

## **IPCC Working Group I Fourth Assessment Report**

### ***Expert and Government Review Comments on the Second-Order Draft***

## **Summary for Policymakers Comments w/ Responses**

The following compilation of review comments and author responses is supplied by the Working Group I Technical Support Unit as a record of the process used to prepare the Working Group I report. These comments and responses are not to be edited and/ or re-distributed in part or in full to others.

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

### **Batch AB (23 August 2006)**

No.	Batch	Page:Line		Comment	Response
SPM-1	A	0:0	0:0	While an impressive achievement of summarizing the large volume of information in the full Working Group I report, the language in the SPM is too technical and awkward for a Policymakers Summary. An additional section with a very short summary of 6-10 key findings would be useful. [Govt. of Canada (Reviewer's comment ID #: 2004-1)]	Headlines have been sharpened to achieve this. Language has been made less technical where practical
0-46	A	0:0		The Second Order Draft embodies a general account of the physical science basis of climate change. It captures the current scientific understanding. Further, the overall text of the Summary for Policy Makers (SPM) as well as Technical Summary (TS) is comprehensive [Govt. of India (Reviewer's comment ID #: 2013-1)]	Thank you
SPM-2	A	0:0	0:0	"In general, the SPM is hard to follow and to extract clear information, in large part due to unclear change in reference points, time scales and time periods."  [Govt. of Canada (Reviewer's comment ID #: 2004-2)]	Time scales, etc., have been made consistent where practical within the information available. Not all variables are available on the same time frames.
SPM-3	A	0:0	0:0	"The SPM communicates some very important new information, i.e, relative to the TAR, but does not provide clear assessment of the implications of that information. For example, what might the new information with respect to the probabilities associated with the emission scenarios in Figure SPM-5 mean?"  [Govt. of Canada (Reviewer's comment ID #: 2004-3)]	Text has been sharpened.
SPM-4	A	0:0	0:0	"The SPM is not clear in the present draft about what results from the TAR still stand. A good example is the lack of a Figure showing future projections for GHGs, temp, SLR etc. as in the TAR. Do the results from the TAR stand, or not? The same SRES scenarios may be being used, but the models have improved - has that made any difference? The AR4 may be meant as an update to the TAR, but the SPM to this WG1 report of the AR4 does not read as a stand-alone document right now. Need some link to findings of the TAR if they are not going to be presented again here."  [Govt. of Canada (Reviewer's comment ID #: 2004-4)]	Text has been sharpened
0-21	A	0:0		new findings compared to the TAR could be summarized at the end of the introductory sub-chapter of the SPM ("what's new ?") [Govt. of Germany (Reviewer's comment ID #: 2011-97)]	SEE COMMENT SPM-1
SPM-5	A	0:0	0:0	"There is very little in the SPM on biological feedbacks."	Noted, but no specific suggestions offered.

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				[Govt. of Canada (Reviewer's comment ID #: 2004-5)]	
SPM-6	A	0:0	0:0	The SPM is in very good shape. I have only two general comments, firstly that in places it is still rather technical for its audience, and secondly a bullet giving some information what we have observed not to change in the climate and why would be a valuable addition. [David Griggs (Reviewer's comment ID #: 90-1)]	Material has been added
SPM-7	A	0:0	0:0	Personal Comment to Co-Chair Susan Solomon: Since I am one of scientists in the energy sector, I have chance to see the draft of SPM by WG-3. I was very surprised to find the sentence related to "feasibility of stabilization level of 400-500ppmv". It means that WG-3 can decide the appropriate stabilization level by themselves without any new scientific findings, which will be provided by WG-1 through SPM of AR4. In other words, I suppose WG-3 has no necessity of the role of WG-1. Why do they do that? I recommend there should be enough meetings and discussion among scientists both in WG-1 and WG-3 about the stabilization level, which is one of critical issues in AR4. [Koki Maruyama (Reviewer's comment ID #: 169-5)]	Assessment of what scenarios are feasible is outside the scope of WG1.
SPM-8	A	0:0	0:0	We would like to thank WG1 for a useful first draft report and are impressed by the level of new work and the quality of many of the graphs. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-82)]	Thank you
SPM-9	A	0:0	0:0	We would however like to make a number of general suggestions regarding presentation and structure of the SPM, which we think would make it much more accessible to a non-technical audience and bring out some of the key conclusions which we find are rather buried in the SPM or even in the underlying report. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-83)]	Specifics addressed where they appear
SPM-10	A	0:0	0:0	We would note that the style is uneven. In general it would be useful to make more of the headings in bold and turn them into short paragraphs which could stand alone as key policy relevant conclusions, leaving more technical info for the bullet points. Indeed the bold text could notionally form a set of very limited key conclusions on their own and should be eminently quotable. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-84)]	Accepted, text has been sharpened with this in mind specifically.
SPM-11	A	0:0	0:0	The language is a little too scientific to be accessible to policy-makers – suggest more plain language would help. And more explanation is required in places – see detailed comments. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-85)]	Specifics addressed where detailed comments appear
SPM-12	A	0:0	0:0	We would suggest a greater use of diagrams would be beneficial. We suggest	Cannot add length. This SPM has one more figure

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				that there are many very useful diagrams in the underlying report that could be used in some form, in the SPM. Examples include 10.3.10, 10.3.16, 10.5.2, 10.6.1. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-86)]	already than the TAR SPM.
SPM-13	A	0:0	0:0	But we would also suggest that the diagrams used in the SPM are simpler and more consistent with each other. We would suggest that a suite of diagram similar to those in Figure 5 of the TAR SPM would be helpful – but they could cover both past trends (say from 1850 or when data allow) and on to 2100 (or beyond). These should cover – ghg and sulphur emissions, ghg and aerosol concentrations, radiative forcing, temperature, sea level rise and extremes, snow and ice, droughtiness and rainfall etc. this would give policymakers a broad feel for the big picture. Other diagrams could be used to make more specific points for example on attribution. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-87)]	Complex figure suggested goes very far beyond TAR spm 5 and would be very difficult to add this level of complexity in any SPM.
SPM-14	A	0:0	0:0	On structure we suggest breaking up the SPM into more manageable sections eg: Observations of global atmospheric and climate change Constituents/temperature/rainfall/circulation/ surface – trop- strat Understanding the causes of global climate change Attribution Predictions of global climate change Constituents/temperature/rainfall/circulation/ surface – trop- strat Specific issues Extremes, Oceanic circulation salinity and temperature, soil moisture, Cryosphere, sea level rise Stabilisation issues [Govt. of United Kingdom (Reviewer's comment ID #: 2022-88)]	Structure we chose was to discuss global material first, followed by regional rather than to split by variable (temps then rainfall, circulation, etc.). Text has clarified our approach to address this.
SPM-15	A	0:0	0:0	Consistency of language – can we use “English” English throughout [Govt. of United Kingdom (Reviewer's comment ID #: 2022-89)]	Copy editing will be done on final copy
SPM-16	A	0:0	0:0	Too many unfamiliar acronyms for any politician - NH, SST etc. Use whole word & no abbrevs [Govt. of United Kingdom (Reviewer's comment ID #: 2022-90)]	Copy editing will be done on final copy, glossary to be added
SPM-17	A	0:0	0:0	References to previous IPCC reports seems to be mixed. Can we suggest we define these early on as AR1, AR2 and AR3 (few will know what the FAR or SAR means!) [Govt. of United Kingdom (Reviewer's comment ID #: 2022-91)]	Copy editing will be done on final copy
SPM-18	A	0:0	0:0	greater use of context setting numbers – e.g. percentage changes [Govt. of United Kingdom (Reviewer's comment ID #: 2022-92)]	Addressed where practical.

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SPM-19	A	0:0	0:0	<p>More careful reference to uncertainty and lack of understanding is required. In several places the casual reader might think we understand very little – but in reality we know a lot but not enough to quantify it. We make specific suggestions at the relevant points.</p> <p>[Govt. of United Kingdom (Reviewer’s comment ID #: 2022-93)]</p>	Specific suggestions considered where offered.
SPM-20	A	0:0		<p>The SPM/TS/chapters of the AR4 seem to have a critical omission compared to the TAR. In the TAR WGI policymakers were provided with information relating to the future time points at which global emissions would have to peak/drop below 1990 levels/ decrease to a small fraction to achieve various CO2 concentrations outcomes. Relevant parts of the TAR include: Synthesis Report Question 6 and Working Group I Technical Summary Section F.10 (including Fig 25). We consider this is a very important line of information for policymakers. Consequently, we believe the topic should be covered in the AR4 - preferably on the basis of any more recent modelling; but otherwise recovering the ground presented in the TAR. This scientific topic is important because it sheds light on the timing of and scale required in global emissions reductions to achieve various stabilisation levels.</p> <p>[Govt. of Australia (Reviewer’s comment ID #: 2001-1)]</p>	Material has been added regarding the WG1 inputs to this issue (carbon cycle feedback), complementing the more detailed material that is covered in WG3.
0-61	A	0:0		<p>It is our view that the SPM is quite heavily loaded with numbers and little background information. This may take away the impression of climate change’s seriousness and urgency, which is expressed in the Technical Summary and the rest of the report. The SPM would benefit from limiting the numbers to be included and adding more explanatory text to these numbers. Focusing more on the robust findings and adding more information from the TS could be one possibility. This would in our view increase the readability and the clarity of the SPM and get the messages better through to policymakers.</p> <p>[Govt. of Norway (Reviewer’s comment ID #: 2018-2)]</p>	Text has been edited
0-60	A	0:0		<p>The SPM should be made clearer and if possible shorter. The coupling between level of emissions and magnitude of climate change should be described more clearly. Furthermore, the key findings in the WGI report should be but in bold text in front of each section. In the draft the message in the text in bold is not always clear. The last page of the report should be rewritten to become a summary of the risk for dangerous climate change, where policy makers can see the effect on climate change depending on the level of emissions during the next 50-100 years.</p> <p>[Govt. of Norway (Reviewer’s comment ID #: 2018-1)]</p>	See SPM-1
SPM-21	A	0:0		The SPM is pretty clear, well structured and has a reasonable length. My	Thank you

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			congratulations. [Govt. of Austria (Reviewer's comment ID #: 2002-1)]	
SPM-22	A	0:0	It should be avoided that two reports of Working Groups include inconsistent information. SPM-11 points to the fact that this might be the case. [Govt. of Austria (Reviewer's comment ID #: 2002-2)]	Text edited
SPM-23	A	0:0	Although it is acknowledged that this document intends to present global information, in order to grab attention from local policy makers, it will always be better to highlight some more local results, for instance stating if results are from studies in different hemispheres. There are some examples in this respect (SPM-6 line 48; SPM-8 line 43, Figure SPM-4). [Govt. of Chile (Reviewer's comment ID #: 2005-1)]	Text edited where practical .
SPM-24	A	0:0	Suggest to add one paragraph to briefly summarize the new findings since TAR at the beginning of SPM. [Govt. of China (Reviewer's comment ID #: 2006-1)]	SEE COMMENT SPM-1
SPM-25	A	0:0	IPCC should provide an objective and balanced report. Therefore, please indicate uncertainties of any conclusion. Also, please indicate to how much degree natural or anthropogenic change can be distinguished. [Govt. of China (Reviewer's comment ID #: 2006-2)]	Specific suggestions considered where they appear.
SPM-26	A	0:0	For any issue, IPCC report should provide various research results in order to keep balance and provide a complete view for policymakers. [Govt. of China (Reviewer's comment ID #: 2006-3)]	Noted. No specific cases given for this concern on SPM?
SPM-27	A	0:0	Considering IPCC only provide policy-neutral report, please delete any conclusions with value judgment in order to avoid misleading policymakers. [Govt. of China (Reviewer's comment ID #: 2006-4)]	No specific cases given for this concern on SPM?
SPM-28	A	0:0	There are various timing in SPM, such as 1850, 1750, 20th century, recently, last 100 years, last 50 years. Please pay attention to the consistency of these timing and keep in mind that the report will be published in 2007. [Govt. of China (Reviewer's comment ID #: 2006-5)]	Subgroups to consider timing issues – minor issue a
SPM-29	A	0:0	The SPM is accurate and well-balanced and presents the findings of WG1AR4 clearly. Congratulation for the excellent work! [Qiang Fu (Reviewer's comment ID #: 78-1)]	Thank you
SPM-30	A	0:0	Please insert a clear statement on future sea level rise including its uncertainty range which takes the full information into account. This means beside of model results the new observations on ice sheet decay and paleoclimatic information also. Otherwise the new evidence of sea level is rising faster than any scenario shown in the TAR, the Antarctic ice sheet appears to be losing mass overall according to the GRACE satellite data (this was not anticipated in the TAR), and	Text edited

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			<p>the ice loss at the fringes of Greenland and Antarctica is more rapid than expected will be not considered. These findings clearly point to greater sea level rise than projected in the TAR. It is the task of IPCC to give a comprehensive assessment of all these findings and state this clearly in the SPM, the TS as well as in Chapter 10.                      [Govt. of Germany (Reviewer's comment ID #: 2011-62)]</p>	
SPM-31	A	0:0	<p>While the first parts of the SPM up to "understanding and attributing climate change" are already in quite a good shape, the section on "projections of future climate change" needs considerable redrafting to improve the presentation, structure and readability as well as highlight the relevant information for policymakers. For example, only two sentences are highlightes with bold letters, and these do not contain hardly any relevant information, as they are of a very general nature. It is hard to extract relevant information on projected temperature increase and sea-level rise and compare it to the information given in the TAR. A summary of information of the estimated risk of abrupt climate change is missing.                      [Govt. of Germany (Reviewer's comment ID #: 2011-235)]</p>	Text edited to address these points where they are consistent with the report.
SPM-32	A	0:0	<p>This Chapter should be deleted. It is repetitive of what is said in the "Technical Summary" which is written by some of the same people, and it is a misnomer, since it is agreed line-by-line by Government Representatives, and so is a "Summary BY Policymakers". It is demeaning to subject grown professionally qualified scientists to this form of coercion by politicians. It tells the world that IPCC Reports are politically controlled documents, not the honest opinions of the scientists. It also holds up publication by being approved after the final draft has been agreed, leading to the problem either of an obvious disagreement or an attempt to amend the Final Draft.                      [VINCENT GRAY (Reviewer's comment ID #: 88-2133)]</p>	Rejected. This suggestion is not consistent with IPCC procedures nor is it correct in the view of the authors.
SPM-33	A	0:0	<p>Some formatting proposals to chapter SPM and chapter TS: Many figures in these chapters are illegibly small. We understand there are severe space constraints, but if figures are not decipherable for an important fraction of your audience (those reading printed versions), they loose much of their information content. If it is not possible to devote more space to some of the tiny but very important figures, at least make sure you enlarge the axis labels and legends in the figures to a font size sufficiently readable in the final print version. Special attention should be paid to maps: the government guidelines for the AR4 were very clearly requesting regionally detailed information. Maps can provide this kind of message in a very concise way, satisfying many audiences in one graph - providing they are sufficiently legible even on a regional scale. Presently your</p>	Figures have been improved

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			<p>maps are so tiny that only crucial global trends can be identified. This is disappointing as they carry very important information. We would strongly recommend enlarging maps, even if this may mean covering less of them and dropping a few.</p> <p>[Govt. of Hungary (Reviewer's comment ID #: 2012-1)]</p>	
SPM-34	A	0:0	<p>The tables defining terms like "low confidence" and "likely" in Chapter 1, pages 26 and 27, should be included in the SPM.</p> <p>[John Hunter (Reviewer's comment ID #: 112-25)]</p>	Accepted
SPM-35	A	0:0	<p>A section (table) in the SPM detailing the new findings since the TAR would be useful.</p> <p>[Govt. of Japan (Reviewer's comment ID #: 2014-22)]</p>	SEE COMMENT SPM-1
SPM-36	A	0:0	<p>In the SPM, there are many descriptions to make it represent by "B1, A1B and A2" as major scenarios among the 35 SRES scenarios. It should be clearly described in the SPM that the reason why these three scenarios have been considered representative.</p> <p>[Govt. of Japan (Reviewer's comment ID #: 2014-23)]</p>	Box added re SRES, as in TAR
SPM-37	A	0:0	<p>It is reported that the volume of the Antarctic ice sheet had decreased from 2002 to 2005 especially in the continental west shore as a result of the analysis of the satellite data by the research team of the U.S. Colorado University (March 24, 2006, Science, pp1754-1756). This is a result different from the description in the SPM. This literature should be also assessed and reflected in SPM as appropriate.</p> <p>[Govt. of Japan (Reviewer's comment ID #: 2014-24)]</p>	Text is consistent with ch 4 conclusions covering this study along with others
SPM-38	A	0:0	<p>In general, there is very little emphasis on carbon cycle feedbacks in the SPM (only a single paragraph in the whole SPM!). Given the demonstrated importance of these feedbacks (amplifying T and CO<sub>2</sub> rises by up to 30% or so), and the fact that much of this work is new since the TAR then the SPM should highlight their effects more. The good coverage in chapter 7 and chapter 10 should be brought out more here.</p> <p>[Chris Jones (Reviewer's comment ID #: 120-4)]</p>	Noted, but no specifics offered. Carbon cycle feedback is covered.
SPM-39	A	0:0	<p>The SPM should clearly state that the default option for uncertainty range is +/- two standard deviations. This is done in the Technical Summary, but the SPM needs to be a stand alone document, since it is often the only part of the report that is read. If WG I persists in presenting some uncertainty ranges as +/- one standard deviation, each and every one of those cases needs to be clearly identified.</p> <p>[Jeff Kueter (Reviewer's comment ID #: 137-2)]</p>	Accepted

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SPM-40	A	0:0	<p>“Carbon dioxide is the most important greenhouse gas” is obvious in Figure SPM-2 but is never stated. Policy makers may not be able to effortlessly absorb information from graphs and numbers the way scientists do. There should be a simple declarative sentence in the SPM stating the obvious.</p> <p>[Daniel Murphy (Reviewer’s comment ID #: 183-1)]</p>	Accepted, but important to note it is the most important anthropogenic GHG
SPM-41	A	0:0	<p>Overall comment -- I found the SPM a useful and informative summary of the WG1 report and my comments are reserved for a few small sections.</p> <p>[Robert Nicholls (Reviewer’s comment ID #: 191-1)]</p>	Thank you
SPM-42	A	0:0	<p>This is a very well written and well-structured document.</p> <p>[David Parker (Reviewer’s comment ID #: 195-117)]</p>	Thank you
SPM-43	A	0:0	<p>Possibly add cross-references to corresponding material in the TS, because it isn't always easy to find TS material relating to statements made in the SPM. Restructuring the TS somewhat may help.</p> <p>[David Parker (Reviewer’s comment ID #: 195-118)]</p>	TS has been made closer in structure to the SPM, particularly the projections section. References need to be to underlying report.
SPM-44	A	0:0	<p>As a general rule, this document should be written for an audience that has no previous knowledge of climate science, but that has a college-educated. I recommend re-reading it to ensure that all passages would fit that standard.</p> <p>[Timothy H. Profeta (Reviewer’s comment ID #: 203-1)]</p>	Noted
SPM-45	A	0:0	<p>I also recommend that you assume that every passage in this document could be quoted in the political debate without the context of the surrounding words. I therefore recommend that the authors read every individual sentence as a stand alone quotation and assure themselves that it cannot be used improperly out of context.</p> <p>[Timothy H. Profeta (Reviewer’s comment ID #: 203-2)]</p>	Noted
SPM-46	A	0:0	<p>The SPM is pretty clear, well structured and has a reasonable length. My congratulations.</p> <p>[Klaus Radunsky (Reviewer’s comment ID #: 204-1)]</p>	Thank you
SPM-47	A	0:0	<p>It should be avoided that two reports of Working Groups include inconsistent information. SPM-11 points to the fact that this might be the case.</p> <p>[Klaus Radunsky (Reviewer’s comment ID #: 204-2)]</p>	Text edited
SPM-48	A	0:0	<p>I feel it would be very important to include Fig. 6.10 panel b or c in the SPM; everything else would be seen as the IPCC backing away from the conclusions of the TAR about the last millennium. The figure should come with a bullet point: "There is new and stronger evidence that the climatic warming of the 20th Century is likely unprecedented for at least a thousand years. A dozen quantitative climate reconstructions for the past millennium now exist which unanimously agree on this point." Many people will specifically look out for this</p>	We feel that the figures cited are not suitable for an SPM as they are too complex. Text has been edited for clarity re. key conclusions.

