refers to Russia and is not necessarily representative of the entire northern hemisphere. [Sharon Smith, Canada]	Noted - text modified significantly making this obsolete
TS-315	TS	8	33	8	35	the southern limit of the Northern Hemisphere permafrost boundary moved north by about 80 km; and the boundary of the continuous permafrost moved north by 15–50 km.' The reference for this statment is to section 4.6.2, in this section the literature referred to is Oberman, 2008. This seems to be an article about permafrost degradation in northern european Russia while this statement in the TS talks about the southern limit of the Northern Hemisphere permafrost boundary.. not just about northern european Russia. Please replace Northern Hemisphere by northern european Russia unless you have more information that justifies this extrapolation. [Line van Kesteren, the Netherlands]	Noted - text modified significantly making this obsolete
TS-316	TS	8	33	8	35	the southern limit of the Northern Hemisphere permafrost boundary moved north by about 80 km; and the boundary of the continuous permafrost moved north by 15–50 km.' The reference for this statment is to section 4.6.2, in this section the literature referred to is Oberman, 2008. This seems to be an article about permafrost degradation in northern european Russia while this statement in the TS talks about the southern limit of the Northern Hemisphere permafrost boundary.. not just about northern european Russia. [Line van Kesteren, the Netherlands]	Noted - text modified significantly making this obsolete
TS-317	TS	8	37		40	These 2 sentences contradict each other. I assume the 1st is missing some qualification. [William Ingram, United Kingdom]	Noted - text modified significantly making this obsolete
TS-318	TS	8	38			"up to 90 cm" expresses only maximum possible warming. What is the range of the temperature changes? [Government of United States of America]	Noted - text modified significantly making this obsolete
TS-319	TS	8	43	8	43	It would be useful to spell out this acronym (TFE) rather than requiring the reader to refer back to the first page to recall the full wording. These boxes are most helpful if they can be read as stand-alone pieces. [Government of Canada]	Noted: It should be clearer in the final printing
TS-320	TS	8	43	8	56	This para gives an introduction for non-experts to the water cycle, and provides very basic information. It is suggested to add such introducing basic information to all the TFE. [Government of Germany]	Noted
TS-321	TS	8	45	8	54	The water cycle describes the movement of water above and below the surface of the Earth, this is the adopted definition (see Page 54 L11-12). River flow in summer (base flow) is also provided by groundwater [LUCILA CANDELA, Spain]	Noted
TS-322	TS	8	47	8	48	The word 'but' is used a number of times here and appears to disqualify previous statements. Rewording is needed. [Government of Australia]	Noted
TS-323	TS	8	48		49	decapitalize [William Ingram, United Kingdom]	noted
TS-324	TS	8	51	8	53	This overlooks the fact that land evapotranspiration returns about 60% of all precipitated water to the atmosphere and is thus an important contributor to land precipitation. References: Oki, T., and S. Kanae 2006, Science, 313, 1068-1072; Seneviratne, S.I. et al., Earth-Science Reviews, 99, 125-161; van der Ent et al. 2010, Water Resources Research, 46, W09525, doi:10.1029/2010WR009127. [Sonia Seneviratne, Switzerland]	Noted, but the point is that this would stop eventually without water from the oceans.
TS-325	TS	8	53		54	It can provide either in either season [William Ingram, United Kingdom]	noted
TS-326	TS	8	55	8	56	the movement of fresh water ... does influence oceanic salinity, which is an important component of the density and thus circulation of the ocean [Government of United Kingdom of Great Britain & Northern Ireland]	Noted
TS-327	TS	8				TS.2.5.6: Provide error bars [Government of United States of America]	Unclear
TS-328	TS	9	6	9	6	I would suggest saying "increases exponentially with temperature" to show how important a relationship it is. [Michael MacCracken, United States of America]	Noted
TS-329	TS	9	6	9	45	You did not examine cloudiness = cloud cover. Did it change? [Andrea Flossmann, France]	Hard to observe change.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-330	TS	9	7	9	7	water vapour suspended' ... Is this technically correct, do you suspend a gas? [Government of United Kingdom of Great Britain & Northern Ireland]	Not technically correct as it could suspend itself in the absence of dry air, but if you think if air as also including water vapor it makes a bit more sense.
TS-331	TS	9	7	9	7	"suspended in the air" - gosh - the amount of water vapour in the gaseous state above a surface is not influenced, to first order, by the presence of air, and it certainly isnt suspended, as it is in a dynamic equilibrium. How about just saying "atmospheric water vapour" and avoiding the suspended business! [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Noted
TS-332	TS	9	7			“suspended” not true in the scientific sense & I can't see useful to the non-scientist: omit [William Ingram, United Kingdom]	Noted, but very common acronym nowadays.
TS-333	TS	9	8	9	8	GPS" is not defined [Government of Kenya]	Noted
TS-334	TS	9	8		9	“water vapour … specific humidity” illucid: make both “specific humidity” [William Ingram, United Kingdom]	Noted
TS-335	TS	9	9	9	17	The terms "specific humidity" and "relative humidity" need to be added to the Glossary (Appendix III). Why does line 17 then refer to "the water content of air"? [David Webb, United Kingdom]	New Glossary entries for "Relative humidity" and "Specific humidity"; Line 17 refers to "water vapour content of air"
TS-336	TS	9	10	9	10	Where does this value of 3.5% come from? Its not in Ch 2. I think it also refers only to TCWV observed over the ocean so that should be stated if it is the case. The fact that water vapour is attributable to human influence with medium confidence is also not in Section 2.5.6. I have pointed to the relevant paper that shows this for surface humidity (Willett et al. 2007) but I believe that this is also demonstrated for tropospheric humidity by Santer et al. (2008, PNAS) but this is not in the tropospheric humidity section. [Kate Willett, United Kingdom]	Noted: Attribution statements are in chapter 10.3.2, to which a reference has been added.
TS-337	TS	9	11	9	12	The statement "rel hum has stayed constant" seems to apply only for the global mean and over oceans, as page 6, lines 26,27 state "As a result, fairly widespread decreases in relative humidity near the surface have been observed over the land areas recently." [Government of Germany]	Stayed constant relative to specific humidity.
TS-338	TS	9	11	9	13	Here it says the RH has stayed about constant (compared to earlier saying it droped over land). I'd suggest that it would be sorth adding a phrase to the effect that the RH staying approximately constant is consistent with some very early assumptions that were consistent with strong water vapor feedback and a 3 C or so sensitivity, and not with a low sensitivity. [Michael MacCracken, United States of America]	Noted
TS-339	TS	9	12	9	13	Delete "The water vapor change can be attributed to human influence with medium confidence",which was not mentioned in 2.5.6. [Qingxiang Li, China]	reference to 10.3.2 has been added.
TS-340	TS	9	12	9	13	There is an attribution statement here but no reference to Chapter 10. [Geert Jan van Oldenborgh, Netherlands]	added
TS-341	TS	9	15	11	22	Please add an uncertainty statement to the observed changes in precip. In the current text it is stated that it is difficult to measure at all, but that attribution to anthropogenic activities is possible with medium confidence. This is confusing. [Government of Germany]	The difficulty in measuring precipitation is stated in words here and details are given elsewhere.
TS-342	TS	9	16	9	16	maybe it would be good to add "especially over the oceans" here? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Noted
TS-343	TS	9	18	9	20	If there has been little change in land-based precipitation since 1900, why is there medium confidence that there has been significant human influence ? [David Webb, United Kingdom]	There is other evidence, such as more robust recent trends and the salinity evidence.
TS-344	TS	9	18	9	21	if time series show “little change in land-based precipitation”, then how can there be “medium confidence that there has been a significant human influence on global scale changes in precipitation patterns”? [David Sauchyn, Canada]	Other evidence of observec change, and using models for detection and attribution.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-345	TS	9	19	9	21	The description of the changing pattern should be in agreement with the newest IPCC AR. Observation doesn't support this result. [Qingxiang Li, China]	it is consistent if all the evidence is considered.
TS-346	TS	9	20	9	20	I am confused by this finding of reduced precipitation in low latitudes--is this in the peak or total amount? So, could it be that precipiitation is now spread out more by latitude? Without an increase in precipitation, it is hard to understand how [Michael MacCracken, United States of America]	Text has been revised and this statement has been eliminated from the TS
TS-347	TS	9	20			“patterns” → “distribution” clearer to non-specialists? [William Ingram, United Kingdom]	Noted
TS-348	TS	9	22			remove "with" [Government of United Kingdom of Great Britain & Northern Ireland]	text revised
TS-349	TS	9	25	9	26	Add "over the oceans" after "between evaporation and precipitation" [Sonia Seneviratne, Switzerland]	Noted
TS-350	TS	9	26	9	27	“The mean regional pattern of sea surface salinity has been enhanced”. Is enhanced the best word to use here? What does it mean? [Ned Dwyer, Ireland]	Text revised to clarify.
TS-351	TS	9	27			“mid-latitudes” isn't right, is it – it's more subtropical? [William Ingram, United Kingdom]	Yes
TS-352	TS	9	28			“tropical” too general, surely - “in the deep tropics”? [William Ingram, United Kingdom]	yes
TS-353	TS	9	29	9	31	This repeats the argument of section TS.2.5.2 (See comment on page 6, lines 43 and 44). I think that it is not 'very likely' that the changes in surface salinity are due to intensification of the water cycle. The well documented increase in ocean stratification appears a more likely candidate. Maybe both should be downgraded to 'may be due to'. [David Webb, United Kingdom]	Noted
TS-354	TS	9	33	9	33	Replace “decreasing numbers of” with “less” [Ned Dwyer, Ireland]	Noted
TS-355	TS	9	34	9	39	this text is repeated verbatim from the previous page (lines 15-19) [David Sauchyn, Canada]	Noted
TS-356	TS	9	43	9	45	It seems to me that these comments need more explanation or the misimpression will be given that any result in the assessment can just be changed and dismissed. Were the previous projections strong or weak; did they apply over the globe or by region. And if IPCC is going to own up to a problem here, what about for Arctic sea ic and global ice sheetse, which are disapperaing much more rapidly that earlier projected. [Michael MacCracken, United States of America]	Noted, but no text added.
TS-357	TS	9	47	10	16	The previous part deals with observations of surface salinity as a component of water cycle change, but there is no discussion of this in the projections of future change. Could this be included. [Government of United Kingdom of Great Britain & Northern Ireland]	Salinity projections have not been included, but E-P trends have been.
TS-358	TS	9	47			Projections of Future Changes: would it be possible to mention here that on regional scales, the reliability of the model ensemble does not encompass the observed changes in more regions than expected by chnce? {Box 11.2} [Geert Jan van Oldenborgh, Netherlands]	The magnitude of these changes relative to natural variability has not been included.
TS-359	TS	9	47			Projections of Future Changes: please also mention that in the near-term, natural variability will amsk many of the longer-term trends (Chapter 11). [Geert Jan van Oldenborgh, Netherlands]	The magnitude of these changes relative to natural variability has not been included.
TS-360	TS	9	49	9	52	As with comment on SPM p14, 38-41: Why are degrees Kelvin being used here, when we've been using C until now. Can this be converted, even if the underlying research used K? Alternatively, throughout the SOD one could be consistent by using Celsius degrees instead of Kelvin [Government of United Kingdom of Great Britain & Northern Ireland]	copy edit; WGI AR5 will use degree C throughout the report for for temperatures, changes in temperature and temperature trends.
TS-361	TS	9	50	9	50	Global-scale precipitation is projected to gradually increase in the 21st century.' If this is the case then why did we not see an increase uptill now? Temperatures have been increasing.. Please add an explanation why we did not see an increase in global scal precipitation until now, see 2.5.1.2 (lack of spacial coverage). [Line van Kesteren, the Netherlands]	This TFE spends several lines explaining why precipitation is difficult to observe.
TS-362	TS	9	50	9	50	Global-scale precipitation is projected to gradually increase in the 21st century.' The underlying material seems to state that wet regions will get wetter and dry regions drier with some exaptions. I did not find a	Noted

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						underlying information stating there will be an overall increase in global-scale precepitation.. In chapter 12 there is no explanation of graph that indicates the increase of global average precipitation until 2100 (this graph is present in Figure SPM.5 c, but this graph should be available in Chapter 12 as well). [Line van Kesteren, the Netherlands]	
TS-363	TS	9	50	9	50	Global-scale precipitation is projected to gradually increase in the 21st century.' If this is the case then why did we not see an increase uptill now? Temperatures have been increasing.. [Line van Kesteren, the Netherlands]	This TFE spends several lines explaining why precipitation is difficult to observe.
TS-364	TS	9	50	9	50	Global-scale precipitation is projected to gradually increase in the 21st century.' The underlying material seems to state that wet regions will get wetter and dry regions drier with some exaptions. I did not find a underlying information stating there will be an overall increase in global-scale precepitation.. 12.4.5.2 p 44 l 19 - 27: 'In general, areas that are currently wet tend to become wetter, while areas that are currently dry tend to become dryer. This holds well in the seasonal averages for the multi-model ensemble mean (Figure 12.22), but it is important to note that significant exceptions can occur in particular regions. In the tropics, an increase of water vapour leads to additional moisture convergence within tropical convergence zones and to additional moisture divergence in the descent zones, increasing the contrast in precipitation between wet and dry regions (Chou and Neelin, 2004; Held and Soden, 2006). This increase in contrast is partly compensated by the slowdown of the tropical circulation due to the water vapour increase (see Section 12.4.4) and the net effect is an increase in tropical precipitation together with a simultaneous suppression in the subtropics (Allan, 2012; Chou et al., 2009).' [Line van Kesteren, the Netherlands]	Yes.
TS-365	TS	9	50	9	50	Global-scale precipitation is projected to gradually increase in the 21st century.' This seems to be in conflict with TS p 41 l 25 - 27 in which it is stated that wet regions will get wetter and dry regions drier, there does not seem to be a global average increase. TS p 41 l 25 - 27: 'It is more likely than not that over the next few decades there will be increases in mean precipitation in regions and seasons where the mean precipitation is relatively high, and decreases in regions and seasons where mean precipitation is relatively low.' [Line van Kesteren, the Netherlands]	The regional changes are larger than the global change beause of the global energy constraint.
TS-366	TS	9	50		52	The text here says that it is 'virtually certain that precipitation increase will be much smaller [than 7%/K], approximately 2%/K'. I think this is much too confident. This is solely based on model projections. But there is some evidence from observations that the models considerably underestimate this rate. See section 10.3.2.2 and Zhang et al. (2007). I would suggest 'It is very likely that the rate of precipitation increase will be much lower than the rate of lower tropospheric water vapour increase (7%/K). Climate models project a rate of approximately 2%/K, though some studies find that models may underestimate this rate compared to observations' (10.3.2.2). Zhang, X., Zwiers, F. W., Hegerl, G. C., Lambert, F. H., Gillett, N. P., Solomon, S., ... & Nozawa, T. (2007). Detection of human influence on twentieth-century precipitation trends. Nature, 448(7152), 461-465. [Nathan Gillett, Canada]	It's not an aspect of models that is very uncertain, but derives from very basic physics.
TS-367	TS	9	51	2	51	A range of incraeses may relect the uncertainty better than only a central value of 2%/K [Geert Jan van Oldenborgh, Netherlands]	Yes, but the range of uncertainty is fairly small compared to the differnce between 2% and 7%.
TS-368	TS	9	51			Reads as if 2%/K smaller than 7 %/K. Anyway, weren't you using 'C? Be consistent! [William Ingram, United Kingdom]	Noted
TS-369	TS	9	54			if "drying" is referring to observations of drying, insert "observed" before "drying" [Government of United Kingdom of Great Britain & Northern Ireland]	Page and line reference does not match question. Drying in the projections sectin refers to model projections.
TS-370	TS	9	54			Omit “with” [William Ingram, United Kingdom]	Noted
TS-371	TS	9	55			“greater amounts of” → “more” [William Ingram, United Kingdom]	noted
TS-372	TS	10	1	10	1	"The largest precipitation changes" absoliute or relative changes? [Geert Jan van Oldenborgh, Netherlands]	Absolute
TS-373	TS	10	6	10	8	Caption needs more as the meaning of the hatching and stippling is not stated nor is the meaning of the top-right-hand-corner numbers stated [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Done

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-374	TS	10	7			figure caption Fig. 1: what are the dots and the shaded areas? [Andrea Flossmann, France]	Caption has been expanded
TS-375	TS	10	8	10	14	Be honest. Tell the reader that sea level rise is far from consistent around the world, as per the 3 figures, each of two long-term tidal stations, in chapter 3. [John McLean, Australia]	reject; this was and still is spelled out explicitly in the very first paragraph of this Section. Regional sea level change is covered in both Chapters 3 and 13 in all detail. FAQ 13.1, for example, focuses on "Why Does Local Sea Level Change Differ from the Global Average?" (Note, comment should refer to p-11)
TS-376	TS	10	11		12	It's fairly clear “drying” means reduced soil moisture rather than rainfall, but it wouldn't hurt to be explicit [William Ingram, United Kingdom]	Noted, but paragraph is about soil mosture and drought.
TS-377	TS	10	11			something missing before "meters" [Government of United Kingdom of Great Britain & Northern Ireland]	No meters in these lines
TS-378	TS	10	17	10	19	You can't make assertions like this unless you can explain the physical processes that warmed the oceans below 700m. [John McLean, Australia]	Should be page 11 - these are direct observations - not the explanations
TS-379	TS	10	19	11	14	This section is confused and incorrect. The "Sea Level" is Relative, the diffeence between the levek of the sea and the position of the equipment attached to the land.Both the sea and the land change from time to time and from one measurement a nd another.The level of the sea may change because of storm protection measures, dredging of the harbour, or measures to increase local water level to enable larger ships to enter. The land position can change in many ways. The extent of the land may increase by reclamation frpm the sea, or decrease by erosion. It may subside from removal of mnierals or ground water or from weight of buildings.The equipment and its ground attachment tend to be damaged by storms and replaced in a different place. Many records are fragmented for this reason. Most of these changes cause an upwards bias, so it is wrong, as is claimed in this Chapter, to claim that they are necessarily related to changes on the climate. or to supposed increases un the level of the ocean caused by melting ice..It also means that the comprehensive amalgamation of sea level "changes" carried out in this Chapter are not a reliable guide to future sea level. In any case it is wrong to attempt to derive "trends" from time series where every point in the graph is different and where the earkuer figures are the least relaible. The most modern measuring equipment (SEA FRAME) and the levelling equipment, based on GPS availability means that the best method for future prediction is to extrapolate the last few relaible and uniform figures. Examples are the recent Pacific Island series, described at http://scienceandpublicpolicy.org/images/stories/papers/originals/southpacific.pdf, which show no change for the past ten years after the GPS levelling was installed. A similar result can be found for Australia.The claim in this Chapter that sea level is rising is not supported by detailed studies of his kind.. [Vincent Gray, New Zealand]	Unfortunately no observation system is perfect. This section reports the observations with the best analysis available
TS-380	TS	10	19			Re. Section on changes in sea level: I would recommend including a summary sentence at the beginning of this section giving values of sea level rise over the last century (similar to other sections) to provide context for the following discussion of paleoclimate records. [Government of United Kingdom of Great Britain & Northern Ireland]	The section has been extensively rewritted, but it does not start with a summary sentence
TS-381	TS	10	19			Again, I would suggest that the word 'integration' is changed to something clearer for the non-scientific reader, perhaps 'combination'. [Government of United Kingdom of Great Britain & Northern Ireland]	This section has been extensively rewritten
TS-382	TS	10	21	10	21	Could this sentence be shortened to '… between 6 and 10 metres above current values.' [Government of United Kingdom of Great Britain & Northern Ireland]	This section has been extensively rewritten
TS-383	TS	10	22	10	22	contemporary sea level change' this statement is very ambiguous without specifying this [Mark Siddall, United Kingdom]	This section has been extensively rewritten
TS-384	TS	10	24	10	25	Is this really significant? Volumetrically, may need justification. [Jeffrey Obbard, Singapore]	Yes it is. It is justified in the chapter. This section has been extensively rewritten
TS-385	TS	10	24			“Anthropogenic processes” → “Human actions” [William Ingram, United Kingdom]	This section has been extensively rewritten
TS-386	TS	10	28	10	28	The first sentence in this para should be moved to the previous para as it belongs to the introduction, not to the this para which is on the Pliocene. [Government of Germany]	This section has been extensively rewritten

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TS-387	TS	10	31	10	31	This uncertainty range seems very large on the low side--going all the way to zero and again opening up the text to criticism and delay by skeptics. [Michael MacCracken, United States of America]	This section has been extensively rewritten - and this statement in particular
TS-388	TS	10	31	10	34	Having mislead the reader into believing that the sea level rise is relatively uniform you now admit that it's not. What a pity then that you don't correctly attribute the changes in the Pacific to the shift in ENSO. [John McLean, Australia]	This section is about observations not their attribution
TS-389	TS	10	31	10	34	confidence? [Mark Siddall, United Kingdom]	This section has been extensively rewritten
TS-390	TS	10	31			What means the expression "10 ± 10 m". Is it correctly written? [José Daniel Pabon-Caicedo, Colombia]	This section has been extensively rewritten, including this statement
TS-391	TS	10	37	10	37	"meters higher"? a few meters? [Government of Germany]	This section has been extensively rewritten
TS-392	TS	10	37	10	37	Please give time period for last interglaciation for non-experts. [Government of Germany]	Done - This section has been extensively rewritten
TS-393	TS	10	37	10	39	remove "s" from "contributions" [Government of United Kingdom of Great Britain & Northern Ireland]	This section has been extensively rewritten
TS-394	TS	10	37			The text says 'to meters higher'. How many metres? This seems to be important information. Give a range if necessary. [Nathan Gillett, Canada]	Done - This section has been extensively rewritten
TS-395	TS	10	37			This is a long clumsy sentence. Say instead "...was between 6 and 10m higher than current values" [Government of United Kingdom of Great Britain & Northern Ireland]	This section has been extensively rewritten
TS-396	TS	10	37			“interglaciation” ->“interglacial” [William Ingram, United Kingdom]	This section has been extensively rewritten
TS-397	TS	10	38	10	38	It might be instructive to include the range of larger values which are suggested by the semi-empirical models. [Government of United Kingdom of Great Britain & Northern Ireland]	this section is about observations
TS-398	TS	10	38	10	39	Sentence is confusing, suggest replacing "was more than 6 m higher than current values and less than 10 m above current values" with "was between 6 m and 10 m higher than current values" [Government of Australia]	This section has been extensively rewritten
TS-399	TS	10	39			Shouldn't “thermosteric” be explained? [William Ingram, United Kingdom]	This section has been extensively rewritten
TS-400	TS	10	40			“are” → “total” for clarity [William Ingram, United Kingdom]	This section has been extensively rewritten
TS-401	TS	10	41	10	46	A problem here is that the rate of warming at present is far above the rate in the paleo periods, and this needs to be said so it is clear that these are not upper limits [Michael MacCracken, United States of America]	This section has been extensively rewritten and this issue addressed
TS-402	TS	10	41			It is suggested to delet "that". [Klaus Radunsky, Austria]	editorial
TS-403	TS	10	43	10	43	Is there a good reason why the units are not the same as used later ie mm yr-1 ?? [Government of United Kingdom of Great Britain & Northern Ireland]	Yes these are rates over millenia - it is approriate to use a different unit
TS-404	TS	10	43	10	55	The use of such different units as m/kyr and mm/yr are not very reader friendly. Any way to compromise and use a common unit--preferably something in terms of m/century? [Michael MacCracken, United States of America]	Prefer to maintain m/millenia
TS-405	TS	10	44	10	46	Some additional information is required to understand the statement that these periods (the last 500,000 years) give only a limited analogy for future anthropogenic climate change. Can some minimum amount of information be added here to make this clearer (e.g., differences in forcing agents). Also, if possible, can it be said whether or not the last interglacial was warmer than present? Having some estimates of global temperature (as was given for the Pliocene on line 30) provides useful context for the discussion of SLR. [Government of Canada]	valid comment - This section has been extensively rewritten
TS-406	TS	10	45	10	45	"upper bounds" - I am not sure of the meaning of this - does it mean that the paleo evidence provides no useful information on contemporary sea-level changes? If so, I would delete this whole paragraph and replace it by a sentence indicating that there is no useful information [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	This section has been extensively rewritten
TS-407	TS	10	49			Should this be, "sea level data… indicate lower rates of sea level change during the late Holocene (order	valid comment - This section has been extensively

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						tenths of mm/yr) than modern rates (order mm/yr) during the 20th century)"? Since the previous sentence is discussing the rate has increased over the past two centuries. [Government of United States of America]	rewritten
TS-408	TS	10	50	10	50	After Holocene add "between x and y years before the present". [Ned Dwyer, Ireland]	included in rewrite of this section
TS-409	TS	10	50			It is suggested to substitute "modern" by "moderate". Otherwise the whole sentence should be rewritten - as it stands the sentence does not convey a clear message. [Klaus Radunsky, Austria]	This section has been extensively rewritten
TS-410	TS	10	54	10	54	In the expression of "virtually certain" does it mean the one is hundred percent certain? Otherwise it is not clear and needs to be clearly certain since it appears in a number of subsequent pages. [Government of Kenya]	now range given as very likely
TS-411	TS	10	54	10	55	On the website ftp://ftp.aviso.oceanobs.com/pub/oceano/AVISO/indicators/msl/, consider the file MSL_Serie_MERGED_Global_IB_RWT_NoGIA_NoAdjust.txt, which is the average of altimeter sea level data. A simple linear regression to fit the 1993-2006 data gives a slope of 3.2 mm per year which is consistent with the sentence written in SOD. However, the slope is reduced to 2 mm per year in the period 2006-2012 and even to 1 mm per year in the period 2008-2012, viz. less than during the 20th century. Please check. I suggest, therefore, addressing and discussing in AR5 the deceleration of sea level rise observed recently. [François Gervais, France]	By carefuly selected short periods any rate can be obtained but these are not meaningful.
TS-412	TS	10	54	11	6	Please give numbers for the total SLR since pre-industrial time, in addition to the rate of increase. [Government of Germany]	20th century value now given
TS-413	TS	10	55	10	55	"since 1993" - until when? 2010? I also find it strange that the text here always refers to the rate of change rather than the absolute change (and so contrasts with the discussion on temperature on page TS-4, for example). I feel it would be useful to add that the overal change is about 20 cm. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	This section has been extensively rewritten - overall change also given
TS-414	TS	10	56	10	57	Where is this rise occurring? It's certainly not in the Pacific where monitoring shows rise and fall associated with ENSO but notjing else, after the data is corrected by the GPS montoring of tidal guages. (Refer Australian SEAFRAME project) [John McLean, Australia]	There are regions of high sea level rise in the Pacific.
TS-415	TS	10	57			I see no chance of policymakers guessing that "altimetry" means satellite-borne radars [William Ingram, United Kingdom]	This section has been extensively rewritten
TS-416	TS	11	1			"the different" → "changes in" [William Ingram, United Kingdom]	This section has been extensively rewritten
TS-417	TS	11	2	11	2	Was not the period of 1930-50 one, at least at early time, of land drying? Could that have been a factor--just less precip onto land? [Michael MacCracken, United States of America]	This is an observation - not an attribution section
TS-418	TS	11	2	11	2	This discussion on 1930-1950 is strange to be, from a casual analysis of TFE.2 Fig 1c on page TS-71. It seems to me that the rate of change of sea-level was also as large, if not larger, between about 1965 and 1983, whereas one might infer from the text that the trend has levelled off in this period. An alternative interpretation seems to be that the trend is somewhat constant but there are occasional step-downs such as in the early 1960s and early 1980s (perhaps volcanically forced?) [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	This section has been extensively rewritten - yes there are short periods of lower and high rates but there is an extended period of larger rates in the first half of the 20th century
TS-419	TS	11	8	10	29	The use of the square bracket notation for uncertainty ranges is very difficult to follow here with no explanation. I think this is the first place where they are used and there is little hope for the reader to understand particularly as they are used first on a range (line 13) then with single number estimates (lines 16,18,19) and then as the range itself line 26. [Government of United Kingdom of Great Britain & Northern Ireland]	This is now introduced earlier
TS-420	TS	11	8	11	14	This paragraph seems to be all about tide gauge data. This should be made clear from the start of the paragraph otherwise the reader questions why coastlines of Northern Europe are being singled out here for the discussion of SLR. Suggest starting with something like this "Tide gauge records are available from some regions of the world, going back to....". [Government of Canada]	This section has been extensively rewritten
TS-421	TS	11	9			Drop "has" (unless "20th" is a mistake for "21st") [William Ingram, United Kingdom]	This section has been extensively rewritten

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TS-422	TS	11	12	11	12	GMSL=global mean sea level? [Luisa Cristini, United States]	This section has been extensively rewritten
TS-423	TS	11	12	11	13	The first use of the [...] notation is on a range following a sentence that talks about ' Two out of three reconstructions...' and is open to misinterpretation. To be able to interpret this sentence with the [..] showing the max and min of the 90% confidence intervals of the 3 reconstructions you have to find the original text in chapter 3.7 p 3-32 , lines 9-11. [Government of United Kingdom of Great Britain & Northern Ireland]	definition now introducedearlier
TS-424	TS	11	18	11	19	“warming below 2000 m has been contributing another 0.15 [0.05 to 0.25] mm yr–1 of sea level rise since the early 1990s.” Is this contradicted by the material in {3.7 } page 29, line 48 – 50 “The estimated total contribution of warming below 2000 m to global mean sea level rise between circa 1992 and 2005 is 0.11 [0.01 to 0.21] mm yr–1 (95% confidence as reported by authors; Purkey and Johnson, 2010).” Or are there additional factors adding 0.14mm ? [Ned Dwyer, Ireland]	this material now removed
TS-425	TS	11	18			Tidy line break [William Ingram, United Kingdom]	editorial
TS-426	TS	11	19	11	19	0.11 [0.01 to 0.21] mm yr -1 in Chapter 3. [David Parker, United Kingdom of Great Britain & Northern Ireland]	this material now removed
TS-427	TS	11	21	11	24	The estimates on page 13-18, lines 12-20, are given in the periods 1993-2009 and 2005-2009 while table 13.1 (page 13-20) and TS-11 line 21-24 used the periods 1993-2010 and 2005-2010. It would greatly improve the report if the same periods were used. [European Union]	now use the same periods
TS-428	TS	11	21	11	24	Pls check the consistence of the individual ice-sheet contributions to SLR as given here with the total contribution of ice-sheets to SLR as mentioned at TS-12, line 56. [Government of Germany]	done.
TS-429	TS	11	21			Tidy line break [William Ingram, United Kingdom]	editorial
TS-430	TS	11	23			Does 'glacier' here include ice sheets as per the standard definition? See my comment on the whole TS. [Nathan Gillett, Canada]	No - glaciers do not include ice sheets.
TS-431	TS	11	26	11	26	Cab you provide an average value as in line 16 for “at a rate between [0.8 to 1.6] mm yr-1 ” [Ned Dwyer, Ireland]	this material now deleted
TS-432	TS	11	26	11	26	Insert 'equivalent to a sea level rise' of' between "rate" and "between" [Government of Australia]	This section has been extensively rewritten
TS-433	TS	11	26	11	26	As above, referring to an increase in 'mass' with units of mm/year needs to be corrected. [Government of United Kingdom of Great Britain & Northern Ireland]	This section has been extensively rewritten
TS-434	TS	11	26	11	29	Explain GRACE and Argo [Luisa Cristini, United States]	this material deleted
TS-435	TS	11	27			please explain "GRACE" or give reference [Barbara Früh, Germany]	this material deleted
TS-436	TS	11	33			Explain “Warm Pool” or at least put it in quotation marks [William Ingram, United Kingdom]	this material deleted
TS-437	TS	11	41	11	58	Please give numbers for the total SLR since pre-industrial time, in addition to the rate of increase. [Government of Germany]	done
TS-438	TS	11	43	11	47	Include time spans for LGM and Holocene [Luisa Cristini, United States]	done
TS-439	TS	11	50	12	2	Another poor excuse for a Figure that is far too complex and needs a caption of more than 10 lines. Remove the modelled component from this graph because you have no evidence that it is credible. Also show the pre-1993 observational data and stop pretending that the only observations that matter are those from 1993 onwards. [John McLean, Australia]	Figure redrawn
TS-440	TS	11	51	11	51	Explain what the blue dotted lines are. I assume they are 9 to 95% confidence limits around the tide gauge	figure redrawn

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						measurements [Ned Dwyer, Ireland]	
TS-441	TS	11	51	12	2	Part c of figure – what is the dashed line at approx 20mm? [Ned Dwyer, Ireland]	figure redrawn
TS-442	TS	11	52	11	52	Maybe change “contributions” to “componenets” to be consistent with the graphic or vice versa [Ned Dwyer, Ireland]	Figure redrawn
TS-443	TS	11	53		56	These sea-levels should all be GMSL. I think some more general consistency is needed [William Ingram, United Kingdom]	done
TS-444	TS	12	1	13	11	unforced climate variability - modellers' slang, not clear enough [Petra Seibert, Austria]	This section has been extensively rewritten
TS-445	TS	12	5	12	6	As with the main text, I found it odd that the absolute change in sea-level is never stated (except, oddly, when the RCP scenarios are discussed at TS-13-26) - rather, the discussion is always in terms of rate of change. I believe that the absolute changes should be stated at least once. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	now included
TS-446	TS	12	8	12	9	Note that ocean heat content shows a 10 years-long plateau since the onset of measurements by the ARGO buys, viz. a change of regime with respect to the 0.15 W/m2 increase during the pre-ARGO era. [François Gervais, France]	Incorrect
TS-447	TS	12	20	12	20	Please explain abbreviation AOGCM for non-experts, at least in glossary. [Government of Germany]	copy edit; "Atmosphere-Ocean General Circulation Model" is in the Glossary. Please note that a list of acronyms will be part of the Final Report
TS-448	TS	12	28	12	28	The phrase "anomalous regional variability" is pretty ambiguous--and is it really well-established that it is not in some way related to huamn activities, etc. I would think a bit better explanation is needed--or just say mostly to anthropogenic climate change or something simialr. [Michael MacCracken, United States of America]	To attribute it to anthropogenic warming would be incorrect. Statement retained
TS-449	TS	12	31	12	31	"predicting" or "projecting" warming? [Government of Germany]	corrected
TS-450	TS	12	38			“as a result” → “so”? [William Ingram, United Kingdom]	editorial
TS-451	TS	12	45	12	45	This is the first use of SLE – spell it out. Sea Level Elevation? SLR is used elsewhere. Are they the same thing? [Ned Dwyer, Ireland]	now spelt out
TS-452	TS	12	45			explain abbreviation "SLE" [Barbara Früh, Germany]	now spelt out
TS-453	TS	12	49	13	2	“Contributions”. Should the word components be changed in the figure for consistency? [Ned Dwyer, Ireland]	figure redrawn
TS-454	TS	12	55	12	55	Residual? Please explain jargon for non-experts. [Government of Germany]	figure redrawn
TS-455	TS	12	56	12	56	Pls check the consistence of the observed contribution of ice-sheets to SLR as mentioned here with the sum of the contributions of both polar ice-sheets as given at TS-11 line 21-24 [Government of Germany]	done
TS-456	TS	12	56	12	56	The observed ice sheet contributions given here are smaller than implied on page TS-11, lines 21-24. Check and update to 2010 or beyond. [David Parker, United Kingdom of Great Britain & Northern Ireland]	now consistent
TS-457	TS	13	4	13	4	1901-1990 or 1971-2010? What does this mean? [Government of Germany]	meaning clear - period retained
TS-458	TS	13	8	13	8	Please explain: A residual that is consistent to zero? [Government of Germany]	This section has been extensively rewritten
TS-459	TS	13	8	13	11	So, given this inability to reconcile over the 20th century, how is this difference of .5 mm/yr accounted for in the projections out into the future? Is an adjustment made, or, as for AR4, is the underestimate just ignored in projecting ahead? [Michael MacCracken, United States of America]	This is well within the uncertainty range.
TS-460	TS	13	8			"The residual of 0.5… is also consistent with zero,…." is not a very well chosen formulation [Barbara Früh, Germany]	rewritten
TS-461	TS	13	9			1930s not 1930's [Government of United Kingdom of Great Britain & Northern Ireland]	editorial
TS-462	TS	13	9			It is suggested to delete "a". [Klaus Radunsky, Austria]	editorial

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TS-463	TS	13	11	13	11	On what basis is the climate change during the 1930's "unforced climate variability"? I thought there were major contributions due to increasing solar radiation and decreased volcanic cooling--along with still rising CO2 and methane concentrations. That is all forced variability--not unforced. [Michael MacCracken, United States of America]	This is the component that is not as a result of theforced changes
TS-464	TS	13	11			please use consistent colour key across all 3 diagrams [Government of United Kingdom of Great Britain & Northern Ireland]	diagrams redrawn
TS-465	TS	13	15	13	16	In Greenland it is fast flowing glaciers and not ice shelves [European Union]	reworded
TS-466	TS	13	15	13	17	This sentence could be further clarified. Is it warming ocean waters that are causing the increased ice flow? [Government of Canada]	This section has been extensively rewritten
TS-467	TS	13	26	13	36	This paragraph is very important as it addresses an issue that was much discussed in the AR4 (dynamical contributions to projected SLR). Therefore, some slight revisions to improve the text here would be helpful. In particular, if the likely projected SLR for the different RCPs uses estimates of dynamical contributions from the literature where that literature is based on results form process-based (versus semi-empirical) models, this should be made clear. This would help avoid confusion about how these projections were made and would clarify the reference to higher projections from semi-empirical models on lines 33-34. [Government of Canada]	reworded. This should be clearer
TS-468	TS	13	26	13	36	With international negotiations being based on warming since preindustrial rather than warming since 1990, should not the same preindustrial reference period be used for sea level rise instead of just doing, essentially, the rise during the 21st century? [Michael MacCracken, United States of America]	The projections are for standard periods. I have sympathy for the comment and the last diagram of the chapter on sea level does just that.
TS-469	TS	13	33	13	33	There has been no projection by models of ice dynamics - but by assessment of the literature (page 13-45, line 18-19) [European Union]	reworded
TS-470	TS	13	33	13	33	What is the "likely range"? and how does it link with the AR5 uncertainty guidance notes? [Government of Germany]	The likely range is as outlined in the guidance note
TS-471	TS	13	34	13	36	The opening sentence of this paragraph is too general and doesn't need to be included. [Government of United Kingdom of Great Britain & Northern Ireland]	Not sure what this refers to.
TS-472	TS	13	41			are extreme weather and extreme climate the same phenomena or are they different? [David Sauchyn, Canada]	Taken into account: the glossary definition clarifies the slight difference between the two such that "When a pattern of extreme weather persists for some time, such as a season, it may be classed as an extreme climate event, especially if it yields an average or total that is itself extreme".
TS-473	TS	13	52	13	53	Globally, there is medium confidence that the length of warm spells, including heat waves, has increased since the middle of the 20th century.' Most people would not know the difference between a heat wave and a warm spell so suggest to delete heat wave since it is a warm spell but in the summer season or add (IN SUMMER SEASON) between brackets rigth after heat waves in the sentence. NB from the glossary: Period of several consecutive High temperature days/nights using a fixed or percentile-based threshold. Can be classified within just the summer season (heat waves) or can define any unusually warm period at any time of the year. [Line van Kesteren, the Netherlands]	Taken into account: As suggested by the reviewer the definitions for warm spells and heatwaves are defined in the glossary so we don't feel that it is necessary to duplicate the description here.
TS-474	TS	13	52	13	53	Globally, there is medium confidence that the length of warm spells, including heat waves, has increased since the middle of the 20th century.' Most people would not know the difference between a heat wave and a warm spell so suggest to delete heat wave since it is a warm spell but in the summer season or add (IN SUMMER SEASON) between brackets rigth after heat waves in the sentence. NB from the glossary: Period of several consecutive High temperature days/nights using a fixed or percentile-based threshold. Can be classified within just the summer season (heat waves) or can define any unusually warm period at any time of the year. [Line van Kesteren, the Netherlands]	Taken into account: As suggested by the reviewer the definitions for warm spells and heatwaves are defined in the glossary so we don't feel that it is necessary to duplicate the description here.
TS-475	TS	14	4	14	4	Hydrological in "hydrological drought" should be deleted. (scientific comments) [Qingxiang Li, China]	Accepted: Text amended accordingly
TS-476	TS	14	4	14	5	Note that this assessment had already been revised in the SREX (see chapter 3, page 170 of that report, first	Taken into account: Text has been amended

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						paragraph under "Observed changes" in section 3.5.1.). [Sonia Seneviratne, Switzerland]	accordingly
TS-477	TS	14	4	14	9	I'm confused by the analysis being done. With wet areas becoming wetter and dry areas drier, I think there needs to be a clearer discussion what the phrasing means of no trend in droughts. I guess I am wondering if it is realy right to be talking about global trends rather than trends in drying areas--presumably, one would not be getting increased drought in areas becoming wetter, so why should those areas be included in the analysis, etc. I just don't think a clear message is being conveyed here and with other statements about what is happening. Same comment about the flood trends--I would think there needs to be a subdivision into those areas becoming wetter and those drier. Given the importance of saying there is a difference from earlier, I think this needs to be done with great clarity--rechecking, perhaps, how the statements in AR4 were derived. [Michael MacCracken, United States of America]	Taken into account: The text on droughts and qualification of global and regional trends is now more clearly defined in this section and TFE 9. Regarding the assessment of floods, this falls much more within the scope of WGII and therefore is not covered in detail in WGI. WGII will assess floods in more regional detail accounting for the fact that trends in floods are strongly influenced by changes in river management and not solely driven by changes in climate.
TS-478	TS	14	4			Could drop 1st 4 words [William Ingram, United Kingdom]	Editorial
TS-479	TS	14	5	14	7	I am not convinced that the general assessment of "low confidence" is justified for observed changes in droughts. As assessed in the IPCC SREX (see in particular chapter 3, Seneviratne et al. 2012), although there is low confidence in drought trends in several regions, there are nonetheless a number of regions which are consistently identified as having experienced either drying (southern Europe, West Africa) or wetting (central North America, northwestern Australia) trends independently of the index or datasets' choice. As recently discussed in Seneviratne (2012, Nature), the location of these regions is confirmed even in the more recent analysis of Sheffield et al. (2012), which evaluated the sensivitiy of historical drought trends to different input datasets and model parameterizations. It would be important to distinguish between the _low confidence_ in global trends vs _medium confidence_ in some regional trends (note that "medium confidence in some regions, low confidence elsewhere" is still a revision from the AR4 assessment). The AR4 focused on global changes in drought, which is not really meaningful, since there are both regions with increasing and decreasing drought (see also Sheffield et al. 2012). References: 1) Seneviratne, S.I., N. Nicholls, D. Easterling, C.M. Goodess, S. Kanae, J. Kossin, Y. Luo, J. Marengo, K. McInnes, M. Rahimi, M. Reichstein, A. Sorteberg, C. Vera, and X. Zhang, 2012: Changes in climate extremes and their impacts on the natural physical environment. In: Managing the Risks of Extreme Events and Disasters to Advance Climate Change Adaptation [Field, C.B., V. Barros, T.F. Stocker, D. Qin, D.J. Dokken, K.L. Ebi, M.D. Mastrandrea, K.J. Mach, G.-K. Plattner, S.K. Allen, M. Tignor, and P.M. Midgley (eds.)]. A Special Report of Working Groups I and II of the Intergovernmental Panel on Climate Change. 2) Seneviratne, S.I, Nature, 491, 338-339. 3) Sheffield, J., E.F. Wood, and M. Roderick, 2012, Nature, 491, 435-438, doi:10.1038/nature11575. [Sonia Seneviratne, Switzerland]	Taken into account: See response to TS-1306
TS-480	TS	14	5			Omit “dryness” in favour of its explanation [William Ingram, United Kingdom]	Editorial
TS-481	TS	14	11	14	16	I'm confused. Right after a sentence saying there is no confidence in any trends is a statement saying there is a very robust trend in Atlantic cyclones, etc. Very confusing.And is there any information on the amount of rainfall associated with tropical cyclones, how long the storms are persisting (so tropical cyclone days). how much further the storms may go due to the warming of ocean waters, etc.? On the other trends, is there evidence of shifts on location even it there is no change in total number, etc.? [Michael MacCracken, United States of America]	Taken into account: this section has been rewritten based on some changes to the underlying text and to make it clearer what the confidence levels are reagarding changes in tropical cyclones on a global scale versus our confidence on regional levels.
TS-482	TS	14	20	14	22	Mean significant wave height has likely increased since the 1950s over much of the mid-latitude North Atlantic and North Pacific, with winter season trends of 8 to 20 cm per decade, although confidence is limited by the lack of observations.' How can something be likely when there is limited confidence? Should this not get a lower likelihood? According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: Assign a likelihood for the event or outcomes, for which confidence should be “high” or “very high”. So it seems inappropriate to assign likely to limited confidence. [Line van Kesteren, the Netherlands]	Accepted: Text amended accordingly:There is medium confidence based on reanalysis forced model hindcasts and ship observations that mean significant wave height has increased since the 1950s over much of the North Atlantic north of45°N, with typical winter season trends of up to 20 cm per decade.
TS-483	TS	14	20	14	22	Mean significant wave height has likely increased since the 1950s over much of the mid-latitude North Atlantic and North Pacific, with winter season trends of 8 to 20 cm per decade, although confidence is limited by the lack of observations.' Limited confidence? According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: A level of confidence is expressed using	Accepted: Text amended accordingly:There is medium confidence based on reanalysis forced model hindcasts and ship observations that mean significant wave height has increased since the 1950s over much

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						five qualifiers: “very low,” “low,” “medium,” “high,” and “very high.” So change limited in ''very low,” “low,” “medium,” “high,” or “very high'' confidence. [Line van Kesteren, the Netherlands]	of the North Atlantic north of45°N, with typical winter season trends of up to 20 cm per decade.
TS-484	TS	14	20	14	22	Mean significant wave height has likely increased since the 1950s over much of the mid-latitude North Atlantic and North Pacific, with winter season trends of 8 to 20 cm per decade, although confidence is limited by the lack of observations.' How can something be likely when there is limited confidence? Should this not get a lower likelihood? According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: Assign a likelihood for the event or outcomes, for which confidence should be “high” or “very high”. So it seems inappropriate to assign likely to limited confidence. [Line van Kesteren, the Netherlands]	Accepted: Text amended accordingly:There is medium confidence based on reanalysis forced model hindcasts and ship observations that mean significant wave height has increased since the 1950s over much of the North Atlantic north of45°N, with typical winter season trends of up to 20 cm per decade.
TS-485	TS	14	20	14	22	Mean significant wave height has likely increased since the 1950s over much of the mid-latitude North Atlantic and North Pacific, with winter season trends of 8 to 20 cm per decade, although confidence is limited by the lack of observations.' Limited confidence? According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: A level of confidence is expressed using five qualifiers: “very low,” “low,” “medium,” “high,” and “very high.” So change limited in ''very low,” “low,” “medium,” “high,” or “very high'' confidence. [Line van Kesteren, the Netherlands]	Accepted: Text amended accordingly:There is medium confidence based on reanalysis forced model hindcasts and ship observations that mean significant wave height has increased since the 1950s over much of the North Atlantic north of45°N, with typical winter season trends of up to 20 cm per decade.
TS-486	TS	14	20	14	25	Please gives changes either in [cm] or in [%], or both for each region/period. [Government of Germany]	Accepted: Text amended accordingly
TS-487	TS	14	21	14	21	The trends of 8 to 20cm per decade appear to be in the North Atlantic with smaller trends in the North Pacific in Wang et al. (2009) cited in Chapter 3. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted: Text amended:There is medium confidence based on reanalysis forced model hindcasts and ship observations that mean significant wave height has increased since the 1950s over much of the North Atlantic north of45°N, with typical winte
TS-488	TS	14	27		16	TS.2.8: unsuitable title. The section describes CO2, pH and CH4 (all carbon) and the N cycle is limited to 4 lines and to N2O emissions. [European Union]	rejected. The title carbon and other biogeochemical cycle encompass all
TS-489	TS	14	31	14	31	…also, here emissions are quoted in PgC where elsewhere in TS and relevant chapters GTC is often used. Can consistent units be applied throughout? [Government of United Kingdom of Great Britain & Northern Ireland]	partly accepted. In the relevant literatur cited, the uni mostly used in Pg C
TS-490	TS	14	31	14	31	reference is made to C8.1 to demonstrate that AF is a 'better indicator of GMT response and is therefore emphasised in AR5. However, in C8 and elsewhere in the AR5 most discussion and most, if not all graphics remain about RF, not AF. This makes the statement difficult to credit. [Government of United Kingdom of Great Britain & Northern Ireland]	rejected. It is not clear what the reviewer is talking about here. This section does not talk about AF or C8.1??
TS-491	TS	14	31	14	35	Not very clear. Can we use a tidied up version of SPM p6 3-5 - i.e. Something that simply sums it up as e.g. 40% increase in atmos conc. of CO2 since 1750 with a quarter of that occurring since 1990. [Government of United Kingdom of Great Britain & Northern Ireland]	rejected. There is no point in duplicating the very basic information in the SPM. The TS is intended to provide more detail than the short SPM.
TS-492	TS	14	31		32	Delete 'CO2 emissions from' and insert 'of CO2' at the end of the sentence. [Nathan Gillett, Canada]	rejected. It is better to clarify what types of emissions are covered and which emissions are not covered.
TS-493	TS	14	34	14	34	is there a value for average emissions in 90's? [Luisa Cristini, United States]	yes, it can be estimated from figure TS2 in this section or is given in chapter 6.
TS-494	TS	14	35	14	35	check if 2012 which is still to be finalised is the right year. [Government of Germany]	accepted. It has been corrected to 2010
TS-495	TS	14	42	14	43	Land use change since when? Can a date be given or is it since forever? [Ned Dwyer, Ireland]	taken into account. This whole section is talking about changes between 1750 and 2010.
TS-496	TS	14	42	14	43	Land use change (mainly deforestation), derived from land cover data and modelling, is estimated to have released 180 ± 80 PgC.' For which years? Is this between 1750 and 2010? [Line van Kesteren, the Netherlands]	Yes, it is for 1750 to 2010
TS-497	TS	14	42	14	43	Land use change (mainly deforestation), derived from land cover data and modelling, is estimated to have released 180 ± 80 PgC.' For which years? Is this between 1750 and 2010? [Line van Kesteren, the	Yes, it is for 1750 to 2010

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						Netherlands]	
TS-498	TS	14	43	14	43	Since when have these 180 PgC been released? [Government of Germany]	taken into account. This whole section is talking about changes between 1750 and 2010.
TS-499	TS	14	49	14	49	Of the 545 ± 85 PgC released to the atmosphere from fossil fuel and land use emissions,' Land use emissions is the same as Land Use Change? If so, please use the same terms in the tekst. [Line van Kesteren, the Netherlands]	Rejected. Emissions here refers to both fossil fuel and land use so it would not be appropriate to use change in this case.
TS-500	TS	14	49	14	49	Of the 545 ± 85 PgC released to the atmosphere from fossil fuel and land use emissions,' Land use emissions is the same as Land Use Change? If so, please use the same terms in the tekst. [Line van Kesteren, the Netherlands]	Rejected. Emissions here refers to both fossil fuel and land use so it would not be appropriate to use change in this case.
TS-501	TS	14	49		50	The quoted uncertainty on the change in atmospheric CO2 doesn't seem to be to 'very high accuracy'. [Nathan Gillett, Canada]	noted. The high accuracy is refering to the accumulation in the atmosphere not the emissions.
TS-502	TS	14	50	15	52	Presumably these are global-mean values for CO2 concentration? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	noted. Yes
TS-503	TS	14	51	14	51	Comparison of atmospheric CO2 concentrations in 1750 with a specific value for January 2011 is inappropriate, Due to the seasonal variability of atmospheric CO2 concentrations an annual mean value needs to be given. Furthermore, it is unclear if the given concentration of January 2011 is a global mean value or refers to an observation at a given measuring site [European Union]	noted. The value is a global mean value.
TS-504	TS	14	51	14	51	atmospheric CO2 concentration from 278 ± 3 ppm6 in 1750 to 390.7 ppm in January 2011.' in the underlying material the amount of 392 ppm in 2012 is mentioned. Make these numbers as recent / up-to date as possible. [Line van Kesteren, the Netherlands]	noted. The text has been modified to make the dates more consistent.
TS-505	TS	14	51	14	51	atmospheric CO2 concentration from 278 ± 3 ppm6 in 1750 to 390.7 ppm in January 2011.' in the underlying material the amount of 392 ppm in 2012 is mentioned. Make these numbers as recent / up-to date as possible. [Line van Kesteren, the Netherlands]	noted. The text has been modified to make the dates more consistent.
TS-506	TS	14	51	14	52	The following wording is suggested: CO2 emissions grew by 4.0 PgC yr-1. The sentence as it stands does not make sense. [Klaus Radunsky, Austria]	rejected. The emissions did not grow by 4, the amount of CO2 in the atmosphere grew by 4.
TS-507	TS	14	52	15	4	Approx. 15-20% of global CO2 emissions are due to land use change emissions (tropical deforestation). However, the effect on atmospheric CO2 increase in low latitudes is low due to the rapid mixing of the atmosphere in these latitudes. Atmospheric CO2 concentration differences between the Northern and the Southern Hemisphere would also exist without anthropogenic emissions, since land to sea ratios are largely different (with terrestrial systems having a s trong seasonality of net uptake [summer] versus net emission [winter]). Thus, attributing hemispheric differences in atmospheric CO2 concentrations to anthropogenic emissions alone might be somehow misleading [European Union]	rejected. In the preindustrial the interhemispheric gradient was the opposite of today so it is apporpriate to say that the gradient shows that the source of the emissions is primarily in the northern hemisphere.
TS-508	TS	14				Footnote 6, & further on! "ppm (parts per million) or ppb (parts per billion, 1 billion = 1,000 million) is the ratio of the number of greenhouse gas molecules to the total number of molecules of dry air." is of course horribly untrue. In reality "ppm" is either ppmm or ppmv, and should always be explicitly so written to avoid the possibility of horrible confusion by someone from outside that particular field – but here particularly! [William Ingram, United Kingdom]	rejected. We believe that this is the most simple way to explain and getting into whather it is by volume or mass would confuse the reader even more.
TS-509	TS	15	1			A stronger statement including confidence language is warranted, especially given multiple independent lines of evidence. [Government of United States of America]	noted. It is unclear what statement the reviewer is requesting confidence language for. If it is about the increase in CO2 gradients, we feel that stating it as fact without confidence language is the strongest way to make the point.
TS-510	TS	15	5	15	5	Please give more information on O2 decrease. [Government of Germany]	noted. The TS contains the full text on oxygen from the Ch3 exec summary.
TS-511	TS	15	6	15	6	Shortly provide reasoning why the decrease in the isotopic ratio of CO2 is evidencing anthropogenic CO2	noted. This is fully explained in chapter 6.

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						emissions [European Union]	
TS-512	TS	15	6	15	6	Please explain the C-isotopic ratio for non-experts, at least in the Glossary. [Government of Germany]	"13C" and "12C" are in the Glossary with an explanation of the C-isotopic ratio.
TS-513	TS	15	10	15	10	It isn't clear in Chapter 3 how nutrient data have been used to estimate the anthropogenic carbon inventory. [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted text have been modified
TS-514	TS	15	18	15	18	13C/12C stable isotope ratio in N2O? Delete N2O here and put N2O to the right place. The Figure descriptions are incomplete, so that the Figure cannot be understood. E.g. for temporal trends of CH4 and N2O: which stations are given here? It would be much better to work with headings in the graph or to use a) b),, to explain the individual panels. Abbreviations given in the Figure caption are not given in the graphs and vice versa. [European Union]	accepted text have been modified
TS-515	TS	15	18	15	22	Graphic needs labels a-d to make it easier to interpret. [Ned Dwyer, Ireland]	accepted text have been modified
TS-516	TS	15	19	15	19	"…recorded over recent decades…". [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted text have been modified
TS-517	TS	15	20			figure caption Figure TS.3: the acronyms don't fit the caption [Andrea Flossmann, France]	accepted text have been modified
TS-518	TS	15	27	15	28	Please give more information on O2 decrease. [Government of Germany]	noted. The TS contains the full text on oxygen from the Ch3 exec summary.
TS-519	TS	15	30	15	37	The term "natural terrestrial ecosystems" is misleading here. Much of the terrestrial C sequestration is occurring in plantations (North America/ Europe/ China) (e.g. neither Europe, East coast US nor China have natural terrestrial forest ecosystems which were not affected by land use change at some time) (see also statement in line 34). Large scale afforestation (China), re-growth (North-America) and improved forest management/ ceasing of overexploration (Europe) has contributed to increased rates of C sequestration in natural and semi-natural (plantations) ecosystems, besides the effects of CO2 and N deposition [European Union]	taken into account. The text in this paragraph has been substantially revised. It still refers to natural terrestrial ecosystems (those not affected by land use change), but the text later in the paragraph on how disturbed systems have changed has been removed to eliminate the confusion.
TS-520	TS	15	36	15	37	revise sentence: …gets stored as organic matter in soil and plant carbon pools with different turnover times --> not all soil carbon pools are long-lived. [European Union]	rejected. While it is true that not all soil carbon pools are long-lived, the sentence says that the carbon pools range from short-lived to long-lived. The pools in brackets are just examples and many soils are long-lived carbon pools.
TS-521	TS	15	39	15	39	TS.2.8.2 Carbon and Ocean Acidification' A better explanation of how CO2 causes the ocean acidification would be welcome. At least in the underlying material including chemical reaction formulas explaining this system. [Line van Kesteren, the Netherlands]	rejected the explanation is given in Ch3, Ch6, and in Ch3 FAQ 3.2
TS-522	TS	15	41	15	41	There is very high confidence that oceanic uptake of anthropogenic CO2 has resulted in gradual acidification...' - the confidence here is very high and this is a very important statement to get right I would therefore ask the authors to check this level of confidence. Are the time-series long enough, do they cover spatial variations account for seasonal, interannual decadal variations etc. The phrase in the SPM p6 lines 30-33 does include the time period. [Government of United Kingdom of Great Britain & Northern Ireland]	noted.
TS-523	TS	15	41			replace "cause" with "explanation" [Government of United Kingdom of Great Britain & Northern Ireland]	noted
TS-524	TS	15	43	15	43	'... Observed pH trends range between -0.015 and -0.024 per decade.' should include a word or two on the time period that this covers. [Government of United Kingdom of Great Britain & Northern Ireland]	accepted: text changed to: It is very likely that oceanic uptake of anthropogenic CO2 results in gradual acidification of the ocean. The pH of seawater has decreased by 0.1 since the beginning of the industrial era, corresponding to a 26% increase in hydrogen ion concentration. The observed pH trends range between –0.0014 and –0.0024 per year in surface waters.
TS-525	TS	15	47	15	47	The paragraph on CH4 does not mention the scale of release from permafrost and marine sediments	noted. These topics are covered in detail in chapter 6

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						(although this is discussed on page 30) or of CH4 release related to coal seam gas fracking. [Andrew Glikson, Australia]	
TS-526	TS	15	47	15	47	The paragraph on CH4 does not mention the scale of release from permafrost and marine sediments (although this is discussed on page 30) or of CH4 release related to coal seam gas fracking. [Government of Australia]	noted. These topics are covered in detail in chapter 6
TS-527	TS	15	52	15	56	OH changes' should be 'OH increases' or, more general, 'CH4 lifetime decreases'. The other arguments relate to decreasing emissions with defined sign. Because OH changes can not include OH decreases to explain a reduced growth, the sign of required change in OH should be made clear. [Michiel van Weele, Netherlands]	accepted. The text was significantly revised to eliminate this confusion
TS-528	TS	15	56			These 2 sentences appear inconsistent. The anthropogenic sources are bigger and have more inter-annual variation than the natural ones. [Government of United Kingdom of Great Britain & Northern Ireland]	accepted. The text was significantly revised to eliminate this confusion
TS-529	TS	16	2	16	5	This section is surprisingly short. It must be possible today something about the sources of N2O and how they re changing [Government of United Kingdom of Great Britain & Northern Ireland]	noted. Nitrogen is described more fully in chapter 6
TS-530	TS	16	2	16	8	The CH4 emissions uncertainties seem surprisingly skewed (eg, for anthropogenic emission, the best estimate is 338 Tg, with an uncertainty ranging from 60 Tg less to only 6 Tg more… [Government of United States of America]	accepted. The text was significantly revised to eliminate this confusion
TS-531	TS	16	2	16	22	The report offers no credible evidence whatsoever to support your claim. This is suppposed to be a scientific report, and science requires evidence. [John McLean, Australia]	rejected. This is discussed in greater detail, including the lines of evidence, in chapter 6.
TS-532	TS	16	3	16	6	it is unclear where the number of 206 TgCH4yr-1 comes from. 338 Tg are the human emissions, 160Tg are from natural wetland, the difference is 178Tg and not 206Tg? Please clarify. [Government of Germany]	accepted. The text was significantly revised to eliminate this confusion
TS-533	TS	16	4	16	5	Bracket “)” missing somewhere [Ned Dwyer, Ireland]	accepted. The text was significantly revised to eliminate this problem
TS-534	TS	16	4	16	6	The overall anthropogenic and natural emissions differ from Table 6.7 and have very asymmetric uncertainties relative to the given most likely value. Change "with" to "which have" in line 6. [David Parker, United Kingdom of Great Britain & Northern Ireland]	accepted. The text was significantly revised to eliminate this confusion
TS-535	TS	16	5	16	5	Rank in importance: ruminant animals, rice paddies, waste… [European Union]	rejected. The importance is very regionally dependent
TS-536	TS	16	5	16	5	The situation with methane is described in TS.2.8.3 with dominant sources identified. However, some of the sources are consequential (e.g. melting of the permafrost represented in this section as emissions from natural wetlands) while others are primary (e.g. ruminant animals). (1) causes should be separated from effects; (2) non-ruminant animals (e.g. chickens and pigs) also generate methane through the manure they produce, they should either be included under a broad category of "farm animals" or alternatively animal farm waste should be explicitly stated as a source of anthropogenic GHG; (3) wetlands should be given in categories whith permafrost methane emissions explicitly stated. [Dora Marinova, Australia]	rejected. There was not space for this level of detail in the technical summary. These details are provided in chapter 6
TS-537	TS	16	6	16	6	“Anthropogenic emissions are dominant over natural sources with emissions of 206 TgCH4 yr–1 “ This sentence is confusing and it should be made clearer that the 206 refers to natural sources. Reword as Anthropogenic emissions dominate natural sources which amount to 206 TgCH4 yr–1 Or …the latter amounting to 206 TgCH4 yr–1 [Ned Dwyer, Ireland]	accepted. The text was significantly revised to eliminate this confusion
TS-538	TS	16	7	16	8	troposphere instead of atmosphere. The following sentence on changes in tropospheric OH concentrations cannot be understood by readers. This sentence states that the atmospheric lifetime of CH4 was likely not affected since OH concentration remained stable. Improve wording to reflect this [European Union]	accepted. The text was significantly revised to eliminate this confusion
TS-539	TS	16	7	16	8	The statement that 'OH changes remained within 5% in the period 2000–2009' is made without remark that this is a very loose constraint. I suggest to add a sentence: The 5% is a rather loose constraint and implies that the interannual variability in the small CH4 annual growth rate over this period could be explained equally	accepted. The text was significantly revised to eliminate this confusion

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						by short-term CH4 emission anomalies, CH4 lifetime anomalies, or any combination of both. [Michiel van Weele, Netherlands]	
TS-540	TS	16	10	16	15	To be consistent with the sectins on CO2 and CH4 can it say more about sources of N2O? [Ned Dwyer, Ireland]	there was not enough room to add more detail
TS-541	TS	16	10			This section should be headed 'N2O' for consistency with the previous section 'CH4'. No other nitrogen compounds are discussed here. [Nathan Gillett, Canada]	noted.
TS-542	TS	16	12	16	13	It would be instructive to include a list of forcings where AF is considered a better indicator that RF. [Government of United Kingdom of Great Britain & Northern Ireland]	there was not enough room to add more detail
TS-543	TS	16	12	16	15	This discussion of N2O is not very informative. Suggest at least mention be made of the cause of the rise in atmospheric N2O, and perhaps some statement about whether or not the global N2O budget is well understood. Section 6.3.4 (N2O budget) should be referenced here as well as section 6.4.6. [Government of Canada]	there was not enough room to add more detail
TS-544	TS	16	12	16	15	This is a rather weak/confusing paragraph. Which other N gas is long-lived and has such an effect on the radiative balance as N2O (first sentence?). "Changes in the nitrogen cycle": mankind has doubled the global nitrogen cycle and increases in atmospheric N2O are mostly due to increased use of organic and inorganic fertilizers for feed and food production and emissions of NOX from combustion processes (and re-deposition and microbial conversion with N2O being a side product). This should be clearly stated. Also increases in oceanic N2O emissions are a result of N export to the sea and thus, finally origin form N fertilizer use [European Union]	there was not enough room to add more detail
TS-545	TS	16	12	16	15	should it say "mostly due to the difficulty of fully accounting for..." [Government of United Kingdom of Great Britain & Northern Ireland]	rejected. Unclear what is being asked here.
TS-546	TS	16	13	16	14	"strongly influenced" - I think this is an overstatement. H2O and CO2 certainly exert a strong influence but the contribution of N2O is rather modest. The confusion here may be that the text is referring really to the radiative forcing rather than the wider "radiative properties" but even then "strongly" feels too strong [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	noted
TS-547	TS	16	41	17	6	Re Box TS.1: Radiative Forcing and Adjusted Forcing - fine to have a box explaining this, but what's needed even more is a box to explain the essential difference between the approach used for generating projections used in AR5 and with the method used in previous ARs - not just a table of results, as is presented later in the report (Table TS.1, wherever that will eventually sit). The uninformed reader needs to know what has been excluded/included and why the RCP projections are more scientifically robust. [Government of United Kingdom of Great Britain & Northern Ireland]	Not obviously appropriate for drivers section as I think they're asking about how the scenarios were constructed. Not sure where this goes (TSU?)
TS-548	TS	16	52	16	53	I think this wording should be altered to indicate that the transition from RF to AF is as a result of improved understanding rather than prior negligence! The earlier assessments were always clear that RF has limitations, but there was nothing obviously better [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Agreed, revised.
TS-549	TS	16	54	16	54	check use of comma in this sentence [Luisa Cristini, United States]	Revised, no longer relevant.
TS-550	TS	16	54	16	56	I do not believe there is a single methodology or definition of the AF - the fixed-SST appoach is one approach, but the inclusion of land surface temperature changes is a limitation. The alternative is the Gregory regression method which side-steps the limitation [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	True, rephrased to include regression and fixed-SST methods.
TS-551	TS	16	54	16	56	It is unsatisfactory to imply, as the text does, that the AF is universally applicable. It is not, for the simple reason that many of the forcings that IPCC deal with (for example in the GWP tables) are too small for them to be used in the AF approach and get a statistically significant signal (without enormous scaling and likely unjustified assumptions about linearity). In addition, the computational expense and model dependence of AF calculations needs to be recognised. Without more careful wording, the reader may also be mystified by the frequent discussion of RF in the coming pages. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Agreed, revised.

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TS-552	TS	16	55	16	57	Again, I believe it unsatisfactory to imply that previous IPCC assessments had not discussed the impact of aerosol on cloud microphysics - they certainly had done, within a heavily caveated use of RF. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Revised, no longer relevant.
TS-553	TS	16	56			"aerosols" is wrong, it is either "aerosol" or "aerosol particles"; aerosol is a suspension of fine solid particles or liquid droplets in a gas (see e.g., http://en.wikipedia.org/wiki/Aerosol) [Barbara Früh, Germany]	Aerosols is the conventional usage in the field.
TS-554	TS	16				same comment as SPM p7 line22: Drivers should be a part of Understanding [Government of France]	Of course these are related, but we feel the current separation is helpful for the reader.
TS-555	TS	17	3	17	3	"has been shown to be a better indicator" - it is certainly true that it has been shown to be a better indicator in climate models - we dont know about the real world. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	True, but as models are the only tool we have to assess this, we don't see this is a point to emphasize here.
TS-556	TS	17	3	17	4	This is a useful directive on using the new AF concept but seems to be at odds with the statement in Chapter 8 p.3 line 19-20 'The total AF value is weaker than total RF and has greater uncertainty due to its inclusion of additional impacts on clouds'. [Government of Australia]	We believe these are consistent. The ERF is weaker due to aerosol indirect effects, and as these are actual effects using the weaker value should give a better indication of the climate response. The greater uncertainty in the metric does not lead to greater uncertainty in the response as the ERF simply incorporates some of the uncertainty in the response into the metric (but it's likewise there for the response to RF).
TS-557	TS	17	3	17	4	Well mixed greenhouse gases have not been defined yet at this point in the TS. [Government of United Kingdom of Great Britain & Northern Ireland]	corrected
TS-558	TS	17	3	17	4	It seems odd to state here that AF is preferable to RF in some cases and then move into a series of paragraphs that use RF exclusively. [Dian Seidel, United States of America]	Agree, text in box revised.
TS-559	TS	17	4	17	4	not clear what the word 'continuously' means in this context - does it mean monotonically, or from start to finish (7,000BP to 1750AD) or something else? [Government of United Kingdom of Great Britain & Northern Ireland]	We do not find the word 'continuously' in the Drivers section of the TS.
TS-560	TS	17	11	17	27	Tell the reader the full story about CO2-driven warming, ie. how the theoretical relationship is logarithmic and the warming will be less and less for each incremental unit of CO2. [John McLean, Australia]	This is well-known and documented already in this and previous IPCC reports.
TS-561	TS	17	11	17	58	The only reference to Fig TS.4 is on line 16 with respect to tightly constrained values for CO2 forcing. Should this Figure not be referenced for much of the other material in this section (RF values for various forcing agents)? In addition, it would be very helpful if, similar to the AR4 Fig. TS.5, the RF values as reported on these lines were also provided in Figure TS.4 so the reader isn't left to try to estimate them off the graph. [Government of Canada]	The statement about historical data providing tightly constrained values refers to all the WMGHGs, not just CO2. Additional reference added in ozone discusison (already there for aerosols). All values given in text, so no need to read off graph.
TS-562	TS	17	12	17	12	I would suggest adding "and cloud micophysics" in the parentheses with "atmospheric chemistry" [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Agreed, revised.
TS-563	TS	17	13	17	14	define WMGHG in line 13 not 14 [Luisa Cristini, United States]	corrected
TS-564	TS	17	13	17	14	Again, short-lived GHGs have not been defined yet. [Government of United Kingdom of Great Britain & Northern Ireland]	Agreed, revised.
TS-565	TS	17	13			explain abbreviation "WMGHG" at first occurence (lines 13) not at second (line 14) [Barbara Früh, Germany]	corrected
TS-566	TS	17	24	17	24	"slighly less than 0.3 +- 0.03" does not seem to make sense: Is "slightly less" within the uncertainty interval of +-0.3 or how does it relate to it? [Government of Germany]	Agreed, revised.
TS-567	TS	17	25			replace "reduction" with "removal" [Government of United Kingdom of Great Britain & Northern Ireland]	Again, we do not see this word near this line.
TS-568	TS	17	30			Figure TS.4: The text in Box TS.1 suggests that AF is a more appropriate metric for warming - for long-lived GHGs it provides similar values to RF, and for short-lived climate forcers it provides a better relationship to	Figures revised as suggested. The development of AF (now ERF) in the RCPs is discussed in both ch 8 and

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						temperature response. In this case, and if the authors wish AF to be the more widely-used metric in future, I would have the AF bars in the top panel of the figure with a solid fill and the RF bars with the hatched fill. Similarly in the bottom panel I would use solid lines for AF and dashed lines for RF. It is the AF data that ought to be emphasised. There should also be a quantification somewhere of how AF develops in the RCPs, to demonstrate how similar/different it is to the develpment of RF. [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	ch 12. RF was not diagnosed for the RCPs in a comparable way for all agents, so is not completely comparable.
TS-569	TS	17	30			Figure TS.4: Also show in the top panel the individual contributions to the aerosol radiative forcing through interaction with radiation from sulphate, black carbon, organic carbon, nitrate and mineral dust. These abundance-based RF estimates should be presented in addition to the emission-based radiative forcing estimates shown in Figure TS.5. [Twan van Noije, Netherlands]	As the abundance based RF values are almost the same as the emissions based values in figure TS.5, we do not feel it provides enough additional information to warrant adding more complexity to figure TS.4
TS-570	TS	17	31	17	36	figure TS.4: in the figure it should be spelled out "clear air" for Ari and "clouds" for aci; is there a forcing due to variable cloudiness? [Andrea Flossmann, France]	These are not what the reviewer suggests, and what they actually are is spelled out in the caption.
TS-571	TS	17	31	17	36	You seem to have removed the statements about the level of scientific understanding that accompanied Figure TS4 when it appears in TAR. Be honest and show these statements. [John McLean, Australia]	This information is given in ch 8. Not everything can fit in the summary.
TS-572	TS	17	32	17	32	Forcing is by concentration change between 1750 and 2010. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Agreed, revised.
TS-573	TS	17	39	17	41	Clarification is needed regarding the scale of CH4 release from permafrost and marine sediments (although this is discussed on page 30) or CH4 release related to coal seam gas fracking. [Andrew Glikson, Australia]	We believe that in order to cover all the forcing agents consistently, if we provide details on methane sources we would have to do so for all the other gases and aerosols as well. As space does not permit this, we keep those discussions to the relevant chapters.
TS-574	TS	17	39	17	41	Clarification is needed regarding the scale of CH4 release from permafrost and marine sediments (although this is discussed on page 30) or CH4 release related to coal seam gas fracking. [Government of Australia]	See reply to TS-573
TS-575	TS	17	39	17	41	What drives CH4 concentration? This should be made explicit. [Dora Marinova, Australia]	See reply to TS-573
TS-576	TS	17	43	17	51	Please provide an overview table of the changes of concentration of atmospheric radiatively active gases. [Government of Germany]	Such a table is in the main text (Table 8.3) for the WMGHGs, but even leaving out the short-lived gases which cannot easily be summarized by a single value the table is too long for the TS in our opinion.
TS-577	TS	17	45	17	45	perhaps better "phase-out of EMISSIONS of this chemical" [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Agreed, revised.
TS-578	TS	17	53	17	53	Maybe the term "near-term climate forcers (NTCFs)" should be used instead (to be consistent with chapter 8). [Jan Fuglestvedt, Norway]	This is a narrower discussion on only O3 and H2O. Revised to clarify this.
TS-579	TS	17	53	17	53	Section TS.4.8 From Global to Regional: This section, which discusses Figure TS.9, does not mention or discuss the panels showing precipitation time series. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted. Figure has been revised and does no longer include the precipitation panels (Note: comment should be for p-32ff)
TS-580	TS	17	53	17	55	Recently we have compared the ozone values measured at NARL, Gadanki (a rural environment) during 2010-2011 with that measured during 1993-96 period and we do not find any increase in the value. This indicate that the statement in the report that "increases have continued mainly over Asia" is not true for at least Gadanki a rural location in India that can be considered "undisturbed background location", instead the increase has flattened like over Europe during the last decade. [Government of India]	Several published peer-reviewed studies show increases over this time in Asia. IPCC rules do not allow us to cite this Indian data without a peer-reviewed journal paper, but we have added a comment that observations cover a limited area to highglight that ozone may not be increasing everywhere in Asia.
TS-581	TS	17	53	17	55	The surface level ozone concentration measured during the 2010-11 period at the National Atmospheric Research Laboratory, Gadanki (a rural environment in Andhra Pradesh state) has been compared with that measured during 1993-96 period (by the PRL group) at the same location and using the same technique. It is	See reply to TS-580

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						observed that there is no appreciable variation in the concentration levelsover this period of more than a decade. As the surface level ozone concentration is highly influenced by manmade activities it can be concluded that at least in rural India there is no detectable influence of any increased manmade influence on ozone concentration. (Published during the 39th COSPAR Assembly, July 2012). [Government of India]	
TS-582	TS	17	53		54	The text says that ozone has been increasing 'in the 1990s'. Does this mean during the 1990s? Or in the 1990s relative to some earlier period? Or between the 1990s and present? [Nathan Gillett, Canada]	Clarified.
TS-583	TS	17	58	17	58	What does "species" mean in this context? This may be a difficult term to reconcile in this context for non-scientific readers. [Government of Canada]	Clarified.
TS-584	TS	18	6	18	7	Revise sentence: "This reduced uptake leads to a reduced sink strength of terrestrial ecosystems for atmospheric CO2" [European Union]	The suggested revision seems to us to be a tautology as reduced uptake is equated to a reduced sink. We maintain the sentence we had which states how the reduced uptake then results in increases in the atmospheric concentration.
TS-585	TS	18	6	18	7	The discussion of the impact of NOx on ozone and hence CO2 is interesting, but earlier in the TSM (TS-15-33) the role of nitrogen deposition on increasing CO2 uptake over land was discussed. Is it known/obvious that the impact of NOx on the CO2 budget via ozone damage is greater than its impact via fertilisation? If it is not, I would delete this from the TSM. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	We agree that for Nox, which can fertilize but also leads to ozone that damages plants, the effects are complex. This discussion, however, refers to all the ozone precursors, and is robust for CO, VOCs, and CH4 that do not fertilize plants, and hence the sign of their impact is not in doubt, so we maintain this here.
TS-586	TS	18	7	18	9	How can the value of CO2 RF attributed to lower vegetation CO2 uptake caused by tropospheric ozone concentrations be well known at the same time as the quantitiative estimates have low confidence? [Government of NORWAY]	Clarified that the forcing that's well known is the CO2 forcing, not the portion attributed to physiological response to O3.
TS-587	TS	18	8	18	8	The readers may find this complicated. In order to clarify I suggest adding "and its precursors" after "ozone", and "of CO2" after "direct emissions". [Jan Fuglestvedt, Norway]	Agreed, revised.
TS-588	TS	18	9	17	12	The estimation of a RF for stratospheric water vapour, while other changes in water vapour are considered feedbacks is a source of potential confusion for readers. The link to CH4 oxidation is given as the reason for considering this a forcing, but this point - that this is a forcing while other water vapour changes are a feedback - should be directly addressed. [Government of Canada]	Agreed, revised.
TS-589	TS	18	10	18	10	"Recent observations" - I dont think this statement is correct. Figure 2.5 in chapter 2 shows that there was indeed a step change downwards in the early 2000's (which certainly is not "recent" in the context of IPCC assessments) but there has been an upward trend since then, such that the atmosphere is now back to pre-2000 levels. Admittedly the observational record is not great, but actually the 2000s have been characterised by strong upward trends in stratospheric water vapour, and this is what I read from Chapter 2, Section 2.2.2.1 [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Agreed, we removed this as we now only discuss the part of water vapour changes that is a forcing and explicitly say all others are not (see comment 588).
TS-590	TS	18	13	18	13	There needs to be a section on additional effects of greenhouse gases on the Earth System, eg: effects of carbon dioxide on photosynthesis, plant water use efficiency, and ocean pH. Even if CO2 were not a greenhouse gas, increasing its concentration in the atmosphere would cause changes to the Earth System (so this process is a first-order driver of change, not merely a feedback). The direct effects of CO2 on the biosphere are a critical factor in reducing the atmospheric CO2 rise. This process is discussed at length in chapters 6 and 10 so should be mentioned in the TS in section TS.3 since it is a driver of change. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Such effects are discussed in several chapters in the AR5 (6, 8 and 10), but as there is little information to quantify these we do not feel that adding those discussions again here for the drivers section is a good use of limited space.
TS-591	TS	18	16	18	53	While all the details here about aerosol forcing are very useful, it is easy to get a bit lost in all the numbers. A table presenting the various contributions to aerosol forcing would be very helpful. [Government of Canada]	Added to Figure TS.5.
TS-592	TS	18	17	18	17	The abbreviations RFari RFaci should be consistent in the text and the figures. [Government of Germany]	Harmonized
TS-593	TS	18	17	18	17	ari and aci need spelling out [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Done

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TS-594	TS	18	17			The terminology choices of RFari and RFaci are extremely poor choices. The 'aci' and 'ari' usage offers no intuitive of mnemonic help to the reader. I highly recommend reconsidering them. One suggestion is to drop 'i' since it is in both terms and does not help differentiate or explain them to the reader. One suggestion is to use both a sub and superscript, for example, with 'aer' or 'aerosol' as the common subscript and 'cloud' and 'rad' or 'radiative' as the different superscripts. Please reconsider. [David Fahey, United States of America]	We are following the terminology of ch 7.
TS-595	TS	18	21			Section 3.3: TS-18 line 21 – possible inconsistency: confidence in RF_ari is stated as low here, while in 7-45 line 22, it says “There is also increased confidence in this assessment due to more robust satellite-based estimates and their better agreement with models.” [Government of United States of America]	Corrected.
TS-596	TS	18	24	18	32	Sentence in line 24 states that the anthropogenic contribution to dust forcing is too uncertain to mention it, but then line 31 does it. Please be consistent. [Government of Germany]	We've revised to clarify that we can quantify dust forcing, but cannot attribute reliably to natural vs anthro, and hence do not include it in the total anthro forcing.
TS-597	TS	18	26	18	32	It is unclear how these components of RFari add up to the total of -0.40±0.30 Wm-2. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Components do not necessarily add to total, but are not vastly different. There are non-linearities, and different constraints.
TS-598	TS	18	27	18	28	"Black carbon (BC) aerosol has an RFari 0.3 ± 0.2 W m–2 (fossil and bio fuel sources only) and 0.4 ± 0.2 W m–2 (fossil fuel and biomass burning including a possible small fraction from vegetation feedbacks)." This sentence is unclear. Does the second number also include bio fuel sources? What is meant with the construction: BC ..has an RFari ... and ... Should the numbers be summed? How do these numbers relate to the total of 0.54 W m-2 BC RFari in Chapter 8? (see next comment) [Michiel van Weele, Netherlands]	Revised with clear numbers in Figure TS.5.
TS-599	TS	18	27	18	28	The RFari for BC seems to be 0.54 W m-2 (e.g. ref Chapter 8 Fig 8.17c;) composed of 0.3 ± 0.2 W m–2 (fossil and bio fuel sources) and 0.04 ± 0.2 W m–2 (black carbon on snow, Chapter 8 P27 L24) with an implicit remaining contribution of 0.2 W m-2 from biomass burning. This number of 0.2 Wm-2 seems inconsistent with the number of 0.4 ± 0.2 W m–2 given here in the TS. irrespective of the inpretation of the sentence (see also former comment) [Michiel van Weele, Netherlands]	Revised with clear numbers in Figure TS.5.
TS-600	TS	18	27	18	29	Are biofuel sources considered in the second estimate of BC RFari? The text says the estimate of 0.3W/m2 is for fossil fuel and biofuel, and the estimate of 0.4 W/m2 from fossil fuel and biomass burning. Please clarify. Perhaps this sentence could be reworded to say "BC aerosol has an RFari of 0.3 ± 0.2 W/m2 (fossil fuel and biofuel sources only). If biomass burning sources are included, this estimate increases to 0.4 ± 0.2 W/m2. [Government of Canada]	Revised with clear numbers in Figure TS.5.
TS-601	TS	18	27	18	29	This sentence is unclear. The value of 0.4 W/m2 seems inconsistent with Figure TS.5. Shouldn't it also include bio fuel? [Twan van Noije, Netherlands]	Revised with clear numbers in Figure TS.5.
TS-602	TS	18	27			When readers consult AR5 to learn what the forcing of black carbon is in the global atmosphere they will be disappointed and/or confused, starting with these two sentences that offer 2 equations and 3 unknowns. Further, black carbon from biomass burning is erased from the accounting here (Chapter 7) apparently because its RF is 'cancelled' by co-emissions of organic carbon. This is highly misleading. The revised Bond et al. 2012 now cites black carbon as the 2nd largest anthropogenic forcing term with approx equal contributions from the 3 sources cited above (I am a coauthor). This section should posit that black carbon has 3 principal source terms that need to be evaluated (along with pre-industrial emissions) to understand its role in the atmosphere and ultimately its forcing contribution, and then discuss what we know about these terms separately and combined. [David Fahey, United States of America]	Revised with clear numbers in Figure TS.5.
TS-603	TS	18	28			The stated value for forcing due to BC combining fossil fuel and biomass does not appear to be correct. It is not explictly called out in section 7.5.1.2 and that section indictes an estimate of 0.0 for the biomass component. [Government of United States of America]	Revised with clear numbers in Figure TS.5.
TS-604	TS	18	30	18	30	Please clarify what is encompassed by biomass burning aerosol (BC, OC and ?) as only BC from biomass burning aerosol has been mentioned up to here. You could say "collectively, biomass burning aerosol (encompassing BC, OC and other constituents) has a RF of....". (Also, presumably 0.0 does not need a negative sign.) [Government of Canada]	Revised with clear numbers in Figure TS.5.

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TS-605	TS	18	31	18	32	Please clarify if "mineral aerosol" is the same or different than "mineral dust". Line 25 says the contribution from mineral dust is not included, but here a RF estimate for mineral aerosol is given. [Government of Canada]	Mineral aerosol no longer used. Clarified inclusion/exclusion rationale (see reply to TS-596)
TS-606	TS	18	33	18	33	This complicated paragraph requires a summary/synthesis statement. The main message would seem to be that the two largest components to the total aerosol forcing (RFari) are from sulphates (-0.4 W/m2 and from BC +0.4 W/m2 (with similar uncertainties of ±0.2W/m2). As these are of similar magnitude and opposite sign they offset each other. That leaves all the other aerosol components to contribute a combined effect of about -0.4 W/m2 to arrive at the net value for RFari of -0.4 W/m2. This presentation may not be quite right but the point is, that summary statements like these would help the reader distill key messages from this paragraph, and avoid misinterpretation. [Government of Canada]	These numbers have been removed from the text here in response to the Govt of Canada's earlier suggestion, and are now given in Figure TS.5. A brief discussion is presented there, but the relative import can now be simply read off the figure.
TS-607	TS	18	34	18	36	The discussion of the reduction of the magnitude of the aerosol forcing (presumably relative to AR4) is a bit confused as AR4 didnt systematically use AF [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Revised to try to clarify that many recent estimates are lower than earlier ones (not AR4 specifically), and not to imply AR4 used ERF.
TS-608	TS	18	41			TS-18 line 41 and lower – check consistency between Table 7.1 and this information. It would be helpful if the two agreed one-to-one. [Government of United States of America]	Agreed, will check.
TS-609	TS	18	43	18	47	The phrase "absorbing aerosol" does not appear on Figure TS.4 (which refers only to BC on snow). Consistency is recommended or the different terms should be explained. [Government of Canada]	Revised to say BC.
TS-610	TS	18	43	18	47	Please explain why forcing from BC on ice/snow is so much stronger than atmospheric BC, possibly using the sentence chapter 7, page 47, lines 18-20. "The greater response of global-mean temperature occurs primarily because all of the forcing energy is deposited directly into the cryosphere, whose evolution drives a positive albedo feedback on climate." [Government of Germany]	Revised as suggested.
TS-611	TS	18	43	18	47	UNECE countries have in the recently revised Gothenburg Protocol just committed to significant reductions in SO2 and NOx emissions over the next 8 years which should deliver rapid reductions in sulphate aerosol over Europe. Hence the wording "If rapid reduction...." is either taking into account a global scale compensating increase or seems a value judgement on the likelihood of delivery of the emission reduction. In Western Europe there is probably little relationship between sulphate reduction and black carbon reduction though the absence of a widely accepted and measureable definition of black carbon for emission quantification purposes does not help such discussions to be well founded. There is some concern that both economic pressure and decarbonisation policies to increase biomass combustion may lead to an increase in black carbon emissions in some countries. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted, no changes requested; The text is a physical science-based if-then statement. No reference is being made to actual policy plans, value judgements or similar. (Note: comment seems to refer to for p-38)
TS-612	TS	18	43	18	47	The same suggestions are as the above. The surface temperature change over Tibet is not obvious from Figure SPM. 6(a). [HUA ZHANG, China]	There is no figure showing surface temperature response to this forcing alone. It is not possible to tease this out from the response to many forcings since despite the enhanced response this is a small forcing.
TS-613	TS	18	43			Change "absorbing aerosol" to "BC". [Twan van Noije, Netherlands]	Done.
TS-614	TS	18	50	18	50	Why only WMGHGs? This excludes tropospheric ozone; would just GHGs be better here? [Government of Canada]	Yes, changed to GHG.
TS-615	TS	19	1	19	11	does the land use change include the ocean albedo change due to sea ice melting? [Andrea Flossmann, France]	No, as that's not on land.
TS-616	TS	19	1	19	11	Give examples of anthropogenic land use changes. Also what are "managed" surfaces; e.g. is a national park or a nature reserve a "managed" surface? [Dora Marinova, Australia]	Revised.
TS-617	TS	19	7	19	7	After "difficult to quantify" insert "and their importance relative to albedo changes varies from region to region according to background conditions." And change the next part to "These non-radiative effects tend to offset the impact of albedo changes at the global scale". [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Clarified second part as suggested, which by calling out the global scale implicitly makes the reviewer's first point that they may vary from region to region.

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TS-618	TS	19	13			Emphasize that the RF is positive [Government of United States of America]	Added.
TS-619	TS	19	20	19	21	This sentence (by referring to volcanic forcing and then Figure TS.4) implies that Figure TS.4 includes the volcanic forcing, when it does not [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Reference to figure moved to call out solar only.
TS-620	TS	19	20			Solar and volcanic variability are the dominant natural contributors to _global_ climate change, but I'd argue that internal variability (eg, ENSO) can be dominant on a regional scale… Perhaps add the word "global" as a modifier here? [Government of United States of America]	Agreed, revised.
TS-621	TS	19	22			The text should make clear how this RS due to TSI change is calculated. The text should say the years, and whether this is a trend or a difference in annual means. Since there is a pronounced solar cycle it will make a big difference how this is calculated. (I made the same comment on chapter 8). [Nathan Gillett, Canada]	Clarified.
TS-622	TS	19	23	19	29	I feel this paragraph is seriously misleading, both in how it is written and the science it reports. Line 23 talks of a "downward trend in TSI" which there clearly has NOT been. The discussion needs to restructured to say first that there is a clear quasi-11 year cycle in TSI, such that it has increased and decreased over the observational period, and that it is currently increasing, rather than decreasing! I am guessing that the "downward trend" is referring to the solar minimum. But the text says that satellite analysis all "agree" on downward trend. But Chapter 8, Figure 8.12, doesnt support this at all. The PMOD data shows the 1976 solar minimum was lower than the 1986 minimum (it is not clear why the 1976 mimmum is ignored in the discussion) and about the same as the 1996 one. The ACRIM and RMIB data show the 1986 minimum to be lower than 1996 AND 2008. Hence, I believe the evidence for a long term trend in solar minimum is very weak indeed, and even the statement that the 2008 minimum was "unusually low" (line 28) is not really substantiated in 2 of the three available "long term" data sets shown in Chapter 8. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	This section has been substantially revised to take into account these comments, which we generally agreed with (note that the ch 8 figure mentioned here has also been revised).
TS-623	TS	19	31	19	31	Add statement that solar forcing will be smaller than that of GHG. Possibly from chapter 8, page 32, line 53-54: "Nevertheless, if there is such a diminished solar activity, there is a high confidence that the TSI RF variations will be much smaller than the projected increased forcing due to GHG." [Government of Germany]	Revised as suggested.
TS-624	TS	19	33	19	39	The conclusions here are based on the high solar activity since about 1910 and do not include the possibility that a significant reduction in solar activity will be accompanied by increased cosmic ray background. This is what has been occurring since the prolonged minimum between solar cycles 23-24, which is addressed below together with a suitable reference. [Forrest Mims, United States of America]	The conclusions are based upon a number of studies that found that even with a substantial reduction in solar output the influence of the additional cosmic rays would be very small (as discussed in ch 7).
TS-625	TS	19	33			Idem (GCR contribute to nucleation, but no evidence that they contribute to atmospheric CCN) [Bart Verheggen, Netherlands]	Revised to describe link between nucleation and CCN as possible.
TS-626	TS	19	49	19	49	I believe that the source paper for this 2000-2010 trend (solomon et al in Science) does not attribute the trend specifically to volcanoes but instead, reports it as a trend in stratospheric aerosol. I think this disctintion is quite important [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	This value is based on updated volcanic aerosol data as used in ch 8 for volcanic aerosol forcing, not directly on the Solomon et al paper. Will ensure clear in chapter 8 section 8.4.
TS-627	TS	19	49		50	Here -0.1 W/m^2 is quoted as the minimum radiative forcing due to strat aerosol over the 2000 to 2010 period, but chapter 8 ES quotes this figure as the average RF over this period. Which is it? If it's an average replace 'down to about' with 'with a mean value of'. [Nathan Gillett, Canada]	Clarified.
TS-628	TS	19	50	19	51	"…dependence of climate impact on the amount of material and time of the year of high-latitude injections". [David Parker, United Kingdom of Great Britain & Northern Ireland]	Reordered as suggested.
TS-629	TS	19	50		51	Given that no climate impacts have been observed from eruptions over the past 10 years, how can they have improved understanding on the depdence of climate impacts on the amount of material and the time of the year? [Nathan Gillett, Canada]	Agreed, revised to RF rather than cliamte impact.
TS-630	TS	19	55	19	57	It would help readers if you list (in parentheses) the forcing agents assigned to each confidence level. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Added to figure TS.4.
TS-631	TS	19	55	20	2	Suggest adding these confidence levels to Figure TS.4 in the same way Levels of Scientific Understanding were added to the similar figure in the AR4. [Government of Canada]	See previous comment.

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TS-632	TS	19	55	20	2	These claimed increases in LOSU aren't backed by any credible evidence. Without that evidence the IPCC will be accused of fudging the figures to pretend that advances have been made. If you don't have the evidence then reinstate the 4AR LOSU's. [John McLean, Australia]	Evidence for changes in confidence levels is given in the chapters.
TS-633	TS	19		20		Section 3.6. In this sysnthesis, discussion about Radiative Forcing due to SO2 and sulphate is missing. [Umesh Kulshrestha, India]	This is now present having been moved from earlier where it formerly was.
TS-634	TS	19				Section 3.5. Under natural drivers of radiative forcing, the role of soil-dust is not mentioned. Regions such as India and Sahara and part of China have very high atmospheric cocentrations of soil-derived aerosols which highly influence Radiative forcing at regional scale. Possibly, it can be mentioned with `very low confidence' till the comprehensive studies are reported. [Umesh Kulshrestha, India]	Natural changes in soil dust are considered a feedback to other climate drivers rather than a forcing.
TS-635	TS	20	6	20	6	"continuous" - I would agree that there has been an continuous increase in the total WMGHG forcing, but the concentrations of all gases (notably the CFCs) have not continuously increased [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Agreed, revised to most WMGHGs.
TS-636	TS	20	8	20	8	"robust evidence and agreement": how much agreement? it cannot be robust, is it high? [Government of Germany]	Revised as suggested.
TS-637	TS	20	12			Replace 'Changes in carbon dioxide are' with 'Carbon dioxide is'. [Nathan Gillett, Canada]	Revised as suggested.
TS-638	TS	20	23			Figure TS.5: Vertical bars are very hard to digest - what scale are they on? I suggest writing the specific values in a separate column down the right-hand side of the figure. Also, the caption does not explain what is meant by the white sub-bars within the black carbon and organic carbon bars [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	Taken into account. An explanation of the vertical error bars has been added to the caption. The white bars were confusing to some so the separation of different bar segments has been revised for clarity.
TS-639	TS	20	24	20	28	in figure TS.5 change "Aerosols" to "clouds"; this process refers to Rfaci, thus is associated to clouds in the common sense, even though for the expert drops are just aerosol particles. [Andrea Flossmann, France]	The text entries are listing the emitted components, so this needs to remain aerosols (referring to aerosols and their precursors in general) rather than clouds.
TS-640	TS	20	32	20	38	This wording is a little confusing: does "Industrial era RFari" refer to all forcings (eg, GHG+aerosols+ozone+land-use) or does it not include GHGs? And are "maximum values" positive or negative? In order to make the sentence make sense, I'd think it would be something like, "Industrial era RFari from forcers other than the WMGHG showed maximum cooling over eastern...". The next sentence, starting "Aerosol AF shows similar behavior..." is also confusing, if aerosols are part of the RFari... Also, the sign should be made clear in terms of the strongest land-use forcing by albedo changes (presumably also cooling?). [Government of United States of America]	Revised as suggested.
TS-641	TS	20	32			Industrial era Rfari of what? Aerosols? [Nathan Gillett, Canada]	Revised to clarify this is just RFari (hence aerosols).
TS-642	TS	20	40		41	Give the sign of the RF due to land-us- induced albedo changes. [Nathan Gillett, Canada]	Added.
TS-643	TS	20	41	20	42	This reference to influence on the hydrologic cycle appears out of place in a section that focuses on radiative forcing. Perhaps this is sentence is better placed in TS.3.7 Climate Feedbacks [Government of Canada]	Revised to point out that the proportional response to these inhomogeneous forcings is greater than for equivalent global mean homogeneous forcing, which is one reason why the spatial pattern matters.
TS-644	TS	20	44	20	47	Using the 4 RCPs, which were defined as radiative forcing targets, to support the contention that anthropogenic forcing will increase is odd. [Government of United States of America]	Added comment in text that the RCP emissions for WMGHGs do span a broad range of possible futures, as shown in Figure 8.5.
TS-645	TS	20	44	20	47	Tell the reader the full story about CO2-driven warming, ie. how the theoretical relationship is logarithmic and the warming will be less and less for each incremental unit of CO2. [John McLean, Australia]	This is well know and clear in IPCC assessments (including AR5), but not relevant to this discussion.
TS-646	TS	20	44	20	51	Care should be taken when using the RCPs to define warming - the RCPs were not chosen to span the range of possible changes in short-lived forcers over the next 20 years. [Government of United States of America]	Agreed, noted in text.
TS-647	TS	20	44	20	55	Suggest adding here some information about changes in CH4 in the RCPs. As CH4 is the second largest RF agent, how it is projected to change over time is of interest. [Government of Canada]	We agree this is interesting, but we do not go into detail about the emissions trends for any agent under

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							the RCPs here. This is in chapter 8 (e.g. Figure 8.5), however.
TS-648	TS	20	46	20	47	Wow, the lowest estimate for year 2100 is lower than that for 2050. Does that mean cooling? [John McLean, Australia]	This is consistent with peak and decline emissions in some scenarios.
TS-649	TS	20	49		50	Aerosol AF change contributes a warming to 2100, but negligible change to 2030 according to Figure 8.20b. This is worth mentioning here. [Nathan Gillett, Canada]	Added.
TS-650	TS	20	51	20	51	"substantial increase" - I think this means a substantially more negative radiative forcing? So this is not an increase [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Clarified.
TS-651	TS	20	53	20	53	You make the ridiculous implication that the Earth was in radiative balance. It's always in imbalance. Daytime and nightime radiation are not in balance because days (usually) warm and nights (usually) cool. There's no radiative balance across the annual cycle because if it existed Earth wouldn't have seasons. And radiation doesn't balance across land and sea, so don't try to claim that the Earth is balanced at each moment because the amount of land and ocean exposed to sunlight is always changing. [John McLean, Australia]	The imbalance is averaged globally and over a year as described in the definition of RF, so this statement does not imply balance at smaller spatial or temporal scales.
TS-652	TS	20	57			Suggest that this section on climate feedbacks include surface albedo feedback. [Government of Canada]	Added.
TS-653	TS	20	57			why is there no mention of critical ice and snow energy balance (albedo) feedbacks? [David Sauchyn, Canada]	Added.
TS-654	TS	21	1	21	1	I think the word "driver" should not be used about feedbacks (since driver is often used for emissions or land use changes). It could be replaced by "determining" or "controlling". [Jan Fuglestvedt, Norway]	Agreed, revised.
TS-655	TS	21	1	21	18	It is strange that there is no discussion of cloud feedbacks here - I wondered if a paragraph was missing. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Cloud feedbacks are mentioned here, with a pointer to TFE 6.
TS-656	TS	21	2	21	2	"may reinforce (positive feedback) of 3 dampen (negative feedback) the expected temperature increase". change "of" to "or" [Andrew Glikson, Australia]	Corrected.
TS-657	TS	21	2	21	3	Should this be "or" rather than "of": "reinforce (positive feedback) of dampen (negative feedback)? [Government of Canada]	Corrected.
TS-658	TS	21	2			last word of line should be "or" not "of" (typo) [Andrea Flossmann, France]	Corrected.
TS-659	TS	21	2			The list of feedbacks here seems to omit the lapse-rate feedback and ice-albedo feedback. Replace 'modification in the carbon, water and geochemical cycles' with 'changes in the atmosphere, land surrface and carbon cycle' [Nathan Gillett, Canada]	This section now mentions both lapse-rate and ice-albedo feedbacks. We prefer the broader cycle terms here as the water cycle, for example, includes the oceans that are neither atmosphere nor land surface.
TS-660	TS	21	2			Typo: Last word in line 2 should be changed from "of" to "or". [Forrest Mims, United States of America]	Corrected.
TS-661	TS	21	7	21	7	"In particular, carbon sinks in tropical land ecosystems are vulnerable to climate change." The potential role of fire in burning tropical forrests, i.e. the Amazon, ought to be mentioned. [Andrew Glikson, Australia]	Added.
TS-662	TS	21	7	21	7	"In particular, carbon sinks in tropical land ecosystems are vulnerable to climate change." The potential role of fire in burning tropical forests, i.e. the Amazon, ought to be mentioned. [Government of Australia]	Added.
TS-663	TS	21	7	21	7	Why are particular "carbon sinks in tropical land ecosystems vulnerable to climate change"? Are these model predictions that due to climate change tropical forests will die back? What is the reasoning here? Need to better explain [European Union]	Added.
TS-664	TS	21	10	21	10	"predicted" or "projected"? [Government of Germany]	Changed to projected.
TS-665	TS	21	11	21	18	It seems to me that rather than just say there is little confidence on wetland, premafrost, and seabed emissions, it would be helpful to give an indication of the potential magnitude of possible effects, etc. So, if most of the C came out as methane, how big an effect could there be, if most as CO2, then how much, etc. Might the possibility be that such emissions might push the response up one RCP category or so (or not that	The chapters underlying the TS do not quantify these realeases, so we cannot here, but we have added a statement about the reservoirs being quite large.