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			point, which has attracted lots of media attention since the TAR. [Stefan Rahmstorf (Reviewer's comment ID #: 206-8)]	
0-12	A	0:0	SPM Fig. 6.10 (b or c) should be included in the SPM, with accompanying text that states that there is new and stronger evidence that the warming observed in the twentieth century is likely unprecedented for at least a thousand years. This is supported by quantitative climate reconstructions, which show broad agreement. [ European Commission (Reviewer's comment ID #: 2008-1)]	See SPM-48
0-13	A	0:0	SPM A clear statement on future sea level rise is needed, that takes into account all new recent data from both models and observations. Crucial observations that complement model results include a rise of sea level more rapid than projected by models, the loss of mass of the Antarctic ice sheet, and the more rapid loss of ice at the fringes of Greenland and Antarctica. The SPM should also highlight Chapter 10's finding that current models underestimate observed sea-level rise from 1961 to 2005 by 40%. [ European Commission (Reviewer's comment ID #: 2008-2)]	Text edited
0-18	A	0:0	We disagree with the presentation of low probability but high impact risks as to be "very unlikely" because they do not show up in most model results. That do not warrant the presentation of these events (such as major changes in ocean circulation, rapid ice sheet decay or a mega-drought) as "speculation". We are sure that the IPCC can and should say more about them. Recent observations and paleoclimatic data document the risk for the decay of ice sheets. The risk of major changes in ocean circulation due to freshwater inflow into the deep water formation areas should be discussed by combining information from model sensitivity studies, modern observations and paleoclimatic data, rather than relying on model results as most models lack the key process that could cause a shutdown of deep water formation. The role of the IPCC is not only to describe the most likely future development but "to assess on a comprehensive, objective, open and transparent basis the scientific, technical and socio-economic information relevant to understanding the scientific basis of risk of human-induced climate change" (see Principles Governing IPCC Work). Therefore IPCC have to give a comprehensive risk assessment that also includes a more than cursory discussion of low probability, but potentially high impact risks. We expect a better and adequate consideration and interpretation of these risks from the IPCC especially in chapter 10 of the report.  [Govt. of Germany (Reviewer's comment ID #: 2011-1)]	Text is consistent with the underlying report. Authors have assessed these issues carefully.

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0-19	A	0:0	<p>Please explicitly explain the scientific reasons for the current IPCC assessment that the range of a) future SLR is lower and b) its uncertainty is smaller than assessed in the TAR. Please add to all these explanations the following: "In contradiction to these model projections the observed sea level rise from 1993 to 2005 exceeds all of the projections of the TAR. The assessment of the range of climate sensitivity in this report is higher than in the TAR and the spectrum of considered emission scenarios has been shrunk. Furthermore the uncertainty of ice sheet decay has increased. In conclusion it is very likely that the range of 290 +- 150 mm by 2100 projected by several models underestimates the possible sea level rise drastically."</p> <p>[Govt. of Germany (Reviewer's comment ID #: 2011-2)]</p>	Text edited to clarify
0-62	A	0:0	<p>There remain two different views in the community of how to discuss small probability risks (such as major changes in ocean circulation, rapid ice sheet decay or a mega-drought) in the report:</p> <ul style="list-style-type: none"> <li>- The view that has prevailed in the SOD is to say no more than that such events are very unlikely, based on the fact that they do not show up in most (or any) model scenarios for the future. Supporters of this view have in cross-chapter discussions labelled the discussion of anything beyond what is found in the model scenarios as "speculation" or "scare mongering".</li> <li>- The alternative view is that such risks, though small, could be non-negligible and that there is more that the IPCC can and should say about them. According to this view, model scenarios have limitations and might give a false sense of security. For ice sheets, it is likely that current models do not capture the full dynamics that can lead to rapid disintegration, and the risk for this could perhaps be better discussed on the basis of recent observations and paleoclimatic data. For the ocean circulation, its sensitivity to freshwater influx differs greatly between models for reasons poorly understood, and the estimates for future freshwater inflow into the deep water formation areas of the Atlantic also differ widely. Most of the models cited in chapter 10 do in fact include no meltwater runoff from Greenland at all, so it can be argued that they are missing the key process that could cause a shutdown of deep water formation. The view of some experts is therefore that this risk could be better discussed by combining information from model sensitivity studies, modern observations and paleoclimatic data, rather than relying too strongly on what current model projections say.</li> </ul> <p>On a more fundamental level, the difference of opinion is in whether the IPCC should focus entirely on model scenarios for the most likely outcome of anthropogenic warming (i.e., tell society what the scientific "best guess" for the</p>	Discussed again at LA4, where the author of this comment presented his views and accepted the final conclusions of the team.

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			<p>future is), or whether its task is to provide society with a more comprehensive risk assessment that also includes a more than cursory discussion of low probability, but potentially high impact risks - like a doctor who does not just tell the patient the most likely outcome of an operation, but provides also a thorough and honest discussion of the risks.</p> <p>My view is strongly the latter - I feel the IPCC fails the expectations of society if it dodges this issue. I think this decision should be considered carefully. My review comments to that effect to the FOD were ignored, but I encourage IPCC to look at this issue again - quite a few colleagues have since told me that they see chapter 10 as the weakest of the report, precisely for the reasons outlined above. Yet it clearly is one of the most important chapters, which will be widely cited in public discussions on climate policy.</p> <p>[Stefan Rahmstorf (Reviewer's comment ID #: 206-1)]</p>	
0-63	A	0:0	<p>With respect to future sea level rise the two key messages coming out of the AR4 are: the projected range is lower than in the TAR (with 290 +- 150 mm by 2100 being the largest number to be found in the executive summary of chapter 10 - this is less than a continuation of the current rate of 3.2 mm/yr from chapter 5), and the uncertainty range is smaller. This will attract significant attention (headline: "IPCC: sea level rise much less than stated before"). Yet it remains completely unclear to me where this reduction in the projected value comes from (other than simply a change in methodology compared to the TAR). The observed sea level rise 1993-2005 exceeds all of the projections of the TAR, so this hardly argues for a reduction. The SRES scenarios seem to be the same. Climate sensitivity actually looks higher than in the TAR, with the lower end (1.5-2 °C) being clipped off the expert range, and a substantial likelihood attached to values above 4.5 °C. Uncertainty concerning ice sheet decay has, if anything, increased. The TAR combined all SRES scenarios and the full climate sensitivity range, and added something for ice sheet decay, to derive its full range. That was a very sensible approach. The AR4 seems to come to a lower and smaller range simply by choosing not to explore the full climate sensitivity and scenario range, by citing numbers from models that greatly underestimate the past observed sea level rise, and by not properly considering the possibility of ice sheet contribution to future sea level rise. For Antarctica it flatly states (even in the SPM) that it will contribute negatively to sea level - in contradiction to the observations presented in chapter 5 which conclude it currently already makes a positive contribution to sea level, and in contradiction with all paleoclimatic evidence pointing to a smaller Antarctic ice sheet in warmer</p>	See 0-62

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			<p>climates (e.g. the Pliocene). I personally find the sea level assessment given in the SOD indefensible. [Stefan Rahmstorf (Reviewer's comment ID #: 206-4)]</p>	
SPM-49	A	0:0	<p>The SPM needs a clear statement on future sea level rise, specifying an uncertainty range which takes the full information into account (not just models - also the new observations on ice sheet decay, and paleoclimatic information). In my view evidence since the TAR clearly points to the risk of higher sea level rise than given in the TAR - sea level is currently (1993-2005 satellite altimeter data shown in Chapter 5) rising faster than any scenario shown in the TAR, the Antarctic ice sheet appears to be losing mass overall according to the GRACE satellite data (this was not anticipated in the TAR), and the ice loss at the fringes of Greenland and Antarctica is more rapid than expected. Models do not capture these things, and chapter 10 finds that current models underestimate the observed sea level rise 1961-2005 by 40% - that is a major finding which is well hidden so far in the SOD. The data clearly point to greater sea level rise than projected in the TAR, and this needs to be brought out clearly in the SPM, the TS as well as in Chapter 10! [Stefan Rahmstorf (Reviewer's comment ID #: 206-9)]</p>	<p>Assessment of chapter 5 is that the past decade sea level rise includes a large component of natural variability. Note also the variability in ocean heat content exceeds models. Antarctic data has been carefully assessed.</p>
SPM-50	A	0:0	<p>This is a very robust and solid draft. Well done! My only overarching comment is that in some places, the SPM presumes a degree of scientific understanding that not all of the SPM's core readership may have. As a consequence, some of the very important policy-relevant key messages could get lost, or may be difficult to decipher, for some policy people. I provide some examples and possible solutions in specific comments. I'm also wondering if the presentation of the SPM could be made more attractive, via layout and graphics design, for people who are not used to read and interpret scientific graphs - recognising that preserving the scientific integrity of the information is paramount and graphics should not be turned into "cartoons" just to make them more attractive. This it would probably need a graphically skilled scientist rather than a scientifically skilled graphics artist. [Andy Reisinger (Reviewer's comment ID #: 210-1)]</p>	<p>SEE COMMENT SPM-1  Figures are improved.</p>
SPM-51	A	0:0	<p>The SPM contains in a number of places references to palaeo information. I'm wondering if the message from palaeo data could be strengthened by pulling bullet points in p6 lines18-22, p9/10 lines 12-2, and p11 lines24-35 together into one section? Or perhaps two sections, one on observations including proxies and what sort of ice sheets etc they were associated with, and a second one on our ability to model past changes including relevant feedback mechanisms? In my perception, spreading this information over three sections dilutes rather than</p>	<p>Believe it is important to be clear regarding what can be said from data, and what from attribution and modelling, to retain balance. This should also be consistent with the modern record, avoiding treating paleo as a special case. Structure has been retained.</p>

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			strengthens these important messages. Especially since there is then another section at the very end that talks about future changes, which again relies (to some extent) on palaeo proxies and analogues. [Andy Reisinger (Reviewer's comment ID #: 210-64)]	
SPM-52	A	0:0	If the authors feel the need to make space to accommodate other additions to the SPM: page 6 lines 9-22 could possibly be merged into other sections on palaeoclimate and on understanding and attributing climate change. On page 8 lines 20-23: this is partly contained in Table SPM-1 and, unless there is a clear attribution statement, the rest of the text may not be sufficiently policy-relevant for inclusion in the SPM (IF the authors feel pressed for space that is!) [Andy Reisinger (Reviewer's comment ID #: 210-70)]	Disagree. New information that circulation, winds, storm tracks, precipitation, wave height, etc. are changing is important and cannot be put into a table that deals with extremes.
SPM-53	A	0:0	No comments [Michel Rixen (Reviewer's comment ID #: 215-2)]	Thank you
SPM-54	A	0:0	The lack of mention of surface, tropospheric, and stratospheric temperature trends is surprising, especially given the focus on this topic in the TS and in Ch. 3. [Dian Seidel (Reviewer's comment ID #: 231-3)]	Rejected. Bullet on page 6, line 44-6 and page 10, line 21-23 covers these points at the level practical for the SPM (policy relevance). More technical detail is found in the TS and chapters.
SPM-55	A	0:0	While I generally like the style of the SPM, I am left with an uncomfortable feeling that it is being too positive ... there are a lot of uncertainties, some of them very old, that hardly get a mention, primary amongst them is the continued lack of understanding of cloud feedbacks. [Keith Shine (Reviewer's comment ID #: 236-1)]	Cloud feedbacks noted.
SPM-56	A	0:0	This SPM is, surely and predominantly, a stand alone document. Thus, a brief explanation of SRES scenarios A2, B1, and A1B appears necessary within the SPM. [Herman Sievering (Reviewer's comment ID #: 240-1)]	SEE COMMENT SPM-36
SPM-57	A	0:0	Unless I missed it, SRES is not defined in SPM. [Herman Sievering (Reviewer's comment ID #: 240-3)]	SEE COMMENT SPM-36
SPM-58	A	0:0	Fig.s SPM-5 (all 9 panels) and SPM-6 (lower two panels) could be larger to make them as nicely readable as Fig.s SPM-1, -2, -3. Also, is there a way to make Fig. SPM-4 a bit larger and still fit to the page?? [Herman Sievering (Reviewer's comment ID #: 240-4)]	Figures have been improved.
SPM-59	A	0:0	looks clear regarding both observed changes and the expected changes. It is from a policy perspective more interesting with the shorter time perspective of 2030 which is given in this report compared to the 100-years perspective. This report gives a better basis for evaluating consequences of policy options and how these can support the stabilization of the climate.	Thank you

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			[Govt. of Sweden (Reviewer's comment ID #: 2020-20)]	
SPM-60	A	0:0	The SPM should start with an introduction giving the outline of what will be discussed and end with summarised conclusions. Even if the title says that it is a summary the document still contains many details that should be better summarized in a few bullets (3-4). [Govt. of Sweden (Reviewer's comment ID #: 2020-9)]	See SPM-1
SPM-61	A	0:0	I feel the text in the SPM gives insufficient attention to the key messages, which are often lost in detail - the key points should be at the front of each section. Specific examples identified are listed in further comments. [Blair Trewin (Reviewer's comment ID #: 266-3)]	See SPM-1
SPM-62	A	0:0	In its current state, the SPM is a major source of dissatisfaction with this IPCC report, and the U.S. Government feels strongly that there is considerable room for improvement. It reads more like a zero-order draft than a polished effort ready for public comment. The SPM, which some might argue is the most important (and certainly most widely read) component of the IPCC report, appears to have been neglected then pieced together quickly before the Expert and Government Review. The inadequacies fall into several different categories: [Govt. of United States of America (Reviewer's comment ID #: 2023-697)]	The SPM was not neglected. Specific suggestions for improvement will be considered where given.
SPM-63	A	0:0	1) General Approach. The general approach seems to be to provide policymakers with answers to the basic questions, as defined by the respective subsections. This is not a problem per se, but policymakers have been given these answers before. What they want to know is what is new since the Third Assessment Report (TAR). While some statements are made along this line, there seems to be no organized, systematic effort to provide this perspective. It should be the first issue discussed in each of the general sections. For example: Will it really take a 3°C warming to melt the Greenland Ice Sheet? If so, that appears to be a crucial difference from what the TAR reported. One of the approaches that was well received in the TAR Synthesis Report (e.g. SPM-10a and SPM-10b on pgs. 33-34 in the synthesis report), and could be of benefit for this report, is to use graphs that combine past (proxy data), present (instrumental records), and future projections. This gives the total picture, which is somewhat missing in this SPM. The examples used in the TAR Synthesis Report are CO <sub>2</sub> and surface temperature. The full comparison of past and present data, with future projections should be made for other quantities as well, such as those shown in SPM-3 in the present report: sea level and snow cover. [Govt. of United States of America (Reviewer's comment ID #: 2023-698)]	See SPM-1. Text has been edited for clarity. Combination of past, present, and future was discussed. Consensus of author team was that this would not be appropriate here. Among other issues, authors of this report feel the present presentation style conveys the uncertainties better than could be achieved by mixing observations and projections, to avoid 'implied' attribution where it has not been proven.
SPM-64	A	0:0	2) Errors of Omission. There are numerous aspects of the report which would be	WG1 does not cover impacts; these are in WG2 so

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			<p>of great interest to policymakers, yet somehow have been omitted. Table SPM-1, which is summarizing some of the important elements of what is new, is somewhat confusing and it omits major new scientific understandings, with little or no justification for why particular elements in the table have been chosen for inclusion. Somehow the loss of sea ice in the Arctic has been neglected, and benign aspects of climate change, like the longer growing season, doesn't make it in either. It is unclear what the basis is for selecting various aspects of projected change to report on, e.g., precipitation patterns are described, but information on projected changes in heavy and extreme precipitation events is not included. A list of some missing candidates follows:            [Govt. of United States of America (Reviewer's comment ID #: 2023-699)]</p>	<p>although frost-free days are mentioned more detail on benign aspects of climate change are found elsewhere (WG2). Table SPM-1 is on extremes, not all climate change such as sea ice. Will edit text to make purpose of table clearer.</p>
SPM-65	A	0:0	<ul style="list-style-type: none"> <li>• sea ice melting and tundra thawing</li> </ul> [Govt. of United States of America (Reviewer's comment ID #: 2023-700)]	In text. Not in table because these are not extremes
SPM-66	A	0:0	<ul style="list-style-type: none"> <li>• ecosystem movements and impact</li> </ul> [Govt. of United States of America (Reviewer's comment ID #: 2023-701)]	Not WG1. See WG2 report
SPM-67	A	0:0	<ul style="list-style-type: none"> <li>• hurricane intensity increase</li> </ul> [Govt. of United States of America (Reviewer's comment ID #: 2023-702)]	In text and table
SPM-68	A	0:0	<ul style="list-style-type: none"> <li>• sea level increase</li> </ul> [Govt. of United States of America (Reviewer's comment ID #: 2023-703)]	In text, not table where not extremes
SPM-69	A	0:0	<ul style="list-style-type: none"> <li>• increased rainfall intensity</li> </ul> [Govt. of United States of America (Reviewer's comment ID #: 2023-704)]	In both text and table
SPM-70	A	0:0	<ul style="list-style-type: none"> <li>• increased temperatures</li> </ul> [Govt. of United States of America (Reviewer's comment ID #: 2023-705)]	In text where appropriate and in table where appropriate
SPM-71	A	0:0	<ul style="list-style-type: none"> <li>• land temperatures</li> </ul> [Govt. of United States of America (Reviewer's comment ID #: 2023-706)]	Not an extreme. In text
SPM-72	A	0:0	<ul style="list-style-type: none"> <li>• warmer highs and lows</li> </ul> [Govt. of United States of America (Reviewer's comment ID #: 2023-707)]	In table
SPM-73	A	0:0	<ul style="list-style-type: none"> <li>• ocean temperatures</li> </ul> [Govt. of United States of America (Reviewer's comment ID #: 2023-708)]	In text, not an extreme
SPM-74	A	0:0	<ul style="list-style-type: none"> <li>• change in storm tracks for mid-latitude cyclones.</li> </ul> [Govt. of United States of America (Reviewer's comment ID #: 2023-709)]	In text as appropriate
SPM-75	A	0:0	<p>Chapter 11 has been touted in the main report itself as representing the first time we are capable of providing even an estimate of regional changes; somehow this point, and everything from Chapter 11, has been omitted. Also missing are things scientists are now less certain of, such as the magnitude of past solar variations, a development, which is of great importance with its implications for our ability to</p>	<p>Disagree re. solar and little ice age – covered here.            Text edited to make points clearer.</p>

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			<p>explain not only the Little Ice Age, and perhaps a Medieval Warm Period, but the warming of the first part of the 20th century. One wonders how much thought has been put into reconciling the real achievements discussed in the overall document with what has gotten into the SPM.                      [Govt. of United States of America (Reviewer’s comment ID #: 2023-710)]</p>	
SPM-76	A	0:0	<p>3) Errors of Commission. By concentrating on Greenland's contribution, this report leaves the impression that sea level during the previous interglacial was 2 to 3 m above current day values, when the chapter clearly states it was 4 to 6 m - a very important difference. Chapter 6, the paleo chapter, recognizes that there are many uncertainties concerning how orbital variations are connected to ice ages, yet the SPM makes it seem like a solved problem, and the associated 'radiative forcing' that is supposed to compare with that driving climate change, but is of a much different nature (latitudinal and seasonal variation, rather than global) than that of the today -- a possibly significant difference that is not mentioned. The scientific community still has little confidence in upper tropospheric water vapor trends, yet a comment is made that they are consistent with the observed warming (which is also, for this region of the atmosphere, poorly constrained). There are other examples, but it is not only that these points are incorrect, they are also inconsistent with the detailed discussion in the individual chapters, which will undoubtedly be pointed out by motivated readers.                      [Govt. of United States of America (Reviewer’s comment ID #: 2023-711)]</p>	<p>Text edited.                       Disagree re upper trop water; see chapter.</p>
SPM-77	A	0:0	<p>4) Premature Conclusions. There are debates or uncertainties within the chapters that accurately represent scientific debate that has not yet been resolved, but in a number of cases one particular point of view has worked its way into the SPM. For example, Chapter 3 provides a land temperature change for 1850-1910 that is very different from the SST change (which dominates the global average) given in Chapter 1. However, after 1910, the land and ocean records are very similar; do we really have good ocean temperature reconstructions for the earlier time period, globally? Before using such a controversial figure in the SPM, this should have been evaluated closely. The observations chapters say Antarctica is currently a source for sea-level rise, while the future projections have it as a sink; the SPM ignores the current assessment in favor of future modeling studies without mentioning this contradiction. The modeling studies that indicate reduced tropical storm activity in the future are very uncertain, given that models do not have the proper resolution to resolve such storms, yet somehow this very important conclusion is given credence in the report and the SPM. There is also a disconnect between Chapter 4’s observational recognition that the major ice sheets are already making a net contribution to sea level, and Chapter 10’s</p>	<p>Reviewer is incorrect re ice sheets, no chapter says Antarctica is definitely a source currently (it could be within error bars, see chapter 4). Text has been edited re. future sea level.                       Basis for temperature data is Brohan et al., which is consistent with other datasets as discussed in ch 3, where uncertainty discussion can be found in detail.                       Uncertainties in projections and differences to observations are clearly stated re tropical cyclones.</p>

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			<p>projection that they will not do so until after the year 2100. It appears that the desire to provide proof of 'progress' has led to a tendency to give conclusions in areas that are not yet decided, without the proper scrutiny.                      [Govt. of United States of America (Reviewer's comment ID #: 2023-712)]</p>	
SPM-78	A	0:0	<p>The overall impression is that, for whatever reason, an insufficient amount of time has been devoted to the SPM to date. With additional time and attention, the U.S. Government is sure that this document will be brought up to the standards set by the previous IPCC SPM documents. A recommendation is to study TAR and SAR summaries for guidance on how to improve the current draft and incorporate some of the needed perspective.                      [Govt. of United States of America (Reviewer's comment ID #: 2023-713)]</p>	Noted
SPM-79	A	0:0	<p>A best estimate and a range should be given in the SPM about the residence time of CO2 in the atmosphere. The caption to Figure SPM-2 states "No CO2 time scale is given as its removal from the atmosphere ... cannot be expressed with a single lifetime." This is correct as stated and appropriate for the figure caption. Elsewhere in the SPM re the multiple CO2 lifetimes, it is suggested that the IPCC provide exact numbers and "likely" qualifiers, as appropriate.                      [Govt. of United States of America (Reviewer's comment ID #: 2023-714)]</p>	Text edited, see chapters 2 and 7 for detailed information.
SPM-80	A	0:0	<p>Indicate for each appropriate item in the SPM whether the data summary represents or suggests new, different, additional, confirming, more robust data, evidence, or support for items discussed in the Third Assessment Report (TAR). The point is to make it easy for policymakers and readers to see and appreciate what new data have been obtained and evaluated since TAR publication in 2001. In each subsection, the first comments should indicate what has been learned since the TAR and to go beyond the very general, high level and sometimes lacking italicized statements currently in the document. Overall, this SPM does not really highlight the important new climate research and understanding of the last 5 years. Other than the probabilities of warming (Figure SPM-5), the policymakers could just use the TAR. Moving beyond the hockey stick and reconciling upper atmosphere and surface temperature trends both seem to be excellent candidates for the SPM authors to consider highlighting.                      [Govt. of United States of America (Reviewer's comment ID #: 2023-715)]</p>	SEE COMMENT SPM-1
SPM-81	A	0:0	<p>The SPM should clearly define uncertainties whenever they are used. This is done in the Technical Summary, but the SPM needs to be a standalone document, since it is often the only part of the report that is read.                      [Govt. of United States of America (Reviewer's comment ID #: 2023-716)]</p>	SEE COMMENT SPM-19
SPM-82	A	0:0	<p>All figures need to be pulled from the chapters or directly traceable to elements</p>	Rejected. All material in these diagrams is traceable to

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			of chapter graphics. No new plots should be introduced into the summary documents. [Govt. of United States of America (Reviewer's comment ID #: 2023-717)]	the chapters and the sources are given, but new plots synthesizing across chapters are important for an SPM
SPM-83	A	0:0	All time frames need to be explicitly stated. For instance, "last 50 years" should be "since 1950" to anchor in time, tying the summary document to the TAR time frame and preventing a shifting window. [Govt. of United States of America (Reviewer's comment ID #: 2023-718)]	Text clarified
SPM-84	A	0:0	There are strong conclusions in the SPM (shown in bold) relating human activities to climate warming (a forcing), forcings to observed temperature changes, and "an increasing body of evidence suggests a discernable human influence on other aspects of climate change. What I miss is a clear statement linking human activities to observed temperature changes (all the ingredients are given). [Guus Velders (Reviewer's comment ID #: 276-5)]	Text clarified
SPM-85	A	0:0	I want to complement you, Susan, Dahe and Martin on a very strong first draft of the SPM. Figures SPM-1, -3 and -4 tell a straightforward and convincing story. I enjoyed reading it. [Guus Velders (Reviewer's comment ID #: 276-6)]	Thank you
SPM-86	A	0:0	It seems to me that there ought to be mention of observed and projected trends in the frequency and magnitude of El Nino events and other modes of variability, and trends in the strength of monsoons, especially since these were discussed in the SPM for the Third Assessment Report. [James S. Wang (Reviewer's comment ID #: 281-1)]	Insufficient length here. Dealt with in chapters.
SPM-87	A	0:0	The Summary for Policy Makers would be better served if it was ordered more like a press release. Key statements at the very start, with lesser details below. In other words, the key 1, or maybe 2, broad messages need to be delivered in the first couple of lines. (eg "Observed climate change can be attributed to human activities over the industrial period. Climate will continue to change due to these influences for at least the remainder of the 21st century.") Secondary messages should appear in subsequent paragraphs. Such a cascade of importance will greatly strengthen the document; currently key messages appear buried in the SPM. The key point text could be highlighted. [Andrew Watkins (Reviewer's comment ID #: 283-5)]	See SPM-1
SPM-88	A	1:1	(placement optional but should be somewhere near the top) - what you really need is a "summary of the summary" - in other words rather than just diving straight into bullets you need to step back and summarize in the most general way something like "since TAR there has been a substantial amount of	See SPM-1

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			evidence indicating ever-more convincingly as to the reality of global warming and its detection in a myriad of fields (state a few). models are converging more convincingly onto a non-insignificant (terrible phrase) climate sensitivity, and planetary/continental scale projections can in many cases be made with more confidence (true?). important new results are that the acidity of the ocean is increasing, with potential serious complications for coral reef ecosystems, the Greenland Ice Sheet is more likely to undergo significant meltback in the next few hundred years, and the likelihood of a collapse of the North Atlantic "conveyor" do not appear to be as great as previously thought. although still fraught with uncertainties, there is a tilt towards greater acceptance of the likelihood of more severe hurricanes and tropical storms in a greenhouse world. [Thomas Crowley (Reviewer's comment ID #: 51-12)]	
SPM-89	A	1:1	CONTINUED) uncertainty there is a greater tendency to accept the likelihood of more severe hurricanes in ghg world. WHAT YOU NEED TO DO HERE IS COMPILE THE QUINTESSENCE - THE ESSENCE OF TH ESSENCE that does not necessarily have to be bulletized but could actually be summarized in a normal english paragraph - how novel! [Thomas Crowley (Reviewer's comment ID #: 51-13)]	See SPM-1
SPM-90	A	1:1	policymakers may also want to know what goes next - why do a FAR - may follow up with that too [Thomas Crowley (Reviewer's comment ID #: 51-14)]	Covered in TS and chapters in appropriate detail. Not appropriate here.
0-89	A	3:0	SPM: The footnote to radiative forcing uses the term "a factor" which seems rather vague. It could be changed to "a component of the climate system" or use a specific example (e.g. carbon dioxide) [Richard Allan (Reviewer's comment ID #: 3-54)]	See glossary
SPM-91	A	3:0	2. I suggest including much of the content of the first two paragraphs of the Technical Summary (Section TS.1, Page TS-3 – lines 3 through 19) in the Introduction to the Summary for Policymakers. The first two paragraphs of the Technical Summary provide historical context that I think would be useful for a reader who reads the Summary for Policy Makers as a stand-alone document, without reading other parts of the IPCC report. [Wilmer Anderson (Reviewer's comment ID #: 5-63)]	Text edited in introduction. Text here must be shorter, but have tried to retain key points.
SPM-92	A	3:0	An explanation of 'likelihood' and 'confidence' terminology is needed near the beginning of this Summary. Eg the terminology tables from Box TS1.1 (TS3-4) could be added as a footnote and should be adhered to throughout the report. [Govt. of Australia (Reviewer's comment ID #: 2001-2)]	Accepted
SPM-93	A	3:0	Fig SPM-1: Apart from the LGM grey bars still being present and the dodgy y	Accepted

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				axis this figure is good. However, I don't really see the need for the rate of change panel, it's not discussed in the text and it adds a number of lines to the figure caption [Piers Forster (Reviewer's comment ID #: 73-5)]	
SPM-94	A	3:0		The Introduction is not really a true introduction. It should be rewritten to encapsulate the top "take-away" points from the SPM, probably tracking the headers of the various sections [Timothy H. Profeta (Reviewer's comment ID #: 203-3)]	Text edited. But see headlines for key messages, avoiding 'summaries of the summaries'
SPM-95	A	3:0		The Introduction could benefit from two other items: (1) a statement for a lay audience about the scientific process and how to evaluate the statements of uncertainty, constant questioning of hypotheses, etc.; and (2) a statement about the process by which the SPM was derived, which would anticipate and address similar questions that arose in the SPM for the TAR. [Timothy H. Profeta (Reviewer's comment ID #: 203-4)]	Not practical given length limitations.
SPM-96	A	3:0		Footnote 2. It might be useful to repeat here what the current glossary entry says, that if the change occurs over a specific period of time (or is relative to some specific baseline period - often 1750), then this period should (must be?) be specified. [Andy Reisinger (Reviewer's comment ID #: 210-6)]	See glossary. Can't repeat glossary here. Footnote refers to glossary
SPM-97	A	3:1	3:16	Add a paragraph on what progress has been made since AR3. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-95)]	See SPM-1
SPM-98	A	3:1	6:8	I want to give my endorsement to the approach, scope and wording of the RF discussion. Don't feel pressure to alter it too much, it's pretty nearly there in my own mind. I would specifically like to add my defense to a couple of points which you may come under pressure to alter. 1) Contrails. You may come under pressure to add discussion of these to the SPM, I would resist this as their significance is totally dependent on projections of air traffic growth which the report doesn't cover, excluding this aspect means their RF is tiny. 2) Likewise I would resist suggestions to expand the forcing discussion to other small uncertain RFs. Not mentioning stratospheric ozone (e.g.) is perfectly ok in my mind. 3) adding the pdf to the forcing diagram is not really necessary in my mind. You would need substantial amounts of text to explain the pdf and caveat it properly. In the current draft line 1 and lines 7-8 of page 5 do a very good job at getting this message across. [Piers Forster (Reviewer's comment ID #: 73-6)]	-  Thank you. Contrails, stratospheric ozone retained (policy implications). Small uncertain RFs- discussion not expanded, pdf diagram not added.
SPM-99	A	3:3	3:10	Invert the first two paragraphs. The introduction needs to say first what the SPM is about and then how it has been put together.	Text edited

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				[Govt. of United Kingdom (Reviewer's comment ID #: 2022-94)]	
SPM-100	A	3:3	199:54	Please generate a section entitled ``Acronyms'', which contains all acronyms used in this Report. Since acronyms are widely used in this Report, this section should help readers to find necessary acronym quickly. [Michael Danilin (Reviewer's comment ID #: 55-4)]	A list of acronyms will be added at the end of the report.
SPM-101	A	3:4	3:4	Footnote Number 1. I have argued in the "General" section that the term "climate change" should be abandoned as it is proposed that it should have two different meanings leading to unfortunate confusion with the public and even many scientists. I suggest that the entire Report be titled "Climate Science". The footnote should deal with the subject matter to be dealt with. The one suggested is unsatisfactory. What is meant by "over time" Does this mean, over ANY period, even millions of years, or would you restrict it?. Then why is "natural" change regarded as only possessing "variability" rather than "change"? "Human activities obviously include changes caused by humans in atmospheric greenhouse gas concentrations, but it should be made plain that it also includes "human activities" that modify the climate which are unrelated to greenhouse gases, such as urban development, energy emissions, land and water use changes, and agriculture and forestry. Instead of a footnote the scope of the study should be up front at the beginning of the Report, A suggested wording is "This Report summarises and assesses scientific evidence and results of all changes of the climate, including those which are essentially natural and those which are influenced by humans. It attempts to assess in particular the possible cosequences to the climate of changes in atmospheric concentration of so-called greenhouse gases, whether these have human or natural origin. [VINCENT GRAY (Reviewer's comment ID #: 88-2134)]	Rejected. GHG are well established, they are not 'so called GHGs'. Believe text is clear regarding definitions used and the balance of discussion re forcing terms is also clear.
SPM-102	A	3:7	3:7	SPM comment: The statement is misleading. Replace it with: "This Summary for Policymakers (SPM) describes the current state of understanding ..." with "This Summary for Policymakers (SPM) describes some of the current state of understanding ..."  [Richard Courtney (Reviewer's comment ID #: 49-1)]	Rejected. No basis given.
SPM-103	A	3:8	3:8	Add "may" before "cause climate change" [Govt. of France (Reviewer's comment ID #: 2010-97)]	Text edited
SPM-104	A	3:8	3:8	Replace "cause climate change" with "influence changes in the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-2135)]	Text edited

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SPM-105	A	3:8	3:8	Expand text to "...cause climate change; it summarises observed changes of climate and our progress in attributing the changes to specific causes; and it provides..." [David Parker (Reviewer's comment ID #: 195-119)]	Text edited
SPM-106	A	3:8	3:	Replace "its projected evolution" with "the projected future evolution of the climate system" to remove any ambiguity of what the "its" refers to in this sentence. [Andy Reisinger (Reviewer's comment ID #: 210-2)]	Text edited
SPM-107	A	3:8		Change the word 'evolution' for 'trayjectory' [Stephen J. Hawkins (Reviewer's comment ID #: 102-3)]	Rejected, believe current text is clearer
SPM-109	A	3:10	3:10	Add a sentence such as " Uncertain values are given in the form mean plus or minus 1 standard deviation (65% probability range)" [Govt. of France (Reviewer's comment ID #: 2010-98)]	See footnote
SPM-110	A	3:10	3:10	Delete "substantive". [David Parker (Reviewer's comment ID #: 195-120)]	edited
SPM-111	A	3:12	3:14	The link to the TAR should be more specific because the handling of uncertainty has not been consistent across Working Groups. [Govt. of Austria (Reviewer's comment ID #: 2002-3)]	See footnote
SPM-112	A	3:12	3:14	"Likelihood terminology needs to be briefly given here. It is difficult to read this document with the various ""very likely"", etc. without having an explanation of the confidence given. Suggest having a small box at this point including the Likelihood terminology like that starting on TS-4, line 34. Alternately, could include in parantheses after each occurrence the likelihood, e.g., ""very likely (>90% probablility)."  [Govt. of Canada (Reviewer's comment ID #: 2004-6)]	See footnote
SPM-113	A	3:12	3:14	The link to the TAR should be more specific because the handling of uncertainty has not been consistent across Working Groups. [Klaus Radunsky (Reviewer's comment ID #: 204-3)]	SEE COMMENT SPM-111
SPM-114	A	3:14	3:14	The reference to Box TS-1 is useful, but it might still be helpful to have a footnote here (as we ended up with in the TAR) that gives the quantitative translation of likelihood statements, i.e. "likely" means >66% probability etc. The SPM needs to be able to stand on its own. [Andy Reisinger (Reviewer's comment ID #: 210-3)]	accepted
SPM-115	A	3:15		We suggest an introductory paragraph would be helpful at this point to summarise the key areas of progress since the AR3 (or TAR!) . We suggest something along the lines of :	Rejected. See headlines, where such material is appropriately connected to supporting material.