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						much or whatever)--saying something like that would be useful, especially given there are groups out there saying these terms are being ignored and it could be a run away condition. [Michael MacCracken, United States of America]	
TS-666	TS	21	17	21	18	Suggest adding "of carbon (as CO2 or CH4)" after the words "Release of" (if this is what is implied). [Government of Canada]	Added 'of carbon'. Since it's not quantified, it does not matter to this statement if it's CO2 or CH4.
TS-667	TS	21	18	21	18	Same comment as above [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	Cannot determine which comment 'above' is meant.
TS-668	TS	21	20	21	22	because the strength of the other feedbacks is not reported in Wm-2K-1 it is difficult to place the stated value (0.2) in any context. In any case, the paragraph seems to be saying it is zero or small, and so this is a candidate for deletion or a statement that it is likely zero or negligible [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Agree, quantification removed so this text is now more consistent with remainder of section.
TS-669	TS	21	23			Can we give a likely net sign for the DMS feedback? [Government of United States of America]	No, the sign seems to vary regionally so the global mean sign is not clear. Revised text to point this out.
TS-670	TS	21	24	21	26	"Although the limited evidence is for a rather weak aerosol-climate feedback at the global scale during the 21st century, regional effects on the aerosol may be important." It's not clear that this statement is said with 'low confidence' or some other level of confidence. If this feedback is neglected within the IPCC analysis, please state that clearly. [Government of United States of America]	These effects are included in the IPCC assessment and it's underlying models, but have not been explicitly quantified at regional scales, so we cannot really assess them well and only note that they may in fact be more important at those scales than at the global scale (now clarified).
TS-671	TS	21	28	22	22	This section has a strong bias towards the GTP. Negative statement tend to address GWP and positive GTP. The IPCC mandate (policy relevant, but not policy neutral") must be followed in particular when discussing common metrics, a topic of high relevance for the UNFCCC and financial implications. [Government of Germany]	Revised to balance paragraph on uncertainties and limitation to better cover both GWP and GTP. Discussion is policy neutral throughout.
TS-672	TS	21	28	22	22	Please add a statement on uncertainties of the different metrics. For example: The same factors, that contribute to uncertainties in GWP, cause uncertainty in GTP as well, with an additional contribution from the parameters describing the ocean heat uptake and climate sensitivity." see 8-53 lines 32-33 [Government of Germany]	Revised as suggested.
TS-673	TS	21	28	22	22	Please provide more information of the radiative effects of short lived tracers. This information is needed by decision makers, given the current activities by UNEP, CCAC regarding these species. [Government of Germany]	The radiative effects of short-lived compounds, like those of long-lived compounds, are all included in the underlying chapter in the section referenced here. We restrict ourselves to the general statements about these (last sentences of first paragraph in section TW3.8) and the accompanying figure due to space limitations in the TS.
TS-674	TS	21	28	22	22	This section has a strong bias towards the GTP. Negative statement tend to address GWP and positive GTP. The IPCC mandate (policy relevant, but not policy neutral") must be followed in particular when discussing common metrics, a topic of high relevance for the UNFCCC and financial implications. [Government of Germany]	See reply to 671 (same comment)
TS-675	TS	21	28	22	22	Please provide more information the effects of the radiative effects of short lived tracers. This information is needed by decision makers, given the current activities by UNEP, CCAC regarding these species. [Government of Germany]	See reply to 673 (same comment)
TS-676	TS	21	28	22	23	This section in the Technical Summary identifies issues with GWPs and their inappropriateness for application in target based policies. As GWPs are currently widely used in target based policies and emissions accounting, this needs further explanation. The benefits of the alternative metric (GTPs) should be more clearly articulated, as well as the implications of switching from GWPs to GTPs. If it is being recommended that GTPs are to be used instead of GWPs, the recommended GTP values for each greenhouse gas should be provided in a location that is easily accessible to policy makers, for example, a table in the TS. [Government of Australia]	We have clarified that GTP has benefits for some specific types of analyses (target-based policies) but not in all cases. We do not give a recommendation as to which metric should be used, but rather describe the relative advantages and disadvantages of each. All values are in the underlying chapter (in the interest of space).

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TS-677	TS	21	28	22	23	It seems like it would be useful to cite the conclusions of the IPCC Expert Meeting on metrics here… these are that despite its imperfections, the GWP was a reasonable compromise metric. [Government of United States of America]	The AR5 represents a newer assessment including literature since the IPCC expert meeting. Our text does not imply that GRP is not a reasonable metric, however we provide additional analyses of cases when GWP or GTP might be more or less suitable.
TS-678	TS	21	28			TS.3.8: The section on Common Metrics is very unbalanced in the negative view of GWP as a common metric without presenting the shortcomings of the alternatives. Further examples are provided below and in comments to Section 8.7. [Government of Denmark]	Revised to balance paragraph on uncertainties and limitation to better cover both GWP and GTP.
TS-679	TS	21	30	21	41	Two comments: (1) Figure TS.6a shows that GTP values for the SLCFs (NTCFs) (noticeably OC, BC, SO2) are smaller than the GWP values at similar time horizons. The reason for this is explained in section 8.7 (page 8-53 lines 1-2); it would help to bring this information in here. (2) Regarding lines 39-41, it could be noted that this statement holds true for both positive and negative forcing agents (i.e. non-CO2 impacts are comparable in magnitude (but of either the same or opposite sign) to those from CO2 emissions ...etc.). [Government of Canada]	Revised following both suggestions.
TS-680	TS	21	32	21	32	The sentence "They account for…. radiative efficiency…..and their lifetime…" is only relevant for the GWP, and not the GTP which accounts for reponse of the climate system. Thus, the sentence could be deleted or expanded in order to also cover what other metrics do. [Jan Fuglestvedt, Norway]	Revised (and relocated) to cover what is included in GWP and GTP.
TS-681	TS	21	33	21	34	"Up to AR4 the most common metric has been the GWP..." and "There is now increasing focus on te GTP...". Please justify this statement. For example, is the focus in science, and did you evaluate the literature published on each of the metrics? [Government of Germany]	We did evaluate the literature, as described in ch 8 (8.7), and the statement is based on increasing use of GTP in both science and policy.
TS-682	TS	21	36	21	36	Yes, the choice of timehorizon is highly subjective. But it could be added that it depends on the context and the application. [Jan Fuglestvedt, Norway]	Added.
TS-683	TS	21	36	21	36	I am not sure the time-horizon is highly subjective - rather it is a user-driven choice, and different users may make different (objective) choices. In the case of the so-called dynamic GTP (in a target-based application) the choice of time horizon is not highly subjective, but is driven by the likely time at which a target temperature will be reached. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	This is in some sense a fair point, as the users indeed choose a time horizon based on an 'objective' criteria of being interested in impacts at a particular time. However, the choice of which time is of interest is still subjective (e.g. why is 100 years, for example, the time at which one should worry about impacts?). We have deleted 'highly' but retain 'subjective' and add 'and context-dependent'.
TS-684	TS	21	36	21	37	More reference should be made to black carbon in the Technical Summary, considering the fact that it should be an important message to policymakers that reductions of black carbon can have a cooling effect in the short term but not in the long term. [Government of Japan]	Black carbon is highlighted in Figure TS5, and the first paragraph of this metrics section discusses BC in particular and NTCFs in general.
TS-685	TS	21	36	21	38	It is good to stress how the comparisons depend on chosen time horizon. But it is also needed to stress that the choice of metric (or indicator, i..e integrated RF or dT) has a strong effect. If space, the fundamental difference between the two metrics used here should be explained. [Jan Fuglestvedt, Norway]	Added text on this topic.
TS-686	TS	21	36			The choice of the time horizon depends on the objectives of the policy and the time horizon of the policy. These are mentioned in Chapter 8 page 51 referring to the UNFCCC Article 2 which mentions both the level goal (it can be interpreted as long term-target, e.g. 50-100 years time scale) and rate goal (medium term target, 10-30 years scale) . Please, instead of the words "which is highly subjective" use the words "which depends on the objective and time horizon of the policy." [Government of Finland]	Our point was that there is no objective way to determine which particular time was the most relevant to look at, so while a policy may have a particular time horizon associated with it that choice is a subjective one. This is the underlying rationale for us giving metric values at multiple time horizons. We have revised the remove 'highly' but retain 'subjective' and add 'and context-dependent'.
TS-687	TS	21	36			The choice of the time horizon depends on the objectives of the policy and the time horizon of the policy. These are mentioned in Chapter 8 page 51 referring to the UNFCCC Article 2 which mentions both the level goal (it can be interpreted as long term target, e.g. 50-100 years time scale) and rate goal (medium term target, 10-30 years scale) . Please, instead of the words "which is highly subjective" use the words "which	See reply to 687 (same comment)

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						depends on the objective and time horizon of the policy." [Ilkka Savolainen, Finland]	
TS-688	TS	21	37	21	37	I suggest deleting "a" before "contribution", changing "contribution" to "contributionS" and adding "relative" before "contributionS". [Jan Fuglestvedt, Norway]	Revised as suggested.
TS-689	TS	21	37			Consistency is needed throughout the entire report as to how methane is classified. In some instances it is called long lived, well mixed, near term and here a new term is added - intermediate lifetime species. [Government of Australia]	Removed 'intermediate lifetime' and used NTCF.
TS-690	TS	21	39	21	39	The sentence "Analysis of the impact of current emissions "(one year pulses of year 2000 emissions)" is needed a few lines up; i.e. on line 36. [Jan Fuglestvedt, Norway]	Reordered as suggested.
TS-691	TS	21	56			The following wording is suggested: A large number of other metzrics may be defined down the driver-response-impact chain. [Klaus Radunsky, Austria]	Revised as suggested.
TS-692	TS	21	58	21	58	I suggest adding (something like) "by which one wants to measure climate change" after "particular impact". And delete "being investigated". [Jan Fuglestvedt, Norway]	Revised similar to what was suggested.
TS-693	TS	22	4	22	5	The sentence " The GWP … feedbacks." gives a biased view on one of the metrics (GWP) while the criticism holds for most of the metrics that have been suggested up till now. Line 4-33 of page 54 of chapter 8 give a more balanced view. In particular, it states clearly that vegetation feedbacks (line 12) are not included in GTP either, and that carbon cycle feedbacks (line 20-22) affect GTP as well as GWP. Finally I note that most of the currently criticized aspects of GWP were already known and mentionned when it was introduced in IPCC FAR. I therefore suggest a more general formulation: "GWP and most other metrics have shortcomings and suffer from inconsistencies related to the treatment of indirect effects and feedbacks." [Peter Van Velthoven, Netherlands]	Revised similar to what was suggested.
TS-694	TS	22	4	22	7	This could also be included in SPM. [Jan Fuglestvedt, Norway]	Noted.
TS-695	TS	22	4	22	7	Having read section 8.7, and especially section 8.7.1.4 (Uncertainties and Limitations) the summary here seems biased in highlighting only the uncertainties and weaknesses of the GWP metric and not those of other metrics, including GTP. In section 8.7 it is noted that uncertainties for GTP are larger than for the GWP (as one example of selective reporting here). In section 8.7.1.4 it is explicitly stated that the same factors that produce the 50% uncertainty on GWP over the 100 year time horizon also apply to the GTP (page 8-53 lines 32-34) and then notes there are additional uncertainties related to ocean heat uptake and climate sensitivity that affect GTP estimates. [Government of Canada]	Revised to balance paragraph on uncertainties and limitation to better cover both GWP and GTP.
TS-696	TS	22	4	22	7	An example of the very unbalanced description is on page 22 line 4-7 – It is stated that GWP as a common metric “has received much criticism and there are serious limitations and inconsistencies…”. Furthermore, it is stated that the uncertainty for GWP100 is larger than 50 %. No mention is made of the shortcomings and uncertainties of the alternative common metrics (e.g. GTP). It should be noted that the same issues that are identified regarding GWP also to different extents are valid for the alternatives. It should also be noted when addressing the issue of uncertainty that the uncertainties associated with GTP are significantly higher than those for GWP. The last sentence of the paragraph states that GWP “is not well suited for target based policies”. This seems very judgemental and can hardly stand alone considering the significant challenges for the alternative metrics. [Government of Denmark]	Revised to balance paragraph on uncertainties and limitation to better cover both GWP and GTP.
TS-697	TS	22	4	22	7	This paragraph about GWPs seems not in line with the more factually written underlying section 8.7. In comparison, this paragraph is unduly negative. For example: This paragraph states that the GWP100 uncertainty is larger than 50%, while in fact the underlying section states: "While each study considers different types of uncertainty, the assessed uncertainty for methane are of the order of 20–40% for GWP100 and 60–75% for GTP100" (Chapter 8, page 55, line 10). Thus, a rephrasing could be: "The GWP has received criticism [DELETE MUCH] and there are limitations [DELETE SERIOUS] and inconsistencies related to the treatment of indirect effects and feedbacks. The uncertainty in the GWP increases with THE chosen time horizon, but is generally smaller than uncertainties of the GTP metrics (e.g. 20-40% for GWP100 and 60-75% for GTP100 in the case of methane). Several studies point out that this metric is not TIGHTLY ALIGNED with a policy aim to limit maximal temperatures". The latter re-phrasing is suggested to be more specific rather than simply saying "target based policies". [Government of Germany]	Revised to balance paragraph on uncertainties and limitation to better cover both GWP and GTP.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-698	TS	22	4	22	7	This paragraph about GWPs seems not in line with the more factually written underlying section 8.7. In comparison, this paragraph is unduly negative. For example: This paragraph states that the GWP100 uncertainty is larger than 50%, while in fact the underlying section states: "While each study considers different types of uncertainty, the assessed uncertainty for methane are of the order of 20–40% for GWP100 and 60–75% for GTP100" (Chapter 8, page 55, line 10). Thus, a rephrasing could be: "The GWP has received criticism [DELETE MUCH] and there are limitations [DELETE SERIOUS] and inconsistencies related to the treatment of indirect effects and feedbacks. The uncertainty in the GWP increases with THE chosen time horizon, but is generally smaller than uncertainties of the GTP metrics (e.g. 20-40% for GWP100 and 60-75% for GTP100 in the case of methane). Several studies point out that this metric is not TIGHTLY ALIGNED with a policy aim to limit maximal temperatures". The latter re-phrasing is suggested to be more specific rather than simply saying "target based policies". [Government of Germany]	See reply to 697 (same comment)
TS-699	TS	22	4	22	7	We notice an unbalanced view on GWP. The criticism holds for most of the metrics that have been suggested up till now. Chapter 8 gives a more blanced view. For example, page 53, line 1-10 state, the uncertainty in GTP is even larger due to the inclusion of more processes (e.g. ocean heat uptake). And further line 12, page 54 it is stated that vegetation feedbacks are not included in GTP either, and that carbon cycle feedbacks (lines 20-22) affect GTP as well as GWP. We therefore suggest a more general formulation: "GWP and most other metrics have shortcomings and suffer from inconsistencies related to the treatment of indirect effects and feedbacks." We further suggest to include that other metrics, such as GTP, may have even higher uncertainties due to the inclusion of more processes. [Government of Netherlands]	Revised to balance paragraph on uncertainties and limitation to better cover both GWP and GTP.
TS-700	TS	22	4	22	7	It is suggested to delete these sentences because they are policy prescriptive as pros and cons of other metrics are not addressed. [Klaus Radunsky, Austria]	Revised to balance paragraph on uncertainties and limitation to better cover both GWP and GTP.
TS-701	TS	22	5	22	6	"The uncertainty in the GWP …50%." This gives un unbalanced view by only stating the big uncertainty in GWP, which might suggest other metrics are more certain. However, as chapter 8, page 53, line 1-10 state, the uncertainty in GTP is even larger due to the inclusion of more processes (e.g. ocean heat uptake). I therefore suggest to either drop this sentence or to add something about other metrics, e.g. "Other metrics, such as GTP, have even higher uncertainties due to the inclusion of more processes." [Peter Van Velthoven, Netherlands]	Revised to balance paragraph on uncertainties and limitation to better cover both GWP and GTP.
TS-702	TS	22	6	22	7	The sentence starting with "Several studies ... " must be deleted. It is not policy-neutral: to which "target based policies" is it referring? If limiting global temperature rise in the long term was the target, than GWP would be better suited than GTP which gives more weight to short term tracers. [Government of Germany]	This sentence has been revised in response to other comments (including the suggestion of the Govt of Germany). The statement about GWP vs GTP is incorrect as GWP gives more weight to short-term forcers.
TS-703	TS	22	6	22	7	I would suggest specifically mentioning that the GWP-100can be quite misleading when working with the short-lived species--especially methane, black carbon, and tropospheric ozone--as the UNEP assessment indicated (basically, using GWP-100, there is no way to see how one could cut the warming from 2010 to 2050 in half, but using actual flux changes, that is what would happen with the suggested cuts in methane and black carbon emissions. [Michael MacCracken, United States of America]	While this is correct, in the interest of balance we would have to point out multiple cases where various metrics were not well suited, and not just GWP100. This would require too much space for the TS, but we point out that the last sentence of the first paragraph in this section (TS3.8) does discuss the relative impact of NTCFs vs CO2 on short and long timescales.
TS-704	TS	22	6	22	7	"Several studies also point out that this metric is not well suited for target based policies." Indeed, already from the IPCC FAR onward GWP caveats have received attention and are still doing so as is reflectecd in 7.8. However, 7.8 also lists numerous disadvantages of all other metrics in 8.7.4 (pages 8-53 to 8-55), and it does not provide a better alternative metric. Hence there is no rationale for one-sidedly discrediting GWP. I therefore suggest to remove this sentence or to add "However, a significantly better alternative metric for target-based policies has not been suggested yet." [Peter Van Velthoven, Netherlands]	Revised to balance paragraph on uncertainties and limitation to better cover both GWP and GTP.
TS-705	TS	22	9	22	10	This sentence needs to be reworded. The metrics are not used for attribution emissions to activities. They are used to calculate impacts of various activties which have emissions of various components already attributed to them. [Jan Fuglestvedt, Norway]	Revised.

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TS-706	TS	22	10	22	11	Replace "more" by "additional" so that the sentence reads "Such activity-based accounting can provide additional policy-relevant information ..." The Reason for this change: Activity based metrics provide interesting information, but are not suitable for metrics in existing emission trading schemes. Thus, the current value-laden formulation ("more policy-relevant") especially after the previous negatively biased GWP paragraph seems not to reflect the scientific literature, nor the practical requirements for metrics in important application areas. [Government of Germany]	Revised as suggested.
TS-707	TS	22	10	22	11	Replace "more" by "additional" so that the sentence reads "Such activity-based accounting can provide additional policy-relevant information ..." The Reason for this change: Activity based metrics provide interesting information, but are not suitable for metrics in existing emission trading schemes. Thus, the current value-laden formulation ("more policy-relevant") especially after the previous negatively biased GWP paragraph seems not to reflect the scientific literature, nor the practical requirements for metrics in important application areas. [Government of Germany]	See reply to 706 (same comment)
TS-708	TS	22	12	22	12	Should this read "is" rather than "if": "if often used to quantify the…" [Government of Canada]	Corrected.
TS-709	TS	22	12	22	16	These lines are informing about the GTP, again giving it more weight in the text and thus suggesting that it is better suited than GWP. Please rewrite text in a more balanced way. [Government of Germany]	This text does not state that GTP is better, rather it uses GTP as the metric since that's what is used in Figure TS.6b which is being discussed here. Figure TS.6a presents both metrics, as we do not recommend one over the other, but that is not practical in the time-evolving plot shown in Figure Ts.6b. We have revised to state that the GTP is used to illustrate the results to avoid any implicit reading that we are saying GTP is better.
TS-710	TS	22	12	22	16	These lines are informing about the GTP, again giving it more weight in the text and thus suggesting that it is better suited than GWP. Please rewrite text in a more balanced way. [Government of Germany]	See reply to 709 (same comment)
TS-711	TS	22	13	22	16	These lines provide a further example of the very unbalanced nature of the section. It is stated that using the GTP metric “power generation and industry have the largest contribution to warming…”. Is seems rather superfluous to make such a statement, since it would be difficult to imagine any common metric where the result would be different and therefore it reads as meaningless praise for one specific common metric. The whole paragraph therefore reads as a long praise for GTP compared to the previous paragraph that is only presenting negatives for GWP. [Government of Denmark]	Revised (see reply to 709).
TS-712	TS	22	19	22	20	The sentence: In both cases, …….is difficult to understand without further explanation. Either such explanantion is added or the sentence should be deleted. [Klaus Radunsky, Austria]	Revised to clarify.
TS-713	TS	22	19	22	21	These lines contain a very important message that is not brought out well enough as is. Some additional information would make this clearer. Presume the point is that these two sectors - industry and power - co-emit many substances but in particular emit large quantities of SO2 in addition to CO2. Therefore, the near term temperature response, as shown in Figure TS6.b, is a cooling, driven by the SO2 (sulphate) forcing, but this switches to a warming response as the effect of CO2 and other GHGs become dominate. The corollary (and this is important in a section on emission metrics that are applied to emissions reductions) is that emission reductions from these sectors could induce a near term warming. [Government of Canada]	Revised to add the import of this result as suggested.
TS-714	TS	22	30	22	31	This sentence is questionable due to the plateau of atmospheric and SST temperatures observed since the publication of AR4. See also Comment SPM 0. [François Gervais, France]	Taken into account. This sentence is supported through the weatlh of evidence documented in the report. The hiatus is dealt with more thoroughly through the inclusion of the hiatus box which provides an explanation of the hiatus in global mean temperatures which also needs to be put into context of the other changes in the climate system that are documented in the report.
TS-715	TS	22	30	22	31	Repeating this mantra doesn't make it true. You still have no credible evidence whatsoever for a significant	Rejected. To explain how the evidence for a human

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						human influence. Models don't produce "evidence" at any time, even more so when climate models do NOT accurately simulate all climate forces. [John McLean, Australia]	influence on climate is obtained a new lead in paragraph to this section has been added.
TS-716	TS	22	33	22	34	Categorically false. This is the comment of a lobbyist. You show no credible evidence. [John McLean, Australia]	See above, TS-715
TS-717	TS	22	36	23	43	Readers may find the "multiple lines of evidence" language from AR4 to be useful: our ability to do attribution is not only dependent on model results, but also on broader knowledge of the climate system together with observational evidence that recent warming is unusual in the past 1000 years. [Government of United States of America]	Taken into account, section has been revised, although did not adopt the "multiple lines oif evidence language"although there are multiple lines of evidence presented in the text which do support this statement.
TS-718	TS	22	38	22	57	I am amazed that a group of scientists like yourselves would ever imagine that models produce evidence. [John McLean, Australia]	Rejected. Climate models embody the knowledge of climate change physics and processes, and thus represent the framework for testing observations against theory. Evidence requires both observations and agreement with theory as we set out in the revised text.
TS-719	TS	22	38	22	57	This paragraph is dishonest unless it also mentions that models failed to predict the 16-year absence of statistically significant warming that began in January 1997. [John McLean, Australia]	Taken into account. The hiatusis discussed fully in the new box TS.3; this paragraph is about the longer period changes
TS-720	TS	22	39	22	40	are observational uncertainties comparable to internal climate variability or do they contribute to uncertainty associated with internal climate variability; this sentence is ambiguous. [David Sauchyn, Canada]	Accepted, text revised.
TS-721	TS	22	40	22	40	The role of observational uncertainty has been more thoroughly investigated, and found to make a comparable contribution to uncertainties to internal climate variability. Change to “to uncertainties as internal climate variability. (Remove to) [Ned Dwyer, Ireland]	Taken into account. Text revised.
TS-722	TS	22	41	22	42	"dominant drivers of observed warming" - over what period? Since the mid-20th century? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	accepted, periods now added where greenhouse gas contribution is discussed.
TS-723	TS	22	47			reference to Figure 10.10 is wrong, it should read Figure 10.1 [Barbara Früh, Germany]	accepted. Reference to figure in chapter 10 removed.
TS-724	TS	22	53	22	53	I dont think Figure TS.7 addresses the regional issues discussed in the preceeding sentence [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Taken into account. Reference to this figure removed here.
TS-725	TS	22	54	22	54	Atlantic Multidecadal Variability' - this term is used here but elsewhere 'Atlantic Multidecadal Oscillation' is used. There is currently an attempt by the community to move from using AMO to AMV terminology as a more correct description of the phenomena... but mixing it up in the one document will present difficulties for the readers. [Government of United Kingdom of Great Britain & Northern Ireland]	accepted, AMO now used
TS-726	TS	22	56	22	56	..the past ten years...' need to specify the years. [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account. This text has now been deleted.
TS-727	TS	22				The text states that the uncertainty for GWP100 is larger than 50%. The text in Chapter 8 on the top of the page 55 says that the uncertainty for GTP is even larger than that of GWP. In order to give a balanced picture on GWP/GTP issue this should be also stated here. Please, add a sentence like "The uncertainty of GTP is even larger due to uncertaity accumulation in the longer calculation chain." [Government of Finland]	Not this section, previouse TS.3
TS-728	TS	22				The text states that the uncertainty for GWP100 is larger than 50%. The text in Chapter 8 on the top of the page 55 says that the uncertainty for GTP is even larger than that of GTP. In order to give a balanced picture on GWP/GTP issue this should be also stated here. Please, add a sentence like "The uncertainty of GTP is even larger due to uncertaity accumulation in the longer calculation chain." [Ilkka Savolainen, Finland]	Not this section, previouse TS.3
TS-729	TS	23	3			Figure TS.7: I would remove the CMIP3 data in the panels - I think this would greatly improve clarity, and it is also not clear why CMIP3 data are there (the CMIP3/CMIP5 difference in this figure is not discussed in the	Rejected. CMIP3 have been retained since these are additional valid simulations. We think the figure

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						surrounding text). For additional clarity I would also label the panels themselves (i.e. write 'anthropogenic & natural forcings' in the top panel, 'natural forcings only' in the middle and 'greenhouse gas forcings only' in the bottom). [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	labelling is already clear. CMIP3 and CMIP5 simulations are discussed elsewhere in the text and contrasted.
TS-730	TS	23	4	23	5	I think the unit mm/yr is really unfortunate and hard on a reader to understand. I would prefer seeing it as cm/decade (so no change in the number). It also seems to me that, given the rate of rise can vary year to year, it would be better to given in a time averaged unit. [Michael MacCracken, United States of America]	Not this section, page or line number.
TS-731	TS	23	4	23	11	Shorten the caption of Figure TS.7 by only defining the the thin blue and yellow lines once! [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text revised.
TS-732	TS	23	4			is the word "and" correctly placed? [Andrea Flossmann, France]	Accepted. Text revised.
TS-733	TS	23	5	23	5	The JMA dataset has not been assessed in Chapter 2. [Geert Jan van Oldenborgh, Netherlands]	Accepted, and now not included in figures
TS-734	TS	23	14	23	15	Another baseless assertion. Th emore plausible explanation is that the dominance of ENSO conditions on the El Nino side of absolutely neutral (SOI=zero) after1976 was the cause of the warming. The reported increases in temperature and weakeneing of the Walker Circulation point to this. What's more global average temperature anomalies (HadCRUT3) began rising in January 1977, which was seven months after the June 1976 shift in ENSO. This time lag is consistent with the findings of McLean et al (2009). (The paper was criticised but the criticism didn't focus on the Discussion and Conclusions, and it contained several blantantly false claims about what the paper said. The journal refused to show the basic courtesy of allowing the authors to respond, and surely you don't condone that refusal?) Regardless of what you think of the paper, the shift in ENSO and subsequently in the global average temperature anomalies is empirical evidence of a significant connection. Figure 7 of the paper graphed the monthly averages and showed that the relationship existed back to the start of lower tropospheric temperature data, except for periods of intermittent cooling by volanoes. [John McLean, Australia]	Rejected: this assessment is about the period since 1951 rather than since 1976 and in any case includes an assessment of the possible role of ENSO in causing the observed warming.
TS-735	TS	23	14	23	16	“Extremely likely” refers to 95% possibility, while AR4 states that most of the observed increase in global average temperatures since the mid-20th century is very likely (namely, over 90% of possibility) due to the observed increase in anthropogenic greenhouse gas concentrations. Actually, in the above two statements extracted from AR5 and AR4 respectively, “extremely likely” and “very likely” are not describing exactly the same subject. In our view, in order to avoid misleading decision- or policy-makers, the report should explain the implications of this important conclusion and its difference with AR4 in terms of confidence levels in greater details. Otherwise, policy-makers may mistakenly believe that the AR5 conclusion on climate change attribution is simply an increase of confidence level to 95% (extremely likely) from 90% (very likely) in AR4. In addition, the present expression may mislead policymakers into thinking that it is the human activities conducted after 1950 that resulted in the most (more than 50%) observed average global surface temperature increase since the 1950s. it is recommended to add “since industrial revolution (1750)”after “human activities” in this sentence. [Government of China]	Accepted: new text includes the discussion of AR4 results as well for this period and the basis of this new conclusion. We think it is clear that the period quoted refers to the temperature change not the human activities.
TS-736	TS	23	14	23	16	This is a crucial part of the TSM but it is most (!) ambiguous as written. Firstly, the elaboration that "most" means more than 50% (which may be true in a democratic sense, but maybe not in a scientific or even a generally understood sense) is not clear in the SPM. But more seriously, I do not know whether this statement is a re-statement of the AR4 conclusion or a strengthening of it, without some considerable (and inconclusive) semantic scrutiny of the wording. AR4 states that it is "very likely that anthropogenic greenhouse gas increases caused most of the observed increase in temperatures since the mid-20th century". I feel it is essential that the SPM and TSM make absolutely clear whether the new statement represents a nuanced change in wording since AR4 (which rests on the interpretation of (and the consistency of) the usage of the word "most" in AR4 and AR5) or a real change in the confidence level - if it is the latter, then I also think it should be clearly stated what elements of the AR5 analysis have led to this change in confidence level. Whatever, the reader should not be left to guess whether there has been a change in confidence level. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted: revised text includes the discussion of AR4 results as well for this period and the basis of this conclusion.
TS-737	TS	23	15	23	15	Uncertainty guidance AR5, page 4, para E: if likelihood is high or very high, there is no need to specify the confidence. If you do it here, this implies that there is less confidence for other likelihood statements. Please delete confidence statement, or explain why there is a need to mention confidence. [Government of	Accepted. Confidence level no longer specified here.

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						Germany]	
TS-738	TS	23	15	23	15	The expression "extremely likely" should be avoided in AR5. It is part of the agreed uncertainty language outlines in the AR5 Guidance Notes on Uncertainty, but only mentioned in a footnote. The more uncertainty expressions are used in AR5 the more diluted the messages become and we encourage the authors to stick to the 7 main agreed expressions for AR5, especially in regard to this very important statement. In addition, it is confusing for the reader to find likelihood terms that not are included in Chapter 1, please introduce all terms used in AR5 in Chapter 1." [Government of Germany]	Rejected: The assessment could not justify virtually certain, but could justify a higher likelihood than 'very likely'. It is worth noting here that for attribution statements there is a very large difference between very likely and virtually certain. Guidance from co-chairs indicates that 'extremely likely' is acceptable. Extremely likely is defined in the TS at Box TS.1
TS-739	TS	23	15	23	16	This is questionable for the reasons already given in Comment SPM 0. Note also that more than « more than 50 % » seems to be contradicted by the statement « between 15 and 40 % » of Chapter 6 Page 79 Line 7. [François Gervais, France]	Taken into account: rephrased the sentence this refers too. Note however that the fraction of CO2 remaining in the atmosphere for 2000 years of 15-40% is entirely consistent with the assessment of more than half of the warming being attributable to human influence.
TS-740	TS	23	16	23	16	wording of uncertainty ("most (at least 50%)") and reference to the time period is different from statement in SPM, page 10, line 8. Please use the same wording for this very important statement. [Government of Germany]	Taken into account, sentence is rephrased and alighns with the SPM statements submitted to governments for review
TS-741	TS	23	16	23	18	As with comment on SPM p10, 28-30: Unclear. Of course 0.6-1.4 is greater than 0.6, so why does it say 'very likely'. Obviously the 0.6 means something different to the 0.6-1.4 and this needs explaining more clearly. [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account. Text has been revised and is no longer phrased in this way.
TS-742	TS	23	16	23	18	When referring to chapter 10, it seems that this evaluation is mainly supported by one publication (Jones et al, 2012; an other estimate is included in this range). This finding appears to be of “limited evidence” if we apply to it the uncertainty language of the IPCC guidance note and the statement should be associated to a “low confidence”. The qualification “very likely” applied to the following statement should thus also be re-evaluated. [Serge PLANTON, France]	Taken into account: new figure TS.10 in the TS and from Chapter 10, based on a greater range of publications.
TS-743	TS	23	17	23	17	This range of 0.6ºC - 1.4ºC does not seem to match what is shown on Figure TS.8b (where the GHG warming contribution seems more like 0.5 - 1.2ºC. Please review. [Government of Canada]	Taken into account: Ranges have been revised.
TS-744	TS	23	17	23	18	Detection and attribution analyses show that the greenhouse gas warming contribution of 0.6°C–1.4°C was very likely greater than the observed warming of 0.6°C over the period 1951–2010. This sentence is hard to understand. [Ned Dwyer, Ireland]	Taken into account: relevant paragraph has been rephrased.
TS-745	TS	23	18	23	20	I do not understand the sentence "The response…" where does it refer to? [Barbara Früh, Germany]	Taken into account: this sentence is rephrased and now clearer.
TS-746	TS	23	20	23	20	Figure TS.8 - I feel the top frame of this figure will be very hard to understand without considerable elaboration - there is no statement in the text or caption as to what "scaling factor" refers to. Assuming that the TSM should be readable as a stand-alone document, I would recommend removing this Figure from the TSM and leaving it for the detailed chapter. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Taken into account: this figure has been replaced with TS.10.
TS-747	TS	23	20			Solar + volcanic contributed less than 0.1 degree… but can one bound the negative side of that, and/or provide a likelihood of net warming? Eg, the AR4 statement was " the sum of solar and volcanic forcings would likely have produced cooling, not warming." Does this statement just bound the upper end of that, or does it weaken it, or does it strengthen it? [Government of United States of America]	Taken into account: a specific range is given for natural forcings (volcanic plus solar).
TS-748	TS	23	23	23	23	"extremely unlikely" is not a suitable uncertainty quantifier for AR5 for this very important statement, see uncertainty guidance notes for AR5. [Government of Germany]	Rejected: formally we could not say exceptionally unlikely (ie 1% chance of being right) and so extremely unlikely is appropriate and allowed in guidance language. Fiurthermore it is defined in Box TS.1.
TS-749	TS	23	26	23	27	On the contrary, it is VIRTUALLY CERTAIN that changes in the ENSO, modulated at times by cooling caused	Rejected: not supported by the assessment based on

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						by volcanic eruptions can indeed account for the variation in temperature since 1950. [John McLean, Australia]	the peer reviewed literature
TS-750	TS	23	29	23	29	Given the policy interest in the contribution of short-lived climate forcers to global warming, it might be worth including here a statement indicating what the current state of science is with regard to attributing observed global warming to these species. If this is not assessed in the CMIP5 experiments, then it would be helpful to have this made clear. [Government of Canada]	Taken into account: we now have specific statements about "other anthropgenic forcings".
TS-751	TS	23	31	23	37	Fig. TS.7 is only mentioned once in the text. It needs more explanation or can be deleted.(Scaling factor?) [Government of Germany]	Taken into account: this figure has been replaced with TS.10
TS-752	TS	23	31			Figure TS.8: The top panel is very hard to understand, especially as the 'scaling factor' concept is not explained at all in the chart or the description. I suggest removing the top panel, as I think the bottom panel contains the most relevant information for readers and having just one panel would save space. At the very least the top panel should be explained in more detail. [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	Taken into account: this figure has been replaced with TS.10
TS-753	TS	23	32		34	This last sentence seems a touch strong - also it would need a period and timescale etc [Gabriele Hegerl, United Kingdom]	Wrong line numbers? Appears to refer to a figure caption.
TS-754	TS	23	34			Figure caption for Fig TS.8 mentions "scaling factos" without that term having been explained or defined; Explanation needed prior to the term being used (or when the term is first used). [Bart Verheggen, Netherlands]	Taken into account: this figure has been replaced with TS.10
TS-755	TS	23	35	23	35	The corresponding estimated contributions of forced changes to temperature trends over 8 the 1951–2010 period. Maybe clarify: The corresponding estimated contributions of forced changes (greenhouse gas and other anthropogenic components) to temperature trends over the 1951–2010 period. [Ned Dwyer, Ireland]	Taken into account: Caption to new Figure TS.10 is clearer.
TS-756	TS	23	46	23	46	Please add introduction to the subject (at least one sentence please), similar to TFE on water. [Government of Germany]	Introductory sentence added as requested.
TS-757	TS	23	46	25	5	This TFE is less convincing than the others, please improve readibility, structure, put statements into context, improve clarity of statements. [Government of Germany]	TFE has been improved in terms of readability.
TS-758	TS	23	50	23	50	Why only previous IPCC reports and not AR5? [Government of Germany]	The purpose is to assess how previous IPCC projections compare with the observational estimates. As such, we only consider previous reports.
TS-759	TS	23	57			Including the past ten years of data in attribution analyses helps to constrain themagnitude of greenhouse-gas-attributable warming, and does not compromise attribution results' This is a bit opaque - the last ten years deserve a bit more discussion and a bit more explicit treatment - I see there is more in the grey box, but I would expand here as well or link to the grey box [Gabriele Hegerl, United Kingdom]	Figures have been re-worked to show 1950 and beyond. The discussion has been clarified accordingly.
TS-760	TS	23				Section TS.5: Suggestion to include a para from FAQ 14.1 on page 14, lines 47-53 in the TS: "Near-equatorial latitudes are projected to become wetter, while regions on the poleward edges of the subtropics are projected to get drier, as the subtropical high pressure belts continue to expand towards the poles. Regions still farther poleward are likely to experience more rain—and winter snowfall—as the atmosphere warms, and its average moisture content increases. The largest temperature increases, meanwhile, are projected to occur in the Arctic, while lower latitudes and the Southern Hemisphere warm at rates closer to the global mean (FAQ14.1, Figure 1, left). Moreover, the rate of change of extremes of weather and climate varies regionally, influenced by large-scale change and regional effects." This information is very relevant and therefore useful to policy makers. [Government of Germany]	Accepted. Changes in precipitation are shown in Fig. TS.16 and discussed in TS5.5. Changes in extremes are discussed in TFE.9 and shown in TFE.9 Fig. 1
TS-761	TS	24	1	24	24	These figures are far too small and the captions are far too complex. The caption should be no more than 2 or 3 lines and the rest of what you currently have be incorporated into the text. [John McLean, Australia]	The figures have been re-worked for clarity. Unfortunately, complete figure captions are needed for reproducability.

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TS-762	TS	24	11	24	11	I think "reanalyses" should be "analyses" [Geert Jan van Oldenborgh, Netherlands]	Taken into account. Text has been revised.
TS-763	TS	24	16	24	16	Change "due to" to "after incorporating" [David Parker, United Kingdom of Great Britain & Northern Ireland]	Not relevant to revised figure and caption.
TS-764	TS	24	16	24	17	I struggle to understand the meaning of the grey shading on the middle frame of TFE.3 Figure 1. The caption says that the whiskers represent the observational uncertainty and hence I guess that the shading is predominantly "internal variability" but what does this mean? The size of the grey band seems to increase considerably from 1995-2015, but how can this be, if it represents the internal variability over a fixed 1951-1980 period? Over that period, the extremes in variability in hadCRUT4 are from +0.1 to -0.26 K, or 0.36 K in absolute anomaly - and yet the grey shading is considerably more than this, especially by 2015. I am sure this is because I misunderstand what has been plotted, but I am also sure I wont be alone in misunderstanding. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Not relevant to revised figure and caption.
TS-765	TS	24	24			The paper discussing preinstrumental internal variability is still in review… [Gabriele Hegerl, United Kingdom]	Noted
TS-766	TS	24	29	24	34	Why only previous IPCC reports and not AR5? also in Fig TFE.3 Fig 1 [Government of Germany]	The purpose is to assess how previous IPCC projections compare with the observational estimates. As such, we only consider previous reports.
TS-767	TS	24	33	24	34	Has this rate been increasing or constant over the 20th century? [Government of Germany]	The revised figure shows the CO2 concentration from 1950-present, and that the rate has increased.
TS-768	TS	24	38	24	39	Information about the temperature anomaly w.r.t. pre-industrial levels is urgently needed by policy makers. All the discussion in public and policy is centered about the 2C limit which refers to pre-industrial levels, not to the period 1961-1990. Please be more policy-relevant. [Government of Germany]	The purpose of the TFE is to assess to put previous assessment reports into context with the observational estimates. The emphasis is on the period of 1990-2011; however, it is important to understand how this period compares with the latter half of the 20th century. Comparisons with the pre-industrial period are discussed elsewhere in the report.
TS-769	TS	24	38	24	39	"...larger than 0.25°C since 2001.": This is potentially confusing to those unfamiliar with the various global temperture time series. "0.25°C" applies to the entire record from 2001 to 2011 when compared to the preceding range of dates. It is important to avoid confusion by stating up front that the 2001-2011 trend is essentially flat, as noted by the mention of a zero trend quoted in the next row below: [Forrest Mims, United States of America]	Re-working of figures has removed this confusion.
TS-770	TS	24	39	24	42	Is this sentence referring to TFE.3, Figure 1? The reference to "area enclosed in grey" should be rewritten to provide the necessary context. [Government of Canada]	Re-working of figures has removed this confusion.
TS-771	TS	24	43	24	44	FAR, SAR and TAR should have references, be noted in the glossary or spelled out so the reader has the necessary context. [Government of Canada]	Figure caption has been revised as requested.
TS-772	TS	24	47	24	48	I would also mention the almost back-to-back La Niña events of 2008-2011. [Geert Jan van Oldenborgh, Netherlands]	Relevant for the warming Hiatus and covered in box 9.2
TS-773	TS	24	50	24	51	This is not only the case for the recent trend since 2000 but also applies for the trends since 1990 or 1980. I repeat a comment I made in chapter 1: "It is a bit disappointing that a blogger (Lucia Liljegren at The Blackboard) is able to show better comparisons between observed and modeled trends than the IPCC. Please read one of her latest posts http://rankexploits.com/musings/2012/trends-relative-to-models/ and use the same format in the final report. Quite surprisingly this post shows that even if you use 1980 or 1990 as the start year, the observed trends are at the lower range of the multimodel mean and some models are even rejected. Nowhere in AR5 the readers are made alert that this is the case. The three relevant graphs are http://rankexploits.com/musings/wp-content/uploads/2012/11/TrendsJan2000_Sept2012.png, http://rankexploits.com/musings/wp-content/uploads/2012/11/ModelVObservattionsJan1990-Sept2012.png, http://rankexploits.com/musings/wp-content/uploads/2012/11/ComparisonSinceJan1980.png " [Marcel Crok, The Netherlands]	Figure has been re-worked using 1950 as the start point. The revised figure shows that the observed record is well within, but on the lower end of the assessment report range and the CMIP3 model simulations.
TS-774	TS	24	51	24	51	This is a strange sentence for several reasons. It seems to implictly be answering a question ("has the world	The text has been revised to remove this confusion

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						warmed since 1998?") but I wonder why? If it is, the question should be stated explictly. The sentence could be altered to take out the "While" (which seems to possess nuance) and place a "but" before "it is also consistent" so it becomes a simple factual statement that decadal periods without warming are not that unusual even in a warming climate. But I still would wonder why are you choosing 1998? And is it not significant that 2005 and 2010 were as warm as, and possibly warmer than, 1998, and yet were ENSO-free years? And is it possible that there has been a hiatus and we are missing a significant forcing in our climate models [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	and refer the reader to the Chapter 9 warming Hiatus Box 9.2 for specific temperature changes over the recent period.
TS-775	TS	24	51	24	52	Again you leave until very late in asection something that should have been made explicit at the beginning so that readers don't get a false impression. [John McLean, Australia]	The text has been revised to remove this confusion and refer the reader to the Chapter 9 warming Hiatus Box 9.2 for specific temperature changes over the recent period.
TS-776	TS	24	51	24	52	"...trend in global mean temperature since 1998 is not significantly different from zero….": While this zero trend is the consensus view, it differs with the assertion in the sentence quoted in the previous row above. [Forrest Mims, United States of America]	The text has been revised to remove this confusion and refer the reader to the Chapter 9 warming Hiatus Box 9.2 for specific temperature changes over the recent period.
TS-777	TS	24	51	24	53	While the trend in global mean temperature since 1998 is not significantly different from zero, it is also consistent with natural variability superposed on the long-term anthropogenic warming trends projected by climate models.' A trend not being significanlty different from zero means no change, no increase nor a decrease. But what is the explanation for this? Why is the temperature trend since 1998 zero? Why is global average temp not increasing between 1998-2010 while GHG concentrations have increased in the same period? What mechanisms can explain this? There is a wealth of literature on this [Line van Kesteren, the Netherlands]	The text has been revised to remove this confusion and refer the reader to the Chapter 9 warming Hiatus Box 9.2 for specific temperature changes over the recent period.
TS-778	TS	24	51	24	53	While the trend in global mean temperature since 1998 is not significantly different from zero, it is also consistent with natural variability superposed on the long-term anthropogenic warming trends projected by climate models.' A trend not being significanlty different from zero means no change, no increase nor a decrease. But what is the explanation for this? Why is the temperature trend since 1998 zero? [Line van Kesteren, the Netherlands]	The text has been revised to remove this confusion and refer the reader to the Chapter 9 warming Hiatus Box 9.2 for specific temperature changes over the recent period.
TS-779	TS	24	52	24	53	"consistent with natural variability" can imply that a neglibible observed trend over 10 years is not outside the 95% bounds of modeled trends, or it could mean that actual sources of natural variability have been observed to counter anthropogenic warming - eg, the observed weak solar cycle, La Ninas, and volcanoes can explain the cooling. The latter is a little more satisfying than the former in terms of demonstrating that our understanding of the system can encompass the observed trends. [Government of United States of America]	The text has been revised to remove this confusion and refer the reader to the Chapter 9 warming Hiatus Box 9.2 for specific temperature changes over the recent period.
TS-780	TS	24	52	24	53	More to the point, the temperature is consistent with the relatively small extent of variability in the ENSO across the period in question. [John McLean, Australia]	The text has been revised to remove this confusion and refer the reader to the Chapter 9 warming Hiatus Box 9.2 for specific temperature changes over the recent period.
TS-781	TS	24	52	24	54	Why does IPCC consider a trend since 1998, a very strong El-Nino year und thus an outstanding warm year in the temperature time series? At least it should be mentioned that 1998 is a very warm year due to internal climate variability and that the considered time period is considerably shorter than the typical time period applied in the definition of climate. There is a sentence on the potential underestimation of aerosol and SLCF in the RCPs in the SPM on page 12, line 15,16 ("There is robust evidence that collectively the RCPs represent the low end of future emissions scenarios for aerosols and other short-lived reactive gases.") This is very important information that should be added to the TS as well in order to understand the low T-increase in the last decade in comparison to the projections. If there are other reasons, these should be added too. [Government of Germany]	The figures have been revised to include the start year of 1950. The text has also been revised to remove this confusion and refer the reader to the Chapter 9 warming Hiatus Box 9.2 for specific temperature changes over the recent period.
TS-782	TS	24	53	24	53	Should this be "superimposed" rather than "superposed"? [Government of Canada]	corrected as suggested.
TS-783	TS	24	58	25	5	Please give numbers for the total SLR since pre-industrial time, in addition to the rate of increase. [Government of Germany]	Total sea level rise of the 20th century is between 1.4 and 2.0 mm per year.

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TS-784	TS	25	1	25	1	as the time of this conclusion, "in recent ten years" should be added at the end of the sentence "While the increase is fairly steady, both observational records show short periods of either no change or a slight decrease". [Tianyu Zhang, China]	sentence has be re-worked.
TS-785	TS	25	5			Please consider my comment TS 10 54-55. [François Gervais, France]	The recent warming period is discussed in chapter 9 warming hiatus box 9.2.
TS-786	TS	25	11	25	13	This material focusses on temperature trends in the tropics but neglects to say that elsewhere models and observations are in better agreement. Also, new work based on radiosonde data rather than satellite data should probably reduce the "high confidence" associated with this assessment of model/observational disparities in the tropical upper troposphere. (Seidel, D. J., M. P. Free, and J. S. Wang, Reexamining the warming in the tropical upper troposphere: Models versus radiosonde observations, Geophys. Res. Lett., doi:10.1029/2012GL053850, in press, accepted 16 October 2012) [Dian Seidel, United States of America]	Accepted, text modified.
TS-787	TS	25	12	25	12	How can you have high confidence that the models overestimate the trend in the tropical troposphere when the observational chapter 2 only assigns low confidence to the magnitude of the trend? [Geert Jan van Oldenborgh, Netherlands]	Accepted, text modified.
TS-788	TS	25	13	25	13	Why is this bias "elusive"? The reason why, or examples should be provided. [Government of Canada]	Accepted, text modified.
TS-789	TS	25	13	25	13	The sentence "The cause of this bias remains elusive." sounds very casually but could decrease the credibility of the findings on temperature anomalies in general. Therefore, it needs to be put into context please (at least, the statement should be a subordinate clause to the previous, not a full sentence). [Government of Germany]	Accepted, text modified.
TS-790	TS	25	13	25	16	Why do the two periods not coincide? [Government of Germany]	Taken into account; text now refers to the different lengths of radiosonde and satellite records.
TS-791	TS	25	18	25	20	The absence of any qualifier in the following sentence may subject AR5 (as with AR4) to justifiable criticism by climate skeptics: "We conclude, consistent with the AR4, that it is likely that the warming of the troposphere is attributable to anthropogenic forcings dominated by greenhouse gases. {2.4, 9.4, 10.3}" In view of the significant uncertainties involving clouds and aerosols discussed in AR5 and the absence of a trend in global water vapor (see the discussion below about the 2012 NVAP-M report), assigning a "likely" confidence level to the hypothesis that the entire warming of the troposphere is due to anthropogenic forcings is unwarranted. [Forrest Mims, United States of America]	Accepted, text modified. We now say 'likely….. have contributed'.
TS-792	TS	25	26	25	26	"primarily attributable to ozone depleting substances" - I dont think we can rule out a significant contribution from stratospheric water vapour [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted, text modified. We no longer say primarily attributable to ozone depleting substances but instead refer to "dominated by" indicating that it is the major but not necessarily the only contributor.
TS-793	TS	25	33	26	2	Please add information on ocean heat content. [Government of Germany]	Rejected. Heat content information already included in first paragraph.
TS-794	TS	25	42	25	42	"external forcing" is a bit unhelpful here, as this could be natural or anthopogenic. I believe the statement refers to anthropogenic [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted. Text revised and now describes contribution from anthropogenic forcing.
TS-795	TS	25	45	25	45	What does “amplification in the global water cycle” mean [Ned Dwyer, Ireland]	Accepted. Text changed to 'change in the global water cycle'.
TS-796	TS	25	45	25	46	This again states that "Observed surface salinity changes also suggest an amplification in the global water cycle has occurred". See my comments on page 6, lines 43 to 44. My point is that "Observed surface salinity changes also suggest" that stratification is also (more?) likely. [David Webb, United Kingdom]	Accepted. We have reduced the likelihood level for the statement 'human influence has affected the global water cycle since 1960' (TS.4.6) based in part on this consideration.
TS-797	TS	25	45			amplification' doesn’t quite seem the right word here [Gabriele Hegerl, United Kingdom]	Accepted. We no longer use this word.
TS-798	TS	25	47			This sentence reads strange because it is unclear what is meant by "the mean climate of the surface salinity". It is suggested to reword and clarify. [Klaus Radunsky, Austria]	Rejected. We think the existing wording is clear.