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				<p>“Since the First Assessment Report (AR1) of the IPCC (1990), our understanding and quantification of climate processes has grown considerably. Since the Third assessment Report (AR3, 2001) observations of the climate system have been improved and extended, with a broader geographical coverage [from SPM6, 27-29] and a wider variety of measurements. Discrepancies between observed temperature trends at the earth’s surface and the lower atmosphere have largely been resolved. Evidence for a human contribution to recent climate change has strengthened further and there is increasing evidence of human influence on specific aspects of climate change including land and sea ice, heat waves and other extremes, atmospheric circulation, storm tracks and precipitation. [SPM11, 8-9] Model simulations of many aspects of climate and its variability have also improved and the increase in the number of model predictions available provides a firmer basis for estimating the future climate change, including precipitation patterns.[SPM11, 42; SPM13, 37]. The expected range of temperature rise over the 21st century is the same as suggested in the AR3, but the range of climate sensitivity of the atmosphere to a doubling of CO2 concentrations has been reduced to between 2 and 4.5C. Although some uncertainties still remain, the broad picture of human influence on climate change, both in the past and for the future, is robust.”</p> <p>[Govt. of United Kingdom (Reviewer’s comment ID #: 2022-96)]</p>	
SPM-116	A	3:17	3:17	<p>Change title to "Human and Natural Drivers of Climate Change"</p> <p>[Govt. of United Kingdom (Reviewer’s comment ID #: 2022-97)]</p>	Accepted
SPM-117	A	3:19	3:20	<p>Absorption and re-emission of radiation' implies that only radiation energy absorbed is then emitted by the greenhouse gases and aerosols. In fact, the emission of longwave radiation by the greenhouse gases and aerosols in the atmosphere is a function of their concentration and temperature, and is independent of absorption. Suggest change 're-emission' to 'emission'.</p> <p>[Govt. of Australia (Reviewer’s comment ID #: 2001-3)]</p>	Accepted
SPM-118	A	3:19	3:20	<p>Consider to change "re-emission" to "scattering and emission".</p> <p>[Qiang Fu (Reviewer’s comment ID #: 78-2)]</p>	SEE COMMENT SPM-117
SPM-119	A	3:19	3:20	<p>absorption and re-emission of radiation' implies that only radiation energy absorbed is then emitted by the greenhouse gases and aerosols. In fact, the emission of longwave radiation by the greenhouse gases and aerosols in the atmosphere is a function of their concentration and temperature, it is independent of absorption, and for the atmosphere exceeds the absorption. Suggest change 're-emission' to 'emission'.</p> <p>[William Kininmonth (Reviewer’s comment ID #: 128-92)]</p>	SEE COMMENT SPM-117

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SPM-120	A	3:19	3:20	<p>“re-emission” is an incorrect statement of atmospheric physics. Greenhouse gases and aerosols absorb and emit longwave radiation according to their emissivity. The radiation emitted by greenhouse gases and aerosols depends on their temperature and their emissivity, and their temperature is a result of their energy balance. They do not “re-emit” the same longwave radiation they absorb. Their temperature depends on all the energy absorbed by the atmosphere and not just that from longwave, and the emission depends on all the terms in the energy balance. A more correct way to make this statement is to change “re-emission” to “emission.” -Alan Robock, Rutgers University [Alan Robock (Reviewer’s comment ID #: 217-1)]</p>	SEE COMMENT SPM-117
SPM-121	A	3:19	23:	<p>This seems a bit technical in style, could it perhaps be with footnote 2, which is incidentally a nice explanation. Alternatively, generalise the style, start with the first line of footnote 2 then talk about radiative forcing being changed by solar radiation (start with this natural factor perhaps), greenhouse gases and land surface properties [Joanna House (Reviewer’s comment ID #: 109-1)]</p>	Rejected – the preferred approach (see other review comments on this section) is to keep the logical flow from changes in forcing agents to the consequent RF.
SPM-122	A	3:19		<p>I found the material starting with [SPM-3 line 19] to be very choppy. It shifts back and forth between three concepts: the measured increases in long-lived greenhouse gases, the causes of those increases, and the radiative forcing from those changes. Also, four successive sentences [SPM-3 lines 25-34] each mention a different time interval. Finally, the structure is not parallel for the various gases. The increase from pre-industrial times to present is given for methane but not CO<sub>2</sub>. On the other hand, the absolute value of the recent growth rate is given for CO<sub>2</sub> but not methane. CO<sub>2</sub> and methane have their own bullets but N<sub>2</sub>O does not. [Daniel Murphy (Reviewer’s comment ID #: 183-2)]</p>	Accept with some changes
SPM-123	A	3:19		<p>{continuation} My suggestion is to (a) make simple, parallel bullets for the concentrations and sources of the three gases, with the most detail for CO<sub>2</sub> because it is most important, (b) move the radiative forcing sentences (in both italics and bold face) to the radiative forcing section starting with bold face on SPM-5 line 1, and (c) move the mention of changes in concentration at the last ice age to the paleoclimate section, with a mention there that the confidence in concentrations of these gases at those times is much higher than the reconstruction of the past climates. [Daniel Murphy (Reviewer’s comment ID #: 183-3)]</p>	Accept
SPM-124	A	3:19		<p>{continuation} Here is my suggestion. (Of course the IPCC should determine precise values and dates). This is not as an extensive rewrite as it might appear</p>	Accept

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			because it is mostly rearrangement of words taken from the draft text. [Daniel Murphy (Reviewer's comment ID #: 183-4)]	
SPM-125	A	3:19	{continuation} CHANGES IN HUMAN AND NATURAL DRIVERS OF CLIMATE /bold/Atmospheric concentrations of the greenhouse gases carbon dioxide, methane, and nitrous oxide are the highest experienced for at least 650,000 years. Observed increases in these gases compared to pre-industrial values, are directly linked to fossil fuel use, agriculture, land use change, and other human activities./bold/ • Carbon dioxide is the most important greenhouse gas. Its atmospheric concentration has increased from 277 ppm in 1750 to 380 ppm in 2004. The rate of change in the period 1999-2004 was more than 1.8 ppm yr-1, compared to xx ppm yr-1 for the period 1980-1999. {or other relevant period} [Daniel Murphy (Reviewer's comment ID #: 183-5)]	Accept
SPM-126	A	3:19	{continuation} * Carbon dioxide emissions due to fossil fuel use increased from 6.5 to 7.2 GtC yr-1 in the period 1999-2004. Carbon dioxide emissions associated with land use change are less well known but are estimated to have contributed 5 to 38% of its atmospheric growth in the 1990s. * {Summarize the various lifetimes of CO2: some prompt uptake, some deep ocean, remainder}. • The current atmospheric methane concentration is more than double its pre-industrial value. The rate of increase in the period 1999-2004 was small {value ???}, compared to xx ppm yr-1 for the period 1980-1999. {or other relevant period same as for CO2} [Daniel Murphy (Reviewer's comment ID #: 183-6)]	Accept
SPM-127	A	3:19	{continuation} * The sum of natural plus anthropogenic methane sources has not been increasing over the last two decades but the breakdown of changes in emissions into individual sources is not well determined. The atmospheric lifetime of methane is about xx years. * The nitrous oxide concentration has increased from 270 ppb in 1750 to 319 ppb in 2005. The rate of change during the last two decades has been roughly constant at xx ppb yr-1 [2.3.3]. The atmospheric lifetime of nitrous oxide is about xx years. [Daniel Murphy (Reviewer's comment ID #: 183-7)]	Taken into account. Reject comments on methane and nitrous oxide lifetimes as too detailed for the SPM.
SPM-128	A	3:19	{continuation}/bold/ Human activities since 1750 have very likely exerted a net warming influence on climate./bold/ In italics: The energy balance of the Earth is affected by changes in the	Accept with some re-wording

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				absorption, scattering, and re-emission of radiation within the atmosphere and at the Earth's surface. The resulting changes in energy balance are defined as radiative forcing <sup>1</sup> . Radiative forcing is used to compare natural and human factors that drive warming and cooling influences on global climate. {moved here from page SPM-3} * The radiative forcings of carbon dioxide...{move from SPM-5 line 4} * The sustained increase in radiative forcing over the past century due to carbon dioxide, methane, and nitrous oxide is unprecedented in at least the last 20,000 years {moved here from page SPM-3} * {continue here with SPM-5 line 21... SPM-6 line 7} [Daniel Murphy (Reviewer's comment ID #: 183-8)]	
SPM-129	A	3:21	3:22	Remove "solar radiation and". [Qiang Fu (Reviewer's comment ID #: 78-3)]	Taken into account
SPM-130	A	3:21	3:21	The term forcing is used in different ways in this SPM. In the footnote, I suggest that this term only be used for global, mean-annual radiative forcing, and that this be specified in the footnote. [Haroon Kheshgi (Reviewer's comment ID #: 125-1)]	RF is not always used as global and annual mean, but glossary definition will be changed to say this is the default. SPM footnote definition refers to glossary.
SPM-131	A	3:21		The term "radiative forcing" should be developed, and coupled with the concept of GWP. To explain these terms is vital to use them later, and is at least as important as the (by the way correct and understandable) definition of uncertainties. Suggest to move text from page 13, line 56 to page 14, line 15 and related graph and tables under a separate section at the beginning of TS2 (i.e. TS.2.1, and current TS.2.1. becomes TS.2.2 etc.). [Govt. of Hungary (Reviewer's comment ID #: 2012-2)]	RF definition will be re-considered for the SPM. Rest of this comment applies to the TS.
SPM-132	A	3:21	:22	Since there is a lot more than solar radiation and land surface properties, I suggest to delete the first part of the sentence starting with "Radiative forcing can also be determined.." Simplify to: "Radiative forcing can be estimated for a range of natural and human factors that might drive climate changes, examples are changes in solar radiation and land surface properties." [Govt. of Norway (Reviewer's comment ID #: 2018-3)]	Accept
SPM-133	A	3:22	3:22	Perhaps "certain" should be inserted before "land surface" to distinguish forcings from feedback and strange chapter 7 type stuff? -or you could say "land surface albedo", "surface albedo" or "land surface reflectance" [Piers Forster (Reviewer's comment ID #: 73-1)]	Accepted in part
SPM-134	A	3:22	3:22	"land surface properties" is perhaps a bit vague, many people will not know at all what it means. I suggest putting it in its own sentence and giving it a small amount of explanation. Land surface properties also affect climate other than	SEE COMMENT SPM-133. This suggestion would introduce a more complex set of topics than can be addressed in the SPM. The focus in

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				through radiative forcing, and this is perhaps also a point worth mentioning e.g. the whole vegetation - evapotranspiration - regional rainfall effect. (e.g. deforestation in deserts and rainforests reduces regional rainfall and the cooling during evapotranspiration) Finally, these climate effects tend to be more regional than global - I am not sure if these regional effects are in the IPCC realm, but they should be, at least as an important feedback effect with drought causing loss of vegetation reducing rainfall and exacerbating drought (see Millennium Assessment, House et al., chapter 13 conditions and trends, Victor Brovkin wrote a good summary of this). Suggest something like "Changes in the physical properties of the land surface (such as vegetation cover, snow and soil colour) affect radiative forcing by changing the reflectiveness to solar radiation (albedo). Furthermore, changes in vegetation cover affect recycling of water from soils to the atmosphere via pores in plant leaves (evapotranspiration), affecting regional rainfall patterns and temperature." [Joanna House (Reviewer's comment ID #: 109-2)]	this section is on radiative forcing as the primary driver or climate change.
SPM-135	A	3:22		Insert "it" after the second "and" and change "range" to "number" [Richard Soulen (Reviewer's comment ID #: 248-1)]	Text reworded but accept suggestion to use "range"
SPM-136	A	3:25	3:45	Comparisons of radiative forcing should be made to a consistent reference time (should this be 1750). WGI and WGII need to work out and be consistent [Govt. of Australia (Reviewer's comment ID #: 2001-4)]	WG1 will include 1750 in RF definition in footnote and in glossary.
SPM-137	A	3:25	3:26	Please indicate in which period "Atmospheric concentrations of the greenhouse gases .... are the highest experienced for at least 650,000 years". It is "pre-industrial period" or "the past century"? In line 27, "the past century" is used and in line 31, "pre-industrial" is used. [Govt. of China (Reviewer's comment ID #: 2006-6)]	Accepted – the word "now" is added for clarity.
SPM-138	A	3:25	3:28	SPM Comment: This paragraph is not true and relies on selective use of data. Replace with: "Although ice core analyses indicate that atmospheric concentrations of greenhouse gases carbon dioxide, methane and nitrous oxide are the highest experienced for at least 650,000 years (see Figure SPM-1), this is disputed by other proxy measurements, for example, stomata data. It should be noted that ice core data are inherently incapable of revealing high and low atmospheric concentrations of the gases. There are several reasons for this with the most notable being that gases diffuse from regions of high concentration in unsealed firn in the decades before the ice sealed, and high values of the gas concentrations measured in the ice cores are deleted from the data sets using the assumption that high values are 'biogenic artefacts'. Also, the diffusion reduces	Taken into account. The smoothing effect is considered in all statements made in SPM,TS and ch. 6. We agree with the fact that stomata record are high resolution records for some period of the Holocene. Such records show high variability and it has not been demonstrated that they represent a global atmospheric signal. Stomatal index is a biological index, also potentially influenced by temperature and humidity as well as local CO2 sources and sinks. The Law Dome ice core record covering the last 2 kyr has a 20yr gas resolution and show CO2 variations of

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				the observed rates of change to gas concentrations indicated by the ice core data. Stomata data do not suffer from these problems and indicate that the present atmospheric concentration of carbon dioxide and the recent rates of change to atmospheric concentration of carbon dioxide have repeatedly occurred in recent millennia.” [Richard Courtney (Reviewer’s comment ID #: 49-2)]	less than 10 ppmv Furthermore, ice core results are from different sites with different accumulation rates, temperature and, hence smoothing. The CO <sub>2</sub> and CH <sub>4</sub> ice core record has not been cleaned for high values (biogenic artefact,...). Measurements have been made by several labs around the world and they provide identical results.
SPM-139	A	3:25	3:25	Add at end "probably" [VINCENT GRAY (Reviewer’s comment ID #: 88-2136)]	Rejected – see SPM-138
SPM-140	A	3:25	3:26	The statement about GHG over the last 650kyr is strictly incorrect. There are no published N <sub>2</sub> O data for parts of the record (except for some Dome F results in a PhD thesis), and for methane with its short lifetime it is theoretically possible (although unlikely) that a big peak would not be seen in the low resolution records back 650kyr. Therefore it is dangerous to say that current values are higher, but it is correct to say that current values exceed the range of concentrations measured over the past 650 kyr. [Reto Knutti (Reviewer’s comment ID #: 133-40)]	Accepted
SPM-141	A	3:25	3:25	The reasons for the 650, 000 and 20,000 years (max extent of relevant data) should be stated, otherwise there is doubt as to why they are different [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-98)]	Accepted. It is specified that 17,000 years ago is the end of the last ice age, and the 20,000 year interval is no longer mentioned
SPM-142	A	3:26	3:26	Add after "years. "although the concentration of methane has currently stabilised" [VINCENT GRAY (Reviewer’s comment ID #: 88-2137)]	Accepted but this point is now dealt with in detail in a subsequent bullet point.
SPM-143	A	3:26	3:26	After "years" I would suggest adding "and very likely much longer." [Michael MacCracken (Reviewer’s comment ID #: 152-3)]	Rejected - This would seem to go beyond the direct evidence covered in chapter 6.
SPM-144	A	3:26	3:28	Add estimate of current atmospheric concentrations of ghgs [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-99)]	Accepted. Concentrations provided.
SPM-145	A	3:26		“experienced” is NOT the right word; try “observed in ice cores” [Govt. of United States of America (Reviewer’s comment ID #: 2023-719)]	Accepted. Reworded.
SPM-146	A	3:26		“sustained rate of increase” cannot be supported given the gaps in the ice core record and the diffusion (mixing of abundances over decade). Recommend merely deleting “sustained rate of” and retaining the word “increase”. [Govt. of United States of America (Reviewer’s comment ID #: 2023-720)]	Rejected. The resolution of the data is sufficient enough for the last 20 kyr. The smoothing effect has been taken into account (see ch. 6 and references cited there.)
SPM-147	A	3:27	3:27	"Figure SPM-1 does not go back 20,000 years (as line 27 says), although the original Fig 6.4 does. Prefer the original Figure in any case." [Govt. of Canada (Reviewer’s comment ID #: 2004-7)]	The time period will remain as is in figure, error in the caption will be corrected.

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SPM-148	A	3:27	3:27	It is not at all clear why this statement is limited to 20,000 years--is there anything happening at that particular time to provide a limit to the statement? I would urge somehow making clear that the rate of change in forcing has likely only been surpassed in the past when there have been major asteroid impacts, or give a better sense of how unique the present period is. [Michael MacCracken (Reviewer's comment ID #: 152-4)]	Noted, but explanation too lengthy for SPM. Figure captures present and is illustrative of past few millenia.
SPM-149	A	3:27		Rephrase to "over the 20th century" because we need to anchor *all* statements in time so that the document does not become dated or time periods shift before the assessment is off press. Be explicit. Do not use statements such as "past century" or "past 50 years". Bound the time frames. [Govt. of United States of America (Reviewer's comment ID #: 2023-721)]	Accepted
SPM-150	A	3:28	3:28	Add at end "But this does not mean that it might still be due to natural influences" [VINCENT GRAY (Reviewer's comment ID #: 88-2138)]	Rejected. Inconsistent with large body of scientific literature
SPM-151	A	3:28	3:28	The Figure SPM-1 is inappropriate here, should Fig TS-2 (rather than TS-1) have been inserted here ??? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-100)]	See comment SPM-152
SPM-152	A	3:28	3:28	Figure SPM-1 only goes back to 6 ka BC, but is used to illustrate/justify statements relating to longer periods of time (see comment above) [Govt. of United Kingdom (Reviewer's comment ID #: 2022-101)]	Figure timescale and text revised to match. The 10 ka timescale is preferred in the SPM because it allows the recent anthropogenic increases to be identified more clearly and these are the focus of this section of the SPM.
SPM-153	A	3:30	3:30	Clarify that increases are in concentrations. [Govt. of Canada (Reviewer's comment ID #: 2004-10)]	Accepted.
SPM-154	A	3:30	3:34	SPM Comment: Delete this paragraph that makes two statements which are both untrue. The statement that the rises "are directly linked to fossil fuel use ... and other human activities" is an assumption that is not supported by any available evidence. Although the assumption can be used to attribute the rises to human activity this only shows that it is possible that the assumption may be correct. It is equally valid to assume that the rises are natural as has been demonstrated by e.g. ref. Rorsch A, Thoenes D and Courtney RS, (E&E v10 no2 (2005). And the statement saying "The concentrations of these gases also increased ... were much slower than those in the last century. [2.3, 6.4]" is another use of selective data. It is supported by the ice core data but not the stomata data. It should be noted that ice core data are inherently incapable of revealing high and low atmospheric concentrations of the gases. There are several reasons for this with the most notable being that gases diffuse from regions of high concentration	Rejected. The attribution of CO <sub>2</sub> increases to fossil fuel emissions is amply demonstrated in a large body of literature. Natural sources can not explain either the change in isotopic ratios of CO <sub>2</sub> or the magnitude of changes observed. The limitations in using stomatal indices as proxies for global CO <sub>2</sub> are explained in response to comment SPM-138

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				<p>in unsealed firm in the decades before the ice sealed, and high values of the gas concentrations measured in the ice cores are deleted from the data sets using the assumption that high values are 'biogenic artefacts'. The diffusion also reduces the observed rates of change to gas concentrations indicated by the ice core data. Stomata data do not suffer from these problems and indicate that the recent rates of change to atmospheric concentration of carbon dioxide have repeatedly occurred in recent millennia and during transition from the last ice age.</p> <p>[Richard Courtney (Reviewer's comment ID #: 49-3)]</p>	
SPM-155	A	3:30	3:30	<p>Add at beginning "Most of the".</p> <p>[VINCENT GRAY (Reviewer's comment ID #: 88-2139)]</p>	Rejected. No reason given for suggested change
SPM-156	A	3:30	3:32	<p>"observed increases" in concentrations or emissions - you may initially argue both, but I guess it should be concentrations and you should say this explicitly because some may argue that CO2 emissions due to land use change have gone down or been a sink in some countries in recent years. Alternatively qualify land use change by saying "land use change (e.g. deforestation)</p> <p>[Joanna House (Reviewer's comment ID #: 109-3)]</p>	Accepted – see SPM-153
SPM-157	A	3:30	3:32	<p>The sentence as it stands, with the phrase "and their associated positive radiative forcing (warming effect)" included, is not strictly correct because radiative forcing is a property of greenhouse gases themselves and not linked to their source. It might be better to delete this phrase here, and add a separate sentence that states "The increased concentrations are associated with an increased radiative forcing (warming effect)."</p> <p>[Andy Reisinger (Reviewer's comment ID #: 210-4)]</p>	Text revised for other reasons.
SPM-158	A	3:30	:39	<p>two bullets concerning concentrations of CO2: We suggest deleting the last three lines of the first bullet, they take focus away from the clear increase. The total increase in CO2 since pre-industrial time should be mentioned directly and it should be stated more clearly that the increase is caused by humans (both are virtually true). We suggest the second bullet is started with the following: "It is virtually true that concentrations of CO2 have increased by 36 % since pre-industrial time and that the increase is caused by human activity."</p> <p>[Govt. of Norway (Reviewer's comment ID #: 2018-4)]</p>	Taken into account.
SPM-159	A	3:31	3:32	<p>Given that you are defining terms in footnotes, I would think you would want to define "fossil fuel use" in a footnote as well.</p> <p>[Michael MacCracken (Reviewer's comment ID #: 152-5)]</p>	A footnote will be used to elaborate the term fossil fuel use.
SPM-160	A	3:32	3:32	<p>"linked to" seems a bit weak, surely they are a direct result of these drivers</p> <p>[Joanna House (Reviewer's comment ID #: 109-4)]</p>	Accept – replace with "related"

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SPM-161	A	3:32	3:34	suggest qulaifying statement (obvious really I know but...) "...as the planet warmed DUE TO NATURAL DRIVERS,...." [Joanna House (Reviewer's comment ID #: 109-5)]	Reject due to length constraints
SPM-162	A	3:32	3:34	Its great to see the paleo perspective in here, but I think it is also helpful to point out how much higher concentration are now compared to the several (how many?) glacial-interglacial cycles, over the past 650,000 years mentioned in the headline. otherwise it sounds like the concentrations increased but it may be comparable to today, the whole point is we are way beyond any of these past interglacial highs. Even the headline just says we are hgiher but not by how much. I mean this is really dramatic stuff, suggested in figure 1 with the shaded bars, but not really made head-bangingly obvious. [Joanna House (Reviewer's comment ID #: 109-6)]	Noted; details in TS, inclusion of concentration values too detailed for SPM.
SPM-163	A	3:32	3:32	The term "human activites" is vague and seems to carry the same meaning as anthropogenic (forcing). Rationale: It is unclear to the policymaker whether or not these terms are interchangeable. Additionally, the reader is left to wonder if "human activities" include industrial emissions or only those produced by individual humans (i.e. driving a car). To avoid confusion, the use of this term should be clearly defined in the text. [Govt. of Japan (Reviewer's comment ID #: 2014-1)]	Accepted – will drop “other human activities” as being too ambiguous
SPM-164	A	3:32	3:34	Not only the rate of increase in concentration was smaller after the last ice age also the concentrations were much lower in the last centurt (see Fig 6.4) [Guus Velders (Reviewer's comment ID #: 276-1)]	This should be clear from the accompanying figure – no change made.
SPM-165	A	3:32		Land-use [Stephen J. Hawkins (Reviewer's comment ID #: 102-4)]	Accepted
SPM-166	A	3:32		In this paragraph we propose including the following text from the TS, which we think contains important additional information: “It is very likely that the average rate of increase in radiative forcing from these well-mixed greenhouse gases over the past decade is at least six times faster at present than at any time during the two millennia before the Industrial Era.  [Govt. of Norway (Reviewer's comment ID #: 2018-5)]	Accepted.
SPM-167	A	3:33	3:33	"The increase in concs. of GHGs at end of last ice age seems to have occurred over the period 17,000-10,000 years ago, not just 17,000 years ago. Suggest correcting the sentence, or rephrasing to say ""beginning about 17,000 years ago..." Again - need the longer timescale in the Figure to support this line as well, not the truncated timescale currently in Fig SPM-1. How did 17,000 BP increase compare to present increase? Need context for this."	Text is revised but would not be accurate to say “beginning” as suggested. See also SPM-152

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				[Govt. of Canada (Reviewer's comment ID #: 2004-9)]	
SPM-168	A	3:34	3:34	Replace "last century" with "20th century" to remove any ambiguity. [Andy Reisinger (Reviewer's comment ID #: 210-5)]	Accepted
SPM-169	A	3:34		"...rates of changes were much slower..." mention a magnitude of the rate! E.g. ...the warming rate was 7 times slower... [Govt. of Germany (Reviewer's comment ID #: 2011-63)]	Rejected as too detailed the comparable figure is given for total RF instead.
SPM-170	A	3:34		Insert the word "observed" after "slower than those". [Govt. of United States of America (Reviewer's comment ID #: 2023-722)]	Rejected – we think this is clearly implied already.
SPM-171	A	3:34		this line doesn't appear to described the extent of the difference between past and present trends in the warming clearly enough. Additional words could be added at the end of the sentence to clarify this: "and are not comparable to current trends." [Andrew Watkins (Reviewer's comment ID #: 283-1)]	This concern should be addressed by restructuring and explicit mention that the rate of increase in RF is 6 times higher than in the last 2000 yrs.
SPM-172	A	3:36	3:36	Sentence should read: Carbon dioxide concentrations in the troposphere ... [Wilmer Anderson (Reviewer's comment ID #: 5-64)]	Unnecessarily detailed for the SPM.
SPM-173	A	3:36	3:39	It would be welcome if also a comparison in absolute level as well as rates before the period 1999-2004 would be included in the text. [Govt. of Austria (Reviewer's comment ID #: 2002-4)]	Taken into account with rewording
SPM-174	A	3:36	3:39	"Would be nice if the time periods of reference were consistent in this paragraph (1999-2004 line 36, then 1990s for line 39). Also, what did the rate increase FROM, given that 1999-2004 is such a short period of time?"  [Govt. of Canada (Reviewer's comment ID #: 2004-11)]	Accepted
SPM-175	A	3:36	3:39	"This bullet is awkward as preceding and following bullet points are relative to pre-industrial yet this refers to more recent period (1999-2004). Could be improved by also including the rate of increase prior to 1999 to highlight how this rate may have changed."  [Govt. of Canada (Reviewer's comment ID #: 2004-12)]	Accepted
SPM-176	A	3:36	3:38	The increase in carbon dioxide concentrations in 1999-2004 should be compared with the change in the past. In line 41, the current methane concentrations are compared with the preindustrial values. It helps better understand the level of current GHGs change. Therefore, suggest to add a general sentence at the beginning that describes the comparison of current CO2 concentrations and the preindustrial value. At the same time, the second sentence of this sentence (line 37) should be deleted. Because the concentration is related to many sources (fossil fuel use, LULUCF, etc.), not the direct result of fossil fuel use.	Accepted and new wording introduced. Fossil fuel emissions now covered in a separate sentence.

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				[Govt. of China (Reviewer's comment ID #: 2006-7)]	
SPM-177	A	3:36	3:36	Four years is an insufficient time to judge trends. I suggest that you replace this with the trend of the past 30 years, which is an average of 1.51ppm/yr.. There is no significant evidence of a recent increase for this figure. It represents a linear increase of 0.41% a year [VINCENT GRAY (Reviewer's comment ID #: 88-2140)]	Partly accepted. Average growth rates for three periods cited now. The text does not refer to trends only to actual observed growth rates.
SPM-178	A	3:36	3:37	Replace from "increased" on line 36 to "global" on line 37 by "linearly by 1.51ppm/yr from 1975 to 2004; 0.41% a year" [VINCENT GRAY (Reviewer's comment ID #: 88-2141)]	See SPM-177
SPM-179	A	3:36	3:36	Insert "Average" at the beginning [VINCENT GRAY (Reviewer's comment ID #: 88-2142)]	Now using "Atmospheric concentration" which should cover this.
SPM-180	A	3:36	3:36	After "concentrations" insert "measured over the oceans" [VINCENT GRAY (Reviewer's comment ID #: 88-2143)]	Rejected; incorrect
SPM-181	A	3:36	3:39	I would like to see some quantification of concentration changes here e.g. current ppm, pre-industrial ppm and even glacial-interglacial range in the past. Actually lots of people look direct to the SPM to pull out such numbers quickly across the policy, science and education community, so I don't think this is too much detail. Figure 1 scale is hard to pick out precise numbers. [Joanna House (Reviewer's comment ID #: 109-7)]	Accepted text changed to include specific values.
SPM-182	A	3:36	3:38	Require elaboration of what specifically constitutes "fossil fuel use." [Govt. of Japan (Reviewer's comment ID #: 2014-2)]	Footnote added.  SEE COMMENT SPM-184
SPM-183	A	3:36	3:36	Suggest that "by more than 1.8 ppm" be replaced by "by 1.XX +/- 0.0X averaged over the period 1999-2004" and add a footnote on what measure of uncertainty (1 or 2 standard deviations or 90%?) is being used. This is one of the best constrained aspects of the climate system; reporting it in this way will convey the accuracy and meaning of this change. [Haroon Kheshgi (Reviewer's comment ID #: 125-2)]	After discussion with Chapter 2 it is decided that there is insufficient basis for citing a quantitative uncertainty.
SPM-184	A	3:36	3:39	The underlying chapter (Pg. 2-3, lines 42-43) indicates that the emissions cited here are from fossil fuel use, cement production, and gas flaring. Change the text to indicate that these emissions are from all three sources, not just fossil fuel use. [Jeff Kueter (Reviewer's comment ID #: 137-3)]	Footnote added.
SPM-185	A	3:36	3:36	Change "Carbon dioxide concentrations" to "The global average carbon dioxide concentration"--there is one global average value; her and elsewhere making this value plural needs to be avoided. [Michael MacCracken (Reviewer's comment ID #: 152-6)]	Will drop the plural but "global average" thought to be too detailed for the SPM.

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SPM-186	A	3:36	3:37	Change "2004 while its" to "2004. Its" to simplify the phrasing. In addition, it might be very useful here to have a footnote at the end of the sentence indicating that these numbers are for gigatonnes of carbon, whereas the policy community works with gigatonnes of CO <sub>2</sub> --so explain and give the conversion, right up front. [Michael MacCracken (Reviewer's comment ID #: 152-7)]	Not relevant as text modified in response to other comments.
SPM-187	A	3:36	3:39	It would be welcome if also a comparison in absolute level as well as rates before the period 1999-2004 would be included in the text. [Klaus Radunsky (Reviewer's comment ID #: 204-4)]	Accepted – values now provided
SPM-188	A	3:36	15:39	In order to avoid ambiguity, I suggest to show concentration in ppmv, ppbv, etc. instead of ppm and ppb, respectively. Otherwise, some people may decide that the shown mixing ratios are in mass mixing ratio instead of volume mixing ratio. If this suggestion will be accepted, this changes should be made through the whole Reepport. [Michael Danilin (Reviewer's comment ID #: 55-1)]	Rejected – values are cited throughout the relevant literature as mole fractions. So ppm is correct, see IUPAC recommendations.
SPM-189	A	3:36	:38	Split into two sentences. Place a period after "2004" because the two clauses are disconnected. Make the second clause a sentence. [Govt. of United States of America (Reviewer's comment ID #: 2023-723)]	Not relevant as text modified in response to other comments.
SPM-190	A	3:36	:39	The carbon budget is an appropriate item for the SPM, but these three questions are disconnected from the underlying context and the intent of the bullet is unclear. [Govt. of United States of America (Reviewer's comment ID #: 2023-724)]	Taken into account, text clarified
SPM-191	A	3:37	3:38	Change to: "...global emission due to fossil fuel use, cement production and gas flaring increase from 6.5 to 7.2 GtC/yr." to be consistent with the underlying text (Pg. 2-3, lines 42-43). [Lenny Bernstein (Reviewer's comment ID #: 20-2)]	Accepted. Dealt with in footnote.
SPM-192	A	3:37	3:37	Replace "6.5" by "4.6" [VINCENT GRAY (Reviewer's comment ID #: 88-2144)]	Rejected – no rationale given for change.
SPM-193	A	3:37	3:37	I am a bit confused, if ff emissions have increased from 6.5 to 7.2, presumably 7.2 relates to the 1999-2004 period, does the 6.5 refer to the TAR period 1990s? [Joanna House (Reviewer's comment ID #: 109-8)]	Accepted – text now specifies periods for emissions explicitly
SPM-194	A	3:37	3:37	I strongly suggest that the emission rates either be reported for years (2004) based on data from that year (including estimate of uncertainty) or deleted. Examination of the underlying text shows that this number is (surprisingly) simply an extrapolation of the past trend of CO <sub>2</sub> emissions, not based on data from 2004. If one uses energy statistics and an emission estimation methodology (as was done in the TAR) to estimate recent emissions based on for example BP	CH <sub>02</sub> and CH <sub>07</sub> use FF emissions for 2004 and 2005 from extrapolation of USDoE statistics using the BP statistics in the same way as done in the TAR. Cement is included in the extrapolation on the assumption that it changes in the same proportion as fossil fuels but the error associated with this is small compared to the ~5%

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				energy statistics, then one would arrive at a higher estimate (~7.6) for 2004. Additionally, these numbers do not report emissions from cement as has been past practice in IPCC, or an uncertainty. Additionally, past assessments have only highlighted averages (e.g. decade) of emissions which would diminish errors or variability found in a single year's data. Highlighting an extrapolation of emissions in the SPM and presenting it as fact could degrade the quality of this assessment, and I strongly recommend that this be addressed. [Haroon Kheshgi (Reviewer's comment ID #: 125-3)]	error in the FF numbers themselves. This is explained in Ch07
SPM-195	A	3:37		add "mainly" after "due": fossil fuel burning is but one cause [Govt. of Hungary (Reviewer's comment ID #: 2012-3)]	Text now avoids this issue
SPM-196	A	3:37	:38	Change to "...global emission due to fossil fuel use, cement production, and gas flaring increase from 6.5 to 7.2 Gt C yr-1" to be consistent with the underlying text (page 2-3, lines 42-43). [Govt. of United States of America (Reviewer's comment ID #: 2023-725)]	Accepted Dealt with in footnote
SPM-197	A	3:38	3:39	It may be more meaningful to quote the contribution of land use changes to the pre-industrial to present-day increase in CO2 concentration. This is a better constrained and more meaningful quantity than the contribution of land use changes to the recent growth rate. [Olivier Boucher (Reviewer's comment ID #: 27-1)]	There is no support in the chapters for the contribution of LUC from pre-industrial times being better constrained than recent values. The text will be changed to show the range of LUC emissions for the 1990s coming from detailed coverage in Ch07.
SPM-198	A	3:38	3:38	Insert after "Gt/yr", "over the period" [VINCENT GRAY (Reviewer's comment ID #: 88-2145)]	Now dealt with in modified text.
SPM-199	A	3:38	3:39	Presumably the 5 to 38% refers to atmospheric concentrations, not emissions which is what the rest of this bullet talks about. Suggest instead of "its atmospheric growth" could say "growth in atmospheric concentrations." [Joanna House (Reviewer's comment ID #: 109-9)]	SEE COMMENT SPM-197
SPM-200	A	3:38	3:39	Although the contribution to total concentration change is hard to estimate, you can say that the land was a net source of CO2 during the nineteenth and early 20th centuries, and became a sink sometime around the middle of the last century (in our millennium assessment chapter 13, Condition and trends House et al. we assigned this fact high certainty on expert judgement if that helps) [Joanna House (Reviewer's comment ID #: 109-10)]	Do not have comparable statements in this report - rejected
SPM-201	A	3:38	3:39	5 to 38% (CO2 emissions associated with land use change as a percent of total atmospheric emission growth in the 1990s) is a significant range. A short explanation about the reason for such a range is suggested. [Govt. of Japan (Reviewer's comment ID #: 2014-3)]	See SPM-197
SPM-	A	3:38	3:39	The meaning of "land use change" should be defined or examples given. The	Feel this better left to chapter 7 as any useful

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202				TAR stated "land use change...mostly deforestation." [Govt. of Japan (Reviewer's comment ID #: 2014-4)]	elaboration would be too long for the SPM
SPM-203	A	3:38	3:39	replace 'well' by 'precisely' and hence 38 by 40, given the uncertainty in land clearing [Michael Manton (Reviewer's comment ID #: 157-22)]	See SPM-197
SPM-204	A	3:38		We propose adding the following sentence from the TS to complete the picture: "Atmospheric CO2 concentrations increased by only 20 ppm over the 8,000 years prior to industrialization, and multi-decadal-to-centennial scale variations were less than 10 ppm. However, since 1750, its concentration has risen by over 100 ppm."  [Govt. of Norway (Reviewer's comment ID #: 2018-6)]	This point is instead captured graphically in Fig SPM-1.
SPM-205	A	3:39	3:39	Range of 5% to 38% for land use change CO2 emissions is exceptionally large when it is considered that this fraction is assessed including much larger amounts of fossil CO2 emissions. Suggests a more pessimistic term than 'less well known' is needed. [Govt. of Australia (Reviewer's comment ID #: 2001-5)]	Taken into account, actual values are now given. See SPM-197
SPM-206	A	3:39	3:39	"Its" is poorly expressed - presumably it actually refers to the gross growth in CO2 from fossil & LUC sources. If so, it should read '...5 to 38% of the total atmospheric growth of CO2 concentrations in the 1990s'. [Govt. of Australia (Reviewer's comment ID #: 2001-6)]	No longer relevant , re-worded
SPM-207	A	3:39	3:39	SPM Comment: Delete this paragraph or amend it to cover the period 1975-2004. The IPCC definition of climate is a minimum period of 30 years (lesser time is weather), and the SPM says (p3 lines 12-14) that the SPM uses "standard Terms" that "are consistent with the TAR". Hence, changes for the period 1999-2004 cannot be related to "climate" or "climate changes" according to the SPM's own definitions. [Richard Courtney (Reviewer's comment ID #: 49-4)]	Rejected. Does not pertain to atmospheric concentration measurements for well mixed gases.
SPM-208	A	3:39	3:39	After "contributed" insert "about" [VINCENT GRAY (Reviewer's comment ID #: 88-2146)]	See SPM-197
SPM-209	A	3:39	3:39	The phrasing here is confusing. Change "its atmospheric growth" to "the change in the CO2 concentration." And, personally, my view is that 38% is very likely to be too high; I would have thought one would be providing a central estimate and an indication of uncertainty, and also including an estimate of confidence using the lexicon set of words. [Michael MacCracken (Reviewer's comment ID #: 152-8)]	SEE COMMENT SPM-197
SPM-	A	3:39	3:39	Do the percentages refer to concentration or emission changes? Maybe such	See SPM-197