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TS-799	TS	25	51	25	51	Why CMIP3, not CMIP5? [Government of Germany]	Accepted. We no longer refer explicitly to CMIP3. But the reason is that the available literature on this topic used CMIP3 models.
TS-800	TS	25	51	25	52	I do not understand what the report means by "observations of surface salinity amplification", or how this can be "a function of global temperature increase per degree surface warming" or be "16 +/- 10% /K", 16% of what. It then refers to "8 +/- 5% /K", and to a figure which compares model rate of changes and gives yet another value. This is very messy and should not be in a technical summary. Mixing model results with observations also appears unprofessional. Model clouds are poor and the model hydrological cycle is likely to have significant errors. We need to use observations as independent tests of reality. [David Webb, United Kingdom]	Accepted. The text has been revised and these numbers are no longer reported in the TS.
TS-801	TS	25	51	25	53	Maybe the surface salinity amplification is strengthened by stratification changes, at least in areas of decreased salinity. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. We have revised the text and removed some of the detail on salinity change.
TS-802	TS	25	51	25	54	There is ambiguity here - I think the 8%/K refers to the change in water vapour in the atmosphere. The change in precipitation, per K, is considerably less, because of energetic constraints. So the word "response" on line 53 is ambiguous - response of what? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted. We have revised the text and no longer report this figure in the TS.
TS-803	TS	25	52	25	52	It has been argued in bothe the observations chapter and the projection chapter that the increase in the hydrological cycle is considerably less than the Clausius-Clapeyron relation would indicate due to a slowdown of the circulation; this is also expected on theoretical grounds (Held & Soden 2006). This result seems at odds with this. Can you comment on this? [Geert Jan van Oldenborgh, Netherlands]	Taken into account: we have removed the discussion of clausius clapeyron equation
TS-804	TS	25	55	25	56	Appears to be an extra word here, is it "trends can be explained" or "trends are explained"? [Government of Canada]	Editorial:
TS-805	TS	25	56	25	56	Salinity changes taken from the simulations with only natural forcings do not match the observations at all, thus excluding the hypothesis that observed trends are can be explained by just solar or volcanic variations. Remove “are” [Ned Dwyer, Ireland]	Editorial:
TS-806	TS	25	56	26	1	This states that "it is likely (with high confidence) that observed changes in ocean surface and sub-surface salinity are due in part to anthropogenic increases in greenhouse gases". I am not sure that the argument here would get far in a court of law. In particular it is in conflict with page 6, lines 12 and 13 which states that "the resulting time series shows little change in land based precipitation since 1900". [David Webb, United Kingdom]	Taken into account: the apparent inconsistency is resolved by undertstanding that the staement on page 6 refers to the global mean whereas it is the pattern of precipitation and evaporation change which is important here.
TS-807	TS	25	56			Are' or 'can be'? [JAVIER MARTIN-VIDE, SPAIN]	Editorial:
TS-808	TS	25	56			It is suggested to delete "are". [Klaus Radunsky, Austria]	Editorial:
TS-809	TS	26	4	26	4	Although this is an "ocean" subsection, I presume this is talking about atmospheric oxygen? But the reference to Figure TS.3 is confusing as the text talks about 1960's to the 1990's and yet the figure starts in 1990 and comes to the present day [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Taken into account: discussing ocean oxygen measurements, and text is now more explicit.
TS-810	TS	26	4	26	5	Which kind of oxygen data? (Concentration in water, Vertical gradients?) Are there more data available now or has the analysis now covered larger spatial scales? [Government of Germany]	Accepted: see above comment
TS-811	TS	26	4		9	Missing: the possible feedback of oxygen depletion to nutrient balance (N elimination and P/Fe increase)cf Keeling et al., 2010 [European Union]	Rejected: ot of scope of this section.
TS-812	TS	26	12	27	43	G.L. Stephens, T. L’Ecuyer, R. Forbes, A. Gettlemen, J.-C. Golaz, A. Bodas-Salcedo, K. Suzuki, P. Gabriel, J.	Rejected. Consistent with the existing literature, this

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						Haynes, in J. Geophys. Res., 115 (2010) D24211, recently reported an error bar as large as 17 W/m2 for the Earth thermal budget. This heavily questions the reliability of the numbers given in this paragraph of SOD. In addition, and contrary to what is claimed in TRS4 paragraph, the ORL measured by the Australian Bureau of Meteorology and available at http://www.bom.gov.au/climate/mjo/graphics/region.ts.dateline.gif, shows that Earth is most often COOLING than heating since 2008. [François Gervais, France]	box assesses that the energy budget is closed by relating changes in radiative forcing, heat uptake, and surface temperature; the latter is linked to outgoing radiation via the assessed range of equilibrium climate sensitivity. Direct observations of TOA radiation cannot be used for this purpose, for reasons explained here.
TS-813	TS	26	12	27	43	The box is very well argued really interesting and very useful. I m still a bit nervous about directly deriving feedback estimates by inverting the sensitivity estimates as probabilistic estimates with flat priors in feedback generally tend to yield narrower estimates - worth thinking about [Gabriele Hegerl, United Kingdom]	Noted.
TS-814	TS	26	16	26	16	suggest replacing "resulting in ocean thermal expansion and hence sea level rise" with "resulting in ocean thermal expansion and hence part of sea level rise" to highlight that sea level rise is partly caused by ocean thermal expansion. [Tianyu Zhang, China]	Rejected. The text correctly states that thermal expansion implies sea-level rise.
TS-815	TS	26	23		28	It is stated in line 23 that the focus here is on the energy budget since 1970. In line 28 it is mentioned an increase in solar irradiance. However, there has been a decrease in solar irradiance since 1970. You must be specific on whether you refer to 1970 or 1750. [Terje Wahl, Norway]	Rejected. The contribution has been positive, albeit small, as TFE.4 Figure 1 shows.
TS-816	TS	26	52	26	52	"grey body" - I wasnt sure why it was necessary to invoke such an approximate term here - the atmosphere is not a grey-body [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted, text revised.
TS-817	TS	27	1	27	9	This para contains many parameters, that cannot be easily understood by non-experts. It would be good to use less scientific jargon, if possible. [Government of Germany]	Taken into account. However, this is a technical summary, so the emphasis must be on technical accuracy.
TS-818	TS	27	5	27	10	Reading the following text I am a little confused by the use of the feedback parameter values relating to effective climate sensitivity. The final sentence suggests that the lower value for the feedback parameter actually gives a much higher equilibrium climate sensitivity value (4.5C) than the higher feedback value (2.0C). Is this the wrong way round?: The mid-range value for α, 1.23 W m–2 oC–1, is equivalent to a radiative forcing for a doubled CO2 concentration of 3.7 W m–2 combined with an equilibrium climate sensitivity of 3.0°C (assessed in Box 12.1 to be the most likely value). Following Box 12.1, the climate feedback parameter α is likely to be in the range from 0.82 W m–2 oC–1 (corresponding to an equilibrium climate sensitivity of 4.5°C) to 1.85 W m–2 °C–1 (corresponding to an equilibrium climate sensitivity of 2.0oC). [David Reay, United Kingdom]	Taken into account. The correspondence of the smaller value of climate feedback parameter with the larger value of equilibrium climate sensitivity is correct, because they are inversely related to each other. But sentence has been rephrased such that it can be understood without the need to keep the inverse relationship in mind.
TS-819	TS	27	15	27	15	"has stored energy" - I think this should be "a changing stored energy"? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Taken into account. Text has been revised.
TS-820	TS	27	22	27	30	residual? Please explain quantity, before discussing it. [Government of Germany]	Taken into account. Paragraph has been deleted during revision.
TS-821	TS	27	33	27	34	Is this only because of the low heat capacity of the atmosphere? And are these steady or cooling decades for global mean temperatures or regions? [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account. It is the large heat capacities that primarily must show larger heat content in a warmer climated.
TS-822	TS	27	51	27	51	How can it only be likely that human activities have contributed to retreat of Arctic sea ice? This has to be certain, given no magnitude is given. I would think it "very likely that human activities have led to most of the reduction in sea ice"--how can it be otherwise? Same for Greenland. [Michael MacCracken, United States of America]	Taken into account: arctic sea ice attribution is revised to be very likely to have an anthropogenic forcing contribution.
TS-823	TS	27	51	27	51	"very likely" for Arctic sea-ice according to Chapter 10. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted
TS-824	TS	27	51	28	2	The use of three different descriptors for anthropogenic influence in these lines makes the reader wonder whether there is something different in the meaning of these sentences. For Arctic sea ice reductions, there is likely a contribution from anthropogenic forcings; for snow cover and permafrost, it is likely there has been an	Taken into account: the liklihoods are not all the same in the revised assessment

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						anthropogenic component to reductions; for glaciers, a human influence on their diminishment is likely. If these statements are all saying the same thing, it would be clearer to merge them in one sentence, especially given that the likelihoods are all the same. [Government of Canada]	
TS-825	TS	27	51			I thnk it is virtually certain that anthropogenic forcings have contributed to sea ice loss. Perhaps not all of it, but there is very low confidence that we would see the degree of change that has occurred without the anthropogenic forcing. At least state the probability as very likely and be consistent with the next page. [Ron Lindsay, United States of America]	Taken into account: the statement has been revised to state that it is very likely an anthropogenic contribution.
TS-826	TS	27	52	27	52	Comment pertaining to Footnote 7 (immediately after line 52). A 'J' [for Joule] should be inserted before the period. [Ian Simmonds, Australia]	Taken into account: energy box now in different units
TS-827	TS	28	10	28	11	This seems a terrible understatement--the reverse needs to be said--that it is very likely that human activities have caused most of the melt-back. [Michael MacCracken, United States of America]	Taken into account: text revised to state very likely anthropogenic contribution.
TS-828	TS	28	10			On all recent timescales assessed (up to 50+ years), it is very likely that the most extreme negative summer sea ice trends observed cannot be explained by modelled internal variability alone (Figure TS.9)' I am not sure that's really a useful statement - modelled internal variability nobody really cares about that right - if you think there is an uncertainty in modelled internal variability of sea ice (and there probably is) then assess overall and tune down maybe? [Gabriele Hegerl, United Kingdom]	Taken into account: This sentence is no longer included.
TS-829	TS	28	10			This assesment as very likely is not consistent with the statement on page 27, line 51. [Ron Lindsay, United States of America]	Taken into account: text revised.
TS-830	TS	28	13	28	18	The Antarctic sea ice extent is accompanoied by a decrease in SST in a much wider zonal band. There is some evidence that these may be due to the fresh water resulting from the increased melt from Antarctica discussed in TS.2.5.5 (Bintanja et al, 2012, submitted). This possibility is not captured in teh climate models employed in D&A. Given the high significance of the sea ice and SST trends in the observational dataand the low confidence we have of our understanding of the prcesses involved it may be better to mention the possibility that the trends could also be anthropogenic. [Geert Jan van Oldenborgh, Netherlands]	Taken into account: text revised. Bintanja et al. is now discussed in the chapter, but we don't think these results are mature or robust enough to elevate to the TS.
TS-831	TS	28	13			Since the 1970s, or since 1979? For example, Cavalieri et al. suggests that satellite data pre-1979 showed higher Antarctic extent... [Government of United States of America]	Taken into account: Text now states since 1979.
TS-832	TS	28	13			Here the increase in Antarctic sea ice is stated to be 'not significant'. However, on pg 7, line 18 from the TS it is written that it is 'small but significant'. Which one is right? [Jan Sedlacek, Switzerland]	Taken into account: now clarified
TS-833	TS	28	14	28	14	it is stated here that the Antarctic sea ice increase my not be significant, in contradiction to the statement made on page 7, l. 18 of this chapter [Rolf Müller, Germany]	Taken into account: now clarified
TS-834	TS	28	17	28	17	"small" - the CMIP5 red-line on Fig TS.9 doesnt look too small to me - it seems to indicate an "expected" loss of about 25% of the Antarctic ice in the last 30 years. There seems a lack of consistency here with other paragraphs in terms of the absence of an attribution statement - are the changes in Antarctic sea-ice unlikely due to human activity? And are the lack of changes at least plausibly related to the discussion about the strengthening SAM that is discussed later in the TS (TSM33 lines 2-4)? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Taken into account: There are competing explanations for the Antarctic sea ice changes which are discussed in more detail in chapter 10. It's probably not due to the SAM.
TS-835	TS	28	17	28	18	Our estimates of the magnitude of the internal variability of the ice extent are highly uncertain because of the short time series of observations and the systematic biases of models. We could thus not state without additional caution that observations and CMIP5 models are within the bounds of internal variability. [Hugues Goosse, Belgium]	Taken into account: now explicitly discuss large differences between simulated and observed variability, and that this precludes an assessment of whether or not the observed trend is consistent with internal variability.
TS-836	TS	28	25	28	26	Specify that this is intrusion of warm seawater [Government of United Kingdom of Great Britain & Northern Ireland]	Rejected. Text already says 'intrusion of wam water'. We think it is clear that this is sea water.
TS-837	TS	28	32	28	35	Nothing is cited in the relevant chapters supporting the statement that "ocean warming" per se is "responsible for accelerating melt rates" under Antarctic ice shelves. The evidence supports only that increased transport	Taken into account: now note transport of heat by ocean circulation.

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						of already-warm water is significant. The attribution of this melting is therefore NOT dependent on whether overall ocean warming is anthropogenic. What matters here is whether the changing wind patterns are anthropogenic. I agree that such attribution is premature. [Eric Steig, United States of America]	
TS-838	TS	28	37	28	38	"These factors combined with incomplete models of Antarctic ice sheet mass loss result in low confidence in scientific understanding, and attribution of changes in mass balance of Antarctica to human influence is premature.". It is not clear why the anthropogenic factor underlying warming of the southern ocean and extensive melting of the West Antarctica peninsula (Velicogna 2009; Rignot et al., 2011) does not apply to the Antarctic ice sheets as a whole? [Andrew Glikson, Australia]	Taken into account: the paragraph is revised from this draft and we note the factors that lead to the assessment
TS-839	TS	28	42			"...likely that the substantial mass loss…" Does this mean that it is likely that a substantial fraction of the mass loss is due to human influence, or that 100% of the large ice loss is due to human influence, or something else? This seems a little unclear. [Government of United States of America]	Taken into account. Text now says 'a substantial part of the mass loss of glaciers'.
TS-840	TS	28	48	28	51	I was surprised that the loss of the Greenland ice sheet was not on the list--and also the Antarctic ice sheet--or parts of it. [Michael MacCracken, United States of America]	Taken into account. Ice sheets have been added.
TS-841	TS	28	54	28	55	"For some events there is information on potential consequences, but in general there is low confidence and little consensus on the likelihood of such events over the 21st century". This statement is inconsistent with a number of recent papers, such as by Lenton, Schellnhuber, Rahmstorf et al 2008, Hansen 2012, Trenberth 2012. Already rising ocean surface temperatures, increased evaporation and enhanced hydrological cycle are resulting in floods in several parts of the world (cf. Pakistan, Australia, China). [Andrew Glikson, Australia]	Noted. However, this comment is dedicated to extreme events rather than abrupt changes as defined in the TS.
TS-842	TS	28	54	28	55	"For some events there is information on potential consequences, but in general there is low confidence and little consensus on the likelihood of such events over the 21st century". This statement is inconsistent with a number of recent papers, such as by Lenton, Schellnhuber, Rahmstorf et al 2008, Hansen 2012, Trenberth 2012. Already rising ocean surface temperatures, increased evaporation and enhanced hydrological cycle are resulting in floods in several parts of the world (cf. Pakistan, Australia, China). [Government of Australia]	Noted. However, this comment is dedicated to extreme events rather than abrupt changes as defined in the TS.
TS-843	TS	29	10	29	13	Teleconnection patterns' requires further explanation [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account. Text has been revised.
TS-844	TS	29	26	29	26	Please clarify the meaning of "glacial stadials". [Government of Canada]	Taken into account. Text has been revised.
TS-845	TS	29	30	29	39	This underlines the point made about SPM p15, 24-27: whilst the models indicate that there may not be a collapse of the AMOC beyond the 21st century, geological records show that this has happened before and so could, feasibly, happen again, albeit far into the future. It therefore needs to be underlined in the SPM that this may be due to a limitation of current modelling capabilities rather than a risk that could be modelled, but is shown to be low. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted.
TS-846	TS	29	47	29	49	I don't think I'm the only one to be shocked by the inclusion of a header "Termination Effect of Geoengineering" and a hypothetical "if solar radiation management (SRM) were put into place…" in the AR5 in such a blithe and casual way. The very limited discussion here is not adequate, and yet a more complete discussion is probably not possible or advisable in the TS. I'll be interested to see how this ends up in the final report, but I hope the authors will reconsider this topic. [Dian Seidel, United States of America]	Taken into account.
TS-847	TS	29	47	30	2	This seems totally out of place here. There has been absolutely no discussion of the potential foregone impacts from the geoengineering, which would presumably be very large given they are supposed to be big enough to get the world to changeover its entire energy system, and yet here the discussion is about a speculation that it might be stopped and suggestions of growing potential mismatches without saying that the benefits of geoengineering would also presumably be getting larger. I think this needs to come out. If you want to discuss irreversibilities, choose permafrost/clathrates, Greenland ice sheet, biodiversity, ocean marine life with acidification, etc. instead of this really hypothetical situation that is not at all well presented here. [Michael MacCracken, United States of America]	Taken into account.
TS-848	TS	29	47			The purpose and structure of this TFE is unclear. Is the purpose to discuss irreversibility and abrupt climate change as integrated issues, or as two separate concepts (as they seem to be presented now)? The inclusion of "termination of geoengineering" in the abrupt climate change section is also confusing since this kind of	Taken into account.

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						change is very different (particularly from a policy perspective) than change linked with the AMOC. Suggest that a good way to organize this may be around "unmanaged" and "managed" sources of abrupt change. [Government of Canada]	
TS-849	TS	29	47			The term "geoengineering" used in the AR5 report and can be easily confused with "geotechnical engineering," also often referred to as "geoengineering."Therefore defining the term in the glossary is not sufficient and some explanation (for example the definition of "geoengineering" provided in Chapter 7 page 60 lines 31-32) should be provided in the TS. [Government of Japan]	Taken into account.
TS-850	TS	29	53			The word "natural" needs qualifying eg use the explanation in chapter 6 on page 72 lines 14-16 [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account.
TS-851	TS	30	4	30	30	Please change title: "Irreversibility" is not very meaningful in a TFE with the same name. [Government of Germany]	Taken into account.
TS-852	TS	30	5	30	9	Please simplify sentence, at is very long and cumbersome. [Government of Germany]	Taken into account.
TS-853	TS	30	18	30	20	Only oceanic methane hydrates are mentioned on line 18 in the introduction to this topic, but then terrestrial clathrates are mentioned on line 20. Please clarify if there are both marine and terrestrial sources on line 18 to set the stage appropriately for the discussion in the following lines of this paragraph. [Government of Canada]	Taken into account.
TS-854	TS	30	26	30	29	It would be helpful to have a statement explaining why ecosystem changes are considered irreversible on century timescales. [Government of Canada]	Taken into account.
TS-855	TS	30	38	30	49	The sentence in line 45 "The reduction in surface elevation as ice is lost increases the vulnerability of the ice sheet;" could be moved at the beginning of the para as an introduction as it helps to intuively understand the topic of the para. [Government of Germany]	Taken into account.
TS-856	TS	30	46	30	46	Case study such as "one study estimated a lower threshold of 1.6 [1.8-3.2]" is not suitable to be presented in the comprehensive discusses in the TS. [Ke Xiu LIU, China]	Taken into account.
TS-857	TS	31	7	31	13	If there is high confidence in the human impact on T-rise, why is the confidence in specific humidy increase low? According to physics, the latter is a consequence of the first. Please clarify. [Government of Germany]	Rejected. This refers to attribution of observed changes in specific humidity, not changes inferred based on observed temperature changes.In any event the confidence is medium not low. And specific humidity changes are not a direct consequence of changes in temeprature - they depend on how relative humidity changes.
TS-858	TS	31	9	31	11	Please clarify sentence: If natural and human influences have not been separated in SSMI, how can the anthrop. fingerprint be detected? [Government of Germany]	Rejected. We state that an anthropogenic contribution to changes in tropospheric specific humidity is found with medium confidence. The lower confidence level than high confidence reflects in part the fact that the corresponding study did not explicitly separate the natural component.
TS-859	TS	31	10			The anthropogenic water vapour fingerprint simulated by an ensemble of climate models has been detected in lower tropospheric moisture content estimates derived from SSM/I data covering the period 1988–2006, although anthropogenic and natural influence were not separated.' - isnt in many ways this fingerprint directly related to warming, as LT water vapour follows the SST very closely? So since the SST is attributed maybe this isnt so much a concern that the exercise hasnt been repeated for water vapour? So I am not sure its worth elevating this concern to the TS. [Gabriele Hegerl, United Kingdom]	Accepted. We have deleted the text referring to separation of the natural component.
TS-860	TS	31	15	31	16	Is the reason that the change is only detected and not attributed still that the observed changes are larger than modelled? [Geert Jan van Oldenborgh, Netherlands]	Noted. This is considered in the chapter, but the more recent studies do not find such a large inconsistency.
TS-861	TS	31	16	31	17	how can there be confidence in the detection of a human influence on changes in precipitation patterns, when there are no significant trends in precipitation time series (see comment above on TS-9 18-21) [David Sauchyn, Canada]	Rejected. The statement about 'little change in land-based precipitation' refers to the global mean, but the attribution studies use the spatial pattern of trends.

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TS-862	TS	31	16	31	17	Again this repeats that "there is medium confidence in a significant human influence on global changes in precipitation patterns". This in conflict with page 6, lines 12 and 13 which states that "the resulting time series shows little change in land based precipitation since 1900". [David Webb, United Kingdom]	Rejected. The statement about 'little change in land-based precipitation' refers to the global mean, but the attribution studies use the spatial pattern of trends.
TS-863	TS	31	19	31	19	This is alos dicussed in Box 11.2. [Geert Jan van Oldenborgh, Netherlands]	Noted. But we think the section cited already are sufficient references.
TS-864	TS	31	21	31	24	This states that changes in the water cycle is very likely (high confidence) to be partly attributable to raising greenhouse gases. This may be true for specific humidity but ocean surface salinity may be explained by other means (see comment on page 6, lines 43 and 44), the special role of subsurface salinity is unclear, the "physical understanding of precipitation" is suspect given the large scatter between model climate sensitivities and as stated on page 6, lines 12 and 13 "the resulting time series shows little change in land based precipitation since 1900". It may be tempting to wish this was true but ... . [David Webb, United Kingdom]	Taken into account. We have moderated the likelihood level here from 'very likely' to 'likely'.
TS-865	TS	31	28	32	7	A reference to the SREX should be added, not only to AR4. [Government of Germany]	Accepted. A reference to SREX has been added.
TS-866	TS	31	31			almost the same comment to extremes - while D+A guys get all happy using multiple fingerprints, the fact that extremes are so closely linked to mean temperature make this a useful but not essential exercise so I wouldn't explicitly mention necessarily. [Gabriele Hegerl, United Kingdom]	Rejected. We prefer to keep in the separation of natural since this is new evidence since AR4 and SREX.
TS-867	TS	31	40		41	Replace "some observed heat waves" with "some types of heat waves" or "some types of observed heat waves". [Terje Wahl, Norway]	Accepted. We now say 'heat waves in some locations'.
TS-868	TS	31	44	31	46	"There is now medium confidence that anthropogenic forcing has contributed to a trend towards increases in the frequency of heavy precipitation events over the second half of the 20th century over land regions with sufficient observations.". Note the implications for the origin of major floods (Pakistan, China, Queensland). [Andrew Glikson, Australia]	Rejected. This is a statement about precipitation itself, and global-scale attribution studies on flooding are not available.
TS-869	TS	31	44	31	46	"There is now medium confidence that anthropogenic forcing has contributed to a trend towards increases in the frequency of heavy precipitation events over the second half of the 20th century over land regions with sufficient observations.". Note the implications for the origin of major floods (Pakistan, China, Queensland). [Government of Australia]	Rejected. This is a statement about precipitation itself, and global-scale attribution studies on flooding are not available.
TS-870	TS	31	48			New research demonstrates the sensitivity of cyclone intensity to SST gradients rather than mean SST' is this a generally accepted conclusion? Conversations I had with TC people lately didn't sound like it. [Gabriele Hegerl, United Kingdom]	Accepted. This sentence has been deleted.
TS-871	TS	32	1		7	If difficulties in characterizing drought is one of the reasons the drought statement has been revised, it might be useful to mention here. [Gabriele Hegerl, United Kingdom]	Rejected. Chapter 10 discusses multiple reasons why the assessment has been revised, so we do not wish to focus on only one here.
TS-872	TS	32	1			What can be said about agricultural drought? [Government of United States of America]	Rejected. Attribution studies on agricultural drought are not available to assess.
TS-873	TS	32	3	32	3	Hydrological drought should be drought. [Qingxiang Li, China]	Rejected. The studies referred to in AR4 generally examined hydrological drought.
TS-874	TS	32	4	32	7	"Owing to the low confidence in observed large-scale trends in dryness combined with difficulties in distinguishing decadal-scale variability in drought from long term climate change we now conclude there is low confidence in the attribution of changes in drought over global land since the mid-20th century to human influence" This statement is inconsistent with earlier statements indicating extension of dry subtropical zones toward the poles, cf TS5 45/7 "a poleward encroachment of subtropical dry zones" [Andrew Glikson, Australia]	Rejected. This statement discusses attribution of observed trends. The text on TS-45, ln 5-7 of the SOD referred to projections of future trends.
TS-875	TS	32	4	32	7	"Owing to the low confidence in observed large-scale trends in dryness combined with difficulties in distinguishing decadal-scale variability in drought from long term climate change we now conclude there is low	Rejected. This statement discusses attribution of observed trends. The text on TS-45, ln 5-7 of the SOD

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						confidence in the attribution of changes in drought over global land since the mid-20th century to human influence" This statement appears inconsistent with earlier statements indicating extension of dry subtropical zones toward the poles, cf TS5 45/7 "a poleward encroachment of subtropical dry zones" [Government of Australia]	referred to projections of future trends.
TS-876	TS	32	4	32	7	I agree with the assessment of _low confidence_ in the attribution of changes in drought over global land, which is consistent with more recent evidence (Sheffield et al. 2012, Nature), as well as with the assessment of the IPCC SREX (see in particular chapter 3, page 170, first paragraph under "Observed changes" in section 3.5.1). However, the IPCC SREX highlighted _medium confidence._ in changes in drought patterns in some regions, and more recent evidence is consistent with this assessment (see also Sheffield et al. 2012, and discussion in Seneviratne 2012, Nature). Hence, I would suggest to distinguish between global changes in droughts and regional changes in droughts. For the latter, even though there is low confidence in observed changes (and thus in their attribution) in many regions, there are a number of regions for which the available literature displays either consistent drying (Southern Europe, West Africa) or consistent wetting (central North America, northwestern Australia) as highlighted in the IPCC SREX. In the case of droughts, assessments for regional trends are more meaningful and robust than those for global changes (see also Sheffield et al. 2012, Nature) since there are changes of opposite signs in different regions. This should thus allow some attribution of drought changes for large regions. References: 1) Sheffield, J., E.F. Wood, and M. Roderick, 2012, Nature, 491, 435-438, doi:10.1038/nature11575; 2) Seneviratne, S.I, Nature, 491, 338-339. [Sonia Seneviratne, Switzerland]	Rejected. There is not sufficient evidence in the literature to support a statement on attribution of regional scale changes in drought.
TS-877	TS	32	9	33	27	What is the topic of this section? Please add introductory sentence, and clarify structure. [Government of Germany]	Rejected. We have tried to shorten the section to concentrate on key points. We did not wish to lengthn the section by adding an introduction. The topic is attribution on sub-global scales.
TS-878	TS	32	9			The description of the TCR is unclear, suggest a change to '(TCR, this is the change in global mean surface air temperature at the point when CO2 concentration has doubled in an experiment where atmospheric CO2 increases by 1% per year) [Government of United Kingdom of Great Britain & Northern Ireland]	Rejected. TCR is not discussed on page 32.
TS-879	TS	32	11	32	12	What is meant with "Longer term persective"? It should definitely include the pre-industrial. Consider clarifying the sentence. [Government of Germany]	Rejected. This sentence already explicitly mentions 'pre-industrial times'.
TS-880	TS	32	11	32	12	internal climate variability is a statistical characteristic of the climate system; it is not a cforcing with an “ability to move heat around the climate system” [David Sauchyn, Canada]	Accepted. This has now been deleted.
TS-881	TS	32	11	32	18	Does this paragraph belong in section TS4.8 - it seems misplaced from the title of the section. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Rejected. This is the best place for this paragraph which focuses on hemispheric temperature (i.e. not global).
TS-882	TS	32	20	32	20	"stronger" evidence: stronger than what? [Government of Germany]	Rejected. Stronger than at the time of the AR4, but we think this is clear from the context.
TS-883	TS	32	20	32	20	"now": present day? [Government of Germany]	Rejected. 'Now' just refers to the current state of knowledge. We think this is clear from the context.
TS-884	TS	32	26	32	26	"predicted" or "projected"? [Government of Germany]	Rejected. Since this refers to the past we think that 'predicted' is more appropriate. ,
TS-885	TS	32	33			Figure TS.9: In the key, put a thick blue line through the middle of the shaded blue rectangle and a thick red line though the middle of the shaded red rectangke. A little detail, but I think it will help the reader to understand the chart much more quickly. [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	Rejected. We think the legend is already clear.
TS-886	TS	32	48	32	48	Please explain why Antartica is different. [Government of Germany]	Accepted. A sentence on Antarctica has been inserted.
TS-887	TS	33	7	32	15	Fig. TS 10 needs context and explanations in the text, or it should be deleted. [Government of Germany]	Rejected. SAM trends are discussed and this figure is cited in the second paragraph of TS.4.8 (as in the

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							SOD).
TS-888	TS	33	8	10	15	It is not clear what "control trends" are, although I can guess that it is something to do with internal variability in the models. This figure would be friendlier if it had a legend. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted. A legend has been added to the figure.
TS-889	TS	33	18	33	26	This para could be move up to Fig. TS 9, to which the text refers. [Government of Germany]	Rejected. Most of the discussion of this figure already appears just before it. This paragraph is just summarising results from the section.
TS-890	TS	33	19	33	19	Please exchange the word "remarkable" with a more objective qualifier, this is rather a value judgement. [Government of Germany]	Rejected. We think this word is appropriate here.
TS-891	TS	33	20	33	20	to which "both records" are you referring? [Government of Germany]	Rejected. This refers to surface temperature and ocean heat content, but we think this is already clear
TS-892	TS	33	24	33	26	Box TS 2 needs context! Why a box on model evaluation in the section "From Global to Regional"? [Government of Germany]	Noted. However, boxes are visually set apart from the main text and thus have an element of stand-alone. The position is near the first extensive use of climate models.
TS-893	TS	33	30	36	20	Boxes TS.2 and TS.3: The positions of Box TS.2 on Model Evaluation and Box TS.3 on Paleoclimate immediately after Box TS.2. within section TS.4 on Understanding the Climate System and its Recent Changes are not logical, the flow of reading is interrupted, because there is no link of the contents. Box TS.2 should be moved in section TS.5 on Projections, and Box TS.3 should be moved in section TS.2. [Government of Germany]	Rejected. Models are also used for detection and attribution, so moving Box TS.2 to the projections section would imply that it appears too late.
TS-894	TS	33	32	34	32	The content in Box TS.2 should be coordinated with the ES of chapter 9 and the logic should also be coordinated. Suggest to add the following sentence before line 46, page 33："The ability of climate models to simulate historical climate, its variability, and its change, has improved in many, though not all, important respects relative to the previous generation of models featured in the AR4." [Lei Huang, China]	Accepted, text modified to be consistent with Ch09 ES.
TS-895	TS	33	32			This section discusses improvements in models since the AR4, but does not document areas that have not improved or perhaps found to be not performing well. Suggest reviewing and revising as appropriate. [Government of Canada]	Taken into account. The text is now more explicit about important instances where models do not perform well. The figure caption explicitly states that no important climate quantity shows degradation in its simulation since AR4.
TS-896	TS	33	34	33	36	Please add information on the difference between prediction and projection. There is some confusion in the text. The definition in section TS.5.3.1, page 39, lines 13-16 should be extended moved in Box TS.2. [Government of Germany]	Noted. Paragraph has been completely rewritten.
TS-897	TS	33	36	33	37	"A particular powerful tool..." please use more neutral language. [Government of Germany]	Noted. Paragraph has been completely rewritten.
TS-898	TS	33	43	33	43	Box TS.2, Fig. 1 needs more explanatory text, or should be deleted. [Government of Germany]	Taken into account. Some text added.
TS-899	TS	33	44	33	44	It is not the model "uncertainty" but model "diversity". The real model "uncertainty" cannot be derived from model intercomparisons (putting the word in quotes does not change this fact). [Government of Germany]	Noted. Paragraph has been completely rewritten.
TS-900	TS	33	46	33	46	"improved notably..." please use more neutral language and explain the facts, e.g. the progress of models from AR4 to AR5 (ESM models, C-cycle, ocean bio-geochemistry etc.). [Government of Germany]	Noted. Paragraph has been completely rewritten.
TS-901	TS	33	51	33	52	There is ambiguity here. I agree that the models probably (but not with "very high confidence") respond properly to the LLGHG's but I wouldnt agree that they respond properly to more inhomogeneous forcings and particularly things like black carbon - I dont know how it would be possible to assess, and therefore give a confidence level, to the response to such forcings [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted; sentence has been re-written so that its scope is much reduced.
TS-902	TS	33	51	33	54	The statement "There is very high confidence that coupled climate models generally respond correctly to	Partly accepted; sentence has been re-written so that

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						external forcing like changing greenhouse gasses." is not supported by the evidence. Chapter 9, page 9, line 11 states that "quantities that are tuned cannot be used in model evaluations". On the same page, lines 1 and 2 state that "modelling centres do not describe in detail how they tune their models". Chapter 9, page 21, lines 13 and 14 states that "Surface temperature is perhaps the most routinely examined quantity in atmospheric models". Without further information it is reasonable to assume that all modelling centres tune their model to give a realistic fit to the surface temperature field. On this basis any good agreement between climate models and the surface temperature field needs to be discounted as it may be the result of parameter fixes. At the zero'th level simple radiation balance arguments explains much of the global warming due to CO2 so the fact that the models produce similar increases is no surprise. If the radiation balance performance is considered average, then the climate models can really only be considered to possibly 'respond correctly' if they really can be shown to do better. If we use this test at the next level, the key quantities in the climate models are the humidity field, where the models do well, and the cloud and rainfall fields where they do poorly. There is also the factor of two difference in the climate sensitivities of the models. On any reasonable measure I would have thought this ranks as poor. On this basis I am tempted to rate the climate models as poor, because by any reasonable test their chance of giving accurate predictions is less than average. OK, so you will probably disagree - but you must agree that there is a serious problem with the climate models that needs to be tackled. The statement "... high confidence ... correctly ..." hides this problem from governments and funding agencies and will not help to solve it. [David Webb, United Kingdom]	its scope is much reduced. Concerning the other comments, they quote incompletely from Ch09 and thus distort what is being said there.
TS-903	TS	33	52	33	52	"realistic simulations"? [Government of Germany]	Accepted, sentence rewritten.
TS-904	TS	33	56	33	57	"The simulation of large-scale patterns of precipitation has improved since the AR4, but there is only medium confidence that models correctly simulate precipitation increases in wet areas and precipitation decreases in dry areas on large spatial scales in a warming climate, based on high agreement among models but only limited evidence that this has been detected in observed trends." The word "correctly" is inconsistent with the description "limited evidence", so suggest to remove it. [Shuanglin Li, China]	Accepted, sentence removed.
TS-905	TS	33	56	34	2	Given the differences between the models and observations, having medium confidence in the models is being kind. [David Webb, United Kingdom]	Taken into account, sentence removed.
TS-906	TS	34	2			Add a reference to Box 11.2 for this. [Geert Jan van Oldenborgh, Netherlands]	Taken into account, sentence removed.
TS-907	TS	34	4	34	6	As with comment on SPM p9, 18-20, how can we say there is high confidence about CMIP5 realistically simulating the annual cycle of Arctic sea-ice extent when we know that observed melt is much more extensive and rapid than that modelled even at the most extreme end of models to date? [Government of United Kingdom of Great Britain & Northern Ireland]	Rejected; the comment is incorrect as shown by the new Figure 9.24. One-quarter of the models show a trend as large as, or larger, than the observations.
TS-908	TS	34	5	34	5	high confidence - I am surprised by this conclusion, as a non-expert in this particular area. See Figure TS.14 where it seems clear that models do a poor job; my conclusion seems consistent with two papers I am aware of which seem to say something quite different. Wang and Overland (GRL, 10.1029/2012GL052868) state "While CMIP5 model mean sea ice extents are closer to observations than CMIP3, the rates of sea ice reduction in most model runs are slow relative to recent observations" and indeed say that the range is "rather discouraging". Similarly Stroeve et al (GRL 10.1029/2012GL052676) come up with an almost identical conclusion. In addition, there is a possible inconsistency with the discussion on TS-42-35 where there is an	Taken into account. The statement has been revised, although, as shown by the new Figure 9.24, one-quarter of the models show a trend as large as, or larger, than the observations. When comparing models to observations, one must take into account the large amount of internal variability, as discussed in Section 9.4.