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210				details are unnecessary here. [Govt. of Sweden (Reviewer's comment ID #: 2020-1)]	
SPM-211	A	3:39	3:39	Explain why the contribution of land use change to CO2 atmospheric growth has a very large range (5 to 38%). [Govt. of United Kingdom (Reviewer's comment ID #: 2022-102)]	SEE COMMENT SPM-205
SPM-212	A	3:39		What is the source for the land use emission being 5 to 38% of atmospheric growth rate ? Table 7.3.1 gives 3.2 gtC/yr for the atmospheric growth rate and 0.5 to 2.8 GtC/yr emission from land use change. This does not give the 5 to 38%. Anyhow these relative numbers have little meaning, as the fossil fuel component would contribute to 200% (6.4GtC emission vs. 3.2 for the atmospheric growth rate). I would rather give absolute numbers for land use (as done for fossil above). [Pierre Friedlingstein (Reviewer's comment ID #: 77-42)]	SEE COMMENT SPM-205
SPM-213	A	3:39		"... contribute FROM 5% to 38% ..." (i.e., put units on both numbers). More importantly, this range must be defined: Is it an uncertainty? Is it just EITHER 5 or 38%? Please be explicit. Actually, the whole sentence is ambiguous. Correct it so that the reader need not interpret message on his/her own. [Govt. of United States of America (Reviewer's comment ID #: 2023-726)]	SEE COMMENT SPM-205
SPM-214	A	3:41	3:44	""""This period"" is not clear- is it ""over the past two decades"", or 1999-2004 (line 36). "  [Govt. of Canada (Reviewer's comment ID #: 2004-8)]	Text rewritten in response to other comments
SPM-215	A	3:41	3:42	SPM Comment: The paragraph needs to be replaced because it is grossly misleading. Its first sentence makes an assertion that, "Current levels of atmospheric methane concentration are more than double their preindustrial level." which is a fabrication that must be deleted. The only source of this assertion is the ice core data that is incapable of indicating whether this statement is true or not. And the second sentence is disingenuous: it cannot be known that "The sum of anthropogenic and natural emissions has not been increasing over this period" when "changes in emissions from different sources are not well determined". Ice cores cannot record high atmospheric methane concentrations that existed for less than ~160 years. This inability to record short-lived variations to atmospheric methane concentration results from the time the ice takes to solidify and seal. The ice is formed from snow that becomes firm as it solidifies to ice, and the FAR (IPCC 1990) reported that the firm takes 83 years to seal. Methane and each other gas will diffuse from regions of high concentration through	Rejected. CH <sub>4</sub> concentrations from ice cores at high accumulation sites such as Law Dome have temporal resolution comparable to the CH <sub>4</sub> lifetime so would show any global scale transient increases. In contrast to the reviewers assertions recent rapid rises in CH <sub>4</sub> can be seen in firm air measurements.

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				<p>sealing firm during the 83 years that the firm is permeable. Therefore, high concentrations of methane in the firm will reduce by diffusing to regions of lower concentration unless the high atmospheric methane concentration existed for more than ~160 years (diffusion can occur both up and down through firm that takes 83 years to seal).</p> <p>The present rapid variation of atmospheric methane concentration indicates that the ice cores are incapable of indicating whether the atmospheric methane was or was not higher in the past than now. This is especially important because the second sentence of the paragraph admits that “changes in emissions from different sources are not well determined”. Indeed, the very recent finding of methane emissions from trees demonstrates that this admission is correct.</p> <p>Replace the paragraph with;  “The growth rate of methane in the atmosphere has been declining and it seems that increase to atmospheric methane concentration may have recently ceased. Ice core data imply that the current levels of atmospheric methane concentration are more than double their preindustrial level. However, the present rapid variation of atmospheric methane concentration indicates that the ice cores are incapable of indicating whether the atmospheric methane is or is not higher than its preindustrial levels. This is because methane will diffuse from regions of high concentration in unsealed ice and the ice takes decades to seal. The changes in methane emissions from different natural and human sources are not well determined and, therefore, it is not known if the sum of anthropogenic and natural emissions has been increased or declined since the industrial revolution.”</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-5)]</p>	
SPM-216	A	3:41	3:42	<p>Again I would like to see quantification of values, pre-ind, current and glacial-interglacial range to give context and easy reference for IPCC users.</p> <p>[Joanna House (Reviewer’s comment ID #: 109-11)]</p>	Partly accepted. Values given for pre-industrial and current concentrations
SPM-217	A	3:41	3:44	<p>States, "Methane concentrations are more than double their preindustrial values." These values should be stated.</p> <p>[Govt. of Japan (Reviewer’s comment ID #: 2014-5)]</p>	Accepted, actual values now provided
SPM-218	A	3:41	3:41	<p>Change the start of the sentence as this should be referring to only one methane concentration. Say "The current concentration of methane in the atmosphere is more than ..."</p> <p>[Michael MacCracken (Reviewer’s comment ID #: 152-9)]</p>	This grammar style adopted in rewritten text.
SPM-219	A	3:41		<p>Pre-industrial</p> <p>[Stephen J. Hawkins (Reviewer’s comment ID #: 102-5)]</p>	Copy-editing will be done at a later stage.

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SPM-220	A	3:41	:44	The statement that the sum of anthropogenic plus natural sources of methane has not been increasing although the former has doubled in value implies that the latter has declined. These are extremely weak and dangling sentences that do not say much. The second one ends with a statement that is a bit too vague for an SPM and is not entirely supportable (internally inconsistent). Suggest deletion. [Govt. of United States of America (Reviewer's comment ID #: 2023-727)]	Clarified in rewritten text
SPM-221	A	3:42	3:44	"Awkard and unclear sentence about the sum of anthropogenic and natural methane sources." [Govt. of Canada (Reviewer's comment ID #: 2004-13)]	Clarified in rewritten text
SPM-222	A	3:42	3:42	A very short explanation for decline of CH <sub>4</sub> growth rate should be given, due to measurement improvements or reduction of emission (e.g. improvement of agricultural practice). [Govt. of China (Reviewer's comment ID #: 2006-8)]	Would require too much explanation for the SPM – covered in the TS/ Chapters
SPM-223	A	3:42	3:42	insert "to zero and may become negative" after "declined" [VINCENT GRAY (Reviewer's comment ID #: 88-2147)]	Rejected. This section is on observations not projections
SPM-224	A	3:42	3:43	I don't understand how you get a "sum" of "Sources", so delete from "The" in line 42 to "bu"t in line 43. Capital C for "changes" [VINCENT GRAY (Reviewer's comment ID #: 88-2148)]	Text rewritten in response to other comments
SPM-225	A	3:42	3:44	I think it is useful to give some indication of what the anthropogenic and natural sources and sinks are, and quantify them as far as possible to give policy makers the information on which to base decision making on how they might manage the whole greenhouse gas basket. [Joanna House (Reviewer's comment ID #: 109-12)]	Accepted partly – text added to note most emissions are anthropogenic. Chapter 7 does not give basis for going further.
SPM-226	A	3:42	3:44	This sentence is not consistent with the preceding sentence. The preceding sentence says the growth rate in emissions is changing--so then so must the emissions of methane. The next sentence then says that the total emissions over this period are not changing. Which is it--are emissions changing or not? [Michael MacCracken (Reviewer's comment ID #: 152-10)]	Rejected – reviewer makes a common mistake in equating growth rates with emissions. For constant emissions growth rates will decline to zero exponentially. Explanation too detailed for SPM, will be in TS/ Chapter.
SPM-227	A	3:42	3:43	This might be misinterpreted as "the number of the sources has not been increasing" [Rolf Müller (Reviewer's comment ID #: 181-1)]	Text has been clarified.
SPM-228	A	3:42	3:42	The relevance/meaning of the second statement "The sum of..." is not clear [Govt. of United Kingdom (Reviewer's comment ID #: 2022-103)]	Text has been changed
SPM-229	A	3:42	:43	Replace "growth rates" with "rates of increase". Replace "past two decades" with actual date range in years specified. [Govt. of United States of America (Reviewer's comment ID #: 2023-728)]	Terms "growth rate" and "two decades" are kept for consistency across chapters.

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SPM-230	A	3:43	3:44	This statement needs qualifying with a "likely" or "very likely" - I think it is premature to rule out that changes in methane lifetime may be responsible. [Keith Shine (Reviewer's comment ID #: 236-2)]	Rejected – Chapter 7 explains why lifetime changes are ruled out. However, no basis in literature for a likelihood statement.
SPM-231	A	3:45	3:45	There should be also a mention of N2O [Govt. of France (Reviewer's comment ID #: 2010-99)]	Accepted – text added.
SPM-232	A	3:45	3:45	Could do with a bullet on N2O perhaps too, fertiliser use is a big issue which is easy to address with sustainable farming policies. Or if there is really not too much to say on this, put it with the methane bullet. [Joanna House (Reviewer's comment ID #: 109-13)]	SEE COMMENT SPM-231
SPM-233	A	3:45		Should an additional footnote be added to define what "likely", "very likely" etc mean?? ie include the tables on page 4 of the Technical summary as footnotes in the SPM?? [Andrew Watkins (Reviewer's comment ID #: 283-4)]	Accepted – footnote will be added.
SPM-234	A	3:53		Add the body responsible in footer 1. [Stephen J. Hawkins (Reviewer's comment ID #: 102-6)]	Rejected – editorial styles will be similar to previous IPCC reports.
SPM-235	A	4:0	4:	The fourth figure should have some explanation for the abrupt and temporary decline in rate of change of radiative forcing around 1600-1700. [Govt. of Australia (Reviewer's comment ID #: 2001-7)]	Bottom panel of Figure has been removed.
SPM-236	A	4:0	4:	"A graph that plots changes in CO <sub>2</sub> e concentration in the atmosphere with changes in mean temperature over the past 420,000 years may make a strong link between GHGs and climate change. It would also highlight how climate change is usually observed over much longer periods of time than what we are observing now. Such a graph is in TAR WGI 2001."  [Govt. of Canada (Reviewer's comment ID #: 2004-14)]	Current figure retained. See SPM-152.
SPM-237	A	4:0	4:0	Figure SPM-1: Show the "CO <sub>2</sub> +CH <sub>4</sub> +N <sub>2</sub> O" label near vertical axis of the bottom panel of Figure SPM-1. [Michael Danilin (Reviewer's comment ID #: 55-2)]	Bottom panel has been deleted.
SPM-238	A	4:0	4:	The right hand caption on the three upper graphs should be W/m squared [VINCENT GRAY (Reviewer's comment ID #: 88-2149)]	Accepted – axis labelling will be fixed
SPM-239	A	4:0	4:	I had always thought that radiative forcing was a function of the logarithm of concentration, These graphs do not appear to bring this out. [VINCENT GRAY (Reviewer's comment ID #: 88-2150)]	The reviewer is correct for CO <sub>2</sub> . For CH <sub>4</sub> and N <sub>2</sub> O the RF is also non-linear in concentration which is why the right hand scales are non-linear
SPM-240	A	4:0	4:	Last Panel needs a y axis -- "Combined effect" or something similar. [Robert Nicholls (Reviewer's comment ID #: 191-2)]	Bottom panel has been deleted.
SPM-	A	4:0		In addition to including figure SPM-1, I suggest also including Figure 8 of the	Accepted partly – an insert showing more recent time

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241			<p>Technical Summary of the Third IPCC Assessment which illustrates the increases in carbon dioxide, methane, and nitrous oxide concentrations over the last 1000 years. This plot makes it easy to see that the rapid increases in the LLGHGs began at about the time of the industrial revolution. The figure also illustrates the agreement between the ice core data and atmospheric samples for periods where both types of data were taken.</p> <p>[Wilmer Anderson (Reviewer's comment ID #: 5-65)]</p>	will be added to each panel.
0-90	A	4:0	<p>SPM Figure SPM-1: Would this be more "readable" as a 2 column-2 row plot (e.g. make it square)?</p> <p>[Richard Allan (Reviewer's comment ID #: 3-55)]</p>	Bottom panel deleted – will retain the single column format
SPM-242	A	4:0	<p>The units on the right-hand side of the plots did not print well on my version.</p> <p>[Olivier Boucher (Reviewer's comment ID #: 27-2)]</p>	Axis labelling will be fixed.
SPM-243	A	4:0	<p>Figure SPM-1: This Figure is grossly misleading for two reasons. It stitches together different data sets derived from different sources using different methods, and thus wrongly implies the data sets are directly comparable. And, importantly, it ignores the severe limitations of the ice core data.</p> <p>Either delete Figure SPM-1 and its caption or, preferably,</p> <p>(a) amend its caption to state the limitations of ice core data and</p> <p>(b) add another figure that shows the limitations of ice core data with additional text explaining what it shows.</p> <p>For example, the additional figure could be Figure 2 of Proc Natl Acad Sci U S A. 2002 September 17; 99(19): 12011–12014, Rapid atmospheric CO<sub>2</sub> changes associated with the 8,200-years-B.P. cooling event, Friederike Wagner, Bent Aaby, and Henk Visscher, <a href="http://www.pubmedcentral.nih.gov/articlerender.fcgi?artid=129389">http://www.pubmedcentral.nih.gov/articlerender.fcgi?artid=129389</a></p> <p>This suggested additional Figure compares stomatal data with ice core data from the Taylor Dome for atmospheric carbon dioxide concentration for the period 8,700 to 6,800 calendar years BP. It shows that the stomatal data indicate a higher atmospheric carbon dioxide (CO<sub>2</sub>) level (up to 320+/-15 ppm) than the ice core data (all less than 270 ppm), and the stomatal data shows the extensive averaging (smoothing) which has occurred in the Taylor Dome ice core data</p> <p>This is a brief quote from the paper (see the paper for references):</p> <p>“The conventional iced-based concept of relatively stabilized CO<sub>2</sub> concentrations during the greater part of the Holocene is challenged increasingly by stomatal frequency analysis of fossil leaves (13–15). Species of C<sub>3</sub> plants are often characterized by a plastic phenotype capable of consistent adjustment of</p>	Rejected – see SPM-138.

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			<p>numbers of leaf stomata in response to changes in ambient CO<sub>2</sub> concentration (16–18). Identification of a CO<sub>2</sub>-sensitive gene involved in stomatal development in <i>Arabidopsis thaliana</i> demonstrates the genetic control of the response (19). As a corollary of this responsiveness, stomatal frequency analysis of fossil leaves enables the detection and quantification of atmospheric CO<sub>2</sub> changes at different time scales (14, 17–25).”</p> <p>It should be noted that ice core data are inherently incapable of revealing high and low atmospheric concentrations of the gases. There are several reasons for this with the most notable being that gases diffuse from regions of high concentration in unsealed firn in the decades before the ice sealed, and high values of the gas concentrations measured in the ice cores are deleted from the data sets using the assumption that high values are ‘biogenic artefacts’. Also, the diffusion reduces the observed rates of change to gas concentrations indicated by the ice core data. Stomata data do not suffer from these problems and indicate that the present atmospheric concentration of CO<sub>2</sub> and the recent rates of change to atmospheric concentration of CO<sub>2</sub> have repeatedly occurred in recent millennia.</p> <p>The stomata measurements are obtained from ancient plants. The leaves of plants adjust the sizes of their stomata with changing atmospheric CO<sub>2</sub> concentration and this permits the determination of past atmospheric CO<sub>2</sub> concentrations by analysis of leaves preserved, for example, in peat bogs. (e.g. Retallack (2001), Wagner et al. (2004), Kouwenberg et al. (2003)). The disagreement with the ice core data is clearly seen in all published studies of the stomata data. For example, as early as 1999 Wagner reported that studies of birch leaves indicated a rapid rise of atmospheric CO<sub>2</sub> concentration from 260 to 327 ppmv (which is similar to the rise in the twentieth century) from late Glacial to Holocene conditions. This ancient rise of 67 ppmv in atmospheric CO<sub>2</sub> concentration is indicated by the stomata data at a time when the ice core data indicate only 20 ppmv rise. (refs. Retallack G, Nature vol. 411 287 (2001), Wagener F, et al. Virtual Journal Geobiology, vol.3. Issue 9, Section 2B (2004), Kouwenberg et al. American Journal of Botany, 90, pp 610-619 (2003), Wagner F et al. Science vol. 284 p 92 (1999)).</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-6)]</p>	
SPM-244	A	4:0	<p>Better label bottom panel [Dennis Hartmann (Reviewer’s comment ID #: 100-6)]</p>	Bottom panel has been deleted.
SPM-245	A	4:0	<p>Figure SPM-1. The fourth graph needs axis labelling [Stephen J. Hawkins (Reviewer’s comment ID #: 102-7)]</p>	Bottom panel has been deleted.

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SPM-246	A	4:0	Figure SPM-1: These are great images, very striking. But I would also add panels for each gas that shows the glacial-interglacial context - the humps of max-mins with the huge shoot-up in present day - one for each gas to accompany the panels shown for the last 6000 years. These are really dramatic and much more exciting than just shading an axis which can be easily overlooked. Say how many ice ages covered by the last 650,000 years. Say why the records shown go back 6000 years. For the SPM just show the smoothed lines (you could shade to show the variability around this) - smoothed values is quite technical, and having all the points make these quite powerful figures a bit complicated. What are low accumulation and high accumulation ice cores? - presumably this is to do with the rate of growth, but this kind of detail seems unnecessary to me for the SPM. For the bottom panel the axis needs labelling as "radiative forcing" and use of $10$ to the power $-3$ is not a policy friendly term. I am not sure this bottom panel is really useful to the SPM as it is quite complicated to explain, for one thing you have gone from radiative forcing to rate of change in forcing. Perhaps this should be a separate figure if used at all. [Joanna House (Reviewer's comment ID #: 109-14)]	Bottom panel deleted; suggestions constitute too much detail for SPM. See comment SPM-152
SPM-247	A	4:0	Re. Figure SPM-1. This figure should include elemental nomenclature labels. Also, the bottom graph should be labelled "All greenhouse gases combined." [Govt. of Japan (Reviewer's comment ID #: 2014-6)]	Bottom panel has been deleted. Chemical formulae will not be used in the SPM as a matter of style.
SPM-248	A	4:0	Fig. SPM1: Axis labels are messed up. [Reto Knutti (Reviewer's comment ID #: 133-41)]	Bottom panel has been deleted.
SPM-249	A	4:0	Figure SPM-1. Delete bottom panel, as it plots a variable with different units from the others. Moreover it is too hard to explain for a SPM [Michael Manton (Reviewer's comment ID #: 157-23)]	Accepted
SPM-250	A	4:0	Abscissa caption should be revised and abscissa values (years) should be stated in a more conventional way [Richard Soulen (Reviewer's comment ID #: 248-2)]	Accepted – will use year before present
SPM-251	A	4:0	Consider eliminating the fourth, lowest graph. The upper three tell a powerful story. The fourth one isn't needed and is somewhat confusing because of the path of the black curve and the explanation in lines 8-10 of the caption that will probably be meaningless to most readers [Richard Soulen (Reviewer's comment ID #: 248-3)]	SEE COMMENT SPM-249
SPM-252	A	4:0	Figure SPM-1: Correct the Y-axis (There is an overlapping of some letters): First figure left side "(pp)m" change to "(ppm)"	Axis labelling will be fixed.

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				First, second and third figures, right side, put clear the units (W m <sup>-2</sup> ) Fourth figure, put clear the right side of the Y-axis. [Govt. of Spain (Reviewer's comment ID #: 2019-86)]	
SPM-254	A	4:0		Figure 1: Bottom panel hard to interpret for non-specialist. The level of detail included in the graphs is unnecessary. [Govt. of Sweden (Reviewer's comment ID #: 2020-2)]	SEE COMMENT SPM-249
SPM-255	A	4:0		Figure SPM-1: The black line of the radiative forcing is confusing. The caption of Fig 6.4 gives an explanation for the negative spike, while this caption mentions a hard to understand "high/low accumulation". I would suggest to remove the black line here, it doesn't add anything. [Guus Velders (Reviewer's comment ID #: 276-2)]	Bottom panel deleted
SPM-256	A	4:1	4:1	Repeats what is stated on page SPM-3, line 30-32. [Govt. of Japan (Reviewer's comment ID #: 2014-7)]	Text has been revised
SPM-257	A	4:1	4:1	The large vertical line around 1600 in the fourth panel (presumably showing forcing) seems totally out of context with the panels above, which is supposedly their sum. Are you sure this does not count some change in solar or volcanic forcing? Otherwise, that very unusual jump needs to be explained in the caption. [Michael MacCracken (Reviewer's comment ID #: 152-11)]	SEE COMMENT SPM-249
SPM-258	A	4:1	4:10	Figure SPM-1. What is the cause of the big negative spike in the change in total radiative forcing around 1600? It does not look credible given the uncertainties in the ice trace gas data. [Ronald Prinn (Reviewer's comment ID #: 202-9)]	SEE COMMENT SPM-249
SPM-259	A	4:1	4:10	SPM-1 is not very clear, needs better presentation of trends in the last 2000 years. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-104)]	Accepted – see SPM-241
SPM-260	A	4:1		Figure SPM-1: "Reformat this figure so each panel is bigger. Visually presents the year 2000 as a spike in radiative forcing due to the gas, rather than a gradual increase over 100 years."  [Govt. of Canada (Reviewer's comment ID #: 2004-15)]	Accepted – see SPM-241
SPM-261	A	4:1		In Figure SPM-1, put subtitles above the graphs to ease understanding of what is being presented without having to reference the caption. Consider a marker on the lower axis for the year 1750, as that is a critical marker year per the explanatory text. [Govt. of United States of America (Reviewer's comment ID #: 2023-729)]	Noted, will add subtitles in next draft
SPM-262	A	4:1		In Figure SPM-1, the gray bars on the left are meant to represent the glacial interglacial range of greenhouse gases. It implies that the abundance was higher	Reviewer appears to mistake concentrations at 6000 BC as representative of all Holocene. Grey shading

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				than the holocene. If so, this is BIG news and should be highlighted! If it is just an uncertainty bar, then it should be dropped as it implies higher greenhouse gases in the past. [Govt. of United States of America (Reviewer's comment ID #: 2023-730)]	will be removed to avoid this confusion.
SPM-263	A	4:1		In Figure SPM-1, the concept of rate of change of radiative forcing is being introduced here for the first time, without any connection to climate. There is no physical connection made between $dRF/dt$ and climate change. Moreover, the ability to take a derivative of the paleo record is very weak; this is clearly a poorly determined quantity. Either the figure shows annual records with noise (righthand side) or the authors have fit a very smooth curve through a paleo record that cannot possibly include annualized data (diffusion in the firm). This is misleading. All you show is a large derivative—which becomes a policy statement, not a scientific one. The bottom panel needs to be a simple linear sum of the top three, or deleted. [Govt. of United States of America (Reviewer's comment ID #: 2023-731)]	SEE COMMENT SPM-249 Accepted
SPM-264	A	4:3	4:10	Explanation for symbols used in graphic SPM-1 is missing [Govt. of Germany (Reviewer's comment ID #: 2011-219)]	Meaning of symbols was given in caption but figure will be simplified to avoid confusion.
SPM-265	A	4:3	4:4	It might be missleading for the readers that in the SPM and the figure caption it is stated that atmospheric concentrations are highest for the last 650000 years and only the last 8000 years are shown. Therefore my suggestion: "... in their combined radiative forcing for the last 8000yr. In the upper..." [Christoph, C. Raible (Reviewer's comment ID #: 207-1)]	Gray bars and reference to 650,000 years will be dropped to avoid this confusion.
SPM-266	A	4:6	4:7	To be precise, I suggest changing "over the past 650,000 years" to "from circa 650,000 years ago to circa 250 years ago." The grey bars do not reflect the changes in GHG concentrations from ~1750 to present. [Melinda Marquis (Reviewer's comment ID #: 162-97)]	See SPM-265
SPM-268	A	4:7		Change "since 1750" to "relative to 1750", as values are shown for years prior to 1750 as well as since 1750. [Adrian Simmons (Reviewer's comment ID #: 242-4)]	Accepted.
SPM-269	A	4:7		Replace "since" with "relative to" [Govt. of United States of America (Reviewer's comment ID #: 2023-732)]	Accepted
SPM-270	A	4:8	4:10	It might be useful to explain in mathematical terms the relationship between the upper three and the fourth panel (the rate of change is the first of the time dependence of radiation) [Govt. of Austria (Reviewer's comment ID #: 2002-5)]	Bottom panel has been deleted
SPM-	A	4:8	4:10	It might be useful to explain in mathematical terms the relationship between the	Bottom panel has been deleted

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271				upper three and the fourth panel (the rate of change is the first of the time dependence of radiation) [Klaus Radunsky (Reviewer's comment ID #: 204-5)]	
SPM-272	A	4:8	4:8	Many non-scientific people confuse absolute amounts with rates of change (or don't care too much about the difference). Unfortunately I don't have a clear suggestion for how to avoid this - just to note that you may want to check review comments carefully for any evidence of such confusion, and think of any ways to emphasise that you are talking about rate of change. This is especially important since the GHG concentrations are given as absolute amounts. [Andy Reisinger (Reviewer's comment ID #: 210-7)]	Bottom panel has been deleted
SPM-273	A	4:8	4:8	In caption to Fig SPM-1, emphasise "rate of change" in the final sentence (otherwise it's easily misinterpreted, raising unnecessary queries about the negative spikes in the blue curve) [Govt. of United Kingdom (Reviewer's comment ID #: 2022-105)]	Bottom panel has been deleted
SPM-274	A	4:8	:10	If the lowest graph is eliminated, eliminate the sentence beginning "The rate . . ." [Richard Soulen (Reviewer's comment ID #: 248-4)]	Bottom panel has been deleted and text has been deleted.
SPM-275	A	4:9	:10	Consider explaining what "low accumulation" and "high accumulation" mean, providing ranges to delineate low from high. [Govt. of United States of America (Reviewer's comment ID #: 2023-733)]	Bottom panel has been deleted and text has been deleted.
SPM-276	A	5:0	5:	The terms "cooling" and "warming" due to the radiative forcing should be added to the graphic [Govt. of Germany (Reviewer's comment ID #: 2011-220)]	The relation between positive and negative RF and warming/ cooling is covered in the text.
SPM-277	A	5:0		figure SPM-2: the last columb is titled as "scientific understanding". The "Guidance Notes for Lead Authors of the AR4 on Addressing Uncertainties" (July 2005) give explicit advice how to communicate qualitatively defined levels of understanding (see table 2 on page 3 of this paper): Please consider both the amount of evidence availabele in support of findings and the degree of consensus among experts on its interpretation. Therefore substitute the columb "scientific understanding" ba two new columbs: "Amount of evidence" and "Level of agreement or consensus" and give a qualitative judgement regarding both. [Govt. of Germany (Reviewer's comment ID #: 2011-67)]	The level of scientific understanding is based on two criteria: Evidence and Consensus (as defined in Chapter 2), presented here as a summary. Technical details too lengthy for SPM
SPM-278	A	5:0		The caption to the diagram should be W/metre squared. [VINCENT GRAY (Reviewer's comment ID #: 88-2157)]	Caption is correct
SPM-279	A	5:0		Figure SPM-2. Write 'CO2' on top of 'Long-lived greenhouse gases' and 'Other gases' under. In order to explain what three types of gases are included in the bracket. [Stephen J. Hawkins (Reviewer's comment ID #: 102-8)]	This does not seem to add any clarity to the figure and may make it harder to follow.

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SPM-280	A	5:0	Figure SPM-2. The label of the column 'spatial scale' should be on the top of the column, not at the bottom. [Stephen J. Hawkins (Reviewer's comment ID #: 102-9)]	This is a layout issue – can not fit everything on the top
SPM-281	A	5:0	Figure SPM-2. Expand the column 'scientific understanding' [Stephen J. Hawkins (Reviewer's comment ID #: 102-10)]	See SPM-280
SPM-282	A	5:0	Figures SPM-2: Need to say radiative forcing of emissions of these gases since 1750 as the radiative forcing of the total concentrations of these gases in the atmosphere is of course quite different.. [Joanna House (Reviewer's comment ID #: 109-18)]	Rejected. The RF arises only from the changes in <u>concentrations</u> of the gases
SPM-283	A	5:0	Figures SPM 2: Why are the CH4 N2O and halocarbons stacked in to one bar - i never understood this in the TAR either, I would rather see them separately then you can also show the uncertainty for each [Joanna House (Reviewer's comment ID #: 109-19)]	Rejected, gases grouped according to lifetime
SPM-284	A	5:0	Figures SPM 2: legend line 17. I wholeheartedly agree with not giving a lifetime for CO2, very wise and good explanation, I just suggest adding "...involves many processes that operate on timescales from minutes through to centuries, and cannot be expressed...." just to give the reader a bit more of a clue why it is so hard, and that some of the stuff really does stick around for a hell of a long time. [Joanna House (Reviewer's comment ID #: 109-20)]	Noted. But due to length constraints timescales will be dropped
SPM-285	A	5:0	Figure 2. It is hard to see the error bars within the dark blue negative shading - e.g. where does the error bar end within the "direct" and "indirect" aerosol boxes? [Chris Jones (Reviewer's comment ID #: 120-1)]	Colors will be improved.
SPM-286	A	5:0	Figure SPM-2. I understand why no timescale was provided for CO2, but it is such an essential policy question, it suffers from silence. Please include some indication of the scale, orders of magnitude. Also, given that the TAR contained a timescale, please provide an explanation for the change. [Timothy H. Profeta (Reviewer's comment ID #: 203-5)]	See SPM-284
SPM-287	A	5:0	Figure SPM-2. I recommend that within the figure, or in the text, you explain for a lay audience the significance of the timescale of a GHG. [Timothy H. Profeta (Reviewer's comment ID #: 203-6)]	Too detailed for the SPM – is covered in an FAQ in detail.
SPM-288	A	5:0	Figure SPM-2. The lifetime of some halocarbons is in the order of thousands of years, not just 10-100 years (Table 2.14), so the lifetime figure should be changed to 10-1000s years. [Andy Reisinger (Reviewer's comment ID #: 210-8)]	Taken into account in revised caption.

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SPM-289	A	5:0		Figure SPM-2. If you separate out natural and anthropogenic forcings in this figure, you may have to list volcanic aerosols separately under the "natural" group. Alternatively, you may want to state in the figure caption (similar to the TAR) that volcanic aerosols are not shown because they lead to a episodic cooling of the climate system that lasts only a few years per event. [Andy Reisinger (Reviewer's comment ID #: 210-9)]	Noted. Figure caption explicitly states now that volcanic aerosols are not included
SPM-292	A	5:1	5:1	"net warming" should be replaced by "net positive radiative forcing". These two are not quite equivalent. [Olivier Boucher (Reviewer's comment ID #: 27-33)]	Accepted
SPM-293	A	5:1	5:2	"Consider adding a sentence to the header here about the masking by aerosols of the warming that would have been realized otherwise - an important message for policymakers." [Govt. of Canada (Reviewer's comment ID #: 2004-16)]	Noted. The word "warming" was replaced by "positive radiative forcing". The "net radiative forcing" is the result of positive and negative contributions. Attribution section deals explicitly with the warming and cooling.
SPM-294	A	5:1	5:1	The statement here and "The global average temperature has increased since 1850"(SPM-6, L38) are not consistent in timing. Although it could be understood that one is the starting time of industry revolution and the other is starting time of instrumental observation, this time difference should be clearly expressed. Otherwise, readers will think climate warming has started since 1850, which is not related to the industry revolution. [Govt. of China (Reviewer's comment ID #: 2006-9)]	SEE COMMENT SPM-293
SPM-295	A	5:1	5:2	supplement: anthropogenic forcing as the most important forcing especially in the last decades; add a quantitative description of the anthropogenic influence in comparison with other forcings on climate in the last century [Govt. of Germany (Reviewer's comment ID #: 2011-64)]	See COMMENT SPM-303
SPM-296	A	5:1	5:1	Insert after ".human activities" "such as emissions of greenhouse gases, land-use and urban changes and energy emissions" [VINCENT GRAY (Reviewer's comment ID #: 88-2151)]	Not necessary and too lengthy for SPM
SPM-297	A	5:1	5:1	"very likely" This statement sounds less powerful than in the TAR. I know in IPCC terminology this is strong, but just read out as a statement it sounds weak. Not really sure what to suggest. [Joanna House (Reviewer's comment ID #: 109-15)]	Taken into account. Net forcing was not estimated in TAR and statistical terms were not used to describe the net forcing. The current statement follows from an assessment of the known positive and negative forcings together with their uncertainties
SPM-298	A	5:1	5:2	Relates to a comment made to chapter 2 and also TS. The point whether the net forcing is greater than zero is not very interesting, and stressing that too much implies that because the net forcing is positive, the whole picture of the observed trends is consistent. Several studies (e.g. Forest Science 2002, Knutti Nature	Individual RF uncertainty to be specified in terms of 90% confidence interval. Pdf plot will be redrawn. Questionable whether bottom-up and top-down

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				2002, see Andersen Science 2003 for a summary) have shown that the net forcing must be substantially greater than zero (at least 0.8 W/m <sup>2</sup> depending on the method used) to be able to explain the observed warming (taking into account uncertainties in the observed warming, natural forcing, etc.). So I think we should focus more on whether the net forcing is large enough to be consistent with the observed warming than whether it is positive. Suggest adding to the main bullet 'and the anthropogenic radiative forcing is likely large enough to explain the observed warming', or 'is likely consistent with the observed warming'. [Reto Knutti (Reviewer's comment ID #: 133-42)]	estimates can be intercompared. There are too many caveats in such an exercise.
SPM-299	A	5:1	5:1	Truly data-constrained modeling, along with direct observational & satellite data, should be considered in establishing the RF low- and high-bound values for the ICAE. If this were done, the lesser maximum -RF for the ICAE may lead to use of "have, with virtual certainty,..." instead of "have very likely..." in this important, bolded SPM statement; see comments #7, #8, and #9. [Herman Sievering (Reviewer's comment ID #: 240-6)]	Taken into account in construction of new RF estimate.
SPM-300	A	5:1	5:1	Precise values are being given for all the forcings, but for what period? Replace "since 1750" by "from 1750 to 2004". [Peter Stone (Reviewer's comment ID #: 257-1)]	Accepted. SPM-2 caption revised to give year.
SPM-301	A	5:1	5:2	Taken as plain English, this sentence sounds somewhat weaker than what was stated in AR3. Reword. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-106)]	SEE COMMENT SPM-297
SPM-302	A	5:1		The bullets underneath and the figure do not support the header because it doesn't connect the radiative forcing to the human influence on or composition of temperature change. [Govt. of United States of America (Reviewer's comment ID #: 2023-734)]	Taken into account
SPM-303	A	5:2	5:2	It should be possible to add here a sentence such as: "The only natural change identified as having probably a warming influence on the atmosphere is the changes in solar radiation, but this effect is estimated to be 20 times smaller than the anthropic effect". [Govt. of France (Reviewer's comment ID #: 2010-100)]	Accepted, at least the sense for introduction into the chapter and TS.
SPM-304	A	5:4	5:4	Suggest clarifying: 'The current radioactive forcings resulting from human-caused emissions.....' [Govt. of Australia (Reviewer's comment ID #: 2001-8)]	Rejected. This appears under a headline which points clearly to the human influence so repetition is unnecessary.
SPM-305	A	5:4	5:9	"Are the uncertainty estimates in this page and following pages corresponding to 65%? This is not clear as currently worded."	Accepted, text revised to define uncertainty ranges in footnotes.

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				[Govt. of Canada (Reviewer's comment ID #: 2004-17)]	
SPM-306	A	5:4	5:9	It would be good to add to this paragraph - perhaps to the end of the third sentence - a line about net natural forcings being negative. Could use lines 11-13 of Technical Summary (page 12) which makes such a statement for the period 1978-present.  [Govt. of Canada (Reviewer's comment ID #: 2004-18)]	Accepted, bullet point added
SPM-307	A	5:4	5:4	Insert before "well", "fairly" [VINCENT GRAY (Reviewer's comment ID #: 88-2152)]	Rejected – no rationale given.
SPM-308	A	5:4	5:4	Add at end "Changes in the main greenhouse gas, water vapour, and in clouds, are, however, virtually unknown" [VINCENT GRAY (Reviewer's comment ID #: 88-2153)]	Rejected. These are feedbacks not forcings as is explained in the text.
SPM-309	A	5:4	5:9	Need to say radiative forcing of emissions of these gases since 1750 as the radiative forcing of the total concentrations of these gases in the atmosphere is of course quite different. [Joanna House (Reviewer's comment ID #: 109-16)]	We do now cite the 1750 reference year but RF is strictly due to concentration change not to emission.
SPM-310	A	5:4	5:21	Somewhere in the SPM it should be made clear what is the measure of uncertainty (1 or 2 standard deviations or 90%?), preferably in a footnote so that it is immediately evident. [Haroon Kheshgi (Reviewer's comment ID #: 125-4)]	Accepted – See SPM-305.
SPM-311	A	5:4	5:9	This bullet refers to the radiative forcing of carbon dioxide, methane, ..., but to the contribution of changes in tropospheric ozone and Montreal Protocol gases. The reference to changes in some gases but not in others is confusing. Radiative forcing is quoted in Chapter 2 as the value relative to 1750, so further references to changes should be omitted completely. See also comment on title of Chapter 2, and several other comments related to whether or not radiative forcing refers to the absolute value or the value relative to 1750. [Adrian Simmons (Reviewer's comment ID #: 242-1)]	Accepted. RF now defined explicitly relative to 1750 and wording improved.
SPM-312	A	5:4	5:9	This paragraph is not very clear. It would be helpful to express in percentage the contribution of each gas to the total radiative forcing. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-107)]	Rejected. Unfortunately the total RF, including aerosol forcing, is less well known than some of the components so the relative proportions of the well known parts are not helpful.
SPM-313	A	5:4	6:7	Uncertainty range should be given as +/- two standard deviations (95% range) following conventional scientific practice. [Lenny Bernstein (Reviewer's comment ID #: 20-3)]	Partly Accepted, text revised to 5%-95%. See SPM-305.
SPM-	A	5:4	6:14	All uncertainty ranges should be +/- two standard deviations, following	Accepted see SPM-305.