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						implication that only a subset of models do a decent job of simulating Arctic sea-ice [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	
TS-909	TS	34	5	34	6	This statement is too strong. The magnitude of the CMIP5 multi-model mean trend in summer Arctic sea ice extent is close to the observed one up to 2005, but is significantly underestimated thereafter. [Thierry Fichefet, Belgium]	Taken into account. The statement has been revised, although, as shown by the new Figure 9.24, one-quarter of the models show a trend as large as, or larger, than the observations. When comparing models to observations, one must take into account the large amount of internal variability, as discussed in Section 9.4.
TS-910	TS	34	13	34	17	This optimistic para on the physical understanding of the processes and modeling of the causes of SLR is in contradiction to the statements on future SLR (medium confidence), and that "no consensus" can be reached about the reliability of semi-empirical models. Please clarify and be more consistent. [Government of Germany]	Noted. Paragraph has been removed.
TS-911	TS	34	36	34	36	"how well the CMIP5 models simulate": Please use more neutral language, e.g., "the capacity of the CMIP5 models to simulate" [Government of Germany]	Rejected. Proposed language would reduce clarity.
TS-912	TS	35	13			delete “allow to” [David Sauchyn, Canada]	Accepted. Changed to 'allow'.
TS-913	TS	35	14			It could be useful for readers to add 'long droughts' to 'megadroughts', as a simple definition of this last word. [JAVIER MARTIN-VIDE, SPAIN]	Accepted. The reference to megadroughts has been removed.
TS-914	TS	35	16	35	18	Fast components of abrupt climate events through Cainozoic history may be masked due to limits in the accuracy and frequency of paleo-temperature proxy measurements for periods prior to the ice core evidence. For pre-ice core periods the consequences of abrupt climate events could be identified by their long-term effects – for example: (1) Longevity of atmospheric CO2 on 10^3 to 10^4 time scales; (2) Longevity of changes in pH; (3) Extinction of some species. As have been identified in connection with the PETM (Zachos et al. 2008 and other); (4) isotopic carbon spikes. Other indicators of global and/or regional temperature spikes could include: (1) Charcoal layers in sediments; (2) storm and tsunami layers. The identification of abrupt events in the geological record requires further review of Cainozoic sedimentary records, in particular for warm periods in the Oligocene, Miocene and Pliocene. [Andrew Glikson, Australia]	Noted but beyond the focus timescales for this assessment.
TS-915	TS	35	17			it should be “and” deep ocean not “or” [David Sauchyn, Canada]	Taken into account
TS-916	TS	35	20	35	20	What is "polar amplification"? [Government of Germany]	Taken into account.
TS-917	TS	35	25	35	26	the phrase beginning “Within uncertainties” and ending with “forcing” is unclear in terms of wording and it’s connection with the rest of the sentence [David Sauchyn, Canada]	Taken into account.
TS-918	TS	35	31	35	35	As currently written, the first sentence and the third sentence here seem to say the same thing. Perhaps the third sentence should explicitly say how this conclusion extends that of the AR4 by highlighting the different time periods assessed (650,000 years in the AR4, 800,000 years in the AR5). [Government of Canada]	Taken into account.
TS-919	TS	35	33	35	333	What does "New data extent the AR4 statement" mean? Do they "support" the statement? [Government of Germany]	Taken into account.
TS-920	TS	35	40	35	40	The reference here to the "bipolar seesaw" needs explaining. [Government of Canada]	Taken into account.
TS-921	TS	35	42	35	43	the sense is unclear here the English is cumbersome - is this a good summary of what C14.6.3 indicates? [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account, text has been revised.
TS-922	TS	35	43	35	44	"emissions from early anthropogenic land use are unlikely sufficient to explain this increase.". Does this take account of the widespread burning associated with Neolothic agriculture? [Andrew Glikson, Australia]	Taken into account.
TS-923	TS	35	43	35	44	"emissions from early anthropogenic land use are unlikely sufficient to explain this increase.". Does this take account of the widespread burning associated with Neolithic agriculture? [Government of Australia]	Taken into account.
TS-924	TS	35	51	35	51	What does "CE?" mean? (conformity marking? common era?) CE is not used in the remaining report.	Taken into account.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Government of Germany]	
TS-925	TS	36	3	36	5	It is relevant to note here that GHG levels during the Medieval Warm Period were not significantly higher than those of the 18th century. [Andrew Glikson, Australia]	Taken into account.
TS-926	TS	36	3	36	5	It is relevant to note here that GHG levels during the Medieval Warm Period were not significantly higher than those of the 18th century. [Government of Australia]	Taken into account.
TS-927	TS	36	3			It is suggested to substitute "were" by "was". [Klaus Radunsky, Austria]	Taken into account.
TS-928	TS	36	8			Box TS.3, Figure 1: This is a massively complicated and underexplained figure. I spent 10 minutes trying to understand it and still don't. I would suggest removing the middle row of panels (this is a level of detail that can be left to the main report; a reference to the relevant section is already given in the box text) and possibly also the bottom row. The authors may argue that modelled response to previous volcanic and solar forcings is a key development in AR5 - if so I would argue this needs to be more clearly explained using expanded text and a separate figure. I would then ensure the lines and symbols in the remaining panels are sufficiently explained. For instance, what are the data sources for the lines? Are they proxy reconstructions or models? Are panels b-d showing multiple events in time or different estimates of the same event? What are the large circles and lines in panels e-f? [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	Taken into account.
TS-929	TS	36	9	36	9	Much of this Figure (TS.3 Figure 1) is not discussed in this text, and so it may be better to simplify this figure for the TSM to focus on what is discussed here. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Taken into account.
TS-930	TS	36	9	36	17	Box TS.3, Fig. 1 needs more explanatory text or should be deleted. [Government of Germany]	Taken into account.
TS-931	TS	36	27	36	32	The introduction of the section TS.5 is appreciated, but could be shortened. Instead an brief introduction could be added to each section. [Government of Germany]	Accepted. Shortened as proposed.
TS-932	TS	36	29	36	29	Please define the terms near-term and long-term. [Government of Germany]	Accepted.
TS-933	TS	36	30		32	But the range in near-term aerosol forcing across the RCPs is relatively small. They all assume rapid reductions in SO2 emissions. Figure 8.20 shows a larger difference in WMGHG adjusted forcing in 2030 (add up the bars for all the GHGs) compared to the difference in aerosol AF in 2030, comparing RCP 2.6 versus RCP 8.5. [Nathan Gillett, Canada]	Noted. Comment does not seem to match text, no clear recommendation.
TS-934	TS	36	35			Insert 'plausible' before 'future emissions scenarios'. [Nathan Gillett, Canada]	Accepted
TS-935	TS	36	36	36	37	The new RCP scenarios in the AR5 are all mitigation scenarios with implied policy actions. The report ought to have considered adaptation scenarios as well. This is because developing countries are already suffering from impacts of global warming and have little or nothing to mitigate [Government of Kenya]	Rejected. Adapation is covered in WG2, not WG1.
TS-936	TS	36	36	36	58	Please try to avoid duplication in TS.5.2 and Box TS.4. [Government of Germany]	Accepted. Text shortened.
TS-937	TS	36	39	36	39	The term "to stabilize climate" is contentious as, inherently, both natural climate changes/variations and anthropogenic climate changes/variations can not be "stabilized". I suggest a different term is used, such as "mitigated". [Andrew Glikson, Australia]	Rejected. Stabilization is commonly used in that framing.
TS-938	TS	36	39			Delete 'to stabilised climate'. Since RCP2.6 is mentioned separately, this presumably refers to RCP4.5, but this scenario has ongoing warming through the 21st century. [Nathan Gillett, Canada]	Rejected. The scenarios continue after 2100, and nearly stabilize.
TS-939	TS	36	40	36	46	This information, that the RCP scenarios are framed as a combination of adaptation and mitigation is confusing and not sufficiently well explained. Nor is it mentioned at all in Box 1.2 despite Box 1.2 being referenced here as a source of this information. This is important to understand about the RCPs and should be included in Box 1.2. That said, it is not clear why this assumption, that adaptation strategies will be similar in the near term for the different RCPs, is true. The near term climate change may be similar, but there could be a range of adaptive responses. [Government of Canada]	Accepted. Text on adaptation in near term deleted.
TS-940	TS	36	40		42	But the range in near-term aerosol forcing across the RCPs is relatively small. They all assume rapid reductions in aerosol emissions, as explained in the following paragraph. [Nathan Gillett, Canada]	Noted. Statements are conditional on the RCP, so are correct.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-941	TS	36	49	38	9	I think the RCP scenarios needs to be better explained here. The presentation of the philosophy behind the RCPs are better explained in TFE.8. In particular I miss some more focus on emissions trajectories. [Jan Fuglestvedt, Norway]	Accepted. Text from TFE.8 has been moved to this section.
TS-942	TS	36	49	38	9	Box TS.4: Please explain the difference between CMIP3 and CMIP5 (number of models, new model qualities and processes considered, difference between AOGCM and ESMs, emission vs concentration forcing in ESMs. Box TS.4 should also inform about the difference between the CMIP5 models and the simpler IAMs, which have been used for the development of the RCPs have been developed. This is needed for non-experts to understand the basics of the scenario process, which is of high importance to AR5. [Government of Germany]	Box TS.4 (now Box TS.6) has been substantially revised to include some of the requested material, but space constraints do not allow for a discussion of all details, which can be found in the chapter.
TS-943	TS	36	53	36	57	The description of the RCPs is not accurate, and no reference for the description is given. The RCPs are not all "mitigation scenarios". The RCPs span the full range of radiative forcing associated with emission scenarios (both mitigation and baseline -- like the SRES scenarios) published in the peer-reviewed literature at the time of the development of the RCPs. Descriptions of the RCPs should be drawn from the literature about their development (e.g. van Vuuren et al. 2011, Climatic Change 109:5-31). [HAROON KHESHGI, United States of America]	Accepted. Text revised.
TS-944	TS	36	54	36	54	Box TS.4 Figure 1 is referenced here for a statement about the new RCPs vs the old SRES scenarios. It is not an appropriate Figure for illustrating this difference. A more general Figure illustrating the different approaches is required here. Furthermore, this is the ONLY reference to Box TS.4 Figure 1 and it is a very complicated Figure. This is not adequate. Further discussion of this Figure, if it is to be used here, needs to be brought into the text in this section. A suitable place may be on page TS37 after line 39 as the paragraph on lines 29-39 discusses the uncertainty ranges on the temperature projections with the RCPs (info that is shown in Box TS.4 Figure 1). [Government of Canada]	Accepted. Figure deleted.
TS-945	TS	37	2	37	2	"... are summarized here.": where is here? [Government of Germany]	Accepted. Sentence removed.
TS-946	TS	37	5	37	11	Fig. TS.4: Great figure, but needs much more explanations. [Government of Germany]	Noted. Figure was found to be doo complex by others, and is removed, but remains in the chapter.
TS-947	TS	37	5			Box TS.4, Figure 1: The different uncertainty ranges in the figure do not seem to relate to the points being made in the box. From the box text, I would surmise that the different uncertainty ranges that ought to be shown are a) CMIP5 models using RCP scenarios; b) CMIP3 models using RCP scenarios and rebased to the same reference and future periods as CMIP5; c) CMIP3 models from AR4 using SRES scenarios and old reference & future periods. There may also need to be another range to indicate the difference between concentration- and emissions-driven scenarios. If the Rogelj et al. and Good et al. ranges need to be shown in the figure, a justification/explanation should be given in the text. [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	Noted. Figure was found to be doo complex by others, and is removed, but remains in the chapter.
TS-948	TS	37	7	37	8	Please explain the likelihood ranges and associated uncertainties. See also our comment on section 12.4.1.2 in chapter 12 [Government of Germany]	Noted. Figure was found to be doo complex by others, and is removed, but remains in the chapter.
TS-949	TS	37	13	37	13	The RCPs need to be explained first before the main comparison points are presented. Also, some insight on what mitigation and policy actions are needed would be useful. [Dora Marinova, Australia]	Accepted. RCP description extended at the beginning of the box. Mitigation and policy is not the scope of WG1.
TS-950	TS	37	22	37	23	please write more clearly: the carbon cycle climate feedbacks are considered in AR5 but not in AR4 [Barbara Früh, Germany]	Accepted. Reworded to clarify.
TS-951	TS	37	26	37	26	What is the "fractional uncertainty"? [Government of Germany]	Accepted. Definition just above, added this in brackets to clarify.
TS-952	TS	37	29	37	30	"The 5–95% range of the CMIP5 projections is considered “likely” for projections of global temperature change." Please explain the likelihood ranges and associated uncertainties. See also our comment on section 12.4.1.2 in chapter 12 [Government of Germany]	Noted. Not clear what should be explained, a range across models is assessed to be likely. Details are given in 12.4.1.
TS-953	TS	37	30	37	33	these sentences are repeated below page on TS 38 line 19-22 [Michiel van Weele, Netherlands]	Accepted. Text deleted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-954	TS	37	35			Remove "other" in "aerosols and other short-lived reactive gases" or change to "most". Also mention that in contrast to most short-lived reactive gases the RCPs do span a reasonable range for NH3 (see e.g. van Vuuren et al., Current Opinion in Environmental Sustainability, 3, 359–369 (2011)). [Twan van Noije, Netherlands]	Noted. Text removed.
TS-955	TS	37	36	37	37	Is it correct to state that all RCP scenarios are mitigation scenarios with implied policy? First, I thought the RCPs were very deliberately decoupled from socio-economic assumptions (which are developed separately as SSPs). Second, it is hard to imagine a world which follows RCP8.5 while undertaking mitigation action. Suggest amending this sentence to something like "The new RCP scenarios in the AR5 are not coupled to specific socio-economic scenarios, however they are designed to include the possibility of concerted mitigation throughout the 21st Century." [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text reworded and sentence added as suggested.
TS-956	TS	37	43			please include "on" before "spatial and temporal averaging" since otherwise it is difficult to understand [Barbara Früh, Germany]	Accepted.
TS-957	TS	38	4	38	4	The meaning of the stippling in the figure is not stated in the caption [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted. Added.
TS-958	TS	38	12	38	17	This para would be better situated within Box TS.4. [Government of Germany]	Accepted. Paragraph moved.
TS-959	TS	38	17	38	17	This topic (emission driven vs concentration driven projections) is not discussed in BOX 1.2 although it is given here as a reference. It would help if such information were included in Box 1.2 [Government of Canada]	Accepted. Reference changed.
TS-960	TS	38	19	38	22	The range in anthropogenic aerosol emissions across all scenarios has a larger impact on near-term climate projections than the corresponding range in long-lived greenhouse gases, particularly on regional scales and for hydrological cycle variables {11.3.1, 11.3.6}. The RCP scenarios do not span the range of future aerosol emissions found in the SRES and alternative scenarios (see Box TS.4).' This paragraph is mentioned literally in BOX TS.4 op TS p 37 l 30 - 33. Suggest to cut here and just refer to the box. [Line van Kesteren, the Netherlands]	Accepted. Duplication eliminated.
TS-961	TS	38	19	38	22	The range in anthropogenic aerosol emissions across all scenarios has a larger impact on near-term climate projections than the corresponding range in long-lived greenhouse gases, particularly on regional scales and for hydrological cycle variables {11.3.1, 11.3.6}. The RCP scenarios do not span the range of future aerosol emissions found in the SRES and alternative scenarios (see Box TS.4).' This paragraph is mentioned literally in BOX TS.4 op TS p 37 l 30 - 33. Suggest to cut here and just refer to the box. [Line van Kesteren, the Netherlands]	Accepted. Duplication eliminated.
TS-962	TS	38	19	38	22	these sentences are repeated above on page TS 37 line 30-33 [Michiel van Weele, Netherlands]	Accepted. Duplication eliminated.
TS-963	TS	38	19		22	Exact repetation of text on previous page (pg 37, ln 30-33). [Nathan Gillett, Canada]	Accepted. Duplication eliminated.
TS-964	TS	38	24	38	25	In Western Europe I am not aware of evidence that there is a robust relationship between sulphate reduction and black carbon reduction. Though the absence of a widely accepted and measureable definition of black carbon for emission quantification purposes does not help such discussions to be well founded. There remains concern that both economic pressure and decarbonisation policies to increase biomass combustion may lead to an increase in black carbon emissions in some countries but it is not clear from the scenario descriptions whether this has been included. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted. Assessment of the scenarios and drives is part of WG3.
TS-965	TS	38	24	38	29	Suggest changing the word 'element' to a more commonly used term, such as 'indicator' or 'variable'. [Government of United Kingdom of Great Britain & Northern Ireland]	Rejected. Comment does not match text.
TS-966	TS	38	24	38	30	This paragraph is important and directly addresses a question that is of interest to policymakers. These lines should be considered for inclusion in the SPM. That said, some revisions are suggested to improve clarity here. Suggest that after the words "sulphate-induced warming" the following be added "although the cooling from methane mitigation will emerge more slowly than the warming from sulphate mitigation due to the different timescales over which atmospheric concentrations of these substances decrease in response to decreases in emissions" (or something to that effect, if indeed this is true). [Government of Canada]	Noted. Statement will be considered, but due to space not all propose statement will be kept. Statement changed as proposed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-967	TS	38	24		25	I don't think there is much evidence to support this assertion. It is true that reductions in sulphate aerosols may conribute to warming. But under the RCP scenarios, the total change in adjusted forcing due to aerosols by 2030 is almost zero for RCP 2.6, and small for RCP 8.5 (only a fraction of the AF due to WMGHG increases). See Figure 8.20. Also the aerosol attributable cooling at present is only ~0.4 K - see e.g. Figure 10.4. The aerosol reductions in the RCP scenarios are progressive through the 21st century, so the corresponding warming should only be ~0.4K over the century. This is small compared to the rate of warming associated with GHGs. The studies cited to support this assertion in chapter 11 only show that aerosol reductions will contribute to the near-term warming, they don't demonstrate how large this contribution will be, or indeed show that the resulting warming will be particularly rapid. [Nathan Gillett, Canada]	Noted. Further discussion and evidence is given in section 11.3.6
TS-968	TS	38	32	38	38	What does "abundance pathway" mean? Do you suggest that the projected atmospheric concentrations are potentially underestimated? [Government of Germany]	this term is equvalent to concentration pathway. As noted, , these concentrations are likely 30% larger than the range in RCP concentraitons used to force the climate models in CMIP5
TS-969	TS	38	32		34	I found this sentence confusing. I think it is comparing the uncertainty in CH4 and N2O concentrations for given RCP emissions with the spread in concentrations across the RCP scenarios. This is not a like-for-like comparison. I think it would be more useful to give the uncertainties in concentrations in percentage terms in 2100, say. [Nathan Gillett, Canada]	Interesting suggestion, but the conventions used here in the TS are those carried forward from section 11.3.5 in ch 11
TS-970	TS	38	37	38	38	No where in this technical summary are we given information about how CH4 pathways change in the various RCPs so it is difficult to put the statement here in context. Similar to Canada's comments on the SPM, we suggest that such information would be useful. [Government of Canada]	Noted. Please see discussion in 11.3.5
TS-971	TS	38	39	38	47	Please try to avoid duplications with the Box TS.4. [Government of Germany]	noted, and efforts have been taken to avoid duplication, though some duplication is necessary for continuity between the text and the Box
TS-972	TS	38	40	38	45	The message here that changes in total solar irradiance are uncertain for the future may be true but it also needs to be made clear whether this uncertainty, or to what extent, this affects projected warming from GHGs. Do we expect the relative magnitude of solar forcing vs GHG forcing to be similar over the coming century as it was over the past century (or from 1750 to current)? This perspective is important to know whether we are confident that anthropogenic emissions of GHGs will be the dominant forcing over the coming century or not. [Government of Canada]	As discussed and referred to in section 11.3.1, possible future solar effects are small compared to projected warming from increasing GHGs
TS-973	TS	38	49	38	49	Equilibrium climate sensitivity and transient climate response should be placed in context by citing (or referring to) the number of years required to achieve the equilibrium or over which the transient is diagnosed. [Government of Canada]	Rejected. Details and definition given in the TFE to which the text refers.
TS-974	TS	38	49	38	51	"The assessed literature supports the conclusion from AR4 that equilibrium climate sensitivity (ECS) is likely 50 in the range 2°C–4.5°C, very likely above 1.5°C, and very unlikely greater than about 6°C–7°C. The most 51 likely value remains near 3°C." However, in so far as slow feedbacks include major changes in GHG, ice sheet extent and vegetation cover, curent climate change may correspond to the Pliocene ECS values by Pagani et al. 2009, i..e. ~6 degrees C or higher for doubling CO2. [Andrew Glikson, Australia]	Noted. ESS is explicitly mentioned, and discussed in more detail in the TFE and chapters.
TS-975	TS	38	49	38	57	Please provide more explanations of these scientific terms (ECS, ESS, TCR) for non-experts. [Government of Germany]	Rejected. Details and definition given in the TFE to which the text refers.
TS-976	TS	38	49		50	As written the text implies that the AR4 concluded that ECS is very unlikely greater than 6-7C. But this is a new assessment. Make this clear. [Nathan Gillett, Canada]	Accepted. Changed as proposed.
TS-977	TS	38	50			The words'most likely' appear here. This is an unfortunate statement as it will be latched onto by decision-makers when it is highly uncertain especially since the statement is also made that carbon-cycle feedbacks aren't included. Decision-makers head for these sorts of statements and ignore the range of possible futures which is what the WG1 is based on. I would suggest 'most likely' wording be expunged from the IPCC lexicon altogether as it is highly misleading. It is impossible to make such a judgement based on a range of scenarios as it implies that the sceanio is a prediction and has probailities assigned when that is not how the assessment has been conducted. [Judy Lawrence, New Zealand]	Most likely values are no longer provided.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-978	TS	38				Section TS.5.3: I think that the problem that I have with this section is a major one which runs throughout the whole document and which is also partly reflected in my comment on page 33, lines 51 to 54. My problem is that, if my memory is correct, the spread of AR4 climate sensitivities quoted here differ little from the values quoted 20 years ago. You can check with people like Ulrich Cubasch and Gerald Meehl who attended the same meetings, but the values seem very familiar. How can the report say that it has "high confidence" in the assessments of ECS when the spread is still O(2). What seems to have happened is that the ability to represent the present climate has improved so there is "high confidence" in the models' ability to do this. Some of the improvement will have been due to better physics, especially coming from higher resolution, but other improvements will have come from changed fudge factors in the sub-grid scale models. Given time for enough tests it is always possible to find a mix of sub-grid scale models and constants which gives a better fit to a given data set. However this does not constrain projections and the fact that the agreement predictions is as bad as ever seems to only show that (a) the present climate can result from a wide range of different physics and (b) different physics can generate different futures. It may be a cultural thing - people have to improve models until they agree more or less with the rest of the pack. The zero state, the current climate, is then OK but the perturbation response can be anywhere. People will probably argue that the fact that a model works in different climatic zones means that some of its perturbation response must be right. OK I agree but how can the models all have a good perturbation response and still have O(2) different sensitivities. I like this chapter because it ends with a list of some key uncertainties. But I think that this problem with sensitivity is "the" key uncertainty. To a large extent the report pretends it is not there. Maybe to fix it the report needs to be much more up front with the problem. It need not say the models are bad any more than is already in the document but much stronger statements are needed to help focus future work. [A couple of additional points that may be relevant: 1. As an example of what I suspect happens - in the ocean at least one climate model fits the Indonesian Throughflow to the observations. The result looks good in talks and papers but the perturbation response to any change in the forcing must be dubious. Similarly with the Nordic Sea overflows, other overflows and all those other lines of computer code which seemed sensible at the time. 2. Adding land, vegetation and other sub-models also looks good and may add some improvements but it also adds extra parameters to 'adjust'. These can improve the present climate in the model but make the predictions worse. I am not saying it always does, extra models are usually added for a good reason but the additional code and parameters need to be part of the analysis, like a statistical T test. 3. Twenty years ago clouds were a problem. It seems they still are (page 6, line 36).] [David Webb, United Kingdom]	Noted. Confidence in a range can be high even if that range is large. The range indeed has not changed much but the amount of evidence has increased massively. A detailed assement of the models is given in the model evaluation chapter. More details on ECS /TCR are given in TFE.6, and there are whole sections of the reports just discussing that.
TS-979	TS	39	4	39	7	"The transient climate response to 5 cumulative carbon emission (TCRE) is very likely between 0.8°C–3°C PgC–1 (1012 metric tons of carbon), 6 with a best estimate in the range of 1.5°C–2.0°C PgC–1, for cumulative emissions less than 2000PgC until the 7 time at which temperatures peak". Do these values take the release of CH4 from permamfrost , sediments and bogs into account? [Andrew Glikson, Australia]	Rejected. TCRE is only for CO2.
TS-980	TS	39	4	39	7	Again, the stated values of transient climate response appear strange. A range of 0.8-3C per PgC is cited, but this would be far too high a response for just a 1Pg C increase in cumulative emissions - I'm probably	Accepted. Typo in unit fixed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						misunderstanding this... [David Reay, United Kingdom]	
TS-981	TS	39	4	39	7	the metric TCRE (transient climate response to cumulative carbon emission) is defined here in TS and can not be traced in e.g. Chapter 6. I doubt its usefulness. The unit is unclear (relative to increase in cumulative emissions per year?) as well as the valid range. TCR is strictly defined as deg Celsius at the time of CO2 doubling. Please clarify its usefullness or remove TCRE. The quantity can also not be traced in the SPM [Michiel van Weele, Netherlands]	Rejected. References are given to chapter 10, chapter 12 (e.g. 12.5.4), box 12.2, TFE.8, and this is also part of the SPM.
TS-982	TS	39	6	39	6	What does "best estimate" mean? Is it the likely range? [Government of Germany]	Noted. Text removed.
TS-983	TS	39	11			The term 'prediction' appears when talking about the near term. This seems to be the only section I could find the word used. I expect it is an abberation. Again it is misleading particularly since at line 55-57 it is defined as an estimated change. [Judy Lawrence, New Zealand]	Noted, and please refer to definitions of projection and prediction in section 11.1 of ch 11
TS-984	TS	39	13	39	16	The definition of difference between prediction and projectionin section TS.5.3.1 should be extended and also mentioned in Box TS.2. [Government of Germany]	Box TS.2 deals with radiative forcing. Prediction and projection are terms relating to different time scales and applications of climate models,not radiative forcing
TS-985	TS	39	13			The term 'accurate estimate' is used. This would appear to be an oxymoron. An estimate by definition cannot be 'accruate' . It might be robust across a number of input assumptions but not 'accurate' . Suggest alternative wording be used as this sort of language confuses decision-makers making them think that scenarios and estimates are somehow what will actually happen as they seek certainty in their decision-making. [Judy Lawrence, New Zealand]	Noted, though this is a vernacular interpretation that has no definitive meaning other than in the application of the term.
TS-986	TS	39	16	39	16	"committed warming"? Please specify, see FAQ 12.3. [Government of Germany]	the parenthetical statement that follows explains why committed warming arises
TS-987	TS	39	16	39	18	"Estimates of near-term climate depend on the committed warming (caused by the inertia of the oceans as they respond to historical external forcing) the time evolution of internally-generated climate variability, and the future path of external forcing." As current global temperatures are in part masked by sulphur aerosols, to what extent do the near-term predictions depend on variations (increase or decrease) in this parameter? [Andrew Glikson, Australia]	As stated in this paragraph, committed warming is just one factor that affects near-term climate change, with "the future path of external forcing" (e.g. sulphur aerosols) also contributing
TS-988	TS	39	16	39	18	"Estimates of near-term climate depend on the committed warming (caused by the inertia of the oceans as they respond to historical external forcing) the time evolution of internally-generated climate variability, and the future path of external forcing." As current global temperatures are in part masked by sulphur aerosols, to what extent do the near-term predictions depend on variations (increase or decrease) in this parameter? [Government of Australia]	As stated in this paragraph, committed warming is just one factor that affects near-term climate change, with "the future path of external forcing" (e.g. sulphur aerosols) also contributing
TS-989	TS	39	19	39	21	The references on these lines are the only references to Figures TS.11 and TS.12 and here all that is said about them is that they refer to near-term predictions out a decade (TS.11) and near term projections beyond a decade (TS.12) . This is not sufficient for readers to understand what these Figures are illustrating nor what messages they are to extract from these complicated Figures. Some additional information about these Figures should be provided. This is particularly puzzling since there is a whole paragraph on the following page (TS40 lines 2-14) about climate prediction experiments with references to other Figures in chapter 11 not shown in this technical summary. Perhaps Figure TS.11 should be replaced with one of these other Figures? [Government of Canada]	This has been changed to refer to Fig. TS.13
TS-990	TS	39	20			The section goes on to say 'accurate depiction" of internally generated climate variability. It is not clear whether this relates to historic variability in which case it is correct to use accurate in this way. If it is about future variability then the term accurate is not teh right word to use. [Judy Lawrence, New Zealand]	Again this is a vernacular issue, and the usage here and elsewhere is consistent
TS-991	TS	39	24	39	46	Fig. TS.11 and TS.12 need more explanatory text or can be deleted. [Government of Germany]	This has been changed to refer to Fig. TS.13
TS-992	TS	39	24			Figure TS.11: The caption erroneously states there are only two rows, with the bottom row showing 'AMV'. This needs to be changed to state there is a middle row showing AMO and a bottom row showing IPO. Also AMV on line 12 needs to be corrected to AMO. [Stephen Smith, United Kingdom of Great Britain & Northern	This has been changed to refer to Fig. TS.13

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Ireland]	
TS-993	TS	39	24			Figure TS.11: The skill panels need to be better explained. How does skill relate to 'correlation' quantified on the y-axes? And does the shaded 95% confidence area mean that if a line lies within it, we have little confidence in the projection? [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	This has been changed to refer to Fig. TS.13
TS-994	TS	39	25	39	25	Figure TS.11 baffled me - are these retrospective predictions (a term used on page TS-40)? To be understood in a free-standing TSM more explanation is needed of what is being shown. Is it significant that the models seem to capture the slowing of the trend in recent years, or is this because of some constraint in the models (e.g. specified boundary conditions?)? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	This has been changed to refer to Fig. TS.13
TS-995	TS	39	49	39	49	Statement is not consistent with FAQ 12.3.. How much is the unavoidable warming? [Government of Germany]	As stated earlier, committed warming is just one factor that affects near-term climate change, with "the future path of external forcing" (e.g. sulphur aerosols) also contributing
TS-996	TS	39	49			I don't think it's strictly true that further near-term warming is 'unavoidable' due to the thermal inertia of the oceans. A complete cessation of emissions would approximately prevent near-term warming, as could a volcano or geoengineering. The sentence is true only in the absence of a rapid reduction in radiative forcing. Insert 'In the absence of a large rapid reduction in radiative forcing' at the beginning of the sentence. [Nathan Gillett, Canada]	As stated earlier, committed warming is just one factor that affects near-term climate change, with "the future path of external forcing" (e.g. sulphur aerosols) also contributing
TS-997	TS	39	54	39	55	"Decadal climate predictions, on the other hand, are intended to predict both the externally 55 forced component of future climate change, and the internally generated component ". It is not clear to what extent medium-term decadal preditions take into account (1) the climate effects of a slowing down and possible collapse of the North Atlantic Ocean Current and (2) the rate of CH4 release from Arctic permafrost and sediments. [Andrew Glikson, Australia]	As stated earlier, committed warming is just one factor that affects near-term climate change, with changes in external forcing and internally generated variability (that can be associated with ocean circulation changes) also contributing
TS-998	TS	39	55			The text says near term predictions "provide estimated changes in the time evolution of the statistics of near term climate variability" . The mixing of the word "prediction" with "estimates" is confusing to the reader and particularly to decision-makers who often do not understand what a scenario is compared with an estimate with a prediction. Also this wording has high agreement beside it when it contains huge uncertainties which implies that the experts are in agreement about 'predictions' based on statistical methods. Is this what is intended? [Judy Lawrence, New Zealand]	This again relates to vernacular and how people may or may not interpret such vernacular. Further discussion of vernacular-related issues raised here is given in section 11.2.2 and 11.3.3
TS-999	TS	40	2	40	4	"There is high confidence that the retrospective prediction experiments for forecast periods of 1 to 18 years have statistically significant regional temperature correlations with the observations (exceeding0.6 over much of the globe) "。此句中仅提及全球很多地方，用high confidence会显得信度过高，建议修改。 [Shuanglin Li, China]	this has been changed to read "up to 10 years"
TS-1000	TS	40	2	40	4	"There is high confidence that the retrospective prediction experiments for forecast periods of 1 to 18 years have statistically significant regional temperature correlations with the observations (exceeding 0.6 over much of the globe) ". The description words "high confidence" seem inappropriate for much of the globe. [Shuanglin Li, China]	this has been changed to read "up to 10 years"
TS-1001	TS	40	16	40	16	Projected near-term changes in climate. The ice core record contains examples of abrupt climate events (cf Steffensen et al. 2008) while modern projections discuss abupt climate change and tipping points (Lenton, Schellnhuber, Rahmstorf et al 2008). The AR5 WG1 uses this term in some sections (TFE5) but not in the Summary for Policy Makers - yet the likelihood of tipping points under the current warming trend, which is extreme in a geological perspective, is in my view very high. [Andrew Glikson, Australia]	Your view is noted but not supported enough by the text chapters to be carried forward to the TS
TS-1002	TS	40	16	40	16	Projected near-term changes in climate. The ice core record contains examples of abrupt climate events (cf Steffensen et al. 2008) while modern projections discuss abrupt climate change and tipping points (Lenton, Schellnhuber, Rahmstorf et al 2008). The AR5 WG1 uses this term in some sections (TFE5) but not in the Summary for Policy Makers - yet the likelihood of tipping points under the current warming trend, which is extreme in a geological perspective, is possible. [Government of Australia]	Your view is noted but not supported enough by the text chapters to be carried forward to the TS
TS-1003	TS	40	16	40	22	"Projected" or "predicted"? What does "near-term" mean. Please define these terms. [Government of	"near term" is defined in TS.5.1 as "up to about mid-

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						Germany]	century"
TS-1004	TS	40	18		20	But ammonia emissions increase, resulting in increased nitrate cooling, which in RCP 8.5 approximately compensates for the change in sulphate cooling to 2030. See figures 8.2, 8.20. [Nathan Gillett, Canada]	This sentence refers only to aerosol emissions in the RCP scenarios, not ammonia emissions
TS-1005	TS	40	20	40	21	"given below": where? ref periods are also mentioned below. [Government of Germany]	"given below" is standard usage to refer to what follows
TS-1006	TS	40	25	40	27	This statement that actual warming is more likely than not to be closer to the lower bound of the projected temperature range needs substantiating/explaining. [Government of Canada]	this has been re-worded and clarified in the new TS.5.4.2
TS-1007	TS	40	35	40	36	In the near term, model uncertainty and natural variability therefore dominate the uncertainty in projections of global mean temperature.' This conclusion is correct although it could well be that model uncertainty is at least partly a dominant term in near-term projections because of the applied large changes in aerosol forcing in the near-term projections throughout the RCP scenarios. Models are know to be variable sensitive to aerosol changes. This specific model deficiency might get very prominent in the near-term projections because of the large aerosol changes assumed throughout the RCP scenarios to which models might respond differently. By comparing with model simulations without rapid aerosol changes the importance of the aerosol changes on model uncertainty can be fully assessed. Maybe a short remark could be made on increased model uncertainty during time periods of (assumed) rapid aerosol changes (both over last 30 years and the coming 30 years), see also same comment on same text in Chapter 11 P4 L37-38). [Michiel van Weele, Netherlands]	this has been re-worded and clarified in the new TS.5.4.2
TS-1008	TS	40	39	40	46	Are the likelyhood statements in this para consistent with the AR5 uncertainty guidance, i.e. for example "likely" would mean that more than 66% of the modelled global mean temperature is crosses a threshold? [Government of Germany]	this has been re-worded and clarified in the new TS.5.4.2
TS-1009	TS	40	41	40	41	AR4 states 0.8°C (AR4 SYR: "The 100-year linear trend (1906-2005) of 0.74 [0.56 to 0.92]°C is larger than the corresponding trend of 0.6 [0.4 to 0.8]°C (1901-2000) given in the TAR." An explanation for non-experts is needed, that due to the different reference periodes, the warming is here only 0.6°C. [Government of Germany]	this has been re-worded and clarified in the new TS.5.4.2
TS-1010	TS	40	53	40	53	"possible future reductions in solar irradiance". Unclear why this is stated here, as we have no useful predictive ability for future changes in solar irradiance. What about possible future increases in solar irradiance? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	This is now generalized to "possible future changes in solar irradiance"
TS-1011	TS	41	10	41	11	Is there an inconsistency here? On Page TSM25-12 it is noted that models overestimate the observed tropical upper troposphere temperature trend "with high confidence" and the "cause is elusive" - given the inability of the models to reproduce this trend (and accepting that it may be as much due problems with the observations) should some caveat be placed on whether the modelled projections of warming are likely to be correct if there are doubts about their correctness for the observed record? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	This text has been deleted
TS-1012	TS	41	14			Are the stratospheric circulation changes definitely driven by tropospheric warming? Rather, for example, by changes in the temperature structure around the tropopause? I would replace 'tropospheric warming' with 'greenhouse gas increases'. [Nathan Gillett, Canada]	This text has been deleted
TS-1013	TS	41	54	41	57	I think great care is needed in explaining this so as not to foul up communication with the forecast meteorology community. Francis and Vavrus, for example talk about a weakened jet due to a reduction in the equator-pole temperature gradient, and this allowed greater meandering of the jet. When I dicussed this with a prominent forecast meteorologist, his comment was that the jet stream speed was not reduced. The problem in communication seemed to be that time averaging the jet gives a reduced speed, but this is only becuase the jet is meandering--its speed in the view of a forecast meteorologist had not gone down--what Francis and Vavrus presented were time averages of the zonal jet, not what the speed of the jet actually was when also accounting for the meridional component--and considering the instantaneous state instead of the averaged condition. [Michael MacCracken, United States of America]	This text has been re-written and shortened
TS-1014	TS	42	13	42	15	“ for example ENSO, NAO, AMO and the IPO ”. Add the word "NAO" here. [Shuanglin Li, China]	This text has been deleted

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TS-1015	TS	42	33		35	I think this statement is unhelpful, and leaves open the door to any abrupt changes in the cryosphere being possible with unquantified probability. Rather than discussing abrupt changes in general focus on specific abrupt changes. I think the main one here is abrupt declines in Arctic sea ice. Define 'abrupt' in this context, and then give a probabilistic assessment of the likelihood of such a change, or a confidence statement if you can't give a probabilistic one. If necessary add that such a change cannot currently be predicted deterministically. [Nathan Gillett, Canada]	This text has been re-written and shortened
TS-1016	TS	42	35	42	37	Basing the projection so completely on model simulations that have been lagging in time what is happening in the Arctic seems very strange--the idea that one would not get virtually complete loss for another 40 years or so seems absurd--it is more likely to happen in 4 years than 42 years based on ice volume projections, which are much smoother than the area data. While I realize IPCC is intended to be conservative, this is overly so. In any case, the real issue is that the integrated time-area plot of open water--and that as ice disappears earlier and earlier, the Sun is higher in the sky, and so an even greater impact on the remaining sea ice. [Michael MacCracken, United States of America]	This assessment is based on a subset of models that most accurately simulate recent trends in Arctic sea ice cover, and this is what the literature supports
TS-1017	TS	42	35	42	37	Based on an assessment of a subset of models that more closely reproduce recent observed trends, a nearly ice-free Arctic in late summer before 2050 is a very distinct possibility, even though later dates cannot be excluded. ' This statement should contain a likelihood and/or confidence statement. [Line van Kesteren, the Netherlands]	This statement has been re-written
TS-1018	TS	42	35	42	37	Based on an assessment of a subset of models that more closely reproduce recent observed trends, a nearly ice-free Arctic in late summer before 2050 is a very distinct possibility, even though later dates cannot be excluded. ' This statement should contain a likelihood and/or confidence statement. [Line van Kesteren, the Netherlands]	This statement has been re-written
TS-1019	TS	42	36	42	37	"Based on an assessment of a subset of models that more closely reproduce recent observed trends, a nearly ice-free Arctic in late summer before 2050 is a very distinct possibility, even though later dates cannot be excluded." The opening of the Arctic is likely have profound effects on NH atmospheric circulation. Where in the AR5-WG1 report are such changes discussed? [Andrew Glikson, Australia]	This statement has been re-written
TS-1020	TS	42	36	42	37	"Based on an assessment of a subset of models that more closely reproduce recent observed trends, a nearly ice-free Arctic in late summer before 2050 is a very distinct possibility, even though later dates cannot be excluded." The opening of the Arctic is likely have profound effects on NH atmospheric circulation. Where in the AR5-WG1 report are such changes discussed? [Government of Australia]	This statement has been re-written
TS-1021	TS	42	36		37	Rather than 'a distinct possibility' use calibrated uncertainty language, such 'likely' or 'about as likely as not'. A statement on when the Arctic is likely to become ice free is a key statement people will look for in the report, so this should use calibrated language to express uncertainty. [Nathan Gillett, Canada]	This statement has been re-written
TS-1022	TS	42	37	42	37	"later dates cannot be excluded" - a naive linear extrapolation of current trends in the extent would indicate that much earlier dates than 2050 cannot be excluded either - say 2020! I wasnt sure why the focus here was on later dates. I accept that 2020 is "before 2050" but the nuance here seems to be that later is as likely as not. Figure TS.14 appears to show that observations are clearly well below almost all models [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	This statement has been re-written
TS-1023	TS	42	37	42	40	These percentages are for ice extent, not ice area (see difference in Chapter 4, page 8, lines 46-50). [Thierry Fichefet, Belgium]	This statement has been re-written
TS-1024	TS	42	37	42	47	For the earlier near-term period of 2016–2035 averaged over all the CMIP5 models compared to the 1986–2005 reference period, the projected decreases of sea ice area for the RCP4.5 scenario are –28% for September, and –6% for February for the Arctic. Projected changes for the Antarctic are decreases of –5% for September, and –13% for February. Reductions in Northern Hemisphere sea ice volume for that same set of models, scenario and time period are projected to be –23% for February, and –4% for September, while for the Southern Hemisphere those values are –12% for February, and –7% for September. Multi-model averages from 21 models in the CMIP5 archive project decreases of Northern Hemisphere snow cover area of –4% ± 1.9% (one standard deviation) for the 2016–2035 time period for a March-April average. The projected reduction in annual mean near- surface permafrost (frozen ground) for the 2016–2035 time period compared	This statement has been re-written

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						to the 1986–2005 reference period for the RCP4.5 scenario for 15 CMIP5 models is –2.9 × 106 km2, or a decrease of about 18%. {11.3.4}' All these statements could contain a likelihood and/or confidence statement? [Line van Kesteren, the Netherlands]	
TS-1025	TS	42	37	42	47	For the earlier near-term period of 2016–2035 averaged over all the CMIP5 models compared to the 1986–2005 reference period, the projected decreases of sea ice area for the RCP4.5 scenario are –28% for September, and –6% for February for the Arctic. Projected changes for the Antarctic are decreases of –5% for September, and –13% for February. Reductions in Northern Hemisphere sea ice volume for that same set of models, scenario and time period are projected to be –23% for February, and –4% for September, while for the Southern Hemisphere those values are –12% for February, and –7% for September. Multi-model averages from 21 models in the CMIP5 archive project decreases of Northern Hemisphere snow cover area of –4% ± 1.9% (one standard deviation) for the 2016–2035 time period for a March-April average. The projected reduction in annual mean near- surface permafrost (frozen ground) for the 2016–2035 time period compared to the 1986–2005 reference period for the RCP4.5 scenario for 15 CMIP5 models is –2.9 × 106 km2, or a decrease of about 18%. {11.3.4}' All these statements should contain a likelihood and/or confidence statement. [Line van Kesteren, the Netherlands]	This statement has been re-written
TS-1026	TS	42	37		40	The ranges of model projected ice extent changes are quoted without reference to the fact that the models on average underpredict trends in the Arctic to date, and get the sign wrong in the Antarctic. This needs to be accounted for in the assessed projections. [Nathan Gillett, Canada]	This statement has been re-written
TS-1027	TS	42	38	42	38	See comment on SPM p13, 8-13: insert 'compared with 1986–2005 reference period' to SPM para summarising this. [Government of United Kingdom of Great Britain & Northern Ireland]	This statement has been re-written
TS-1028	TS	42	38	42	47	There is a stylistic problem here throughout this paragraph. For example does "decreases of ... minus 28%" means an increase?? It is either "changes of minus 28%" or "decreases of 28%" [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	This statement has been re-written
TS-1029	TS	42	40	42	41	A cautionary statement about the inability of current models to reproduce the slight increase in Antarctic sea ice extent observed in the last decades should be added here. [Thierry Fichefet, Belgium]	This statement only refers to Arctic sea ice
TS-1030	TS	42	42	42	42	Should these numbers for reductions in northern hemisphere sea ice volume be the other way around (i.e. -23% for September and -4% for February)? [Government of Canada]	This statement has been re-written
TS-1031	TS	42	45	42	47	Near-surface permafrost extent/area is confusing terminiology. No where in chapter 11 or 12 is "near-surface" defined. This is misleading terminologyoften interpreted as complete loss of permafrost. Normally the models on which these statements are based are considering thawing in the upper 2-3 m of the ground and are therefore considering an increase in thaw depth over time rather than a decrease in permafrost extent. In the permafrost chapter of the SWIPA report use of this terminology was avoided when refering to the results of these modelling studies. Instead statements such as "models project that the upper 2 to 3 m of permafrost will thaw over X% of the area currently under by permafrost by XXXX" were.Similar terminology should be used in this report. See additional comments on chapter 11 and 12 [Sharon Smith, Canada]	This statement has been re-written
TS-1032	TS	42	50	42	53	"There are various mechanisms that could lead to changes in global or regional climate that are abrupt by comparison with rates experienced in recent decades. The likelihood of such changes is generally lower for the near term than for the long term. {11.3.6}" As in my comment at TS40/16 - a distinct possibility exists of an abrupt shift in state of the climate, similar to such events in the ice core record and as discussed by prominent climate scientists (Schellnhuber, Rahmstorf, Lenton). [Andrew Glikson, Australia]	Note relevant discussion in TS.5 and TFE.5
TS-1033	TS	42	50	42	53	"There are various mechanisms that could lead to changes in global or regional climate that are abrupt by comparison with rates experienced in recent decades. The likelihood of such changes is generally lower for the near term than for the long term. {11.3.6}" As in my comment at TS40/16 - a distinct possibility exists of an abrupt shift in state of the climate, similar to such events in the ice core record and as discussed by prominent climate scientists (Schellnhuber, Rahmstorf, Lenton). [Government of Australia]	Note relevant discussion in TS.5 and TFE.5
TS-1034	TS	42	55	43	31	It would be good to divide the future air quality changes due to changes in anthropogenic emissions of pollution precursors from the future changes due to projected changes in climate. [Government of United States of America]	This section has been re-written and shortened

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TS-1035	TS	42	56			It seems strange to report that there is 'high confidence' that surface ozone will change in the 21st century, and then quote a range of trends which includes zero. I would delete this first part of the sentence and start 'Projections for background surface ozone differ across the RCP, SRES and...'. [Nathan Gillett, Canada]	This section has been re-written and shortened
TS-1036	TS	43	4	43	8	Also mention that a warming climate will also accelerate the meridional circulation in the stratosphere and thus increase the stratosphere-troposphere exchange of ozone, and that this contributes to increased ozone concentrations mostly in the subtropics and midlatitudes (see comment no. 15). [Twan van Noije, Netherlands]	This section has been re-written and shortened
TS-1037	TS	43	10	43	23	This sentence is not accurate. According to Figures 11.31ab,for East Asia, near term O3 air quality degrades under two scenarios (RCP8.5 and RCP6.0), and PM2.5 air quality degrades only in RCP 6.0. [Shuanglin Li, China]	This section has been re-written and shortened
TS-1038	TS	43	20	43	20	"episodic dust and wildfire transport events". This is the first time fire is mentioned in this chapter, yet drought and heatwave-triggered fires constitute major feedback to warming. [Andrew Glikson, Australia]	This section has been re-written and shortened
TS-1039	TS	43	36	43	37	For RCP2.6 temperatures decrease after about 2040, hence the statement is not correct. [Government of Germany]	Accepted
TS-1040	TS	43	42	43	48	As with comment on SPM p13, 47-48: I read 'likely' as meaning more than 50% and as likely as not as meaning 50/50. This may become slightly confused with the terminology used regarding uncertainty intervals throughout the IPCC reports. We're talking about the odds here, rather than the normal data confidence clarifications - perhaps need to use different terminology here, to avoid confusion. Or perhaps they ARE using the uncertainty intervals and I'm mistaken. Either way, I feel this whole paragraph could be made simpler. [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted. Text clarified.
TS-1041	TS	43	43	43	46	The predicted global temperature for 2081-2100 due to anthropogenic forcing are highly questionable, in particular for model RCP8.5, for reasons already developed in ROFOD but ignored in SOD, and for arguments developed throughout this report. The TCR is over evaluated since (i) hotspots predicted by the models are not observed experimentally, hence feedback parameter would be either zero of negative as shown by R.S. Lindzen and Y.S. Choi (2009) or by R.W. Spencer and W.D. Braswell (2010), (ii) the annual anthropogenic residue of CO2 is found to be only 10 % of human emissions after actions of the carbon sinks (see my comments about SPM 0) and (iii) the model of greenhouse effect which is a black box in AR5 as will be discussed and criticized later on, does not take properly into account the presumably near saturation effect of CO2 greenhouse effect, by exaggerating radiative transfer which is weak compared to heat dissipation by thermal conduction in the troposphere. [François Gervais, France]	Rejected. Unspecific comments that are not supported by evidence. The work by Lindzen and Choi and Spencer and Braswell is widely seen as flawed and is criticized in the literature.
TS-1042	TS	43	43	43	46	Cont. – References cited above are R.S. Lindzen and Y.S. Choi, Geophys. Res. Lett. 36 (2009) L16705 – R.W. Spencer and W.D. Braswell, J. Geophys. Res. 115 (2010) D16109. [François Gervais, France]	Noted.
TS-1043	TS	43	44	43	44	Please explain the choice of the interval boundaries for the ranges, and the for associated uncertainty, if possible in the text. At least provide references where this information can be found in text. Are the choices comparable to AR4? This information is relevant costal protection. [Government of Germany]	Accpeted. Text refers to section 12.4.1 where this is discussed in detail.
TS-1044	TS	43	45	43	45	Presumably the range 1.3 - 3.2 is meant to be for RCP6 and not RCP4.5 as values are already provided for RCP4.5. [Government of Canada]	Accepted. Fixed typo.
TS-1045	TS	43	45	43	45	"RCP4.5" is mentioned twice, the second time it should be "RCP6.0". [Government of Germany]	Accepted. Fixed typo.
TS-1046	TS	43	47	43	47	Same comment as for Chapter 12, page 3, line 19: The current wording "... and about as likely as not to be above 2C warming for RCP2.6" seems to be at odds with the results provided in Table 12.2. In Table 2.12, the multi-model and global-mean warming is provided with maximally 1.0+-0.4C for the middle of the century and 1.0+-0.5 C for the end of the 21st century. In case of the 0.4 one-standard deviation case, and adding 0.6C warming for the 1986-2005 to preindustrial difference, this would result in the complete +-1std range being below 2C, i.e. a chance of exceeding 2C of only about 16% (assuming a normal distribution and 1std range reflecting a 68% range). In the case of the 0.5C std, the exceedance probability might be a bit higher. The point is however, that RCP2.6 with a multi-model mean warming of 1.6C seems to be better characterised with having a likely chance (>66%) of staying below 2C, than merely a (33% to 66%) "as likely as not" chance. A wording suggestion that would avoid to make a definite call on the exceedance probability of RCP2.6 would be	Accepted. Likekihoods updated based on complete set of models.

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						to take the sentence from page 12-24, line 35, which says:"In the CMIP5 ensemble mean, global warming under RCP2.6 stays below 2C above preindustrial levels throughout the 21st century, clearly demonstrating the result of mitigation policies.". [Government of Germany]	
TS-1047	TS	43	47	43	48	"global temperatures are projected to likely exceed 2ºC warming with respect to preindustrial by 2100, and about as likely as not to be above 2°C warming for RCP2.6". Since at present global temperature is only kept below 2 degrees C due to the transient sulphur aerosol masking effect (constituting an "unintended" form of geoengineering), it can not be said committed global temprature is below 2 degees C. (An analogy: where an analgesic is administered to a patient suffering from fever, it can not be claimed the patient's true body temperature is the temporarily mitigated tempeature) [Andrew Glikson, Australia]	Rejected. The text does not say anything about commitment, just about the probability of exceeding a threshold in a particular scenario.
TS-1048	TS	43	52	43	52	Note typo in top left hand frame axis label - minus sign is missing in m^2 [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted. Fixed typo.
TS-1049	TS	44	16	44	20	It is virtually certain that global mean precipitation will increase with global mean surface air temperature in the next century, with an increase per °C smaller than that of atmospheric water vapour. It is likely that the rate of increase of precipitation with temperature will be in the range 1–3% °C–1, for scenarios other than RCP2.6. For RCP2.6 the range of sensitivities in the CMIP5 models is 0.5–4% °C–1 at the end of the 21st century. {12.4.1}' Why will global mean precipitation increase with global mean surface temperature? And then why did global mean precipitation not increase untill now while global mean surface temperature has been increasing? [Line van Kesteren, the Netherlands]	Noted. Discussion of processes are in the underlying chapters. The statement refers to the forced trends. The obeserved changes are strongly affected by interannual variability.
TS-1050	TS	44	16	44	20	It is virtually certain that global mean precipitation will increase with global mean surface air temperature in the next century, with an increase per °C smaller than that of atmospheric water vapour. It is likely that the rate of increase of precipitation with temperature will be in the range 1–3% °C–1, for scenarios other than RCP2.6. For RCP2.6 the range of sensitivities in the CMIP5 models is 0.5–4% °C–1 at the end of the 21st century. {12.4.1}' Why will global mean precipitation increase with global mean surface temperature? And then why did global mean precipitation not increase untill now while global mean surface temperature has been increasing? [Line van Kesteren, the Netherlands]	Noted. Discussion of processes are in the underlying chapters. The statement refers to the forced trends. The obeserved changes are strongly affected by interannual variability.
TS-1051	TS	44	17		18	Does the assessed range of hydrological sensitivity account for observational comparisons, some of which show that models tend to underestimate this ratio compared to observations? (10.3.2.2) I would think that such studies would at least reduce confidence in this projection. [Nathan Gillett, Canada]	Accepted. The ranges are essentially min max ranges that are interpreted as likely, thus allowing for a substnantial probability for the models to be biased.
TS-1052	TS	44	18			It is not clear in the various IPCC reports if increasing temperature would increase droughts or precipitations. Versus latitude ? Please clarify. [François Gervais, France]	Noted. Changes in droughts and precipitation are discussed, see also the TFE on the water cycle.
TS-1053	TS	44	25	44	27	Please add an uncertainty qualifier to this statement. [Government of Germany]	Rejected. Simply a statment of what the models show.
TS-1054	TS	44	27	44	29	There is misplaced confidence here - consistency between the models in getting this maximum is undermined by the discussion on TSM25-12 where there was inconsistency "with high confidence" that the same models overestimated the observed trends in tropical upper tropospheric temperatures. Until we understand the "elusive" (to quote TSM.25) causes of this difference, the confidence level for the future should be modified [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted. Statement removed.
TS-1055	TS	44	43	44	46	There is no mention here of one of the most (if not the most) important cloud change: the ubiquitous upward shift of cloud tops, which gives a strong positive feedback. [Government of United States of America]	Noted. Statement may be correct but few if any papers have looked at cloud changes in CMIP5, and the authors had limited resources to look at all aspects.
TS-1056	TS	44	53			In all seasons except DJF the CMIP5 models simulate an increase in the SAM. Even in DJF the CMIP5 models simulate either no significant SAM trend or a positive trend (Gillett and Fyfe, 2012). N. P. Gillett and J. C. Fyfe. Annular mode changes in the CMIP5 simulations, Geophs. Res. Lett., submitted, 2012. [Nathan Gillett, Canada]	Noted. Statemement reflects the assessment of the underlying chapter.
TS-1057	TS	45	12	45	12	...increase in warmer climates.' Clarify ambiguity- does this mean in regions that are warmer or during periods when the climate is warmer? [Government of United Kingdom of Great Britain & Northern Ireland]	Accpeted. Changed to warming climate.

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TS-1058	TS	45	48		50	This sentence on soil moisture would belong better on line 30. [Nathan Gillett, Canada]	Noted. Section largely rewritten.
TS-1059	TS	45	53	46	4	As with comment on SPM p15, 46-51: need to give a baseline period, against which to compare quoted reductions in sea ice under CMIP5 projections. [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted. Base period is indicated and is identical throughout the projections chapter.
TS-1060	TS	45	54	45	54	I was confused by the top plot of Figure TS.14. It is plotted as an anomaly, as I understand it, from a historical mean of about 7 million sq.km. Hence it is impossible for the anomaly to be any more negative than this, as it would constitute zero sea ice (which is a big event). Wouldnt it be better to plot this on an absolute scale, so it is clear when zero sea ice is appoached? It would also be more consistent with the discussion in the text that talks about the vanishing of sea ice. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accpted. Sea ice now shown in absolute values.
TS-1061	TS	45	56	45	57	This seems a very soft conclusion (ice free by the end of the century) given current trends and the inability of models to reproduce those trends. It would appear more likely than not that sea-ice will have disappeared by the end of this decade,let alone century, at current loss rates! It is hard to calibrate the "distinct possibility" for loss of sea ice by 2050 (on TS.42-37) with this very likely conclusion [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Accepted. Wording changed. Taking into account model performance is dificult but the revised version does show projections from a subset of models that perform well.
TS-1062	TS	46	1	46	1	Greater than 2degC or 'of 2 degC or more'? - there's a subtle difference [Government of United Kingdom of Great Britain & Northern Ireland]	Noted. Text no longer there.
TS-1063	TS	46	28	46	31	How about the Southern hemisphere? Please add a statement. [Government of Germany]	Accepted. Added.
TS-1064	TS	46	31	46	31	If the percentages given refer to decreases in snow covered area, there should be minus signs before the numbers. Please explain the reference period. [Government of Germany]	Accepted. Sign changed. Reference period mentioned in the caption. All projections use the same reference period.
TS-1065	TS	46	33	46	36	"A retreat of permafrost extent with rising global temperatures is virtually certain. The projected changes in permafrost are a response not only to warming, but also to changes in snow cover, which exerts a control on the underlying soil. By the end of the 21st century, diagnosed near-surface permafrost area is projected to decrease by between 37% (RCP2.6) to 81% (RCP8.5)." A comment regarding the effects of consequent CH4 release from permafrost and its feedback forcing is relevant here. [Andrew Glikson, Australia]	Rejected. The available model results do not include that. Section 12.5.5. discusses this but confidence is low.
TS-1066	TS	46	33	46	36	"A retreat of permafrost extent with rising global temperatures is virtually certain. The projected changes in permafrost are a response not only to warming, but also to changes in snow cover, which exerts a control on the underlying soil. By the end of the 21st century, diagnosed near-surface permafrost area is projected to decrease by between 37% (RCP2.6) to 81% (RCP8.5)." A comment regarding the effects of consequent CH4 release from permafrost and its feedback forcing is relevant here. [Government of Australia]	Rejected. The available model results do not include that. Section 12.5.5. discusses this but confidence is low.
TS-1067	TS	46	33	46	36	Near-surface permafrost extent/area is confusing terminiology. No where in chapter 11 or 12 is "near-surface" defined. This is misleading terminology often interpreted as complete loss of permafrost. See previous comment and additional comments on chapter 11 and 12 [Sharon Smith, Canada]	Rejected. Permafrost is defined in the glossary and used consistenly in the report.
TS-1068	TS	46	39	46	41	"It is very likely that the AMOC will weaken over the 21st century. It also is very unlikely that the AMOC will undergo an abrupt transition or collapse in the 21st century and it is unlikely that the AMOC will collapse beyond the end of the 21st century." The AMOC repeatedly collapsed during the Daansgaard-Oeschger cycles of the LGM. It is relevant to examine the level of RF which drove these cycles, with implications for future prospects of the AMOC [Andrew Glikson, Australia]	Rejected. The evidence from the LGM is not applicable to the current climate state. The paleoclimate chapter and 12.5.5. provide more detail on this topic.
TS-1069	TS	46	44		45	It doesn't follow that the rate of ocean heat uptake will increase as the radiative forcing increases. The radiative forcing is defined without adjustment of the surface temperature. But if the surface temperature adjusts this will reduce the TOA energy imbalance and hence the flux into the ocean. For example in RCP 4.5 the radiative forcing increases progressively through the 21st century, but by the end of the century the rate of increase of ocean heat content is decreasing (Figure 13.9). [Nathan Gillett, Canada]	Accepted. Paragraph rewritten.
TS-1070	TS	46	53	46	55	As indicated in ROFOD, this prediction of 12 W/m2 is unrealistic. The observations over more recent years contradict this alarmist projection : troposphere temperature shows a plateau for 16 years, SST as well, ocean heat content shows a plateau since 2002, viz. the beginning of more precise ARGO buys measurements, sea	Rejected. This section discusses model projections, observed changes are discussed in chapters 2-4.

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						level rise (according to AVISO data discussed in TS 10 54-55) shows a slope of only 1 mm per year since 2008, smaller than during the 20th century, and even Antarctic data (temperature, sea ice area and extent) indicate cooling rather than heating. Note also that ORL measured by the Australian bureau of meteorology shows more cooling than heating since 2008. [François Gervais, France]	
TS-1071	TS	46	55	46	55	The missing information that is needed to understand why global temperature reduces in the RCP2.6 extension is that in RCP2.6, net negative emissions are sustained after 2070 throughout the extension (Ch. 12 page 20). So this means that atmospheric CO2 concentrations will actually be lowered over time, to which global temperature will respond. Please add the pertinent info regarding the RCP2.6 extension. [Government of Canada]	Noted. Paragraph deleted.
TS-1072	TS	47	1		5	While the statement that the persistence of the warming is longer than the gas lifetimes is clearly true for short-lived forcings, I think this statement is misleading for CO2. It may be true in the sense that the warming has a longer e-folding timescale than the concentration. But it is fundamentally the CO2 sinks which set the timescale for recovery from CO2 warming. The lifetime of part of the anthropogenic CO2 is so long that the heat transfer into and out of the ocean and the nonlinear absorption affects don't matter much. For CO2 I think key information, which is missing here, is an estimate of the lifetime of the CO2 itself. [Nathan Gillett, Canada]	Noted. Paragraph deleted.
TS-1073	TS	47	8	47	19	Please add an introduction to the concept of Earth's energy balance for non-experts. on the other hand, it is suggested to shorten the present text which is quite general and does not provide specific information. [Government of Germany]	Noted. Paragraph deleted.
TS-1074	TS	47	8	49	13	In the entire box biosphere feedbacks are not considered. Increase in temperature (and precipitation) will potentially volatilize ecosystem C stocks, which will further accelerate increases in atmospheric CO2 concentrations (climate warming). Wonder why this is not picked up here in the frame of the ECS concept discussion. Furthermore, albedo effects or effects of atmospheric pollutants (aerosols, O3) on plant performance are not mentioned at all. The box only shows the view of atmospheric chemists and climate modellers, without having an earth system view. E.g. also feedbacks of climate change on marine systems are not mentioned. Sure, part of this is presented in TS5.4 but should already mentioned in the box. [European Union]	Noted. Paragraph deleted.
TS-1075	TS	47	12			"feedbacks may be much faster than the surface warming" is awkward phrasing and may not even be accurate. Feedbacks as they are defined in this report require a change in x brought about by a change in surface temperature that impacts the TOA radiation. Thus feedbacks that occur faster than a change in surface warming does not make sense. [Government of United States of America]	Rejected. That is precisely the point, because they happen faster than the surface warming, they do not fit into the traditional feedback definition. See section 12.5.3.
TS-1076	TS	47	19	47	19	The term "mitigration" is more realistic than "stabilization" (i.e. there is no such thing as a "stable" climate) [Andrew Glikson, Australia]	Accepted. Reworded to "stabilization or targets", which is the use of the words in this report.
TS-1077	TS	47	21	7	21	Why is there a hyphen in "water-vapour/lapse-rate"? [Government of Germany]	Accepted.
TS-1078	TS	47	23	47	23	The unfortunate circumstance that most known feedback mechanisms amplify warming might deserve a bit more attention than given here, where it is buried in the text. [Dian Seidel, United States of America]	Rejected. That is implicit in the assessment of ECS. Actually only water vapor and albedo clearly are, clouds only likely, lapse rate is not.
TS-1079	TS	47	23		23	Explain the meaning of "positive" /"negative" feedback [Government of Germany]	Rejected. This is technical summary, not a textbook.
TS-1080	TS	47	24	47	29	The use of the term 'black body' here might not be meaningful to all readers. If it's not necessary, to include this phrase, suggest it be deleted. [Government of Canada]	Rejected. This is technical summary, not a textbook.
TS-1081	TS	47	26	47	27	Section TFE.7: Carbon Cycle: This section should include a comment on estimated reserves of carbon stored in methane hydrates and the likelihood of their destabilisation. [Government of United Kingdom of Great Britain & Northern Ireland]	Rejected. Comment does not match text.
TS-1082	TS	47	37	47	38	"The net radiative feedback due to all cloud types is likely positive, although a negative feedback (damping global climate changes) is still possible.": This sentence is troubled by its juxtaposition of "likely positive" and "still possible." It such cases it is always much better to simply state there is uncertainty. Furthermore, the use of "global" is overly broad and should be tempered by inserting "regional or" before "global". A suggested	Accepted. Deleted this part.

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						revision to this sentence is given in the row below: [Forrest Mims, United States of America]	
TS-1083	TS	47	37	47	38	Suggested revision to sentence discussed in previous row above: "While the net radiative feedback due to all cloud types may be positive, a negative feedback (damping regional or even global climate changes) is still possible." [Forrest Mims, United States of America]	Accepted. Deleted this part.
TS-1084	TS	47	41	37	41	"remarkable consistency": please use more neutral language. [Government of Germany]	Rejected. Correlations between the patterns are indeed very strong, see pattern scaling section in chapter 12.
TS-1085	TS	47	53	47	56	It is confusing to have the CMIP5 results for ECS compared to the assessed likely range for ECS before that assessment is presented (which it is, later in this Box, on page TS48 lines 37-48). Suggest the focus here remain on the CMIP5 results. The later discussion makes clear that the model results are only of a number of lines of evidence used to arrive at the assessed likely range for ECS. [Government of Canada]	Accepted.
TS-1086	TS	47	53	48	10	The content of this para seems quite important, but the language and wording are not suitable for non-experts. For example, the sentence "No correlation is found between biases in global-mean surface temperature and ECS." would probably mean that although models differ in the simulated absolute global-mean surface temperature, this does not affect their temperature response to forcing from increased CO2, and models show similar warming to a given CO2-concentration change. If this is the case, it should be clearly said, and likewise other sentences containing scientific jargon should be translated in common language. [Government of Germany]	Rejected. Given the large spread in sensitivity, it is not clear that the bias in simulated mean temperature does not affect the response. The current ensemble is simply not sufficient to state that. It gives no positive evidence, but also not exclude it.
TS-1087	TS	47	55	47	55	What is a "perturbed parameter ensembles model (PPE)"? [Government of Germany]	Rejected. This is technical summary, not a textbook. Terms are explained in the glossary and the relevant sections.
TS-1088	TS	48	1	48	3	"A negative correlation..." very long and difficult sentence, please move the verb closer to the subject. The content of this sentence remains unclear. [Government of Germany]	Accepted. Verb moved as suggested.
TS-1089	TS	48	3		4	The implication here is that there might be a physical mechanism whereby large ECS is associated with large aerosol cooling and weak ECS with weak aerosol cooling. But I think the proposed 'mechanism' is model tuning or bias in selecting forcing datasets. I suggest replacing 'a mechanism' with 'model tuning' or similar. [Nathan Gillett, Canada]	Accepted.
TS-1090	TS	48	8			This material is decribed as "most Likely" with high confidence which seems a strong statement given the uncertainties. Does the range or the 'most likey' number have high confidence? I would have thought the rannge could but not the "most likely" number. Suggest removing any reference to a 'most likely' as it is highly speculative and also highly misleading given the range. [Judy Lawrence, New Zealand]	Accepted. Most likely values are no longer given.
TS-1091	TS	48	9	48	10	The last sentence of the para is unclear. [Government of Germany]	Rejected. Unspecific comment.
TS-1092	TS	48	23	48	25	"Very likely" is not credible, in my view. The models tend to incorporate optimistic assumptions about CO2 fertilization, do not include potential constraints on plant movement, do not account for potential changes in disturbance (e.g., fire or pest outbreaks), do not consider the potential carbon implications of shifts in inter-species competition, and are surely incomplete with respect to interactive effects of multiple ongoing global changes. Attaching a 90% chance to the statement is inaccurate and could be damaging to the credibility of the chapter and the overall assessment. [Paul Higgins, United States of America]	Rejected. The text at this position is not about CO2 fertizilation, nor does is mention 90% confidence.
TS-1093	TS	48	29			Awkwardly phrased - should be split into two (or maybe three) sentences (one noting the bayesian estimates can, in principle, narrow the range, the other noting that bayesian estimates depend on the prior and that other difficulties exist. [Government of United States of America]	Accepted.
TS-1094	TS	48	32			Replace 'statistically' with 'robustly'. [Nathan Gillett, Canada]	Accepted.
TS-1095	TS	48	37	48	48	Most of the statements in this para are already mentioned above: please shorten para. [Government of Germany]	Partly accepted. Some of the statements removed in earlier paragraphs.
TS-1096	TS	49	1	49	6	To understand why TCR is a more useful indicator of 21st century warming, it would help to be reminded here	Rejected. Defined the beginning of TFE.6

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						of how TCR is defined and to be informed approximately when in the 21st century under the 1%/year increase experiments CO2 doubling is reached. [Government of Canada]	
TS-1097	TS	49	1			This material is decribed as "most Likely" with high confidence which seems a strong statement given the uncertainties. Does the range or the 'most likey' number have high confidence? I would have thought the rannge could but not the "most likely" number. Suggest removing any reference to a 'most likely' as it is highly speculative and also highly misleading given the range. [Judy Lawrence, New Zealand]	Accepted. Most likely values are no longer given.
TS-1098	TS	49	16	51	58	Please add general information on the N-cycle and its climate influence, potentially in TFE.7, or in TS.5.4. [Government of Germany]	rejected: the components of the nitrogen cycle that are relevant to climate are already discussed in these sections and in TS.2.8. A more general description of the N-cycle is not appropriate for the technical summary.
TS-1099	TS	49	23	49	27	As with comment on SPM p16, 53-56: By saying that global land will become a net carbon sink for scenarios with decreasing areas of anthropogenic land-use, can we also make it clear what the role of socio-economics (i.e. Land use changes) and climate would have here? [Government of United Kingdom of Great Britain & Northern Ireland]	accepted: The text in this section has been extensively modified. The revised text should better address this concern.
TS-1100	TS	49	26	49	27	Give reasoning why tropical regions are in the focus [European Union]	accepted: Text has been revised to explain that the models are in high agreement that tropical systems will store less carbon and medium agreement on the high latitudes. Section 3 points our that tropical areas are particularly vulnerable to drought and fire.
TS-1101	TS	49	28	49	28	C^4MIP? [Government of Germany]	taken into account: The text in this section has been extensively revised so there is no longer a reference to C4MIP. C4MIP is mentioned in TFE.7, but the superscript is not used.
TS-1102	TS	49	34	49	37	What is the conclusion from this para? and what are the reasons? And are the statements also valid for other RCPs? [Government of Germany]	accepted: The text in this section has been extensively modified. The revised text should better address this concern.
TS-1103	TS	49	34	49	37	The difference between ESM results from emission vs concentration forcing need to be explained in Box TS.4 [Government of Germany]	accepted: The text in this section has been extensively modified. The revised text should better address this concern.
TS-1104	TS	49	34	49	37	The sentence is too condensed to be understandable. Does it mean if CMIP5 ESM is forced by emissions rather than by concentrations the temperature change output is higher than for forcíng the same models by concentrations? And what does that mean für policy makers? [Government of Germany]	accepted: The text in this section has been extensively modified. The revised text should better address this concern.
TS-1105	TS	49	34		37	The suggestion here is that this difference in CO2 concentration between the CMIP5 models and the RCPs is somehow fundamental ('The value of 60 ppm is uncertain within a range of +/- 70ppm'). But it only tells us about differences between the carbon-climate-concentration interactions in CMIP5 models and MAGIC or whatever model was used to derive the concentrations for the RCPs. Make this clear or omit this entirely. [Nathan Gillett, Canada]	accepted: The text in this section has been extensively modified. The revised text should better address this concern.
TS-1106	TS	49	39	49	44	It's not only nitrogen liniting ecosystem carbon sequestration, but also other trace nutrients (P, K, etc.). However, these mechanisms have so far rarely been implemented in earth system models. This all limits our predicting capability of future ecosystems states and responses to climate and atmospheric composition change. This should be made clear. [European Union]	accepted: text has been modified to talk about nutrient limitation rather than jus nitrogen limitation.
TS-1107	TS	49	44	49	45	An uninformed reader may think that the best would be to provide surplus nitrogen to ecosystems. I am missing an earth system view here taking into account all N effects (climate, biodiversity, health, etc.). Moreover, the role of other nutrients is fully neglected. The models cited, are only starting to capable to explore biosphere processes an spatio-temporal scales. Our uncertainty on climate feedbacks on ecosystems is surely much higher as outlined here [European Union]	accepted: text has been modified to talk about nutrient limitation rather than jus nitrogen limitation.

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TS-1108	TS	49	52	49	53	It would be helpful to add the information that for projections not requiring negative emissions to be consistent with RCP2.6, close to zero emissions are required. [Government of Canada]	accepted: additional information has been given about the average and range of emissions reductions under RCP2.6
TS-1109	TS	50	6	0	6	Please explain "aragonite" for non-experts [Government of Germany]	rejected: There was not sufficient room in this summary to expain basic minerology, it should be clear from the context.
TS-1110	TS	50	16		17	Is 'a stabilisation of CO2 and climate' realistic? Global mean temperature could be stabilised by cutting CO2 emissions to appoximately zero, but this wouldn't correspond to stable CO2, at least not for many centuries to millenia. If CO2 concentrations were stabilised (and other forcers), then global mean temperature would continue to rise for centuries to millenia. How about 'stabilisation of CO2 or climate'. [Nathan Gillett, Canada]	taken into account: The text in this section has been extensively revised but TFE7 does note that ecosystems will continue to respond to climate change and atmospheric CO2 increases created during the 21st Century, even for centuries after any stabilization attempt.
TS-1111	TS	50	19	50	21	a qualification on CDR methods should be given along the lines of chapter 6 page 5 lines 47 to 55. [Government of Germany]	taken into account: CDR is discussed in BOX TS.7
TS-1112	TS	50	19			Suggest mentioning here that large-scale CDR at present is impractical (as is stated elsehere in this report). [Government of United States of America]	taken into account: CDR is discussed in BOX TS.7
TS-1113	TS	50	20	50	21	SRM is only "likely to impact the carbon cycle" if it is implemented, and this is a huge "if". Also, the pronoun "their" does not have a plural antecedent in this sentence or elsewhere. [Dian Seidel, United States of America]	taken into account: CDR is discussed in BOX TS.7
TS-1114	TS	50	24	51	57	I suggest adding the figure from Box 6.2 or FAQ 6.1 figure 2 to the TFE.7. I find these figures essential for communicating a core aspect of man-made perturbation to the carbon cycle. [Jan Fuglestvedt, Norway]	accepted: figure has been added.
TS-1115	TS	50	24	54	34	TFE.7 and TFE.8 do not link to the text (as there is no text between them). Please try to connect these boxes to relevant text. [Government of Germany]	accepted: links back to the original chapter text has been added.
TS-1116	TS	50	24			"Thematic Focus Elements" is unnecessary jargon. Replace with "policy relevant topics" (throughout the AR5) [Government of United Kingdom of Great Britain & Northern Ireland]	rejected: the TFE terminology was considered more more appropriate.
TS-1117	TS	50	26	50	27	AR4 concluded that "most of the observed increase since 1950" is due to greenhouse gas increases - the paraphrasing here misses the "most" [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	accepted: most was added.
TS-1118	TS	50	26		30	Is this conclusion confirmed by AR5? If yes, please insert paragraph in SPM at an adequate place, e.g. introduction? [Government of Germany]	accepted: statement about AR5 conclusion was added
TS-1119	TS	50	27	50	27	I would write "three of the most influential" - CFC-12 was in third place until recently and without decomposing the contribution of each gas to the warming since 1950 (I would guess that the 1950-2012 CFC-12 forcing exceeds that due to N2O) it might be better to be a bit more cautious [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	accepted: text changed as recommended
TS-1120	TS	50	28	50	28	How is 80% calculated? Seems to be inconsistent with table 8.3 (1.83 + 0.48 + 0.17) / 2.83 = 0.87 [Jan Fuglestvedt, Norway]	accepted: revised to say more than 80%
TS-1121	TS	50	33	50	33	Suggest revising this sentence to make clear how the AR5 conclusion extends that of the AR4 (by extending the time frame of analysis from 650,000 to 800,00 years). [Government of Canada]	accepted: text modified
TS-1122	TS	50	34	50	34	800.000 years -> 800 ka as defined in line 33. [Government of Germany]	accepted: text changed
TS-1123	TS	50	35			This kind of statement - eg, high confidence for 800,000 years, but indirect methods provide some evidence back to 2.7 million years - is useful because too often "the highest in 800,000 years" is read to mean "800,001 years ago it was higher". This should be done more consistently throughout the chapter. [Government of United States of America]	noted
TS-1124	TS	50	45	50	45	Possibly define land use change differently, e.g. land use change is related to the conversion of natural ecosystems into managed ecosystems for food, feed and timber production. [European Union]	accepted: text changed as recommended

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TS-1125	TS	50	45	50	47	Should fire related LUC emissions be mentioned here? As written, it suggests that the only CO2 emission from forest clearing are from slow decomposition of plants and soil carbon. [Government of Canada]	accepted: text modified to include fires
TS-1126	TS	50	46	50	46	Organic carbon stored in soils [European Union]	accepted: changed to soil organic carbon
TS-1127	TS	50	49		56	The description of the global carbon cycle is missing important interfaces that might contribute. These are (i) the "so-called" aquatic continuum formed (soil water, rivers, lakes, estuaries, coastal zones) that laterally transports, transforms, sequesters or eliminate C and nutrients from land to the ocean (e.g. the boundless carbon in Battin et al. 2009, Nature Geosciences 2(9), 598-600). According to Battin et al., 2.7 to 2.9 PgC/y are exported from land to inland waters; and (ii) the Arctic and Antarctic sea-ice which can exchange CO2 with both the atmosphere and the ocean (Gosink et al., 1976 Nature 263, 41-42; Rysgaard et al., 2011, Tellus B, 63(5), 823-830).In particular, Rysgaard et al estimate that CO2 uptake during the seasonal sea-ice cycle almost equals half of the net atm CO2 uptake in ice-free polar seas. [European Union]	accepted: lateral fluxes were added to the text.
TS-1128	TS	50	51	50	52	Better: An excess of atmospheric CO2 supports photosynthetic CO2 fixation by plants and, thus ecosystem C sequestration, with organic C being stored in plant biomass or in the soil. Residence times of stored carbon depends on compartments (plant/ soil) and C compound quality and composition, with time horizons varying from days to centuries. [European Union]	accepted: wording was modified
TS-1129	TS	51	4			What does "before the end of the 21st century" mean here? That surface waters will NOT become more corrosive to aragonite shells until late in the 21st century? Or that the trend towards increased corrosivity will reverse? This could be more clear… [Government of United States of America]	accepted: this wording has been changed.
TS-1130	TS	51	8	51	8	Poor choice of words - carbon, as such, does not 'thaw' but rather the soil and organic matter containing carbon thaws - thus leading to microbial biodegradation to release carbon in the form of CO2 (aerobic) and CO2 and CH4 (anaerobically). [Jeffrey Obbard, Singapore]	accepted: wording was modified
TS-1131	TS	51	8	51	9	"The thawing of carbon in frozen soils constitutes a positive radiative forcing feedback that is missing in current coupled carbon-climate models projections." This is a major omission --- attempts need to be made to roughly estimate the effects of permafrost and sediment-released CH4. [Andrew Glikson, Australia]	noted
TS-1132	TS	51	8	51	9	"The thawing of carbon in frozen soils constitutes a positive radiative forcing feedback that is missing in current coupled carbon-climate models projections." This is a major omission --- attempts need to be made to roughly estimate the effects of permafrost and sediment-released CH4. [Government of Australia]	noted
TS-1133	TS	51	15	51	17	It seems inappropriate for models with negative feedbacks referred to in section 6.4.6.2 (page 64 line 19) to be omitted from discussion here? Suggest adding "although some models not included in CMIP5 exhibited a negative feedback." to account for this difference. [HAROON KHESHGI, United States of America]	accepted: this section was significantly modified
TS-1134	TS	51	17	51	18	It does not get better if the story of a diminishing carbon sink strength in the tropics is repeated several times. These are model predictions, based on changes in the areal extend of tropical forests. H276 [European Union]	noted
TS-1135	TS	51	17	51	19	I was not able to find a traceable account for the very likely statements in chapter 6. Moreover, I suggest that the CMIP 5 models alone do not indicate likelihood and so the rationale presented in these lines for the very likely judgment are not sufficient. [HAROON KHESHGI, United States of America]	accepted: wording was modified
TS-1136	TS	51	19	51	19	C^4MIP? [Government of Germany]	accepted: wording was modified
TS-1137	TS	51	37		57	Connection between C and other biogeochemical cycles focuses on N for obvious reasons. However other biogenic elements (P, Si, Fe etc…) should not be neglected. The case of P is particularly important as reserves are decreasing fast to the synthesis of chemical fertilizers. This does not diminish the great value of the work accomplished but this limitation should be shortly recognized. [European Union]	accepted: wording was modified
TS-1138	TS	51	39	51	39	"Natural CH4 emissions from wetland and fires are sensitive to climate change." This is the second time fire is mentioned in this chapter, yet drought and heatwave-triggered fires constitute major feedback to warming. [Andrew Glikson, Australia]	noted
TS-1139	TS	51	39	51	39	Are fires really associated with CH4 emissions of any significance? [Jeffrey Obbard, Singapore]	accepted: wording was modified

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TS-1140	TS	51	39	51	40	Human perturbation of the N cycle has already resulted in an acceleration by a factor of two. So far, the perturbation may have led to a net cooling due to aerosol effects and accelerated C sequestration by terrestrial ecosystems and the ocean due to N fertilization (Butterbach-Bahl et al., 2011; Erisman et al. 2011; Pinder et al. 2012), but this will change in a few decades, i.e. perturbation of the N cycle will result in a net climate warming. Erisman J, Galloway J, Seitzinger S, Bleeker A, Butterbach-Bahl K, 2011, Reactive nitrogen in the environment and its effect on climate change. Current Opinion in Environmental Sustainability 3, 281-290. Butterbach-Bahl K, Nemitz E, Zaehle S, Billen G, Boeckx P, Erisman JW, Garnier J, Upstill-Goddard R, Kreuzer M, Oenema O, Reis S, Schaap M, Simpson D, De Vries W, Winiwarter W, Sutton M, 2011, Nitrogen as a threat to the European greenhouse balance. In: Sutton MA, Howard CM, Erisman JW, Billen G, Bleeker A, Grennfeldt P, Van Grinsen H, Grozetti B (eds.), The European nitrogen assessment: sources effects, and policy perspectives, pp. 434-462, Cambridge University Press, Pinder RW, Davidson EA, Goodale CL, Greaver TL, Herrick JD, Liu L 2012 Climate change impacts of US reactive nitrogen. PNAS doi.1073/pnas.1114243109 [European Union]	noted
TS-1141	TS	51	43	51	43	C^4MIP? [Government of Germany]	accepted: wording was modified
TS-1142	TS	51	49		46	Repeated from pg 41, ln 41-46. [Nathan Gillett, Canada]	accepted: wording was modified
TS-1143	TS	52	2	53	41	I think this presentation of the RCPs works better than the one given in Box TS.4. In particular the focus on emission trajectories is useful (TFE.8, Figure 1 c). [Jan Fuglestvedt, Norway]	Noted, the decision was to move all text describing the RCPs under Box TS.6
TS-1144	TS	52	2			TFE.8. I think the first half of this TFE on 'Climate Targets' is redundant. Most of the material in this part of the TFE is either an exact copy of text in the main body of the TS Text or is described in different words elsewhere. The first paragraph is redundant given the information in box TS.4. The second paragraph is an exact copy of TS.5.3.2.1. The third paragraph includes an exact repeat of material in TS.5.3.2.7 (pg 46, ln 53-56 and pg 52, ln 29-32 are the same). The fourth paragraph contains some new information but some is repeated from pg 49, ln 34-37. If this section of the TFE.8 is deleted, some of the text from the fourth paragraph could go there. [Nathan Gillett, Canada]	Accepted. Text on RCP description as been moved and merged with text under Box TS.6. Assessment on projections is now focussed on targets and stabilization only. Reducing the overalp with the projection section.
TS-1145	TS	52	12	52	12	Please add "by the scientific community" after "The RCPs were developed". [Government of Germany]	Noted, text has been moved to Box TS.6
TS-1146	TS	52	17	52	18	Statement is not really true for RCP2.6 for which T stays almost constant, as stated in line 21 of the same para on page 52. [Government of Germany]	Noted, text removed
TS-1147	TS	52	20	52	20	How can RCP8.5 be a non-mitigation pathway, if TS.5.2. states "The new RCP scenarios in AR5 are all mitigation scenarios...2? [Government of Germany]	Noted, text removed
TS-1148	TS	52	23	52	23	Suggest adding here a note to indicate that to express the projected temperature changes relative to pre-industrial, ~0.6ºC needs to be added to the given ranges. [Government of Canada]	Noted, text removed
TS-1149	TS	52	23	52	23	Delete "CO2" as not only CO2 concentrations were prescribed for the RCP runs. [Government of Germany]	Noted, text removed
TS-1150	TS	52	27	52	27	Same comment as for Chapter 12, page 3, line 19: The current wording "... and about as likely as not to be above 2C warming for RCP2.6" seems to be at odds with the results provided in Table 12.2. In Table 2.12, the multi-model and global-mean warming is provided with maximally 1.0+-0.4C for the middle of the century and 1.0+-0.5 C for the end of the 21st century. In case of the 0.4 one-standard deviation case, and adding 0.6C warming for the 1986-2005 to preindustrial difference, this would result in the complete +-1std range being below 2C, i.e. a chance of exceeding 2C of only about 16% (assuming a normal distribution and 1std range reflecting a 68% range). In the case of the 0.5C std, the exceedance probability might be a bit higher. The point is however, that RCP2.6 with a multi-model mean warming of 1.6C seems to be better characterised with having a likely chance (>66%) of staying below 2C, than merely a (33% to 66%) "as likely as not" chance. A wording suggestion that would avoid to make a definite call on the exceedance probability of RCP2.6 would be to take the sentence from page 12-24, line 35, which says:"In the CMIP5 ensemble mean, global warming under RCP2.6 stays below 2C above preindustrial levels throughout the 21st century, clearly demonstrating the result of mitigation policies.". [Government of Germany]	Accepted. Change above 2degC is now considered unlikely for RCP2.6 (see Table 12.3).
TS-1151	TS	52	32	52	46	The terminology regarding the "projection" of emissions by the CMIP models is potentially confusing. "Projection" calls to mind IAM projections (eg, starting from economics to developing emissions): Taking a	Noted, text removed

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						forcing pathway and back-calculating an emissions trajectory would be better described as "back-calculating", not "projecting". [Government of United States of America]	
TS-1152	TS	52	41	52	41	The text doesnt refer to RCP3PD which is the scenario used in TFE8 Figure 1. Also in this figure I could not understand what the dashed lines in the lower two plots were [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Noted, figure removed
TS-1153	TS	53	1	53	12	The information given in this para comes from IAMs, not from CMIP5. Therefore IAMs and there difference needs to be explained, as suggested above maybe in Box TS.4. [Government of Germany]	Rejected, information on cumulative emissions does not come from IAMs, but from ESMs and EMICs. Sentence on multi-gas emission pathways does (which indeed does come from IAMs) has been removed.
TS-1154	TS	53	1	53	41	Please add information on the peak year as this is of high political relevance. [Government of Germany]	Noted, text removed
TS-1155	TS	53	5	53	7	Similar comment as on Chapter 12, page 6, line 49ff and SPM-29, line 29.: The current wording "In cumulative terms, the 2C temeprature target implies cumulative carbon emissions by 2100 of about 1000-1300Pg in the set of scenarios considered, of which about 550 Pg were emitted by 2100." seems to be wrong or misleading for four reasons. (1) This budget calculation is NOT based on any "set of scenarios". The underlying language on page 12-66 explains how the 1000-1300 GtC number is derived, namely from the best-estimate range of TCRE of 1.5C to 2C warming. Thus, the number 1000-1300 GtC is hence NOT tied to a specific set of scenarios. (2) The 2C temperature target is NOT a target that concerns only the CO2-induced warming, but the total anthropogenically induced global warming. The TCRE however only refers to the CO2-induced warming, ignoring any additional warming by non-CO2 forcing agents. Thus, the real carbon budget in line with a 2C target will be lower than 1000-1300GtC, because of non-CO2 forcings (which will be positive in the future). COMMENT CONTINUED IN NEXT BOX. [Government of Germany]	Accepted. This whole part has been extended and clarified. Non CO2 and the dependence of the budget and the likelihood are both explicitly mentioned. A budget for likely 2°C is given.
TS-1156	TS	53	5	53	7	CONTINUED COMMENT FROM PREVIOUS BOX: (3) The next sentences (as does the preambular text in the international communities's UNFCCC agreements in Durban, 2011) refer to a "likely chance" of staying below 2C. This latter carbon budget however is derived from a BEST-ESTIMATE, hence implying only a 50:50% chance of staying below 2C. And lastly, (4) the provided timeframe "by 2100" is misleading given that the 2C temperature target is not defined only to apply over the 21st century, but as a limit not to exceed at any moment in time. Given the definition of the TCRE, the sentence would be more correct without specifying the timeframe "by 2100". Summa summarum, these four issues could be addressed by re-phrasing this sentence into something like (in line with a re-phrasing of the parallel section in Chapter 12, page 6, line 49ff.) : "In cumulative terms, 1000-1300GtC of carbon emissions would imply a best-estimate warming of 2C only due to the effect of CO2 emissions. For having a likely chance of staying below 2C and accounting for non-CO2 forcings, the cumulative carbon emission budget would be substantially lower than 1000-1300GtC, of which about 545 [460 to 630] PgC were emitted by 2011." Finally, ensure consistency of the 550 PgC number between Chapter 12 and here, and add "C" to the "Pg" unit. [Government of Germany]	Accepted. This whole part has been extended and clarified. Non CO2 and the dependence of the budget and the likelihood are both explicitly mentioned. A budget for likely 2°C is given.
TS-1157	TS	53	5	53	7	Cumulative carbon emissions of 1000-1300 PgC since industrialisation? Perhaps need to make that clear. [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted, sentence rephrased.
TS-1158	TS	53	5	53	12	Does "carbon emissions" refer to CO2-only, or to all C-containg gases, or to CO2-equivalents? [Government of Germany]	Accepted, it refers to carbon only. Rephrased as CO2 emissions to avoid confusion.
TS-1159	TS	53	6			The term "2 C temperature target" should be avoided in this report since this may be misunderstood as an indication that the IPCC reinforces/supports this particular political goal. Considering the role of IPCC to provide rigorous and balanced scientific information to policy makers, it is essential for the IPCC to keep its neutrality; therefore, the sentence is better being rephrased to address the above concern. [Government of Japan]	Rejected. It is not stated that the IPCC supports this goal. The information is included as it is a commonly discussed target. We believe this to be policy relevant not prescriptive.
TS-1160	TS	53	7	53	7	Although this paragraph provides TFE.8 Fig 1 as a reference, it follows TFE.7 Figure 1. TFE.7 Figure 1 shows historical cumulative fossil fuel emissions of ~300PgC. Here, historical cumulative carbon emissions are given as 550PgC. To avoid potential misunderstanding, it would help to make note that this is combined fossil fuel and LUC emissions. [Government of Canada]	Accepted, we now refer to ALL anthropogenic emissions.

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TS-1161	TS	53	8	53	8	exactly 10 Pg C per year? [Government of Germany]	Accepted, sentence rephrased.
TS-1162	TS	53	9	53	12	As we note, it is not an integral quotation. In Chapter 12, this issue is stated in the section of “limitation and conclusion” to indicate that there are still uncertainties with the estimation of global cumulative emissions, as found between lines 55-57 of page 66, Chapter 12: “It is important to note that the cumulative budget constraint does not consider non-CO2 forcings. Also, since those ranges are based on a set of scenarios available in the literature the interpretation in terms of likelihood is difficult.” It is inappropriate for such an argument with much uncertainty to be cited as a key conclusion in the Technical Summary. Therefore, it is proposed to take out relevant words from the Technical Summary. If there is an insistence to have such elements reflected in the Technical Summary, the representation must be integral, with an emphasis placed on the fact that it is an estimate with limitations and uncertainties, coupled with a quotation of lines 55-57, page 66, Chapter 12. [Government of China]	Rejected, the discussion on cumulative carbon has been substantially revised and improved, and caveats on non CO2 are explicitly discussed. The evidence fo the linearity between global temperature change and cumulative carbon is very clear and robust across models. It is a key conclusion from chapter 12, and therefore is mentioned in the technical summary.
TS-1163	TS	53	11	53	11	Where do the ranges come from? How is likely defined here? [Government of Germany]	Noted, text removed
TS-1164	TS	53	11	53	11	Why do you give numbers for 2020 and 2050? It would be more interesting to get information about the trajectories and emissions reductions needed for different peak years. [Government of Germany]	Noted, text removed
TS-1165	TS	53	12	53	12	What is the purpose of the information about the median? [Government of Germany]	Noted, text removed
TS-1166	TS	53	14	53	14	Please delete "policy relevant", or provide a reason for your statement that the new quantity is policy relevant. It would be good to give it a name [Government of Germany]	Accepted, sentence rephrased.
TS-1167	TS	53	14	53	14	Does this refer to stabilisation temperatures? [Government of Germany]	Noted, "climate targets" and "climate stabilization" subsections have been removed.
TS-1168	TS	53	14	53	14	"global temperature" - should this be the "target global temperature"? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Rejected, TCRE is a relationship between cumulated CO2 emissions and global temperature change. It does not need to be a particular target.
TS-1169	TS	53	14			Presumably this global temperature refers to long-term temperature - in the short-term, the relationship is less linear and more dependent on other factors. That should be made clear in a couple of the sentences here. [Government of United States of America]	Noted, this is indeed in the context of long term climate response.
TS-1170	TS	53	17			Insert '(the Transient climate response to cumulative carbon emissions, TCRE)' after 'carbon emissions'. The term is used later in the paragraph, but the definition is not clear as written. [Nathan Gillett, Canada]	Accepted. This whole part has been extended and clarified.
TS-1171	TS	53	20	53	20	Would "transient climate response" imply that your new quantity does not refer to stabilisation temperature but to an average of 20 years..as described in the Glossary? [Government of Germany]	Noted, the definition of TCRE is not as strictly defined as TCR is.
TS-1172	TS	53	20	53	23	Sentence is unclear. Where do the ranges come from? What does best "estimate mean"? what does "until the time at which temperatures peak" mean (may be for cumulative emissions lower than 2000PgC?) ? [Government of Germany]	Accepted. This whole part has been extended and clarified.
TS-1173	TS	53	22	53	22	If DT/totC is independent of the scenario (same para line 18), why do the numbers only hold for cumulative emissions < 2000 PgC? [Government of Germany]	Noted, it might hold for higher emissions but there are too few studies for emissions above 2000Pg to provide an expert judgement.
TS-1174	TS	53	23	53	24	Sentence is unclear. [Government of Germany]	Accepted, sentence removed.
TS-1175	TS	53	27		32	Repeated from TS.5.3.2.7. I suggest that this is retained here and deleted there (pg 46, ln 57 - pg 47, ln 5). [Nathan Gillett, Canada]	Accepted, section TS 5.3.2.7 has been removed.
TS-1176	TS	53	29		31	While the statement that the persistence of the warming is longer than the gas lifetimes is clearly true for short-lived forcings, I think this statement is misleading for CO2. It may be true in the sense that the warming has a longer e-folding timescale than the concentration. But it is fundamentally the CO2 sinks which set the timescale for recovery from CO2 warming. The lifetime of part of the anthropogenic CO2 is so long that the heat transfer into and out of the ocean and the nonlinear absorption affects don't matter much. For CO2 I think key information, which is missing here, is an estimate of the lifetime of the CO2 itself. [Nathan Gillett, Canada]	Accepted, sentence removed.

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TS-1177	TS	53	32		35	If emissions of shortlived GHGs such as CH4 are reduced to zero at the same time, doesn't this offset much of the warming due to reduced aerosols in the near-term? [Nathan Gillett, Canada]	Noted, but from the sudies available, the effect of aerrosols dominates.
TS-1178	TS	53	43	54	34	The paragraphs about Geoengineering (GE, SRM + CDR) should not be included in this TFE.8. As it now follows the section „ Climate Targets" and "Climate Stabilisation" this gives the impression that GE-methods (or more precisely CDR and SRM) are seen as the only solutions to reach climate targets and stabilisation. This it not the case as the WGIII report will prove, and in addition it attributes an importance to CDR and SRM that is too high. We suggest to shift the two paras to page 50 after line 22. It should be added that GE has far reaching implications as for other areas/ levels (economics, politics, ethics, legislation,…). There should be a reference to other parts of AR 5 (Capter 7, page53 line 47). [Government of Germany]	Accepted, discussion on geoengineering has moved under Box TS.7
TS-1179	TS	53	48	53	48	All CDR methods are mentioned together, without any differentiation. It should be at least stated that there are enormous differences among the various methods as for e.g. technologies, effects, potential risks. Add following text at the end: there are enormous differences among the various methods as for e.g. technologies,expected effects, potential associated risks, feasibility. [Government of Germany]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1180	TS	53	53	53	53	Please rephrase "CDR schemes do not rapidly affect climate ..." deleting the statement on CDR as a potential "viable option". This statement that is not justified here, because (mitigation) options have not been investigated. [Government of Germany]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1181	TS	53	55	53	56	…'temperatures have increased since' [and] 'This warming [i.e. that which has happened, as written in the previous phrase] is 'virtually certain' are incompatible phrases - In this context 'Have' is a word that defines certainty, not near certainty. This needs cross-checking with Chapter 2 [Government of United Kingdom of Great Britain & Northern Ireland]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1182	TS	53	55	54	5	It is suggested to delete "currently" and to add another sentence along the lines: Some CDR schemes such as direct air capture have not be assessed so far with respect to their removal potential. [Klaus Radunsky, Austria]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1183	TS	54	2	54	5	This para talks about "side effects" and "effects". Additionally, the word "risk" should be used. The current wording is not adequate, because a lot of/various effects would be negative: impacts on precipitation ground water, food vs land usefor e.g. CDR measures, biodiversity, etc. In order to be policy relevant, IPCC should use neutral language, but not play down risks. Delete "side effects" and "effects" in line 4 and insert "impacts" and add after de-oxygenation, "biodiversity, ground water etc." Moreover it should be stated additionally that "Further research is needed before an assessment of CDR is possible." [Government of Germany]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1184	TS	54	2		5	Suggest qualifying the constraints for land-based CDR methods further or revising or softening the statement. It has not been explained why the constraints on land-based options make them unfeasible, whereas constraints on other geoengineering or mitigation options are manageable. [Government of Canada]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1185	TS	54	7	54	34	This write-up really fails to indicate the potential benefits (foregone impacts) of climate change while suggesting that there are numerous unintended consequences. In a relative risk analysis there would be consideration of the relative importance of each--that is, this is not an evaluation of geoengineering on top of a stable climate creating no significant impacts; the issue is global warming with or without SRM, and what needs to be presented is a sense of what the relative risks and benefits would be. And as to uncertainties, it seems to me there are many more for global warming without geoengineering as the climate is taken to conditions not experienced in tens of millions of years as compared to imitating volcanic eruptions and seeking to keep the global climate near to present conditions. I just don't think the text here is at all balanced--the foregone benefits are not discussed specifically and just summarized in part of a sentence, whereas a number of possible complications that apply to only some of the approaches is covered in a full paragraph. And as to "unanticipated and unexplored impacts"--I would suggest there are a lot more such impacts for global warming without geoengineering than with. [Michael MacCracken, United States of America]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1186	TS	54	7	54	34	I think it has to be pointed out more strongly in this discussion that SRM, although it could substantially offset a GLOBAL temperature rise, sensitive regions such as high latitudes (i.e., Arctic/Antarctica) will continue to warm.	Noted, discussion on geoengineering has moved under Box TS.7

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						[Jana Sillmann, Canada]	
TS-1187	TS	54	9	54	9	Are there observations of SRM methods? We assume, that this is not the case and the word "observation" should be deleted. [Government of Germany]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1188	TS	54	9	54	10	The sentence "Theory, model studies and observations suggest that some Solar Radiation Management (SRM) methods, if realizable, could substantially offset a global temperature rise and some of its effects. {7.7}" could be understood as a headline for the SRM-chapter in the TS. However, the sentence almosts suggests SRM as a feasible option. The aspects of negative side effects and risks should be mentioned here as well. Moreover the aspect of uncertainty and further research needs before assessing SRM. Therefore insert the following sentence: However as mentioned below numerous side effecs and risks have been identified. Overall uncertainties are very large and further research is necessary. [Government of Germany]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1189	TS	54	9	54	10	Theory, model studies and observations suggest that some Solar Radiation Management (SRM) methods, if realizable, could substantially offset a global temperature rise and some of its effects. {7.7}' This statement should contain a likelihood and/or confidence statement. [Line van Kesteren, the Netherlands]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1190	TS	54	9	54	10	Theory, model studies and observations suggest that some Solar Radiation Management (SRM) methods, if realizable, could substantially offset a global temperature rise and some of its effects. {7.7}' This statement should contain a likelihood and/or confidence statement. [Line van Kesteren, the Netherlands]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1191	TS	54	9	54	34	Section TFE.8: TS -54 Confidence levels (medium, very high) regarding SRM are not found in Section 7.7 [Government of United States of America]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1192	TS	54	12	54	12	"There is medium confidence (medium evidence, medium agrrement) that…" should be deleted since there is not enough evidence up to now. [HUA ZHANG, China]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1193	TS	54	29	54	29	mention the time scale over which polar ozone depletion would be a problem; perhaps until about 2100 [Rolf Müller, Germany]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1194	TS	54	29	54	32	“Moreover, if SRM were used to counter a large RF by greenhouse gases and then terminated, most of the warming that had been offset would become evident within a few decades, and the rate of climate change would exceed the rate that would have occurred in the absence of SRM.” Could add a sentence that suggests this rapid warming would stress the ability of ecosystems and humans to adapt. [Government of United States of America]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1195	TS	54	31	54	31	Does it really take „a few decades“ after the stop of CDR? Chapter 7 indicates 1-2 decades. [Government of Germany]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1196	TS	54	34	54	34	mention the problem that SRM measures would have to be sustained for extremely long time periods, otherwise the warming would 'catch up' quickly after the SRM measures are stopped. [Rolf Müller, Germany]	Noted, discussion on geoengineering has moved under Box TS.7
TS-1197	TS	54	41	54	43	This is contradicted by most recent sea level measurements according to AVISO data discussed in TS 10 54-55. [François Gervais, France]	Rejected. We think there must be a misunderstanding. These numbers are projections for the end of the 21st century; the earlier section reports observartions for the late 20th and early 21st century.
TS-1198	TS	54	41	54	43	Please explain the choice of the interval boundaries for the ranges, and the for associated uncertainty, if possible in the text. At least provide references where this information can be found in text. Are the choices comparable to AR4? This information is relevant costal protection. [Government of Germany]	The text adequately takes these points into account. The periods 1986-2005 and 2081-2100 are used for long-term projections of all quantities in chapters 12 and 13, and TS5. The interpretation of the ranges as likely ranges follows TS5.5.1 and TFE6, and is discussed in more detail in 13.5.1. The text of TS5.7.1 mentions that the inclusion of ice sheet rapid dynamical change is the main reason for larger projections than in the AR4; there is a more detailed comparison with the AR4 projections in 13.5.3, for which there is not space in the TS.

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TS-1199	TS	54	44	54	44	Please check: here the value is given as 0.11m for the central projection of changes in ice-sheet outflow, and on page 55 line 19, it is given as 0.12m. Also, is the intent here to indicate that 0.11m is added to each of the projected SLR ranges for the 4 RCPs? Please clarify. [Government of Canada]	Taken into account by giving the number is only one place. Also, a range is now given instead of a single number.
TS-1200	TS	54	44			Use of a central projection is also misleading as those using this information will use it and ignore the range. Care needs to be taken around such statements. [Judy Lawrence, New Zealand]	Taken into account by giving a range instead of just the central projection.
TS-1201	TS	54	55	54	55	Please explain the sentence in regards to the meaning of and are treated as having uniform propability distributions, and explain, if the assumption is justified. [Government of Germany]	Taken into account by adding more explanation to the caption of Figure TS.21.
TS-1202	TS	55	7	55	9	In all RCP scenarios, thermal expansion is the largest contribution, accounting for 30–50% of the total in the central projections, and glaciers are the next largest. The increase in surface melting in Greenland is projected to exceed the increase in accumulation.' This statement should contain a likelihood and/or confidence statement. [Line van Kesteren, the Netherlands]	Rejected. These are assertions about the projections (like those of the previous paragraph), for which likelihood and confidence has already been given.
TS-1203	TS	55	7	55	9	In all RCP scenarios, thermal expansion is the largest contribution, accounting for 30–50% of the total in the central projections, and glaciers are the next largest. The increase in surface melting in Greenland is projected to exceed the increase in accumulation.' This statement should contain a likelihood and/or confidence statement. [Line van Kesteren, the Netherlands]	See TS-1202.
TS-1204	TS	55	18			Use of a central projection is also misleading as those using this information will use it and ignore the range. Care needs to be taken around such statements. [Judy Lawrence, New Zealand]	See TS-1200.
TS-1205	TS	55	23	55	23	Has the effect of the disappearance of glaciers been considered in semi-empirical models? [Government of Germany]	We note the question. No change to the TS text has been proposed. This particular question is addressed in 13.5.2 but there is not space in the TS to make an adequate summary of that complicated discussion.
TS-1206	TS	55	23	55	34	there is a question about whether 'semi-empirical' belongs in WG1 - the PHYSICAL science basis. There is a general clash here. The approach seems more akin to WG2. [Mark Siddall, United Kingdom]	We note the question. If the reviewer is proposing that semi-empirical models should not be considered by WG1, we must reject the suggestion, because they do relate to the physical science basis of sea level projection, and they raise important issues of policy relevance.
TS-1207	TS	55	24	55	24	no consensus' would 'semi-empirical models are highly controversial' be better. Because those working on SE models are skeptical about ice sheet dynamical models, one could say the same about ice sheet models. On the other hand the controversial nature of SE models can be established objectively by citing the numerouse comments and responses on SE models in the literature [Mark Siddall, United Kingdom]	Rejected. We think "no consensus" is more objective way of describing the lack of agreement than "controversial" would be. Moreover, "measure of consensus" is a phrase which appears in the uncertainty guidelines (Mastrandea et al.)
TS-1208	TS	55	43	55	43	Why is it not "virtually certain" that SLR would continue beyond 2100? By which process could it be stopped? [Government of Germany]	Accepted and changed.
TS-1209	TS	55	43	55	46	the contents from this paragraph has nothing to do with Fig3.10 and Fig3.11. [Tianyu Zhang, China]	Accepted; figure references removed.
TS-1210	TS	55	44	55	45	The amount of ocean thermal expansion increases with global warming (0.2–0.6 m °C–1).' This statement should contain a likelihood and/or confidence statement. [Line van Kesteren, the Netherlands]	Taken into account by indicating that this is a model range. We are not able to assess a likelihood for it.
TS-1211	TS	55	44	55	45	The amount of ocean thermal expansion increases with global warming (0.2–0.6 m °C–1).' This statement should contain a likelihood and/or confidence statement. [Line van Kesteren, the Netherlands]	See TS-1210.
TS-1212	TS	55	56	55	56	Case study such as "one study estimated a lower threshold of 1.6 [1.8-3.2]" is not suitable to be presented in the comprehensive discusses in the TS. [Ke Xiu LIU, China]	Taken into account by emphasising the large uncertainty, given the limited evidence, which means that we cannot assess likelihood.
TS-1213	TS	55	57	56	1	As with comment on SPM p17, 47-48: Do any of the models show temperature declining any time soon? I'm not aware that any do. Suggest rewording to the reflect that under even the most ambitious GHG mitigation scenario we would expect partial loss of the Greenland ice sheet at some point over the next 300 years, and	Rejected. This text does not suggest there will no contribution from the Greenland ice sheet in the next 300 years. That is actually to be taken as read. This

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						that the rate and extent of ice loss is linked to the ambition of mitigation. [Government of United Kingdom of Great Britain & Northern Ireland]	text is about the long-term commitment to irreversible loss.
TS-1214	TS	56	5	56	8	Using ppm as a metric now, rather than referring to the RCP scenarios - could mention which RCPs most closely match the quoted ppms. [Government of United Kingdom of Great Britain & Northern Ireland]	Rejected. We use ppm because most of this evidence comes from idealised scenarios which are not RCPs, and the RCPs themselves are only for the 21st century.
TS-1215	TS	56	6	56	7	This is unreliable on inspection of most recent sea level measurements according to AVISO data discussed in TS 10 54-55 and also of other arguments given throughout this reviewer report. [François Gervais, France]	See TS-1197. This is a statement about projections, not observations.
TS-1216	TS	56	16	56	18	By the end of the 21st century, about 72% and 77% of the global coastlines are projected to experience a sea-level change within 20% of the global mean sea level change, for RCP4.5 and 8.5, respectively.' This statement should contain a likelihood and/or confidence statement. [Line van Kesteren, the Netherlands]	Rejected. This is an assertion (modified in detail in the final draft) about the projections, for which likelihood and confidence have already been assessed.
TS-1217	TS	56	16	56	18	By the end of the 21st century, about 72% and 77% of the global coastlines are projected to experience a sea-level change within 20% of the global mean sea level change, for RCP4.5 and 8.5, respectively.' This statement should contain a likelihood and/or confidence statement. [Line van Kesteren, the Netherlands]	See TS-1216.
TS-1218	TS	56	29	56	29	Please give an order of magnitude of regional SLR in absolut terms and relative to the global mean. [Government of Germany]	Rejected. These are qualitative statements to give background. More quantitative information can be found in the figures in chapter 13.
TS-1219	TS	56	30	56	30	Ocean dynamical ocean change...' is this a typo? if not, clarify. If it is, correct. [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted and corrected.
TS-1220	TS	56	30	56	30	Ocean dynamical ocean change results from changes in wind forcing,' Suggest to delete second time ocean: Ocean dynamical change results from changes in wind forcing, [Line van Kesteren, the Netherlands]	See TS-1219.
TS-1221	TS	56	30	56	30	Ocean dynamical ocean change results from changes in wind forcing,' Suggest to delete second time ocean: Ocean dynamical change results from changes in wind forcing, [Line van Kesteren, the Netherlands]	See TS-1219.
TS-1222	TS	56	30			It is suggested to delete "Ocean" and strat the sentence: Dynamical ocean change results … [Klaus Radunsky, Austria]	See TS-1219.
TS-1223	TS	56	40	56	40	... ...increase in occurrence of future extreme sea level...' I think there is something slightly confusing in this wording that removing the word future would help. As worded it suggests that the 'future extremes' increase in frequency rather than the number of times we see levels of a particular level would increase. The difference in interpretation of the wording is quite subtle but I think is real. [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account by rewording.
TS-1224	TS	56	51	56	51	What is surface wave height? Please explain, at least in the Glossary [Government of Germany]	Taken into account by rewording, in order not to use the phrase "surface wave", which just means "wave" in this context. No entry has been added to the glossary.
TS-1225	TS	57	30	57	30	There is medium to high confidence that the global measure of monsoon precipitation is likely to increase in the 21st century while the monsoon circulation weakens.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that the global measure of monsoon precipitation will increase in the 21st century while the monsoon circulation weakens (medium to high confidence).' [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1226	TS	57	30	57	30	There is medium to high confidence that the global measure of monsoon precipitation is likely to increase in the 21st century while the monsoon circulation weakens.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that the global measure of monsoon precipitation will increase in the 21st century while the monsoon circulation weakens (medium to high confidence).' [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1227	TS	57	32	57	33	There is high confidence that extreme precipitation will very likely increase in all monsoon regions (Figure	The use of the uncertainty language has been

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						TS.17), a change much more robust than the seasonal mean.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is very likely that extreme precipitation will increase in all monsoon regions (Figure TS.17), a change much more robust than the seasonal mean (high confidence). [Line van Kesteren, the Netherlands]	improved.
TS-1228	TS	57	32	57	33	There is high confidence that extreme precipitation will very likely increase in all monsoon regions (Figure TS.17), a change much more robust than the seasonal mean.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is very likely that extreme precipitation will increase in all monsoon regions (Figure TS.17), a change much more robust than the seasonal mean (high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1229	TS	57	33	57	35	There is medium confidence that interannual rainfall variability is likely to increase in the future and the relationship between monsoon and El Niño is likely to remain, subject to slow natural modulations.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that interannual rainfall variability will increase in the future and the relationship between monsoon and El Niño is likely to remain, subject to slow natural modulations (medium confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1230	TS	57	33	57	35	There is medium confidence that interannual rainfall variability is likely to increase in the future and the relationship between monsoon and El Niño is likely to remain, subject to slow natural modulations.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that interannual rainfall variability will increase in the future and the relationship between monsoon and El Niño is likely to remain, subject to slow natural modulations (medium confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1231	TS	57	37	57	39	There is medium to high confidence that overall precipitation associated with the Asian-Australian monsoon system is likely to increase but with a north-south asymmetry: the Indian monsoon rainfall increases while the changes in the Australian summer monsoon rainfall are small.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that overall precipitation associated with the Asian-Australian monsoon system will increase but with a north-south asymmetry: the Indian monsoon rainfall increases while the changes in the Australian summer monsoon rainfall are small (medium to high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1232	TS	57	37	57	39	There is medium to high confidence that overall precipitation associated with the Asian-Australian monsoon system is likely to increase but with a north-south asymmetry: the Indian monsoon rainfall increases while the changes in the Australian summer monsoon rainfall are small.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that overall precipitation associated with the Asian-Australian monsoon system will increase but with a north-south asymmetry: the Indian monsoon rainfall increases while the changes in the Australian summer monsoon rainfall are small (medium to high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1233	TS	57	43	57	35	There is medium confidence that over the Maritime continent monsoon the austral summer precipitation is likely to increase.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that over the Maritime continent monsoon the austral summer precipitation will increase (medium confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1234	TS	57	43	57	35	There is medium confidence that over the Maritime continent monsoon the austral summer precipitation is likely to increase.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that over the Maritime continent monsoon the austral summer precipitation will increase (medium confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1235	TS	57	45	57	47	There is high confidence that the Australian summer monsoon over the Java archipelago and northernmost Australia will very likely to be delayed and shortened while there is a medium confidence in the delay of monsoon over the interior of Australia. " The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is very likely that the Australian summer monsoon over the Java archipelago and northernmost Australia will be delayed and shortened (high confidence) while there is a medium confidence in the delay of monsoon over the interior of Australia. [Line	The use of the uncertainty language has been improved.

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						van Kesteren, the Netherlands]	
TS-1236	TS	57	45	57	47	There is high confidence that the Australian summer monsoon over the Java archipelago and northernmost Australia will very likely to be delayed and shortened while there is a medium confidence in the delay of monsoon over the interior of Australia. " The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is very likely that the Australian summer monsoon over the Java archipelago and northernmost Australia will be delayed and shortened (high confidence) while there is a medium confidence in the delay of monsoon over the interior of Australia. [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1237	TS	57	47	57	48	There is medium confidence that the Western North Pacific monsoon is likely to weaken, but compensating moisture effects will enhance precipitation.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that the Western North Pacific monsoon will weaken, but compensating moisture effects will enhance precipitation (medium confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1238	TS	57	47	57	48	There is medium confidence that the Western North Pacific monsoon is likely to weaken, but compensating moisture effects will enhance precipitation.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that the Western North Pacific monsoon will weaken, but compensating moisture effects will enhance precipitation (medium confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1239	TS	57	50	51	57	Please explain SAMS and NAMS for non-experts [Government of Germany]	Acronyms are no longer used. NAM and SAM are defined in glossary.
TS-1240	TS	57	55	57	56	There is medium confidence that a small delay in the development of the West African mean rainy season is likely; but with an intensification of late-season rains.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that a small delay in the development of the West African mean rainy season; but with an intensification of late-season rains (medium confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1241	TS	57	55	57	56	There is medium confidence that a small delay in the development of the West African mean rainy season is likely; but with an intensification of late-season rains.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that a small delay in the development of the West African mean rainy season; but with an intensification of late-season rains (medium confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1242	TS	58	3	58	5	There is medium to high confidence that annual rainfall change over tropical oceans is likely to follow a 'warmer-get-wetter' pattern, increasing where the sea surface temperature (SST) warming exceeds the tropical mean and vice versa.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that annual rainfall change over tropical oceans will follow a 'warmer-get-wetter' pattern, increasing where the sea surface temperature (SST) warming exceeds the tropical mean and vice versa (medium to high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1243	TS	58	3	58	5	There is medium to high confidence that annual rainfall change over tropical oceans is likely to follow a 'warmer-get-wetter' pattern, increasing where the sea surface temperature (SST) warming exceeds the tropical mean and vice versa.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that annual rainfall change over tropical oceans will follow a 'warmer-get-wetter' pattern, increasing where the sea surface temperature (SST) warming exceeds the tropical mean and vice versa (medium to high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1244	TS	58	5	58	5	I don't think "vice versa" is exactly what is meant here, that "cooler-get-drier". [Dian Seidel, United States of America]	Reworded.
TS-1245	TS	58	9	58	15	Please explain SPCZ events, SACZ and MJO for non-experts, at least in the Glossary. [Government of Germany]	New entry introduced for "South Pacific Convergence Zone (SPCZ)" and "Madden Julian Oscillation (MJO)"

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TS-1246	TS	58	10	58	11	There is medium confidence that the frequency of zonally-oriented SPCZ events is likely to increase, with the SPCZ lying well to the northeast of its average position.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that the frequency of zonally-oriented SPCZ events will increase, with the SPCZ lying well to the northeast of its average position (medium confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1247	TS	58	10	58	11	There is medium confidence that the frequency of zonally-oriented SPCZ events is likely to increase, with the SPCZ lying well to the northeast of its average position.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that the frequency of zonally-oriented SPCZ events will increase, with the SPCZ lying well to the northeast of its average position (medium confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1248	TS	58	17	58	19	There is medium to high confidence that the tropical Indian Ocean is likely to feature a zonal pattern with reduced (enhanced) warming and decreased (increased) rainfall in the east (west), a pattern especially pronounced during August-November.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that the tropical Indian Ocean will feature a zonal pattern with reduced (enhanced) warming and decreased (increased) rainfall in the east (west), a pattern especially pronounced during August-November (medium to high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1249	TS	58	17	58	19	There is medium to high confidence that the tropical Indian Ocean is likely to feature a zonal pattern with reduced (enhanced) warming and decreased (increased) rainfall in the east (west), a pattern especially pronounced during August-November.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that the tropical Indian Ocean will feature a zonal pattern with reduced (enhanced) warming and decreased (increased) rainfall in the east (west), a pattern especially pronounced during August-November (medium to high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1250	TS	58	18	58	18	This use of parentheses for alternatives is very hard to read, and the subject of quite a few "rants" in the literature. For the sake of a few extra words, it could be expressed much more clearly [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Reworded.
TS-1251	TS	58	19	58	20	There is high confidence that the Indian Ocean dipole mode will likely remain active, with interannual variability unchanged in SST but decreasing in thermocline depth.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that the Indian Ocean dipole mode will remain active, with interannual variability unchanged in SST but decreasing in thermocline depth (high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1252	TS	58	19	58	20	There is high confidence that the Indian Ocean dipole mode will likely remain active, with interannual variability unchanged in SST but decreasing in thermocline depth.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that the Indian Ocean dipole mode will remain active, with interannual variability unchanged in SST but decreasing in thermocline depth (high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1253	TS	58	26	58	27	There is high confidence that ENSO very likely remains as the dominant mode of interannual variability in the future.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is very likely that ENSO remains as the dominant mode of interannual variability in the future (high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1254	TS	58	26	58	27	There is high confidence that ENSO very likely remains as the dominant mode of interannual variability in the future.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is very likely that ENSO remains as the dominant mode of interannual variability in the future (high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1255	TS	58	26	58	30	Teleconnection patterns' requires further explanation [Government of United Kingdom of Great Britain & Northern Ireland]	Reworded.
TS-1256	TS	58	27	58	28	There is high confidence that both El Niño and La Niña-induced teleconnection patterns over the extra-tropical	The use of the uncertainty language has been

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Northern Hemisphere are likely to move eastwards in the future.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that both El Niño and La Niña-induced teleconnection patterns over the extra-tropical Northern Hemisphere will move eastwards in the future (high confidence). [Line van Kesteren, the Netherlands]	improved.
TS-1257	TS	58	27	58	28	There is high confidence that both El Niño and La Niña-induced teleconnection patterns over the extra-tropical Northern Hemisphere are likely to move eastwards in the future.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that both El Niño and La Niña-induced teleconnection patterns over the extra-tropical Northern Hemisphere will move eastwards in the future (high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1258	TS	58	28	58	30	I struggled with Figure TS.18 - as far as I could tell, it was just saying that the ENSO variability would likely stay the same in future warmed world. Does this need a figure, or would a sentence do? [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	We felt that ENSO is an important phenomenon that merits a figure to feature.
TS-1259	TS	58	28		30	Is it really true that confidence is low due to large natural modulations in El Nino? This means that given a long enough time period we are confident that the teleconnection patterns would move eastwards, it's just that for a short period this forced signal may be contaminated by noise. Don't we also have low confidence in the forced signal? [Nathan Gillett, Canada]	The revison puts changes in the amplitude and pattern of ENSO itself low confidence. Despite this, the PNA may shift eastward.
TS-1260	TS	58	29	58	30	Why the models which claim to be able to predict the climate until 2300 are unable to predict ENSO phenomena which are merely the result of winds, pressures and oceanic currents ? These phenomena are remarkably explained and discussed in the book of Bob Tisdale « Who turned on the heat ? The unsuspected Global Warming Culprit, El Niño-Southern Oscillation » which deserves mention because the disruptions in ocean heat contents are shown to occur over time scales of about two years which have obviously nothing to do with the longer time dependence of CO2 and other greenhouse gases growth rates. ENSO fluctuations are able to heat or cool the Pacific Ocean by several degrees with a global impact as large as 0.4°C, viz. the same order of magnitude as the heating observed during the 20th century. [François Gervais, France]	The ENSO effect as commented here is precisely the reason we evaluate its change. Climate models typically have skills predicting individual ENSO events two seasons to a year in advance.
TS-1261	TS	58	41			this paragraph refers to SAM but not to NAM? [David Sauchyn, Canada]	Reworded.
TS-1262	TS	58	43	58	44	There is high confidence that future boreal wintertime NAO is very likely to exhibit large natural variations of similar magnitude to those observed in the past.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is very likely that future boreal wintertime NAO is to exhibit large natural variations of similar magnitude to those observed in the past (high confidence). [Line van Kesteren, the Netherlands]	The sentence removed.
TS-1263	TS	58	43	58	44	There is high confidence that future boreal wintertime NAO is very likely to exhibit large natural variations of similar magnitude to those observed in the past.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is very likely that future boreal wintertime NAO is to exhibit large natural variations of similar magnitude to those observed in the past (high confidence). [Line van Kesteren, the Netherlands]	The sentence removed.
TS-1264	TS	58	44	58	46	The sentence starting with: It is also very likely .. Should be worded as its meaning is unclear. [Klaus Radunsky, Austria]	Reworded.
TS-1265	TS	58	44		45	What does it mean that the NAO 'is very likely to differ quantitatively in long-term trend from individual climate model projections'? . It's obvious that the actual trend will not be exactly the same as an individual climate simulation. So does this mean that the projected trend will differ significantly from individual climate model projections. If so, then which ones? Given that the variability in the NAO is large, this means that the simulated and observed trends would have to be very different. I suggest deleting this phrase. [Nathan Gillett, Canada]	The sentence removed.
TS-1266	TS	58	46	58	48	There is high confidence that the austral summer/autumn positive trend in SAM is likely to weaken considerably as ozone depletion recovers through to the mid-21st century.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that the austral summer/autumn positive trend in SAM is to weaken considerably as ozone depletion recovers through to the mid-21st century (high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-1267	TS	58	46	58	48	There is high confidence that the austral summer/autumn positive trend in SAM is likely to weaken considerably as ozone depletion recovers through to the mid-21st century.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that the austral summer/autumn positive trend in SAM is to weaken considerably as ozone depletion recovers through to the mid-21st century (high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1268	TS	58	46		48	What is the evidence that the autumn SAM is likely to weaken considerably as ozone depletion recovers? The dominant influence will be in DJF. Admittedly Figure TS.10 shows a small ozone influence in MAM, but I don't know that there is a lot of literature demonstrating a weakening of the MAM SAM trend due to ozone recovery. [Nathan Gillett, Canada]	The summary was drawn from 14.5.2.
TS-1269	TS	58	49		52	I disagree with this statement, at least for stratosphere-troposphere interaction and ozone chemistry. Most CMIP5 models have high enough resolution that they should be able to resolve the response to ozone depletion e.g. Karpechko et al. (2008). Son et al. (2010) found no significant differences in the SAM trends simulated in the CCMVal-2 models and the CMIP3 models, in contrast to Son et al. (2008) who found a difference using a smaller sample of CCMVal-1 models. So I think the evidence that chemistry-climate coupling is needed to resolve the SH circulation response to ozone is weak, although admittedly there is some evidence that the 3D structure of the ozone may be important. Karpechko, A. Y., Gillett, N. P., Marshall, G. J., & Scaife, A. A. (2008). Stratospheric influence on circulation changes in the Southern Hemisphere troposphere in coupled climate models. Geophysical Research Letters, 35(20), L20806. Son, S. W., Gerber, E. P., Perlwitz, J., Polvani, L. M., Gillett, N. P., Seo, K. H., ... & Yamashita, Y. (2010). Impact of stratospheric ozone on Southern Hemisphere circulation change: A multimodel assessment. Journal of Geophysical Research, 115(null), D00M07. [Nathan Gillett, Canada]	The sentence removed.
TS-1270	TS	58	56	58	58	There is low to medium confidence that it is likely that the frequency of Northern Hemisphere and Southern Hemisphere blocking will decrease under increasing GHG concentrations, while trends in blocking intensity and persistence are uncertain.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that the frequency of Northern Hemisphere and Southern Hemisphere blocking will decrease under increasing GHG concentrations, while trends in blocking intensity and persistence are uncertain (low confidence). [Line van Kesteren, the Netherlands]	Reworded.
TS-1271	TS	58	56	58	58	There is low to medium confidence that it is likely that the frequency of Northern Hemisphere and Southern Hemisphere blocking will decrease under increasing GHG concentrations, while trends in blocking intensity and persistence are uncertain.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that the frequency of Northern Hemisphere and Southern Hemisphere blocking will decrease under increasing GHG concentrations, while trends in blocking intensity and persistence are uncertain (low confidence). [Line van Kesteren, the Netherlands]	Reworded.
TS-1272	TS	58	56	59	2	Fig TS.2 Can the data be updated to 2011? [Government of United Kingdom of Great Britain & Northern Ireland]	Done
TS-1273	TS	58	56			I don't think 'it is likely that' is needed here, given the low confidence assessment. [Nathan Gillett, Canada]	Reworded.
TS-1274	TS	58	57	58	57	What is blocking? Please explain, at least in the Glossary [Government of Germany]	New Glossary entry added for "Blocking"
TS-1275	TS	59	1			NAM is identified here but no in the previous subsection where it is relevant [David Sauchyn, Canada]	Reworded.
TS-1276	TS	59	8	58	15	Please explain QBO in the Glossary. [Government of Germany]	New Glossary entry added for "Quasi-Biennal Oscillation (QBO)
TS-1277	TS	59	10	59	11	Several recent papers investigate model projections of the PDO. Does this work represent “have not been investigated in any depth”. [David Sauchyn, Canada]	We felt that the work on PDO changes does not have enough depth to be discussed in the current TS.
TS-1278	TS	59	12	59	13	See comment 17 above. AMO or AMV? [Government of United Kingdom of Great Britain & Northern Ireland]	AMO here.
TS-1279	TS	59	13	59	15	However, natural fluctuations in interdecadal modes such as the PDO and AMO over the coming few decades are likely to influence regional climates at least as strongly as will human induced changes.' This applis to which regions? And how will these be influenced? [Line van Kesteren, the Netherlands]	In regions affected by these modes.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-1280	TS	59	13	59	15	However, natural fluctuations in interdecadal modes such as the PDO and AMO over the coming few decades are likely to influence regional climates at least as strongly as will human induced changes.' This applis to which regions? And how will these be influenced? [Line van Kesteren, the Netherlands]	In regions affected by these modes.
TS-1281	TS	59	18			Would it be appropriate to repeat the storm surge/SLR link here? Also, can anything be said about changes in the AREA of tropical cyclones, in addition to frequency, maximum intensity, and precip? [Government of United States of America]	Region-specific changes are uncertain.
TS-1282	TS	59	22	59	23	For individual basins, the SST warming pattern and multi-decadal climate variability will affect tropical cyclone activity through much of the 21st century (Figure TS.19, see also TFE.9).' Affect how, positively or negatiely? Or we do not know? [Line van Kesteren, the Netherlands]	The sentence removed.
TS-1283	TS	59	22	59	23	For individual basins, the SST warming pattern and multi-decadal climate variability will affect tropical cyclone activity through much of the 21st century (Figure TS.19, see also TFE.9).' Affect how, positively or negatiely? Or we do not know? [Line van Kesteren, the Netherlands]	The sentence removed.
TS-1284	TS	59	39			It is suggested to include a definition of extra-tropical cyclone in the glossary. [Klaus Radunsky, Austria]	New entry introduced for "Extratropical cyclone"
TS-1285	TS	59	41	59	43	There is high confidence that the global number of extra-tropical cyclones is unlikely to decrease by more than a few percent due to global warming and that future changes in storms are likely to be small compared to natural interannual variability and substantial variations between model simulations of storms.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is unlikely that the global number of extra-tropical cyclones is to decrease by more than a few percent due to global warming and it is likely that future changes in storms will be small compared to natural interannual variability and substantial variations between model simulations of storms (high confidence). [Line van Kesteren, the Netherlands]	Reworded.
TS-1286	TS	59	41	59	43	There is high confidence that the global number of extra-tropical cyclones is unlikely to decrease by more than a few percent due to global warming and that future changes in storms are likely to be small compared to natural interannual variability and substantial variations between model simulations of storms.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is unlikely that the global number of extra-tropical cyclones is to decrease by more than a few percent due to global warming and it is likely that future changes in storms will be small compared to natural interannual variability and substantial variations between model simulations of storms (high confidence). [Line van Kesteren, the Netherlands]	Reworded.
TS-1287	TS	59	43	59	45	There is high confidence that a small poleward shift is likely in the Southern Hemisphere storm track, but the magnitude is model-dependent.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that there will be a small poleward shift in the Southern Hemisphere storm track, but the magnitude is model-dependent (high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1288	TS	59	43	59	45	There is high confidence that a small poleward shift is likely in the Southern Hemisphere storm track, but the magnitude is model-dependent.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is likely that there will be a small poleward shift in the Southern Hemisphere storm track, but the magnitude is model-dependent (high confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1289	TS	59	45	59	48	There is medium confidence that a poleward shift in the North Pacific storm track is more likely than not, and that storm activity over the North Atlantic is likely to increase along the storm track and extends farther downstream into Europe, and to decrease on both the north and south flanks, especially over the Mediterranean.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is more likely than not that there will be a poleward shift in the North Pacific storm track, and it is likley that storm activity over the North Atlantic is to increase along the storm track and extends farther downstream into Europe, and to decrease on both the north and south flanks, especially over the Mediterranean (medium confidence). [Line van Kesteren, the Netherlands]	The use of the uncertainty language has been improved.
TS-1290	TS	59	45	59	48	There is medium confidence that a poleward shift in the North Pacific storm track is more likely than not, and	The use of the uncertainty language has been

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						that storm activity over the North Atlantic is likely to increase along the storm track and extends farther downstream into Europe, and to decrease on both the north and south flanks, especially over the Mediterranean.' The use of the uncertainty language in this sence is different than in the rest of the report. Suggest to change the sentence into: It is more likely than not that there will be a poleward shift in the North Pacific storm track, and it is likley that storm activity over the North Atlantic is to increase along the storm track and extends farther downstream into Europe, and to decrease on both the north and south flanks, especially over the Mediterranean (medium confidence). [Line van Kesteren, the Netherlands]	improved.
TS-1291	TS	59	58	59	58	SREX=special report on... [Government of Germany]	Noted: The acronym has been expanded.
TS-1292	TS	60	3	60	6	Please explain "Extreme Value Theory" and "fraction of attributable risk" in the Glossary. [Government of Germany]	Taken into account. References to the relevant sections where these are described have been added after these sentences. However it was not deemed necessary to add these to the glossary as appropriate references are highlighted in the relevant sections of the underlying chapter text. Note that the the glossary is intended for terms used in multiple chapters.
TS-1293	TS	60	28	60	39	This whole paragraph does not contain uncertainty language. Suggest to add this where necessary. [Line van Kesteren, the Netherlands]	Accepted: Some uncertainty language has been added
TS-1294	TS	60	28	60	39	This whole paragraph does not contain uncertainty language. Suggest to add this where necessary. [Line van Kesteren, the Netherlands]	Accepted: Some uncertainty language has been added
TS-1295	TS	60	29	60	29	Given all the uncertainties in our field, showing such results to 3-figure precision, even with error bars, seems overdone--for these numbers and quite a number of others. I realize that some particular data set gives this result, but it would have uncertainties as well, and so it just all seems a bit too precise. [Michael MacCracken, United States of America]	Accepted: The numbers have been removed and uncertainty language has been used instead.
TS-1296	TS	60	29	60	29	For warming of warm days and nights replace 2.48±0.64 by 2.88 ±1.22 [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The numbers have been removed and uncertainty language has been used instead.
TS-1297	TS	60	37	60	39	The statement in this sentence comes from FAQ 2.2. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account: This text has been amended
TS-1298	TS	60	42	60	42	“[climate models] tend to overestimate (underestimate) the warming of warm (cold) extremes”. Please see my discussion above (comment #4-7) concerning this statement in chapter 9. I don't think that this statement can be justified with the few studies that exist which actually asses global trends in temperature extremes in observations and models. Preliminary results show that observed trends in cold and warm temperature extremes are simulated well in the CMIP5 ensemble on a global scale , but on regional scale (i.e. North America) there seem to be some deficits. [Jana Sillmann, Canada]	Accepted: This text has been amended based on the revisions to the Ch 9 text
TS-1299	TS	60	42		45	This doesn't make sense. Delete. [Nathan Gillett, Canada]	Noted: This sentence refers to the ability of downscaling to offer a credible representation of extremes and also to highlight the improvements between CMIP3 and CMIP5 which we believe are important. Therefore the sentence remains unchanged.
TS-1300	TS	60	56	60	56	"greater extremes"? [Government of Germany]	Noted: We are unsure what the comment means as the sentence does not refer to "greater extremes" but to a greater warming of extremes.
TS-1301	TS	61	5			TFE.9, Figure 1: Bottom two maps are unviewable because the hatching is so dense - suggest making it less dense. [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The figure and associated caption have been updated following changes to the underlying chapters.

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TS-1302	TS	61	6	61	10	Replace the first two sentences of the caption of TFE.9, Figure 1 by: Timeseries plots show global mean projections for the occurrence of extremes from CMIP5 for the RCP2.6, RCP4.5 and RCP8.5 scenarios relative to 1986–2005. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account: The figure and associated caption have been updated following changes to the underlying chapters.
TS-1303	TS	61	6	61	18	Please join descriptions for each plot. [Government of Germany]	Taken into account: The figure and associated caption have been updated following changes to the underlying chapters.
TS-1304	TS	61	35		36	Delete 'The temperature dependence (Clausius-Clapeyron relationship) of' and re-write 'Short duration precipitation extremes are likely to increase…'. The Clausius-Clapeyron equation predicts the rate of change of saturation vapour pressure with temperature. It does not predict changes in precipitation. [Nathan Gillett, Canada]	Taken into account: This paragraph has been rewritten following changes to the underlying Chapters.
TS-1305	TS	61	53	61	53	For the projections a reference to Chapter 12 is needed. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account: This specific reference is only considering near-term projections which relates to Chapter 11 and not Chapter 12. Where longer term projections are considered, reference has been made to the relevant section of Chapter 12.
TS-1306	TS	61	55	61	57	This statement overlooks the fact that some regions consistently display a drying (southern Europe, West Africa) or wetting (central North America, northwestern Australia) trend since the 1950s, as recently highlighted in the IPCC SREX (see also chapter 3 of that report). For this reason, the IPCC SREX assessed that there was _medium confidence_ that these regions had experienced enhanced drying, respectively enhanced wetting (see IPCC SREX SPM "There is medium confidence that some regions of the world have experienced more intense and longer droughts, in particular in southern Europe and West Africa, but in some regions droughts have become less frequent, less intense, or shorter, for example, in central North America and northwestern Australia"; see also Table 3.2 of IPCC SREX chapter 3, Seneviratne et al. 2012, for details for all SREX regions). These regional assessments are also consistent with more recent evidence (Sheffield et al. 2012, Seneviratne 2012). References: 1) Sheffield, J., E.F. Wood, and M. Roderick, 2012, Nature, 491, 435-438, doi:10.1038/nature11575; 2) Seneviratne, S.I, Nature, 491, 338-339; 3) Seneviratne, S.I., N. Nicholls, D. Easterling, C.M. Goodess, S. Kanae, J. Kossin, Y. Luo, J. Marengo, K. McInnes, M. Rahimi, M. Reichstein, A. Sorteberg, C. Vera, and X. Zhang, 2012: Changes in climate extremes and their impacts on the natural physical environment. In: Managing the Risks of Extreme Events and Disasters to Advance Climate Change Adaptation [Field, C.B., V. Barros, T.F. Stocker, D. Qin, D.J. Dokken, K.L. Ebi, M.D. Mastrandrea, K.J. Mach, G.-K. Plattner, S.K. Allen, M. Tignor, and P.M. Midgley (eds.)]. A Special Report of Working Groups I and II of the Intergovernmental Panel on Climate Change. [Sonia Seneviratne, Switzerland]	Taken into account: This section has been rewritten following updates to the underlying chapters to which it refers. However, given the compelling arguments in the literature both for and against a global scale trend in droughts, our assessment is somewhat different to SREX. That is we assign "low confidence" to global scale trends but assess that it is "likely" that some regions have experienced increases or decreases in drought. This does not disagree with the assessment of SREX but rather frames the assessment in a different light.
TS-1307	TS	61				Section TFE.9: TS-61 Confidence levels (medium, very high) regarding precipitation extremes are not found in Section 7.6 [Government of United States of America]	Taken into account: This section has been rewritten following updates to the underlying chapters to which it refers. The assessment is derived from many chapters. Assessments from Chapter 7 come from sections 7.2, 7.3 and 7.4.
TS-1308	TS	62	2	62	4	Sentence unclear. [Government of Germany]	Taken into account: This sentence has been rewritten accordingly.
TS-1309	TS	62	12	62	16	"Recent re-assessments of tropical cyclone data do not support the AR4 conclusions of an increase in the most intense tropical cyclones or an upward trend in the potential destructiveness of all storms since the 1970s. There is low confidence that any reported long-term changes are robust, after accounting for past changes in observing capabilities. However over the satellite era, increases in the intensity of the strongest storms in the Atlantic appear robust. {2.6.3}".The first and second parts of the sentence are inconsistent since the first part implies ~1970-2011 but the second part qualifies it to !1970-1990 (onset of the satellite era). [Andrew Glikson, Australia]	Taken into account: This section has been rewritten accordingly to incorporate IPCC uncertainty language. However the satellite era refers to the 1970s through to the present day rather than just the onset of the satellite era.
TS-1310	TS	62	12	62	16	"Recent re-assessments of tropical cyclone data do not support the AR4 conclusions of an increase in the most intense tropical cyclones or an upward trend in the potential destructiveness of all storms since the 1970s. There is low confidence that any reported long-term changes are robust, after accounting for past	Taken into account: This section has been rewritten accordingly to incorporate IPCC uncertainty language. However the satellite era refers to the 1970s through

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						changes in observing capabilities. However over the satellite era, increases in the intensity of the strongest storms in the Atlantic appear robust. {2.6.3}".The first and second parts of the sentence are inconsistent since the first part implies ~1970-2011 but the second part qualifies it to 1970-1990 (onset of the satellite era). [Government of Australia]	to the present day rather than just the onset of the satellite era.
TS-1311	TS	62	19			Delete 'but' and start a new sentence 'There is low confidence'. The two statements are not logically linked. [Nathan Gillett, Canada]	Accepted: This sentence has been amended accordingly.
TS-1312	TS	62	44	65	18	TS.6 Uncertainties. Please improve consistency and mention only lack of knowledge for each parameter, or provide context and also certain aspects for each parameter. Please reconsider each section with care as highlighting uncertainties out of context could decrease the credibility of other statements, where confidence is high, but which are not mentioned in this section. [Government of Germany]	Noted and partly accepted. The text has been modified
TS-1313	TS	62	48	62	48	Delete "only" in the first sentence. [Government of Germany]	Done
TS-1314	TS	62	48	62	50	This bullet needs information about time period. Also, it should probably include a clear statement that the stratosphere has cooled. [Dian Seidel, United States of America]	Noted: The statement is true for the period for which estimates are made in the chapter.
TS-1315	TS	62	48		50	The likelihood statements on the sign of tropospheric and stratospheric trends, and the confidence statements in their rates are not consistent in my view. If there is 'low confidence' in the rates of change, then how can we be 'virtually certain' that they are of one sign or another? I would suggest replacing 'medium to low confidence' with 'considerably uncertainty' and 'only low confidence' with 'considerable uncertainty'. Alternatively (less preferable) replace 'rate of change' with 'magnitude of the warming rate' and 'cooling rate' with 'magnitude of the cooling rate' (since this at least indicates that we do not have low/medium confidence in the sign of the rate). In the chapter, quote a broader range on the rates of temperature change, so that they can be associated with a higher confidence/likelihood level. [Nathan Gillett, Canada]	it is the case that confidence is high that the trends are increasing in the case of the troposohere and decreasing in the case of the stratosphere, but the rate of warming is less certaint.
TS-1316	TS	62				Section TS.6: Following on the previous comment, this section needs a sub-section on "Model Uncertainties". [David Webb, United Kingdom]	Noted
TS-1317	TS	63	1	63	2	"There continues to be insufficient evidence and thus low confidence for consistent trends in the magnitude or frequency of floods on a global scale. {2.6.2}". The Munich ReInsurance reports suggest an increase in a range of extreme weather events and catastrophes. http://www.munichre.com/en/reinsurance/business/non-life/georisks/natcatservice/great_natural_catastrophes.aspx [Andrew Glikson, Australia]	Noted
TS-1318	TS	63	1	63	2	"There continues to be insufficient evidence and thus low confidence for consistent trends in the magnitude or frequency of floods on a global scale. {2.6.2}". The Munich ReInsurance reports suggest an increase in a range of extreme weather events and catastrophes. http://www.munichre.com/en/reinsurance/business/non-life/georisks/natcatservice/great_natural_catastrophes.aspx [Government of Australia]	Noted
TS-1319	TS	63	4	63	6	"Not enough evidence exists at present to suggest anything else than low confidence in observed large scale trends in dryness (lack of rainfall), due to lack of direct observations, dependencies of inferred 6 trends on the index choice and geographical inconsistencies in the trends. {2.6.2}" The Munich ReInsurance reports suggest an increase in a range of extreme weather events and catastrophes. http://www.munichre.com/en/reinsurance/business/non-life/georisks/natcatservice/great_natural_catastrophes.aspx [Andrew Glikson, Australia]	Noted
TS-1320	TS	63	4	63	6	"Not enough evidence exists at present to suggest anything else than low confidence in observed large scale trends in dryness (lack of rainfall), due to lack of direct observations, dependencies of inferred 6 trends on the index choice and geographical inconsistencies in the trends. {2.6.2}" The Munich ReInsurance reports suggest an increase in a range of extreme weather events and catastrophes. http://www.munichre.com/en/reinsurance/business/non-life/georisks/natcatservice/great_natural_catastrophes.aspx [Government of Australia]	Noted
TS-1321	TS	63	9	63	10	The sentence starting with "However over the satellite ..." is not consistent with the other paras, where only uncertainties are mentioned. [Government of Germany]	The second statement has been removed.
TS-1322	TS	63	9	63	10	The sentence starting with "However over the satellite ..." is not consistent with the other paras, where only	The second statement has been removed.

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						uncertainties are mentioned. [Government of Germany]	
TS-1323	TS	63	18	63	18	Cross-reference should be 3.2.3. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted, reference changed to 3.2
TS-1324	TS	63	23	63	24	The sentence starting with "Towards the bottom..." is not consistent with the other paras, where only uncertainties are mentioned. [Government of Germany]	accepted, text has been modifed.
TS-1325	TS	63	26	63	27	The first sentence of the para is not consistent with the other paras, where only uncertainties are mentioned. [Government of Germany]	accepted, text has been modifed.
TS-1326	TS	63	32	63	32	What are "contrasting regions" [Government of Germany]	Text modified to remove phrase
TS-1327	TS	63	36	63	36	Change "seem to" to "may". [David Parker, United Kingdom of Great Britain & Northern Ireland]	Text has been removed
TS-1328	TS	63	36	63	39	"Time series of global glacier mass change seem to overestimate recent loss. On a global scale the mass loss from melting at calving fronts and iceberg calving are not yet comprehensively assessed. The largest uncertainty in estimated mass change from glaciers comes from the Antarctic periphery where most recent estimates show lower losses than previously estimated. {4.3.3}" How is this reconciled with Velicogna 2009 and Rignot et al. 2011, who states: "We find excellent agreement between the two techniques for absolute mass loss and acceleration of mass loss. In 2006, the Greenland and Antarctic ice sheets experienced a combined mass loss of 475 ± 158 Gt/yr, equivalent to 1.3 ± 0.4 mm/yr sea level rise. Notably, the acceleration in ice sheet loss over the last 18 years was 21.9 ± 1 Gt/yr2 for Greenland and 14.5 ± 2 Gt/yr2 for Antarctica, for a combined total of 36.3 ± 2 Gt/yr2. [Andrew Glikson, Australia]	Text has been removed
TS-1329	TS	63	36	63	39	"Time series of global glacier mass change seem to overestimate recent loss. On a global scale the mass loss from melting at calving fronts and iceberg calving are not yet comprehensively assessed. The largest uncertainty in estimated mass change from glaciers comes from the Antarctic periphery where most recent estimates show lower losses than previously estimated. {4.3.3}" How is this reconciled with Velicogna 2009 and Rignot et al. 2011, who states: "We find excellent agreement between the two techniques for absolute mass loss and acceleration of mass loss. In 2006, the Greenland and Antarctic ice sheets experienced a combined mass loss of 475 ± 158 Gt/yr, equivalent to 1.3 ± 0.4 mm/yr sea level rise. Notably, the acceleration in ice sheet loss over the last 18 years was 21.9 ± 1 Gt/yr2 for Greenland and 14.5 ± 2 Gt/yr2 for Antarctica, for a combined total of 36.3 ± 2 Gt/yr2. [Government of Australia]	see TS-1328
TS-1330	TS	63	36			How do we know that the timeseries of glacier mass change overestimate loss? Are there independent observations we can verify them against. [Nathan Gillett, Canada]	Sentence has been deleted
TS-1331	TS	63	41	63	43	High confidence (please put in italics) altough obs are poor? Not logical. [Government of Germany]	Text for bullet is significantly altered - no longer relevant
TS-1332	TS	63	50			The solar activity (past and future) should still be listed as a key uncertainty among the Drivers of Climate Change. [Terje Wahl, Norway]	Noted. However, chapter 8 found that this forcing is very small, so that despite a large relative uncertainty it still makes a small contribution to the overall uncertainty of the Drivers.
TS-1333	TS	63	51	63	51	dwarf' - would a more neutrally toned word be more appropriate. [Government of United Kingdom of Great Britain & Northern Ireland]	Editorial. This text was removed during revisions, so no longer applicable.
TS-1334	TS	63	51	63	53	This doesnt seem to be an uncertainty, so I wondered why it was included here. [Keith Shine, United Kingdom of Great Britain and Northern Ireland]	Editorial. This text was removed during revisions, so no longer applicable.
TS-1335	TS	63	51			The phrasing here implies that the forcing at the end of the RCP is a projection, when actually that was a predetermined target. It would be more accurate to state that, "The forcing at the end of the century in all four RCP scenarios dwarfs natural forcing" or a statement along the lines of "sustained natural forcing variations are expected to be in the range of -1.5 to +0.5 W/m2, much smaller than the forcing at the end of the century from any of the four RCP scenarios.". Moreover, the RCPs, like the SRES scenarios before them, do not (yet) have any probability estimates associated with them. While it may seem unlikely that even in a future with policy that forcing will be below RCP2.6, that's an economics statement for WGIII not a science statement for WGI. [Government of United States of America]	Editorial. This text was removed during revisions, so no longer applicable.

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TS-1336	TS	63	52	63	52	Too subjective - quantify. [Jeffrey Obbard, Singapore]	Editorial. This text was removed during revisions, so no longer applicable.
TS-1337	TS	64	2	64	4	The discussion on feedbacks should include surface albedo feedback. [Government of Canada]	Editorial. This text was reduced to discussin only one feedback (clouds), so it is now clear that this is not meant to be a comprehensive discussion.
TS-1338	TS	64	6	64	6	Shouldn't there be something as to what needs to be better understood, e.g. methane? [Dora Marinova, Australia]	Noted, however this is addressed in the underlying chapters (ch 6 in the case of methane) and space is too limited to cover this in this portion of the TS.
TS-1339	TS	64	13		14	limits in process understanding should be exemplified. [European Union]	Noted. Limits in process understanding is one of the key uncertainties we highlight here.
TS-1340	TS	64	15	64	15	Why the use of more in “more difficult to attribute” [Ned Dwyer, Ireland]	Taken into account. Bullet point revised.
TS-1341	TS	64	19			Replace 'natural variability' with 'internal variability'. [Nathan Gillett, Canada]	Accepted.
TS-1342	TS	64	28			Delete 'Increased understanding of the'. It isn't our increased understanding of the uncertainties which makes the assessment of causes of trends in the upper troposphere less confident, it is the uncertainties themselves. [Nathan Gillett, Canada]	Noted. Bullet point dropped.
TS-1343	TS	64	34	64	34	After "due to" insert "regioanlly-dependent decadal variability and" [David Parker, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Bullet point completely re-written.
TS-1344	TS	64	41	64	44	Given the spread of climate sensitivities and the problems in validating the models against almost any field except surface temperatures (used to tune the models) I am surprised that you have as much as medium confidence in any part of the predictions - other than that which would come out of a simple model of the effect of enhanced green house gases on the radiation budget. [David Webb, United Kingdom]	noted, and recall these are key uncertainties
TS-1345	TS	64	41			Replace 'predictability of yearly to decadal averages of temperature' with 'predictability of temperature for 1-10 years based on observed initial conditions'. Need to flag to non-specialists that we are describing initial value predictions. [Nathan Gillett, Canada]	these include both initialized and uninitialized simulations
TS-1346	TS	64	52	64	52	The term "robustness" should be defined quantitatively if possible. [Government of Canada]	this statement has been deleted
TS-1347	TS	65	2	65	4	see comment on 12.5.5 below - given that many models produce the wrong mean AMOC excluding the AMOC from the low confidence statement seems odd… [Meric Srokosz, United Kingdom of Great Britain & Northern Ireland]	this statement has been re-formulated
TS-1348	TS	65	2		4	This is not a useful statement as it leaves open the door to any abrupt or nonlinear change happening with unquantified probability. Better to list specific types of abrupt change. Then express probability of their occurrence. If for specific events there is low confidence in assessments of their likelihood of occurence, then say this. But for most events I have seen assessed in the report, the probabilities of occurence are low. Alternatively delete this bullet completely. [Nathan Gillett, Canada]	this statement has been re-formulated
TS-1349	TS	65	2			How about, "There is ??? Confidence regarding a change in the AMOC in the 21st century. For other abrupt or nonlinear changes there is generally low confidence…" (otherwise this statement leaves the open question of just what the confidence is in the AMOC changes). [Government of United States of America]	this statement has been re-formulated
TS-1350	TS	65	4	65	4	Change "excluded to occur" to "ruled out". [David Parker, United Kingdom of Great Britain & Northern Ireland]	this statement has been re-formulated
TS-1351	TS	65	7	65	7	There is limited confidence in future methane emissions from natural sources due to changes in wetlands and gas hydrate release from the sea floor. Add “the magnitude of” between in and future? [Ned Dwyer, Ireland]	this statement has been re-formulated

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TS-1352	TS	65	15	65	16	Suggest to change this sentence ”There is low confidence in projections in many aspects of climate phenomena, including changes in the amplitude and spatial pattern of modes of climate variability. {9.5.3, 14.2–14.6}”to "There is low confidence in projections in SOME aspects of climate phenomena IN REGIONAL SCALE,including…." [Lei Huang, China]	this statement has been re-formulated
TS-1353	TS	66	3	66	4	Table TS.1: It is important to include, right in the Table title, the information that these projections are relative to 1986-2005. Somewhere, in the title text or the caption, the information should be provided to add ~0.6ºC to these ranges to get values relative to pre-industrial. [Government of Canada]	Accept. This information is now included in footnote (a).
TS-1354	TS	66	7			Tab. TS1: Further explanation is needed on the "format". This seems to be the likelihood range used, not the format? Please explain. [Government of Germany]	Accept. The table has been revised and clarified. The information about the ranges provided is now explictly given in the footnotes to the table.
TS-1355	TS	66	7			Tab. TS1: Further explanation is needed on the "format". What does the number format "mean" mean? Is it the arithmetic average of all model values at each grid cell? Or have they been averaged before? Please describe or give ref to the arithmetic method used. Please explain the choice of the interval boundaries for the ranges, and the for associated uncertainty, if possible in the text. At least provide references where this information can be found in text. Are the choices comparable to AR4? [Government of Germany]	Accept. The table has been revised and clarified. The information about the ranges provided is now explictly given in the footnotes to the table.
TS-1356	TS	66	7			Tab. TS1: Again: the reference period. Policy makers will add 0,8 °C to all the values, indicated in AR4 as the current global mean T-increase. If you wish to avoid mistakes from such simple calculations, please provide more information, it is needed. [Government of Germany]	Accept. This information for the standard reference period, and additional reference periods to allow comparison with AR4, are included in footnote (a).
TS-1357	TS	66	11			Please explain the sentence in regards to the meaning of and are treated as having uniform propability distributions, and explain, if the assumption is justified. [Government of Germany]	The explanations given in the footnote have been expanded. More details can be found in Chapter 13, Table 13.5.
TS-1358	TS	66				Tab. TS1: Please add a horizontal line between DeltaT and SLR, and vertical lines around one time period and its associated uncertainties each. [Government of Germany]	Accept. Actually these lines were there in the doc, but were lost in the conversion to pdf.
TS-1359	TS	66				Tab. TS1: Please replace "Level of Confidence" by "Level of Uncertainty", because both likelihood and confidence levels are given. [Government of Germany]	Accept. The header now says "likely range".
TS-1360	TS	66				Table TS.1: Be specific how the likley uncertainty ranges were derived. Looking at Table 12.2, it seems that the multi-model mean is used plus two times the standard deviation. Clarify in the footnote. [Government of Germany]	Accept. The footnote has been expanded to clarify how the likely ranges are determined. Details can be found in Chapter 12.
TS-1361	TS	66				Table TS.1. Add a footnote explaining that the warming and sea level rise in RCP6.0 by 2046-65 are less than those in RCP4.5 owing to smaller anthropogenic forcings at that stage, as noted in Chapter 12. Otherwise readers may be mystified! [David Parker, United Kingdom of Great Britain & Northern Ireland]	Reject. We don't think this needs to be specifically highlighted as it can be seen from Figure TS.15.
TS-1362	TS	67	5	67	6	TFE.1 TABLE 1: perhaps need explanation as to how there could be higher confidence (i.e. medium) over RCP projections to the end of the 21st century compared with next few decades ('low confidence') as some may expect the future to become less predictable, the further forward we project [Government of United Kingdom of Great Britain & Northern Ireland]	Taken into account: The explanation of how uncertainty language is used throughout the IPCC WGI report clarifies how there might be differences between the assessments of near-term and longer-term projections. The use of uncertainty language is outlined in many places throughout the main body of the report and is repeated in each chapter to reinforce how the assessment is carried out.
TS-1363	TS	67	5	67	6	"Increases in frequency and/or intensity of drought" is "Not assessed" when it comes to "Likelihood of future trends based on projections for the next few decades" is a miss opportunity to inform policy makers of what we do and do not know. Even an assessment of 'Low confidence' would be of more value than "Not assessed". If IPCC AR5 is "virtually certain that changes in average precipitation in a much warmer world will not be uniform, with regions experiencing increases, or decreases or no significant change", then a statement on the dominant role of natural variability is equally informative [Robert Webb, United States of America]	Taken into account: The assessment in the table is based on the underlying chapters and the available literature. While we agree that such a statement on near-term changes in drought would be informative for policymakers, in Chapter 11 there was insufficient literature to make an appropriate assessment and therefore the "not assessed" statement stands.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-1364	TS	67	5	67	6	The absence of an assessment for coastal inundation in this table on extreme weather and climate events is a step backwards from the AR4 inclusion of an assessment of "increased incidence of extreme high sea level". Chapter 13 Executive Summary on page 5 states "It is very likely that there will be an increase in the occurrence of future extreme sea level and flooding events." [Robert Webb, United States of America]	Accepted: An assessment of extreme high sea level has been added to the table.
TS-1365	TS	67	5	67	6	The absence of an assessment for "floods" in this table on extreme weather and climate events is a miss opportunity to inform policy makers of what we do and do not know. [Robert Webb, United States of America]	Noted: The assessment of floods falls much more within the scope of WGII and therefore is not covered in detail in WGI. WGII will assess floods in more regional detail accounting for the fact that trends in floods are strongly influenced by changes in river management and not solely driven by changes in climate.
TS-1366	TS	67				Table 9.1. Is there an inconsistency between the 'medium confidence' in the observed trend for warm spells, and the 'likely' assessment for an anthropogenic contribution? [Nathan Gillett, Canada]	Taken into account: The medium confidence statement for observations is now qualified as is the "likely" attribution statement. In Chapter 2 it is clear, that the reason for medium confidence is due in large part to lack of data in Africa and South America but also to different formulations of warm spells/heat waves depending on region and application. The "likely" attribution statement is now qualified as "that human influence has substantially increased the probability of some observed heatwaves in some locations".
TS-1367	TS	67				Tab. TFE.9 T1: Please replace "Likelihood" by "Level of Uncertainty", because both likelihood and confidence levels are given. "Level of Uncertainty could be mentioned just once, not in each column. The # in the column on the next decades is not needed, this could be mentioned in the caption. [Government of Germany]	Taken into account: The caption and table have been amended accordingly where appropriate.
TS-1368	TS	67				Tab. TFE.9 T1: Is the column four on the next decades also based on the RCPs? [Government of Germany]	Taken into account: The caption now qualifies how projections have been presented.
TS-1369	TS	67				Tab. TFE.9 T1: line 2 and 3 both mention warmer days/nights over land. Is this a duplication? [Government of Germany]	Taken into account: Row 2 refers to "Warmer and/or fewer cold days and nights" and Row 3 refers to "Warmer and/or more frequent hot days and nights". Thus they are referring to the cold and warm ends of the temperature distribution respectively and are therefore not duplications.
TS-1370	TS	69	1	69	1	Use the same verticale scale for LSAT and SST so that the differences are clearly visible and explain these differences in Chapter 2. [Marcel Crok, The Netherlands]	Plotting suggestion is editorial. The relative rates of change of LSAT and SST are discussed in Chapter 2.
TS-1371	TS	69	1	69	1	Showing snow cover for only March/April is slightly misleading. Over the whole year there is no trend in snow cover in the last 30 years. In October there is an increasing trend. The same criticism could be given for September sea ice but in this case most people are known with this dataset and most people know that sea ice trends declined for the whole year as well so in that case showing only September as the worst case is acceptable. [Marcel Crok, The Netherlands]	March/April is most commonly used in the literature.
TS-1372	TS	69	1	69	10	In the figure TS.1,it would be helpful to define which data set each color represents for clarity [Government of Kenya]	Details are in the Appendix to Chapter 2. To indicate the specific data sets for each line would make the plots extremely complex without adding anything to the evidence.
TS-1373	TS	69	1	69	11	Fig. 1: Reference period for the various graphs should be explained- if possible, change reference period to "preindustrial", or at least make it possible for the reader to get an idea on how much change has occurred since the preindustrial era. [Government of Germany]	Reference periods are given in the suporting material. It would not be possible to use the preindustrial era as the referenc period, since most all of these observations are not available then.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-1374	TS	69				Figure-TS1: - explain the meaning of the grey shading - give reference to the data sets plotted - mark reference period in each panel [Barbara Früh, Germany]	Shading is explained. Other suggested changes would make the plot very busy. The information is give in the appendix to Chapter 2.
TS-1375	TS	69				Fig. TS.1: vertical lines each 50 years would be helpful [Government of Germany]	Editorial
TS-1376	TS	69				Fig. TS.1: What is the "common reference period"? [Government of Germany]	It varies from variable to variable, depending on the length of record. The reference periods are given in the Appendix to Chapter 2. For many of the longer records it is 1961-1990. for others it is a later and shorter reference period. For example, ocean heat content is 1993-2009.
TS-1377	TS	69				Fig. TS.1: What is the difference between sea surface and marine air? [Government of Germany]	One is a water temperature and one is an air temperature
TS-1378	TS	69				Fig. TS.1: how is Troposphere defined for tropospheric temperature? the vertical mean from surface to tropopause? Not obvious for non-experts, please extend caption. [Government of Germany]	There is no simple answer to this question since the various data sets come from both satellite and radiosonde estimates. One would have to go to the data source references given in the Appendix to Chapter 2 and investigate. In general, though, it is an attempt to estimate the average temperature of the air between the surface and the tropopause.
TS-1379	TS	69				Figure TS.1. Marine air temperature should include HadNMAT2 (Kent et al., 2012, accepted subject to revision) as commented in review of Chapter 2. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted
TS-1380	TS	69				gray area in figures should be explained [Petra Seibert, Austria]	Done
TS-1381	TS	69				Fig. TS.1 should probably include a time series of stratospheric temperature, as it is included in the SPM in Fig. SPM.1. [Dian Seidel, United States of America]	It was decided not to include it
TS-1382	TS	70				Please explain the meaning of hatching and stippling areas in Fig. 1 [Government of Spain]	Done
TS-1383	TS	70				Fig. TS.2 legend should explain the cross hatching and dots. [Dian Seidel, United States of America]	Done
TS-1384	TS	71	0	99		General comments on figures- the text on the figs seem to be at an earlier stage of completeness to the main text. Many of the captions do not refer adequately to the figures themselves particularly where there are multiple panels, some are labelled a, b, c etc while the captions refer to upper middle lower panels. Also many abbreviations in the legends are not clear or are differently/inadequately explained in the caption. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted; updating figures, captions and how they are embedded in the text has been a focus of the revisions for the Final Draft of the TS
TS-1385	TS	71	0			The axis label to panel b) 'SLE' is not explained in the caption [Government of United Kingdom of Great Britain & Northern Ireland]	Figure modified
TS-1386	TS	71	1	71	15	this would be better as a column of three figures with the same aspect ratio. It is odd to distort equivalent aspect ratios like this. [Mark Siddall, United Kingdom]	Figure modified in line with comment
TS-1387	TS	71	4	71	13	Fig. TFE.2 F1: The caption of this figure should help non-experts to understand the figure. Usually these do not know, what altimeters and tide gauges are, and why their results differ that much. This needs to be explained either in the text or in the caption please (at least in the Glossary). An explanation is also needed for "additional terms". [Government of Germany]	"Altimetry" and "Tide gauge" are in the Glossary. "tide gauge" is no longer used, "satellite altimetry " is to a large degree self explanatory.
TS-1388	TS	71	8			"set to have the same…":does it mean that there is a bias correction applied? [Barbara Früh, Germany]	Figure and caption text have been completely revised. Text questioned no longer appears.
TS-1389	TS	71	8			What is meant by "likely range"? [Government of Germany]	Figure and caption text have been completely revised.

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							Text questioned no longer appears.
TS-1390	TS	71	9	71	10	remove "s" from "contributions" [Government of United Kingdom of Great Britain & Northern Ireland]	Figure and caption text have been completely revised. Text questioned no longer appears.
TS-1391	TS	71	11			"very high confidence" Why is this not certain? [Government of United Kingdom of Great Britain & Northern Ireland]	Comment appears to be misplaced, very high confidence can not be found at the specified location - no change
TS-1392	TS	71	12		13	The inclusion of a term due to unforced climate variability in the model projections seems to be a kludge. Better not to include this term, and only include the response to known forcings (adding in ice sheet contributions and preindustrial volcanic forcing correction seems reasonable). [Nathan Gillett, Canada]	This term arises when the Marzeion et al. glacier estimate includes forcing by the observed climate. It is thus in the estimate of the "observed" glacier mass changes.
TS-1393	TS	71				Figure-TFE-2a: what is "sum of the contributions"? It could not even be the mean of those plotted since the red line lies below all others in about 1998 [Barbara Früh, Germany]	Figure and caption text have been completely revised, and clarified.
TS-1394	TS	71				Fig. TFE.2 F1: The axes of Figs a) and b) should have identical maxima of and be labelled identically. [Government of Germany]	Figure and caption text have been completely revised, and clarified.
TS-1395	TS	71				Fig. TFE.2 F1: Fig c) What does the gray, dashed, horizontal line in mean? And what are the gray lines? [Government of Germany]	Figure and caption text have been completely revised, and clarified.
TS-1396	TS	71				Fig. TFE.2 F1: Fig a) What does "sum of components (legend) or contributions (caption)" mean? [Government of Germany]	Figure and caption text have been completely revised, and clarified.
TS-1397	TS	71				Fig. TFE.2 F1: Fig a) What do the dotted lines mean? [Government of Germany]	Figure and caption text have been completely revised, and clarified.
TS-1398	TS	71				Fig. TFE.2 F1: The usefulness of having both Fig a) and b) is not obvious. There is no difference in the scenarios in b) and the curves for altimeter and tide gauges are the same in a) and b). It is suggested to only one of these Figs. [Government of Germany]	Figure and caption text have been completely revised, and clarified.
TS-1399	TS	71				Fig. TS.2, nice color scheme! [Dian Seidel, United States of America]	Thank you.
TS-1400	TS	72				Figure-TS-2: "coal" is hardly visible [Barbara Früh, Germany]	noted. This should be more visible in high resolution version
TS-1401	TS	72				Figure is misleading; should also show land-use CO2 emissions going back to 1750. [Stephen E Schwartz, United States of America]	rejected. This figure is designed to only show fossil fuels
TS-1402	TS	73	0			The caption does not match to the figure at the moment, not all of the time-series are labelled, The delta notation for isotope ratio is used on the axis label but not in the caption. ESTOC is on the fig but not in the caption. [Government of United Kingdom of Great Britain & Northern Ireland]	taken into account. This figure has been substantially revised.
TS-1403	TS	73				Figure-TS-3: the stations cannot be assigned to the lines in the plot [Barbara Früh, Germany]	taken into account. This figure has been substantially revised.
TS-1404	TS	73				Figure TS.3. First the lines in the plots are not all clearly identified. Better to include a key in each panel. Also some of the stations metioned in the caption don't seem to be on the plot. [Nathan Gillett, Canada]	taken into account. This figure has been substantially revised.
TS-1405	TS	73				Fig. TS.3: Please add labels to the x-axis of all graphs, even if they are the same, to improve readibility. The letters are too of legends and labels are too small. Please improve colors, they are too similar. If there are lines in one graph, that pertain to different y-axis, please group them by similar line types, to help the reader identify what is presented. Numbering the figures from a) to d) would also help referring to them in the text. [Government of Germany]	taken into account. This figure has been substantially revised.
TS-1406	TS	73				Fig. TS.3: Numbering the figures from a) to b) would also help referring to them in the caption and in the text. [Government of Germany]	taken into account. This figure has been substantially revised.

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TS-1407	TS	73				Figure TS.3 would benefit from legends in each panel rather than just the 2nd panel as at present. [David Parker, United Kingdom of Great Britain & Northern Ireland]	taken into account. This figure has been substantially revised.
TS-1408	TS	73				Figure TS. 3 Why does the O2 concentration (ppm) start around 1900 with -30 ppm, why the negative number? [Line van Kesteren, the Netherlands]	taken into account. This figure has been substantially revised.
TS-1409	TS	73				Figure TS. 3 Why does the O2 concentration (ppm) start around 1900 with -30 ppm, why the negative number? [Line van Kesteren, the Netherlands]	taken into account. This figure has been substantially revised.
TS-1410	TS	74	0			Figure TS.4: Suggest adding to this Figure a column to indicate the RF values (so readers don't have to estimate them from the graph) and to consider adding information about the confidence or level of understanding about each of the forcings as was done for the comparable Figure in the AR4 WGI Technical Summary (Fig TS.5). [Government of Canada]	Accepted (in part). A column showing confidence levels has been added. We believe that also addint the numbers would make the figure too complex, but point out that these are in ch 8 so they do not need to be estimated from the graph.
TS-1411	TS	74	1	74	10	In the Figure TS.4.Top: in the Legend it is indicated the following: "Forcing by concentration between 1750 and 2010". However, in the upper part of the Top figure it is written the following: "Radiative forcing of climate between 1750 and 2011". Please, verify. Also in this Top Figure, at the level of "Contrails", there is a horizontal green segment indicating the error bar and a point ("green diamond with associated uncertainty") with an indication "AR4 estimates", that needs to be verified since it is two high (the corresponding mean value is as high as the CO2 contribution, about 1.8 Wm-2). [Rubén D Piacentini, Argentina]	Accepted. 2010 has been corrected to 2011. The AR4 estimate on the figure is a legend referrring to all the green AR4 estimates, as also described in the caption, not AR4 contrails. We have improved the figure to clarify.
TS-1412	TS	74	5			Please consider having this graph professionally rendered. We can do far better in aesthetically and effectively displaying this information. This graph potentially will be seen and displayed by many thousands of scientists and policy makers over the next 6-7 years, which is motivation to make it the best possible. [David Fahey, United States of America]	Accepted. We have improved the graphics.
TS-1413	TS	74		74		This is a useful figure, but it contains too much information for use in a presentation to non-specialists. I suggest adding a simplified version that we can use in our talks to general audiences. One simplification would be to include only adjusted forcing, as the difference between RF and AF is a technical detail that will just confuse general audiences. Another simplification is to omit the comparison with AR4; again, that's not a detail that matters in a general talk. If we want to discuss the changes from AR4 to AR5, we can always use the original figure. [John Ogren, United States of America]	Accepted. We have simplified the figure by showing only ERF in many cases (e.g. for WMGHG, in the lower panel).
TS-1414	TS	74				Figure TS.4: Please specify if AR4 estimates have been corrected for different reference years (2005, AR4 vs 2011, AR5). [Andrew Ferrone, Germany]	Rejected. The AR4 values were not altered, which we believe is implied as is (i.e. by not stating that they are).
TS-1415	TS	74				Figure-TS-4: in title "2011" but in the caption (line 6) "2010" - last sentence should be moved before "Bottom", i.e., from line 9 to line 7 - the green uncertainty range is hardly visible on the blue and red bars (dyschromatopsia) [Barbara Früh, Germany]	Accepted. 2010 has been corrected to 2011. The caption has also been revised. We have darkened the green to try to make it clearer, and used a smaller symbol to distinguish on the basis of something other than color.
TS-1416	TS	74				How can Aerosol aci have a solid bar corresponding to RF? I thought RF was defined as the radiative repsonse without any tropospheric adjustments. But the response of clouds is inherent in aci. I must be missing something here, even though I have also read the whole of chapter 8. Clarify. [Nathan Gillett, Canada]	Accepted. ACI now has only one bar (ERF).
TS-1417	TS	74				Fig. TS.4: Numbering the figures from a) to d) would also help referring to them in the caption and in the text. [Government of Germany]	Noted. There are only 2 panels though, not 4, and we believe top and bottom clearly distinguishes those.
TS-1418	TS	74				Fig. TS.4: Please remove the green line labelled AR4 estimates from the upper graph, this is confusing. Instead, explain lines in the caption. [Government of Germany]	Rejected. We do explain the green lines in the caption, but feel it's also useful to have the legend on the figure.
TS-1419	TS	74				Fig. TS.4: Upper Fig: The word "Natural" should not overlap on "total anthropogenic" [Government of Germany]	Rejected. This is centered on the solar forcing, and we feel this is clear to readers.

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TS-1420	TS	74				Fig. TS.4: Lower Fig: WMGHG instead of GHG as in caption, explain WMGHG [Government of Germany]	Accepted. WMGHG revised to GHG in caption to match lower panel.
TS-1421	TS	74				Fig. TS.4: Here the reference is pre-industrial conditions, which is much appreciated. [Government of Germany]	Thank you.
TS-1422	TS	74				Fig. TS.4: Lower Fig: legend for AR4 should not contain a minus sign, but a komma [Government of Germany]	Accepted. Revised.
TS-1423	TS	74				Fig TS.4 contains an entry for contrails but not for cirrus or other clouds. I do not think this is a good balance. Certainly cirrus are more important for radiative forcing than contrails. Bottom panel: cannot find ArI and AcI as mentioned in the caption. [Rolf Müller, Germany]	Rejected (point 1), accepted (point 2). Contrails are a forcing while the others are feedbacks and so not appropriate here. Text on ari and aci removed.
TS-1424	TS	74				The top headline "Radiative forcing of climate between 1750 and 2011" is somewhat misleading because it only shows the difference between the RF at the start and end of the interval, not the history of the RF during the period. Because of the ocean's slow response to radiative forcing, a better headline would be "Radiative forcing: 2011 vs. 1750". [Terje Wahl, Norway]	Rejected. This description is used throughout AR5 with community approval.
TS-1425	TS	75	1	75	8	In the upper part of Figure TS.5: "Well Mixed GHG", the horizontal histogram corresponding to CH4, actually has different colors and not only that of CH4. So, this label needs to be generalized incorporating the other gases. [Rubén D Piacentini, Argentina]	Rejected. Other compounds respond to methane emissions, so multiple colors are correct.
TS-1426	TS	75	4			Please consider having this graph professionally rendered. We can do far better in aesthetically and effectively displaying this information. This graph potentially will be seen and displayed by many thousands of scientists and policy makers over the next 6-7 years, which is motivation to make it the best possible. [David Fahey, United States of America]	Accepted. We have improved the graphics.
TS-1427	TS	75	4			This graph is highly inconsistent with the description of black carbon forcing on TS-18 ln 16. Repeat of earlier comment on TS-18 ln 27: When readers consult AR5 to learn what the forcing of black carbon is in the global atmosphere they will be disappointed and/or confused, starting with these two sentences that offer 2 equations and 3 unknowns. Further, black carbon from biomass burning is erased from the accounting here (Chapter 7) apparently because its RF is 'cancelled' by co-emissions of organic carbon. This is highly misleading. The revised Bond et al. 2012 now cites black carbon as the 2nd largest anthropogenic forcing term with approx equal contributions from the 3 sources cited above (I am a coauthor). This section should posit that black carbon has 3 principal source terms that need to be evaluated (along with pre-industrial emissions) to understand its role in the atmosphere and ultimately its forcing contribution, and then discuss what we know about these terms separately and combined. [David Fahey, United States of America]	Accepted. The BC term is now cleare to readers and consistent with the more complete discussion of contributions of different BC emission sources in ch 7.
TS-1428	TS	75				Figure TS.5: The vertical error bars in this graph do not seem to make sense. [Andrew Ferrone, Germany]	Accepted. The caption now explains these more clearly.
TS-1429	TS	75				Figure-TS-5: meaning of biomass burning arrow? [Barbara Früh, Germany]	Noted. The visual distinction between the sections of the BC and OC bars and their labeling has been clarified.
TS-1430	TS	75				Why are the white boxes included for Biomass burning aerosols? Second the vertical error bars for uncertainties in individual components are confusing. There is no vertical scale to interpret the error bars (presumably it's the same as the horizontal scale). How about using thin coloured uncertainty bars just above the large bars to indicate uncertainties in components? [Nathan Gillett, Canada]	Accepted. The white boxes have been replaced. The vertical bars are now explained more clearly in the caption.
TS-1431	TS	75				Fig. TS.5: Please use the same colors as for Fig. TS.4., as far as possible (e.g., CO2) [Government of Germany]	Noted. These figures are independent and convey different information, so while we are sypathetic to the consistency idea we do not feel it is key in this instance.
TS-1432	TS	75				Fig. TS.5: What does the little arrows for carbon aerosol mean? [Government of Germany]	Noted. The visual distinction between the sections of the BC and OC bars and their labeling has been clarified.
TS-1433	TS	75				Figure TS.5: Like in Fig TS.4, why no entry for cirrus/clouds when contrails are shown? [Rolf Müller, Germany]	Rejected. Contrails are a forcing while the others are

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							feedbacks and so not appropriate here.
TS-1434	TS	76	3			The colored areas in the lower panel of the figure are quite unclear, there are too many colors and sectors. Could you reshape the figure. Also the figure caption does not explicitely refer to the lower panel. [Ilkka Savolainen, Finland]	Accepted. Both panels revised.
TS-1435	TS	76	13	76	13	Figure TS caption: replace Figure 8.32 by Figure 8.33 [Michel Petit, France]	Accepted. Revised.
TS-1436	TS	76	13			Figure TS caption: replace Figure 8.32 by Figure 8.33 [Government of France]	Accepted. Revised.
TS-1437	TS	76				Figure TS.6: Given that some of the colours in TS.6b are similar, it would help if the order to the categories in the legend matched the order in which they appear in the Figure. [Government of Canada]	Accepted. Revised.
TS-1438	TS	76				Fig. TS.6: please move legend out of upper graph,. [Government of Germany]	Rejected. There is plenty of blank space within the graph, so it seems more efficient to include the legend there.
TS-1439	TS	76				Fig. TS.6: Why is 50 yr only shown for GTP? There should be the same information for both metrics. It would be easier to have GWP and GTP next to each other for each time horizon. [Government of Germany]	Accepted. Timescales harmonized.
TS-1440	TS	76				Fig. TS.6: upper graph: SO2 or SO4? and what kind of aerosol forcing has been considered? [Government of Germany]	Noted. Emissions are almost all SO2, so this is correct. The upper plot is Rfari, the lower adds in Rfaci, as stated in the caption.
TS-1441	TS	76				Fig. TS.6: lower graph: only for GTP? Putting this graph here suggests that GTP is more suitable to assess the effects of different forcing agents. Please be more balanced and provide a corresponding figure also for GWP, even though the units would not be the same, the visual impression would be helpful. [Government of Germany]	Rejected. There is extensive discussion in AR5 (e.g. ch 8) about how no single metric is appropriate for all impacts and thus none can be called 'better' or 'more suitable' in a general sense. The lower panel of this figure is an example illustration, not an endorsement of a particular metric, and it's part of an already very complex, full figure with both GWP and GTP in the uppor portion fo multiple time horizons along with all the various sectors in the lower portion so we believe that adding another complex panel would make this figure overly busy.
TS-1442	TS	76				Fig. TS.6: lower graph: strange changes in colors at DT=0.What is the unit of the y-axes (1mK=0.001K)? [Government of Germany]	Accepted. The colors and y-axis label are revised.
TS-1443	TS	76				Fig. TS.6: Numbering the figures from a) to d) would also help referring to them in the caption and in the text. Please improve readibility of the caption. [Government of Germany]	Noted. There are only 2 panels though, not 4, and upper and bottom seems clear enough.
TS-1444	TS	77	0			The caption refers to the panels starting with bottom/c) first. Should be ordered a,b,c. [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted.
TS-1445	TS	77	1	77	2	As usual you omit the two most important contributors to the supposed temperature ris. The are the ocen oscillations , notable ENSO, and the effect of urban and land use changen which you have underestimated [Vincent Gray, New Zealand]	Rejected. These effects are included in the analyses and simulations shown in the figure.
TS-1446	TS	77	10	77	11	Fig. TS.7: Confusing information about the calculation of anomalies, please clarify. [Government of Germany]	Noted. However no changes are made because it is necessary to specify the method used to calculate the anomaliies and it is unclear where the difficulty lies with the current text..
TS-1447	TS	77				Figure is misleading in that it does not (also) show temperature (as opposed to temperature anomaly) from the several studies; should show in separate panel to show how much temperature spread there is among models; and include actual GMST in figure. . Caption refers to this as temperature. It is _anomaly_. That needs to be explicitly stated. But essential also to	Noted. Ch09 shows the time-mean GMST over the reference period 1961-1990, to bring out what the reviewer requests. However, the primary observational quantity related to GMST is the anomaly, not the absolute, owing to the much more

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						plot temperature out of the models, not temperature anomaly, and compare to actual GMST. See Tredger E (2009) On the evaluation of uncertainty in climate models. PhD thesis, London School of Economics, London http://cats.lse.ac.uk/homepages/edward/TREDGER_Thesis.pdf; Figure 3.1 p. 71. Also Stevens B. and Schwartz S. E.: Observing and Modeling Earth's Energy Flows. Surveys Geophys. 33 779-816 (2012). DOI 10.1007/s10712-012-9184-0 Figure 11. Also Mauritsen, T., et al. (2012), Tuning the climate of a global model, J. Adv. Model. Earth Syst., 4, M00A01, doi:10.1029/2012MS000154. These figures show that the spread in GMST of AR4 models greatly exceeds the change in GMST over the twentieth century and indeed over expected temperature change in the 21st century, about 3 K This would be expected to have major effects on ice lines, vegetation, etc, and ultimately in climate response to forcing. So it is misleading to present only temperature anomaly and not temperature itself. The departures of modeled temperature from observations and its implications must be shown and discussed. [Stephen E Schwartz, United States of America]	robust construction of the global mean in the observations when working with anomalies. Furthermore, the primary interest is in the change simulated by the models. Lastly, Figure 9.42 shows that there is no correlation between absolute GMST and equilibrium climate sensitivity for the range simulated by the models. In summary, the figure does display the most relevant information in the clearest form.
TS-1448	TS	77				Figure TS. 7 b and c: In panel b you can see that the natural forcings have been more or less constant (with a small dip) between 2000 and 20120. In panel c you can see that the anthropogenic forcings have been increasing between 2000 - 2010. The temperature observations curve shown in both panel b and c shows a temperture decrease between 2000 - 2007 and an increase between 2007 - 2010. Why does the temperature observation curve show a decrease when the natural forcings are constant and the anthropogenic forcings are increasing? Please add an explanation. [Line van Kesteren, the Netherlands]	Noted. Box TS.3 discusses this; notice the large influence of internal variability in the observed record over periods as short as mentioned in the comment.
TS-1449	TS	77				Fig. TS.7 Why does this figure not run until 2011? [Geert Jan van Oldenborgh, Netherlands]	Taken int account. While all model data runs to 2010 for consistency in chapter 10, observational data up to an including 2012 has been added to this figure.
TS-1450	TS	78	0			Figure TS.8: The upper part of this graph (scaling factors) does not seem to be described or discussed in the text of the technical summary and many readers would likely require an explanation to understand how this graphic should be interpreted. Suggest deletion of the upper panel be considered unless it is crucial to understanding the lower panel. [Government of Canada]	Taken into account. This figure has been replaced by a simplified and much clearer figure that does not include scaling factors.
TS-1451	TS	78	1	78	8	Figure TS.8 has a very limited quality (it seems that it was incorporated as a copy of another figure). Please improve significantly its quality and also suppress in the upper part of this figure the red line and letters "Greenhouse ga" (without the "s") since this information is given in the Legend. [Rubén D Piacentini, Argentina]	Taken into account. This figure has been replaced by a simplified and much clearer figure that does not include scaling factors.
TS-1452	TS	78	4	78	4	Please explain what is included in the 'other anthropogenic' category (aerosols, LUC?) [Government of Canada]	Taken into account. The figure caption to the revised figure explains this.
TS-1453	TS	78				Fig. TS.8: What does this scaling mean? [Government of Germany]	Taken into account. The figure has been replaced by a simplified and much clearer figure that does not include scaling factors.
TS-1454	TS	78				Fig. TS.8: Are there blue lines in the lower figure? [Government of Germany]	Taken into account. This figure has been replaced by a simplified and much clearer figure in which all the lines are visible.
TS-1455	TS	78				Figure TS. 8 a: This figure is not clear, for me it does not show anything. The meaning of scaling factor is unclear to me. Suggest to clarify this graph with more explanation or to remove it. [Line van Kesteren, the Netherlands]	Taken into account. This figure has been replaced by a simplified and much clearer figure that does not include scaling factors.
TS-1456	TS	78				Figure TS. 8 a: This figure is not clear, for me it does not show anything. The meaning of scaling factor is unclear to me. Suggest to clarify this graph with more explanation or to remove it. [Line van Kesteren, the Netherlands]	Taken into account. This figure has been replaced by a simplified and much clearer figure that does not include scaling factors.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-1457	TS	79	1	79	1	I suggest the middle panel be modified (or a further panel includes) to show individual GCM simulations year-on-year from the TAR and AR4 multi-model ensembles (CMIP2 and CMIP3). This will make it clear that the models do simulate interannual variability as seen in the observations. The current version of the figure gives the impression that the IPCC expected temperature to warm continuously year on year, which of course was not the expectation - the projections shown here are just the long-term trend either from averaging the GCMs or using simple climate models. Showing individual GCM simulations with interannual variability will show that interannual variability does emerge from the models, and some showed sequences of consecutive years without major warmings simply as an emergent property of the internal variability. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Taken into account. Figure has been completely revised, and does show interannual variability in both models (individual simulations) and observations.
TS-1458	TS	79	1	79	2	Shows that the models have predicted carbon dioxide but they are very poor at predicting temperature or sea level. Tou have omitted methane because the model predictions are disastrous. Why should we believe they can reliably predict future figures? [Vincent Gray, New Zealand]	Noted. No action item discernible.
TS-1459	TS	79	1	79	2	This comment is about Figure TFE.3, Figure 1, which is similar to Figure 1.5 (Chapter 1, page 40, lines 1-11). I understand that this is a PLACEHOLDER and will be updated when new observational data sets become available. These figures, particularly the center figure that compares the observed surface temperature change to the AR4-predicted globally and annually averaged surface termperature change, needs to be brought into the SPM and discussed. This figure could easily be interpreted to show that the observed temperature has leveled off over the past few years (since ~ 2004) at a level near the extreme lower end of the AR5 predictions. Those persons opposed to the IPCC conclusions will use this figure to demonstrate that the climate is not changing as greatly as predicted, and that the models are inaccurate and overly conservative. To clarify this, the figure should be brought into the SPM, and its relevance, meaning, and uncertanties should be openly discussed. [Julian Levy, U.S.A.]	Figure has been completely revised.
TS-1460	TS	79	12			I think it would be useful to say 'Projected values are aligned...' unless I have misunderstood. [Government of United Kingdom of Great Britain & Northern Ireland]	Figure has been completely revised.
TS-1461	TS	79				TFE-3-Figure-1: use the same scenarios for the shading in each panel - observed temperature in this figure ends in 2011 whereas that in Figure-TS-7 already ends in 2009; it would be better showing the same last year [Barbara Früh, Germany]	Figure has been completely revised.
TS-1462	TS	79				Fig. TFE.3 F1: Very useful figure! It would be very good to have AR5 in this figure too. [Government of Germany]	Figure has been completely revised.
TS-1463	TS	79				Fig. TFE.3 F1: Lower graph: it looks like SLR was zero in 1990? [Government of Germany]	Figure has been completely revised.
TS-1464	TS	79				Fig. TFE.3 F1: Middle Graph: it would be good indeed to show the anomaly wrt to pre-industrial times. [Government of Germany]	Figure has been completely revised.
TS-1465	TS	79				Fig. TFE.3 F1: Here, uncertainty ranges are linkes to 90%, not 95% as in the rest of the report? [Government of Germany]	Figure has been completely revised.
TS-1466	TS	79				TFE3 Fig. 1: I continue to argue that this figure is misleading and dangerous, and I am surprised to see the same discussion and figure again in this draft. First, the scenarios in early IPCC reports were incomplete in terms of forcing, and most models did not include all relevant forcings, even up to AR4. The text in TFE3 incorrectly states that Pinatubo was included in the AR4 simulations. Some models did, others not. Why would we expect agreement if the world has not followed those scenarios and if the models did not have all forcings? Second, the figure mixes model response uncertainty and scenario uncertainty by showing ranges across both.Third, aligning the temperature data at a single year makes no sense, the figure may look very different when the average of 20yrs is used to align. Fourth, comparing a model average (or energy balance models without variability) is problematic. Even if the variability band is shown, the visual impression is very different than from a figure which shows individual ensemble members along with observations, as for example in TS.7 or TS.12. But fundamentally IPCC never made predictions but projections, so I argue that this comparison should not be made. [Reto Knutti, Switzerland]	Figure has been completely revised.
TS-1467	TS	79				TFE.3, Figure 1 (middle): the observed global annual temperature change relative to 1961-1990 shows no increase nor decrease from 2001 - 2011.. This seems to be in conflict with Figure TS. 7 panel b and c: which	Figure has been completely revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						shows the observations graph of the temperature decreasing between 2001-2007 and then increasing again up to 2010. [Line van Kesteren, the Netherlands]	
TS-1468	TS	79				TFE.3, Figure 1 (middle): the observed global annual temperature change relative to 1961-1990 shows no increase nor decrease from 2001 - 2011.. There should be an explanation of why this is the case. [Line van Kesteren, the Netherlands]	Figure has been completely revised.
TS-1469	TS	79				TFE.3, Figure 1 (middle): the observed global annual temperature change relative to 1961-1990 shows no increase nor decrease from 2001 - 2011.. This seems to be in conflict with Figure TS. 7 panel b and c: which shows the observations graph of the temperature decreasing between 2001-2007 and then increasing again up to 2010. [Line van Kesteren, the Netherlands]	Figure has been completely revised.
TS-1470	TS	79				TFE.3, Figure 1 (middle): the observed global annual temperature change relative to 1961-1990 shows no increase nor decrease from 2001 - 2011.. There should be an explanation of why this is the case. [Line van Kesteren, the Netherlands]	Figure has been completely revised.
TS-1471	TS	80	1	80	12	In TFE.4, Figure 1, part b), there is a quantity alpha with numbers that it is not explained in the Leyend. Please, introduce alpha and explain the origin of the different values. [Rubén D Piacentini, Argentina]	Accepted. Definition of alpha added and range explained.
TS-1472	TS	80	4	80	12	Fig. TFE.4 F1:Caption should be improved for non experts. The last sentence and the right figure remain unclear. [Government of Germany]	Taken into account. Caption comprehensively revised.
TS-1473	TS	80	9			The word "inferred" should be avoided here because it can easily be confused with "infrared". Better to use "obtained from" or "deduced from". [Terje Wahl, Norway]	Rejected. These words differ in their meanings in subtle yet important ways.
TS-1474	TS	80				TFE-4-Figure-1: where do the different values of alpha come from? Give reference [Barbara Früh, Germany]	Accepted. Definition of alpha added and range explained.
TS-1475	TS	80				Fig. TFE.4 F1:y-axis: should it be "cumulative flux of energy" on the left and "change of cumulative energy" on the right? what is alpha in the right graph? Please explain residual. [Government of Germany]	Taken into account. Figure, legend, and caption comprehensively revised.
TS-1476	TS	80				TFE.4, Figure 1: It is not clear to me why solar forcing provided a monotonously increasing positive contribution to the cumulative energy since 1970. Since then (especially since about 1990) TSI was rather decreasing. Please note that solar activity was on a local maximum around 1970 and since then the average solar activity was lower (which should have led to a negative cumulative energy?). [Raimund Muscheler, Sweden]	Noted. Numbers are best estimates from Ch08.
TS-1477	TS	80				TFE. 4, Figure 1 b: It is unclear to me what these alphas mean. Please add a clear explanation. [Line van Kesteren, the Netherlands]	Accepted. Definition of alpha added and range explained.
TS-1478	TS	80				TFE. 4, Figure 1 b: It is unclear to me what these alphas mean. Please add a clear explanation. [Line van Kesteren, the Netherlands]	Accepted. Definition of alpha added and range explained.
TS-1479	TS	81	1	81	5	With respect to: TFE.5, Figure 1:"Atlantic Meridional Overturning Circulation strength (Sv) as a function of year, from 1850 CE to 2100 CE as simulated by different OAGCMs in response to scenario RCP2.6 (left) and RCP8.5 (right). {Figure 12.35}." Please, explain the use of CE after the years 1850 and 2100. Also, since two scenarios are described, the text at the end of the legend must be: "in response to scenarios RCP2.6 (left) and RCP8.5 (right)". [Rubén D Piacentini, Argentina]	Taken into account, figure caption has been revised.
TS-1480	TS	81	1	81	5	TFE.5 Figure 1 - see comments on Figure 12.35 below - might be useful to include onservational estimates of the AMOC on this lot as they exist and give an indication of what confidence can be placed in the models [Meric Srokosz, United Kingdom of Great Britain & Northern Ireland]	Taken into account, observational estimates have been added on the figure.
TS-1481	TS	81				Fig. TFE.5 Fig.1 Has the FIO-ESM model, in which the AMOC collapses in the 21st century, been included in this plot? If not, is there a reason to exclude this model as unrealistic? [Geert Jan van Oldenborgh, Netherlands]	All models were included that provided the necessary data. No data was available for FIO-ESM
TS-1482	TS	82	8	82	9	….For surface temperatue the shading refers to the 5% to 95 % interval of the ensemble…" giving the ensemble size would also be good [Barbara Früh, Germany]	Noted. There is detailed information about this figure in the Supplementary Information.

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TS-1483	TS	82	11	82	11	Please explain abbreviation OHC and it explain it in the Glossary. [Government of Germany]	Taken into account. OHC is defined at use of term Ocean Heat Content in revised text.
TS-1484	TS	83				Fig. TS.10: This figure and its usefulness remain unclear (SAM? SLP? synthetic indices? different lines?) The figure needs more explanations in the text or it can be deleted in the TS. [Government of Germany]	Taken into account. Figure has been redrawn for clarity including the addition of a linekey but has been retained as it shows an important indication of circulation changes.
TS-1485	TS	83				Figure TS. 10: This figure is not clear, for me it does not show a clear conclusion. There are too many different parameters shows for each season. Suggest to simplify this graph or to remove it. [Line van Kesteren, the Netherlands]	Taken into account. Figure has been redrawn for clarity including the addition of a linekey but has been retained as it shows an important indication of circulation changes.
TS-1486	TS	83				Figure TS. 10: This figure is not clear, for me it does not show a clear conclusion. There are too many different parameters shows for each season. Suggest to simplify this graph or to remove it. [Line van Kesteren, the Netherlands]	Taken into account. Figure has been redrawn for clarity including the addition of a linekey but has been retained as it shows an important indication of circulation changes.
TS-1487	TS	84	14			ArctSIE is labelled ArcSIE in the figure itself. [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted. The figure and caption are now consistent.
TS-1488	TS	84				I think the label on the vertical axis should be something other 'model quality'. How about 'Agreement with observations' or 'Consistency with observations'. For example, Arctic sea ice extent has high model quality, but Antarctic sea ice extent has low model quality. But the models used to predict these are exactly the same. Also, how come there is higher confidence in trends in precip extremes than trends in surface air temperature extremes in panel (b). The latter must be better observed than the former. Why is confidence in the assessment of model agreement lower for TAS? [Nathan Gillett, Canada]	Taken into account. Y-label modified
TS-1489	TS	84				Box TS.2 F1: Suggestion to change colors of entries in the matrix and to use the colors of traffic lights (green is best) to improve intuitive reading. [Government of Germany]	Accepted. Thank you for this suggestion.
TS-1490	TS	84				Box TS.2 F1: Provide information on how values for model quality and confidence have been obtained (expert judgements?, statistics?) [Government of Germany]	Rejected. The caption already provides pointers to where the assessments are provided in detail. It is impossible to summarise this information in a single figure in the TS.
TS-1491	TS	85				Box TS.3 F1: lower row of figures: what do all the letters and dots mean? and the different colors? [Government of Germany]	Taken into account. Figure has been completely revised.
TS-1492	TS	85				Box TS.3 F1: Caption: First sentence does not apply to all figures, please adapt. [Government of Germany]	Taken into account. Figure has been completely revised.
TS-1493	TS	85				Box TS.3 F1: Explain all abreviations and extend the caption to explain the purpose of these graphs (if not deleted) [Government of Germany]	Rejected. Abbreviations are explained in the referenced chapter sections. The purpose of the graphs is explained in the text of Box TS.5.
TS-1494	TS	85				Box TS.3 F1: There is too much information in this figure and it is only mentioned once in the text (p35, line 53) without specifying which of the many graphs is the relevant one. We suggest deleting all graphs except for the upper one. The caption should carefully explain all the lines in the graph. [Government of Germany]	Taken into account. This figure has been completely revised.
TS-1495	TS	85				Box TS. 3, Figure 1 e and f: These two graphs (e and f) need more explanation. For me these two graphs are not clear. Suggest to explain these graphs more elaborately. [Line van Kesteren, the Netherlands]	Taken into account. These panels are no longer included.
TS-1496	TS	85				Box TS. 3, Figure 1 e and f: These two graphs (e and f) need more explanation. For me these two graphs are not clear. Suggest to explain these graphs more elaborately. [Line van Kesteren, the Netherlands]	Taken into account. These panels are no longer included.
TS-1497	TS	86	0			Figure TS.4 Figure 1: This Figure is not discussed in the text of the Technical Summary (the only reference to it is in a line noting that RCPs and not SRES scenarios were used in CMIP5. The caption does not sufficiently	Noted. Figure is no longer part of the document.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						explain the results here either. In particular, the explanation for the blue bars is not adequate. Why is this information critical? The 5-95% range from CMIP5 should be identified as the likely range so the reader knows why the grey bars (likely range using AR4 definition) are being shown. [Government of Canada]	
TS-1498	TS	86	8			Box-TS4-Figure-1: include CMIP5 before median [Barbara Früh, Germany]	Noted. Figure is no longer part of the document.
TS-1499	TS	86				First replace RCP3-PD with RCP2.6. Second why is the 5-95% range assessed as a likely range rather than very likely? Some discussion is needed on the assumptions underlying the interpretation of the model range in this way. Third - why is a puls- response emulation of CMIP5 models needed if we have actual simulations? [Nathan Gillett, Canada]	Noted. Figure is no longer part of the document.
TS-1500	TS	86				Box TS.4 F1: Please: reference period should be pre-industrial for this figure to be useful for policy makers. [Government of Germany]	Noted. Figure is no longer part of the document.
TS-1501	TS	86				Box TS.4 F1: Please explain briefly, what "concentration driven" means. [Government of Germany]	Noted. Figure is no longer part of the document.
TS-1502	TS	86				Box TS.4 F1: Please explain, why 5-95% intervals have been chosen, and the link to the standard deviations. [Government of Germany]	Noted. Figure is no longer part of the document.
TS-1503	TS	86				Box TS.4 F1: Why is the yellow box for Rogelji broader than the others? And why is the median instead of the mean as for the other studies used, and different ranges (33-66/10-90%?) [Government of Germany]	Noted. Figure is no longer part of the document.
TS-1504	TS	86				Box TS.4 F1: Box for Good: 50%ile = median? as in Rogelji Box. [Government of Germany]	Noted. Figure is no longer part of the document.
TS-1505	TS	87	0			The stippling is not explained in the caption. [Government of United Kingdom of Great Britain & Northern Ireland]	Accepted. Stippling dscribed in revised caption.
TS-1506	TS	87				Box TS.4 F2: The period shown differs by 1 year for CMIP3 and CMIP5: Is there a reason? Please explain dotted areas in plots. [Government of Germany]	Noted. The periods differ because AR4 and AR5 used slightly different time intervals, partly as a result of different availability of data. Stippling is described in the revised caption.
TS-1507	TS	87				Box TS.4 F2: Please use pre-industrial conditions as reference. [Government of Germany]	Rejected. Combining the simluated and observed warming over the historical period is not straightforward. All maps are using present day as a reference. For global mean temperature the difference to prei-industrial is given in Table. 12.2/3.
TS-1508	TS	88	4	88	14	The descriptions of the panels in the caption do not match the titles of the panels and there is only mention of two panels. [Government of Canada]	Accepted. Caption revised.
TS-1509	TS	88				Figure-TS-11: - explain abbreviation "SAT" in header - line 5: AMV or AMO, if AMV explain abbreviation - line 6: should read "(middle row)" - explaination of content of bottom row missing, give definition of IPO as for AMO - line 6: which ENSEMBLES do you mean? Give reference - line 6: "CMIP5 Assim" and "CMIP5" in the figure - line 6: "No Assim" in the caption or "CMIP5-hist as in the figure? - line 8: longitude range of the subtrahend - line 11: include "(left column)" after "grey" [Barbara Früh, Germany]	Figure caption has been completely re-written taking all of these comments into account.
TS-1510	TS	88				AMV is referred to in the caption but not shown on the plot. (Is AMV the same as AMO? - if so, use a consistent acronym). [Nathan Gillett, Canada]	AMV - Altantic Multidecadal Variability is defined in the revised caption.
TS-1511	TS	88				Fig. TS.11: This figure and its usefulness remain unclear (AMV? DePreSys? ERSST, IPO...?)The figure needs more explanations in the text or it can be deleted in the TS. The figure is only mentioned once in the text (page 39,line 19), but there is no clear reference to its content. It is suggested to delete this figure. [Government of Germany]	Figure and text have been revised for clarity as suggested.

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TS-1512	TS	88				Figure TS11: This figure is not consistent with Figure 11.6. For example, Fig 11.6 now shows a big difference between CMIP initialized and non-initialized forecasts for the AMO in the 2000's, which is not visible in TS11. (I.e. I might even draw contradictory conclusions from the two figures). [Timothy Stockdale, United Kingdom of Great Britain & Northern Ireland]	Figure is now consistent with figure in chapter 11.
TS-1513	TS	89	0			Figure TS.12: Suggest adding to bottom panel (or to Figure caption) that the 5-95% range of CMIP5 projections is considered the likely range. [Government of Canada]	The figure caption explains how the 5-95% range was calculated. Similarly with the text.
TS-1514	TS	89	1	89	1	Figure T@.12 is very important, especially panel (a) as it makes clear that the Earth System Models do simulate interannual variability and temperatures are not projected to rise consistently year-on-year. I suggest that this figure ought to be in the SPM. [Richard Betts, United Kingdom of Great Britain & Northern Ireland]	Agreed
TS-1515	TS	89	1	89	1	It would be more informative to show trends. It is a bit disappointing that a blogger (Lucia Liljegren at The Blackboard) is able to show better comparisons between observed and modeled trends than the IPCC. Please read one of her latest posts http://rankexploits.com/musings/2012/trends-relative-to-models/ and use the same format in the final report. Quite surprisingly this post shows that even if you use 1980 or 1990 as the start year, the observed trends are at the lower range of the multimodel mean and some models are even rejected. Nowhere in AR5 the readers are made alert that this is the case. The three relevant graphs are http://rankexploits.com/musings/wp-content/uploads/2012/11/TrendsJan2000_Sept2012.png, http://rankexploits.com/musings/wp-content/uploads/2012/11/ModelVObservattionsJan1990-Sept2012.png, http://rankexploits.com/musings/wp-content/uploads/2012/11/ComparisonSinceJan1980.png [Marcel Crok, The Netherlands]	Comment appreciated. Box TS.3 discusses the warming during the last 15-years in detail.
TS-1516	TS	89	1	89	2	This Figure shows that all the models are hopelessly exaggerated [Vincent Gray, New Zealand]	Comment appreciated - this has been considered in the assessment.
TS-1517	TS	89	1	89	11	In Figure TS.12 a) and b), the vertical axis: "Temperature anomaly", do not have its unit. Please includes the corresponding unit. [Rubén D Piacentini, Argentina]	Corrected
TS-1518	TS	89				Figure-TS-12: the plot "global mean temp near-term proj" SHOULD be identical to the corresponding figure 11.33. It is especially critical that in Fig. TS 12 the observations end with an temperature increase whereas in Fig 11.33 it ends with a temp DECREASE! - panels are not marked with "a)" or "b)" - line 4: include "near" before "surface temp" - line 9: give reference to ASK method [Barbara Früh, Germany]	Corrected
TS-1519	TS	89				The one sigma range is not needed on this plot, given that the 5-95% range is shown. [Nathan Gillett, Canada]	The figure provides the complete assessment of the ranges.
TS-1520	TS	89				Fig. TS.12: This figure and its usefulness remain unclear (lines, abreviations need more explanations). The figure is only mentioned once in the text (page 39,line 20), but there is no clear reference to its content. It is suggested to delete this figure. The results from CMIP5 should be included in figure TFE.3 Figure 1. [Government of Germany]	CMIP5 results are now included in TFE.3 Figure 1. However, TS.12 includes the assessed temperature changes whereas TFE.3 Figure 1 only includes the CMIP5 results.
TS-1521	TS	89				Figure TS.12: Why does the observations graph stop in 2005? Suggest to add also the more recent years to the graph. [Line van Kesteren, the Netherlands]	revised figure includes most recent observations
TS-1522	TS	89				Figure TS.12: Why does the observations graph stop in 2005? Suggest to add also the more recent years to the graph. [Line van Kesteren, the Netherlands]	revised figure includes most recent observations
TS-1523	TS	90	1	90	17	In Figure TS.13, the upper left figure has a vertical axis corresponding to "Radiative forcing", but the unit is not correct since it is written as [Wm2]. The correct unit is [Wm-2]. [Rubén D Piacentini, Argentina]	Accepted. Unit corrected.
TS-1524	TS	90	6	90	7	Figure-TS-13: skip the sentence "Global mean near surface temperature change." [Barbara Früh, Germany]	Accepted. Sentence removed.
TS-1525	TS	90	14			If retained the caption needs to say what the internal variability is of (annual mean SAT)? Of course this would be different e.g. for decadal mean SAT. I think this hatching could be removed. Also, why not show 5-95% ranges for consistency with the rest of the TS, rather than one sigma? [Nathan Gillett, Canada]	Accepted. Hatching and stippling explained in the revised version and consistent with chapter 12. Likely ranges (5-95%) shown in the timeseries for consistency with the SPM warming ranges.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
TS-1526	TS	90				Fig. TS.13: Explanation on what an "energy balance model" is in contrast to the CMIP5 model is needed for non-experts. The upper left graph shows what has been used to created the RCP-scenarios or results from MAGICC? The lower left graph shows results from CMIP5 using the input from the RCPs? The maps show results from CMIP5? Color bar for maps does not need so much blue. [Government of Germany]	Partly accepted. Space limits do not allow to explain the different model types in the caption of a summary. The caption notes that the forcing is from MAGICC and the projections are from CMIP5. Color bars are chosen to be consistent with the chapter.
TS-1527	TS	90				Fig. TS.13: Caption: Please explain, what "concentration driven" means. Reference period shold be pre-industrial. The maps show results from CMIP5 ("multimodel ensemble average ...from CMIP5"?) [Government of Germany]	Partly accepted. Space limits do not allow to explain all details in the caption of a summary, but those are provided in the chapters. Combining the simluated and observed warming over the historical period is not straightforward. All maps are using present day as a reference. For global mean temperature the difference to prei-industrial is given in Table. 12.2/3.
TS-1528	TS	91	0			Fig TS-14: The figure may be misleading as it implies that permafrost is completely thawed over this area whereas the caption indicates that the figure refers to "near-surface" permafrost. A previous comment was included on the SPM that suggested that the use of the term "near-surface" permafrost is confusing as it can be interpreted as complete loss of permafrost, whereas normally the models on which these statements are based are considering thawing in the upper 2-3 m of the ground and are therefore considering an increase in thaw depth over time rather than a decrease in permafrost extent. In other reports (e.g., SWIPA report by AMAP) the permafrost science community has avoided wording like this and instead used statements such as "models project that the upper 2 to 3 m of permafrost will thaw over X% of the area currently underlain by permafrost by XXXX". It is suggest that alternate wording (other than near-surface) be used in the SPM, TS and Chp 12. [Government of Canada]	Rejected. The use of "near surface permafrost" was carefully discussed and agreed upon, and the quantity is defined in the glosary.
TS-1529	TS	91	0			Figure TS.14 upper panel: As per comments on this Figure in the SPM, it would help if the y-axis were adjusted to better reflect what reductions in sea extent are possible. Under RCP8.5, projections are for a decline of 94% by the end of the century and yet this is not evident at all on this graphic. [Government of Canada]	Accepted. Sea ice timeseries are now shown in absolute terms rather than anomalies.
TS-1530	TS	91	12	91	13	For sea ice, the shading denotes the 5-95% range of the CMIP5 ensemble (see Figure 12.28 of Chapter 12). [Thierry Fichefet, Belgium]	Accepted. Figures and captions have been revised in collaboration with the reviewer.
TS-1531	TS	91				Figure-TS-14: line 9: explain abbreviation MMD - line 10: explain "box smoothed" - line: 11 & 12: skip sentence " Blue:…" it is double [Barbara Früh, Germany]	Partly accepted. MMD removed, duplicated sentence deleted. Space does not allow to define/explain all details in the caption.
TS-1532	TS	91				Better to show 5-95% ranges for consistency with the rest of the report than one sigma. [Nathan Gillett, Canada]	Rejected. One standard deviation is shown as an illustration of model spread in many figures of CMIP5, and is not a formal uncertainty estimate.
TS-1533	TS	91				Fig. TS.14: Numbering the figures from a) to d) would help referring to them in the caption and text. The x-axis are different, please give briefly the reason. Please explain the shades, why one standard deviation? What does "MMD average" mean? first graph: what is the difference between "observations" and "historical"? [Government of Germany]	Partly accepted. Figures and captions revised. Space does not allow to define/explain all details in the caption. Details are given in chapter 12.
TS-1534	TS	91				fig. 14C Why are there no error bands for the historical period? Was this generated using observed or reanalysis data prior to 2005? Were the models bias-corrected to make them agree on an initial permafrost extent? Also, can you add independent, non-model derived estimates of permafrost extent over the observational period? (same comments given on figure 12.33) [Government of United States of America]	Noted. Historical refers to the historical CMIP5 simulations throughout the projection chapters. Observed and simulated permafrost are difficult to compare, and observations are limited. Details are given in the chapter.
TS-1535	TS	91				Figure TS.14, top: Please add a line showing at what anomaly level all ice has disappeared. [Ron Lindsay, United States of America]	Accepted. Sea ice timeseries are now shown in absolute terms rather than anomalies.
TS-1536	TS	91				Fig. TS-14 The figure is misleading as it implies that permafrost will completely thaw over this area when according to the caption only near-surface permafrost is being considered. The Y-axis label would seem to indicate that the figure shows the total area underlain by permafrost which is not the case. See previous	Rejected. Figure and caption refer to near surface permafrost, which was carefully discussed and defined.

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						comments and ch 12 comments regarding the use of terminology "near-surface permafrost area/extent" and the suggestion that it be avoided. [Sharon Smith, Canada]	
TS-1537	TS	92	1	92	13	In TFE.7, Figure 1. the vertical axis, in both figures, has only the indication of the units and not of the variable. Please, include the variables. In the legend it is indicated that the Top figure display the "timeseries of instantaneous emission rate." However the unit is Gtyr-1(please correct also this unit, including the minus sign) which means that the emission rate is given one value each year. So a better expression would be: "timeseries of yearly emission rate". [Rubén D Piacentini, Argentina]	Rejected. The figure title indicates what quantity is being plotted. This is also stated in the caption .Unit has been corrected. Variable name simplified to "fossil fuel emissions". Note that the former Figure TFE.7, Fig1. is Fig. TS.19 in the Final Draft
TS-1538	TS	92	5	92	5	Fig. TFE.7 F1: What does "time series of instantanous emission rate" mean? [Government of Germany]	Now Fig. TS.19 in the Final Draft. Variable name simplified to "fossil fuel emissions". Legend now reads "Timeseries of annual emissions"
TS-1539	TS	92	8	92	13	Fig. TFE.7 F1: This caption explains "concentration driven models": explanation should be provided in the text or a footnote, not in this caption, as it is needed several times in the TS. [Government of Germany]	Rejected. Space does not allow to define/explain all details in the caption. But more information can be found in Box TS.6: The New RCP Scenarios and CMIP5 Models. Almost all figures are are based on the same simulations driven by atmospheric concentrations rather than emissions. further details are given in the chapters.
TS-1540	TS	93	4	93	4	Fig. TFE.8 F1: RCP2.6 not 3PD [Government of Germany]	Noted, figure removed
TS-1541	TS	93	8	93	8	Fig. TFE.83 F1: How is "likely" defined here? What is an emission corridor? what is the difference between panel b and d? [Government of Germany]	Noted, figure removed
TS-1542	TS	93				Fig. TFE.8 F1: How have the pathways in figure b) been obtained? And does this mean, that T-increase from RCP2.6 is about 1.6 °C wrt to pre-industrial levels? what is the gray shading? why are there 2 lines in fig c) in addition to the shading? label of c) should be total emissions per year? [Government of Germany]	Noted, figure removed
TS-1543	TS	94				Fig. TS.15: How is likely defined here? ref should be pre-industrial. [Government of Germany]	Rejected. Combining (uncertain) observations and simulated past changes is difficult. Projections are given relative to present day throughout. Likely is defined as >66% throughout the report (see uncertainty guidance note).
TS-1544	TS	95	0			The global Pacific focused projection makes it difficult examine European seas. Consider whether it would be apropos this plot be produced as Indian Ocean, Pacific Ocean and Atlantic Ocean, centred panels (similar to fig 3.16)? [Government of United Kingdom of Great Britain & Northern Ireland]	Rejeced. Ocean maps are generally centered on the Pacific in order not to cut the Pacific into two pieces.
TS-1545	TS	95				Fig. TS.16: Please: ref should be pre-industrial. [Government of Germany]	Rejected. Combining (uncertain) observations and simulated past changes is difficult. Projections are given relative to present day throughout.
TS-1546	TS	96				Could an acronym other than 'SAM' be used for South America. SAM is used frequently in the TS for Southern Annular Mode. [Nathan Gillett, Canada]	Rejected. SAM has traditionally been used in IPCC for South America as well. Given that the area is shown and labeled in the map, there should be not confusion.
TS-1547	TS	96				Figure TS.17: All these small graphs together do not form a clear view. Maybe it is possible to enlarge graph b with the world map and add the small regional outcomes next to the region in the map - making it one big overview graph (a bit the idea of Figure TS.19 on p 98 TS). [Line van Kesteren, the Netherlands]	Accepted. Figure has been revised for clarity.
TS-1548	TS	96				Figure TS.17: All these small graphs together do not form a clear view. Maybe it is possible to enlarge graph b with the world map and add the small regional outcomes next to the region in the map - making it one big overview graph (a bit the idea of Figure TS.19 on p 98 TS). [Line van Kesteren, the Netherlands]	Accepted. Figure has been revised for clarity.
TS-1549	TS	97				Fig. TS.18: What is the purpose of the SOI? The figure remains unclear for non-experts, and as it is only mentioned once in text without explanation, deletion is suggested. [Government of Germany]	Accepted. Figure replaced.

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TS-1550	TS	97				Figure TS. 18: Incomprehensible graphs. Please add more explanation or remove the graphs. [Line van Kesteren, the Netherlands]	Accepted. Figure replaced.
TS-1551	TS	97				Figure TS. 18: Incomprehensible graphs. Please add more explanation or remove the graphs. [Line van Kesteren, the Netherlands]	Accepted. Figure replaced.
TS-1552	TS	98				Fig. TS.19: How is likely defined here (67% confidence interval?) units should be added [Government of Germany]	Accepted. Likely is defined as >66% throughtout the report. Figure, caption and units revised.
TS-1553	TS	99	1	99	17	With respect to TFE.9, Figure 1. bottom left, the map is covered with squares that have almost all the same false color. So, it is not possible to derive any conclusion from it. Please, change the map and improve significantly the color codes in order that scientific information can be derived from this map. [Rubén D Piacentini, Argentina]	Accepted:The figure and associated caption have been updated accordingly.
TS-1554	TS	99	5	99	6	Fig. TFE.9 F1: "annual maximum of max daily T" \= annual max of daily T? [Government of Germany]	Accepted:The figure and associated caption have been updated accordingly.
TS-1555	TS	99	5	99	9	Fig. TFE.9 F1: There is a doubled description of the panels a-d, starting at the end of line 9 ist seems to be the correct description, Please correct the description for the panels a-d. [Government of Germany]	Accepted:The figure and associated caption have been updated accordingly.
TS-1556	TS	99				Fig. TFE.9 F1: lower row, left map: please modify color bar, blue is not needed. [Government of Germany]	Accepted:The figure and associated caption have been updated accordingly.
TS-1557	TS	99				Figure TFE9, Figure 1: This figure needs some improvement. The time series shown should be in the same style as time series displayed in previous figures. The temperature indices in the time series don't match the indices shown in the maps. The stippling/hatching in the maps dominates the whole figure. Stippling could be shown only for areas that are not significantly. Alternatively it would be worth considering to use a similar methodology of stippling/hatching as in the other figures in the TS. [Jana Sillmann, Canada]	Accepted:The figure and associated caption have been updated accordingly.