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314				conventional scientific practice. [Jeff Kueter (Reviewer's comment ID #: 137-4)]	
SPM-315	A	5:4	6:7	It should be made clear that RF changes with time. Differences from the time of the TAR can be due to reduced uncertainty, but mainly due to the change in base time. What is the common base time for all the RF estimates? [Michael Manton (Reviewer's comment ID #: 157-24)]	Base year of 1750 now stated explicitly
SPM-316	A	5:4	6:7	Would it be helpful to Policymakers to give them an indication of the order of magnitude of the global-mean temperature change expected for a particular value of radiative forcing? Wm-2 may not mean too much to some. [Adrian Simmons (Reviewer's comment ID #: 242-2)]	Noted, but is in TS
SPM-317	A	5:4		bullet starting at line 4: Start with what is the most important: "Increases in greenhouse gases are the dominant cause of radiative forcing (the size)." Then the details. [Govt. of Norway (Reviewer's comment ID #: 2018-7)]	Accepted
SPM-318	A	5:4	:5	The first sentence in this paragraph contains too much information and numbers in our view. The information on the three greenhouse gases should be separated, and we believe they deserve one sentence each. The sentences in italics on page TS-6 could be used for this purpose. That is, the text on lines 28-29 for CO <sub>2</sub> , on lines 39-40 regarding methane and on lines 47-48 for nitrous oxide. [Govt. of Norway (Reviewer's comment ID #: 2018-8)]	Accepted – text rewritten
SPM-319	A	5:4		Replace "forcings" with "forcing increase since 1750 due to". [Govt. of United States of America (Reviewer's comment ID #: 2023-735)]	RF reference year of 1750 now stated explicitly
SPM-320	A	5:5	5:8	I hope that all these confidence figures are for TWO standard deviations. If they are only ONE they must all be doubled. [VINCENT GRAY (Reviewer's comment ID #: 88-2156)]	Accepted see SPM-305.
SPM-321	A	5:5	9:6	Figure SPM-2: Lifetime halocarbons can range beyond the indicated 100 years. [Govt. of Netherlands (Reviewer's comment ID #: 2016-1)]	SEE COMMENT SPM-288
SPM-323	A	5:5		This comment applies to $0.16 \pm 0.02$ , in this particular instance, but needs to be considered for all other $\pm$ ranges as well. Define what this range means? 2/3 likelihood? 1-sigma, 2-sigma? If it has no quantitative meaning (i.e., a model range as in the Third Assessment Report), then the range should be dropped entirely. [Govt. of United States of America (Reviewer's comment ID #: 2023-736)]	Accepted see SPM-305.
SPM-324	A	5:6	5:6	The notation for the range is not clear. I assume you mean a range of [0.25,0.50]. [Olivier Boucher (Reviewer's comment ID #: 27-3)]	Text clarified
0-91	A	5:6		SPM: Suggest changing "0.35 (+0.15, -0.1)" to "0.25 - 0.5"	Accepted.

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				[Richard Allan (Reviewer's comment ID #: 3-56)]	
0-92	A	5:7		SPM: Suggest adding "(including Halocarbons)" after "Montreal Protocol gases" [Richard Allan (Reviewer's comment ID #: 3-57)]	Accepted
SPM-325	A	5:6	:7	It sounds somewhat strange that "the contribution due to the changes in the Montreal Protocol gases is of a certain magnitude radiative forcing". We suggest replacing this text with the following sentence from the TS: "The Montreal Protocol gases (primarily CFCs and HCFCs) as a group contributed 0.32 Wm <sup>-2</sup> to radiative forcing in 2004 with CFC-12 continuing to be the third most important long-lived radiative forcing agent."  [Govt. of Norway (Reviewer's comment ID #: 2018-9)]	Montreal Protocol gases has been deleted and the text now reads "halocarbons"
SPM-326	A	5:7	5:7	Suggest clarifying: 'Increases in the concentration of greenhouse gases in the atmosphere are....' [Govt. of Australia (Reviewer's comment ID #: 2001-9)]	Accepted
SPM-327	A	5:7	5:7	Need to define the "Montreal Protocol gases" in the SPM [Govt. of Australia (Reviewer's comment ID #: 2001-10)]	Replaced "Montreal Protocol gases" by halocarbons
SPM-328	A	5:7	5:9	SPM Comment: Replace the untrue and misleading sentence that says; "Increases in greenhouse gases are the dominant cause of radiative forcing (2.9 ± 0.3 W m <sup>-2</sup> total). (See Figure SPM-2, where smaller terms are also shown.) [2.3]" with "Increases in greenhouse gases may be the dominant cause of positive radiative forcing (2.9 ± 0.3 W m <sup>-2</sup> total) but (as shown in Figure SPM-2) the sum of negative forcings may be similar (-1.5 ± 1.2 W m <sup>-2</sup> total, also shown in Figure SPM-2). [2.3]"  [Richard Courtney (Reviewer's comment ID #: 49-7)]	Rejected. The headline for this section says it is very likely that the GHGs +ve terms are larger than the aerosol -ve terms.
SPM-329	A	5:7	5:7	Insert before "increases"" If water vapour and clouds are ignored completely" [VINCENT GRAY (Reviewer's comment ID #: 88-2154)]	Water vapor and cloud changes are feedback effects and dealt with below. Their net contribution is also to cause further warming so the suggestion would be misleading.
SPM-330	A	5:7	5:7	Readers may not know what the "montreal Protocol" gases are. Coupld you say "halocarbons" or put in a footnote if it is more comlicated than this [Joanna House (Reviewer's comment ID #: 109-17)]	SEE COMMENT SPM-327
SPM-331	A	5:7	5:8	"Increases in greenhouse gases are the dominant cause of radiative forcing (2.9+/-0.3 W/m <sup>2</sup> total). (See Figure SPM-2, where smaller terms are also shown)."	Rejected. Long-lived anthropogenic greenhouse gases taken together, and even the sum of all anthropogenic gases, exceeds most estimates for the aerosol effect.

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			<p>I believe this statistic and Figure SPM-2 are misleading because they ignore the contribution of fossil-fuel plus biofuel soot, the second leading cause of global warming after carbon dioxide and ahead of methane. By combining the effect of fossil-fuel plus biofuel soot together with the effects of all other aerosol components, the SPM implies that global warming cannot be reduced by controlling any particle emissions, that only greenhouse gases cause global warming, and that methane is the second-most important of global warming after carbon dioxide. All three implications are incorrect and make it more difficult for policy makers to develop useful strategies for slowing global warming. Already, policy makers in several U.S. states and some countries are using information about the climate effects of soot to address climate change and human health simultaneously, but in the future, they will look to IPCC to develop policies, so it is important that the SPM be correct about this issue.</p> <p>Fossil-fuel soot (which consists primarily of black carbon, organic matter, and sulfate), just like methane, is a specific component whose emission can be controlled by specific technologies (e.g., the particle trap in the case of vehicle exhaust). Biofuel soot, similarly, which originates from the burning of wood, dung, and other materials for indoor and outdoor heating and cooking, is also readily controlled by new technologies. As such, fossil-fuel plus biofuel soot should be listed as specific components for potential emission control to slow global warming.</p> <p>Figure 10 of the following paper,                      Jacobson, M.Z., The climate response of fossil-fuel and biofuel soot, accounting for soot's feedback to snow and sea ice albedo and emissivity, <i>J. Geophys. Res.</i>, 109, D21201, doi:10.1029/2004JD004945, 2004                      shows that controlling ff+bf soot would slow global warming faster and to a greater extent than controlling methane and faster, but to a lesser overall extent, than controlling carbon dioxide. This figure has been requested and used by many policy makers to date, and I believe the figure would be useful to include in the SPM since it is simple and shows the relative effects, over time, of controlling the three major sources of global warming.</p> <p>Results from Jacobson (2004) are supported by two additional papers:                      Chung, S.H., and J.H. Seinfeld, Climate response of direct radiative forcing of anthropogenic black carbon, <i>J. Geophys. Res.</i>, 110, D11102, doi:10.1029/2004JD005441, 2005.                      who found a similarly strong climate response of BC from all sources (although not looking at the inclusion of organics and sulfate along with BC), and from Jacobson, M. Z., Control of fossil-fuel particulate black carbon plus organic</p>	

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				<p>matter, possibly the most effective method of slowing global warming, J. Geophys. Res., 107, (D19), 4410, doi:10.1029/2001JD001376, 2002 who looked at the climate response and direct forcing of controlling fossil-fuel soot (BC+OM in that case) with a stronger inventory but without looking at the effect of BC on snow and ice albedo (as was done in the Jacobson 2004 paper). Figure 3 of</p> <p>Correction to "Control of fossil-fuel particulate black carbon and organic matter," J. Geophys. Res., 110, D14105, doi:10.1029/2005JD005888, 2005. which is similar to Figure 10 of Jacobson (2004), corrects Figure 1 of Jacobson (2002). Thus, both Figure 10 of Jacobson (2004) and Figure 3 of Jacobson (2005), which show results from two separate studies of the effects of soot from sources aside from biomass burning, show consistently that non-biomass-burning soot is the second-leading cause of global warming after carbon dioxide.</p> <p>A new paper has now quantified an additional climate response of black carbon: Jacobson, M.Z., Effects of absorption by soot inclusions within clouds and precipitation on global climate, J. Phys. Chem., in press, 2006, www.stanford.edu/group/efmh/jacobson/soot_incl_clouds.htm.</p> <p>To be consistent, it would also be useful if Figure SPM-2 were modified to separate out direct forcing from ff+bf soot (BC+OM), which is a distinct, emitted component that causes warming, from other aerosol components, which cause cooling. The most rigorous models to date suggest that the direct forcing due to ff+bf soot (BC+OM) should be approximately +0.25 W/m<sup>2</sup>, as discussed in my Comment #5, below.</p> <p>Finally, it would be useful to point out that the climate response per unit direct forcing of ff BC+OM is greater than that of CO<sub>2</sub> or CH<sub>4</sub> (as explained in Paragraph 63 of Jacobson, 2002 and in Comment #15, below), which explains why the ff+bf soot has a greater effect on temperature than does CH<sub>4</sub> although the direct forcing of CH<sub>4</sub> is slightly greater than is that of ff+bf soot.</p> <p>[Mark Jacobson (Reviewer's comment ID #: 116-1)]</p>	
SPM-332	A	5:7	5:7	<p>The Montreal protocol gasses are called "halocarbons" in Fig SPM-2, use consistent wording.</p> <p>[Rolf Müller (Reviewer's comment ID #: 181-2)]</p>	SEE COMMENT SPM-327
SPM-333	A	5:7	5:7	<p>Reword the sentence to explain what Montreal Protocol gases are.</p> <p>[Govt. of United Kingdom (Reviewer's comment ID #: 2022-108)]</p>	SEE COMMENT SPM-327
SPM-334	A	5:7		<p>To complete the picture, we suggest adding the following sentence on industrial fluorinated gases: "The concentrations of industrial fluorinated gases covered by</p>	Rejected, both Kyoto and Montreal Protocol gases as halocarbons, see Figure SPM-2

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				the Kyoto Protocol (HFCs, PFCs, SF6) are relatively small but are increasing. Their total radiative forcing in 2004 was 0.015 W m <sup>-2</sup> . [Govt. of Norway (Reviewer's comment ID #: 2018-10)]	
0-93	A	5:8	5:8	Revise the magnitude of the total error (Is it 0.4 instead of 0.3?) [Govt. of Spain (Reviewer's comment ID #: 2019-16)]	This value no longer used in the text
SPM-335	A	5:8	5:8	Clarify sentence by expanding: '.....total), and therefore of warming of the atmosphere and associated climate changes'. [Govt. of Australia (Reviewer's comment ID #: 2001-11)]	Text removed for other reasons
SPM-336	A	5:8		Replace "forcing" with "forcing increase since 1750" [Govt. of United States of America (Reviewer's comment ID #: 2023-737)]	RF reference year now stated explicitly
SPM-337	A	5:11	5:12	Although timescales are present, at first glance (as may occur in an SPM), this figure is mis-leading and almost gives the impression that the Aerosol effects largely cancel out the greenhouse gases. Of course the latter are very long-lived and it is the timescale that is critical. The figure does not highlight this sufficiently. [ Message given by this figure needs to be consistent with drafting at SPM 10, line 17] [Govt. of Australia (Reviewer's comment ID #: 2001-12)]	Reject. RF is defined as a snapshot and timescales are dealt with in other ways. Here they are summarized qualitatively in the timescale column.
SPM-338	A	5:11	5:12	There should be some discussion up front defining scientific uncertainty and the terminology that is being used throughout the WGI document. [Govt. of Australia (Reviewer's comment ID #: 2001-13)]	Uncertainties now defined in footnotes with reference to Box TS-1 where details provided.
SPM-339	A	5:11	5:12	Figure SPM-2: I suggest to remove the Contrail cirrus line from Figure SPM-2, since its radiative forcing is tiny compared with other anthropogenic terms shown here. Additionally, the quoted value (0.01 W/m <sup>2</sup> ) correspond to the direct contrail RF and does not account for contrail indirect effect. Hence, the contrail bar creates more questions than answers and could confuse policymakers. [Michael Danilin (Reviewer's comment ID #: 55-3)]	Rejected, it appeared in TAR and is shown for comparison as it is frequently requested
SPM-340	A	5:11	5:12	Figure SPM-2: question: Is it allowed to add or subtract the radiative forcings even when different time and space scales are valid for the different agents and mechanisms? [Govt. of Germany (Reviewer's comment ID #: 2011-65)]	Yes it is – see chapter 2.
SPM-341	A	5:11	5:16	I think the authors really need to rethink using the lexicon for scientific understanding that is used for radiative forcing. The problem that persists, even with the current modification, is that there does not seem to be any account taken of the relative importance of the uncertainty for the problem at hand. This continues to lead to criticisms of scientific understanding that confuse the public. Looking at Figure SPM-2, for example, the uncertainty ranges (or range of estimates) for stratospheric water vapour and for contrail cirrus are a tenth of a	Noted. The LOSU is based on Evidence and Consensus, now included in figure caption and see Chapter 2. Only in the FAQ section were the qualifying columns removed.

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				<p>W per square meter or less, yet the level of understanding is "Low" whereas the same level of understanding is given for direct aerosol effects when the uncertainty range spans about 0.8 W per square meter. There are other similar contradictions (though I am glad that the level of understanding on solar has been increased). What is really needed here is an indication about whether the uncertainty is likely to be significant or not--for aerosols and perhaps land surface it is; for other species, the uncertainties are likely not really that important--or maybe they are, but it would really help to change that column to something indicating likely relative importance in calculating the overall radiative forcing and the response. At the very least, I do not think the brief referral to this column in the figure caption is adequate for the figure will frequently be shown without any explanation of what is meant--a new column/description needs to be created. I found it interesting that the figure most like this in their chapter, which is for Question 2.1, does not have this column in it, so why have it in the SPM?</p> <p>[Michael MacCracken (Reviewer's comment ID #: 152-12)]</p>	
SPM-342	A	5:11	5:11	<p>I think it is a serious mistake in Figure SPM-2 (and so then also in Chapter 2) not to separately list the reflecting (cooling) and absorbing (warming) aerosols. They have separate sources and so will need to be separately addressed by SBSTA and the national measures.</p> <p>[Michael MacCracken (Reviewer's comment ID #: 152-13)]</p>	Noted but raises too much detail to be covered in the SPM. New observations yield quantitative estimates of the total atmospheric aerosol which help constrain the total aerosol RF, rather than the individual components.
SPM-343	A	5:11	5:30	<p>Fig. SPM-2: Total aerosol negative RF low- and high-bound values are not well supported by direct observational and satellite data. A less negative total aerosol RF may be appropriate. Too much reliance on models - not constrained by data - appears, to this reader, to be a major reason for the overly large low- and high-bound total aerosol -RF values.</p> <p>[Herman Sievering (Reviewer's comment ID #: 240-2)]</p>	Partially taken into account. RF estimates of aerosols updated. Models for aerosol indirect effect, constrained by obs, now considered in the estimate.
SPM-344	A	5:11	5:30	<p>As a long-standing (38 yrs. now) aerosol researcher, I am quite concerned about the <math>-0.9 \pm 0.5</math> W/m<sup>2</sup> for the "indirect cloud albedo effect" (ICAE); especially since it is based only on modeling (8 [not data constrained] models' mean, etc. - Ch. 2). I'd suggest (see comments #8 and #7) that zero may be a better low-bound RF value and that a smaller -RF value is appropriate for the high-bound negative RF for the ICAE. An important implication of such revised ICAE RF values is stated in comment #6.</p> <p>[Herman Sievering (Reviewer's comment ID #: 240-5)]</p>	See comment 240-2. Zero is not considered appropriate lower bound.
SPM-345	A	5:11	5:12	<p>Reorganise figure SPM-2 by moving "spacial scale" from the caption to the picture.</p>	Rejected – suggestion would make caption too long.

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				[Govt. of United Kingdom (Reviewer's comment ID #: 2022-109)]	
SPM-346	A	5:11	5:12	The last column in figure SPM-2 is not very helpful, it is not clear whether what is meant is understanding or quantification. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-110)]	Consider - retain in chapter of course SEE COMMENT SPM-341
SPM-347	A	5:11	5:12	figure SPM-2 should add surface water vapor and tropospheric water vapor. [Zong-Ci Zhao (Reviewer's comment ID #: 302-3)]	Tropospheric and surface water vapor are considered to be feedback variables, in contrast to stratospheric water vapor increase from methane oxidation, displayed in Figure SPM-2
SPM-348	A	5:11		In Figure SPM-2, the error bars are not associated with scientific uncertainty, but the policymaker might not realize such. The caption should be more careful and explicit as to what is contained in the figure. [Govt. of United States of America (Reviewer's comment ID #: 2023-738)]	Taken into account. Caption re-worded
SPM-349	A	5:11		In Figure SPM-2, consider making two columns on the right wider so that labels/titles can all be placed at the top of the columns. Consider adding, "Level of" before "Scientific Understanding" at top of last column. [Govt. of United States of America (Reviewer's comment ID #: 2023-739)]	Accepted
SPM-350	A	5:12		Fig SPM-2: The intent of "the approximate duration of the change/variation" is not clear. First, it is not referring to variation or change in the agent but to forcing attributable to the agent since 1750. Suggested change: "approximate duration of the forcing agent" [Govt. of Canada (Reviewer's comment ID #: 2004-19)]	Taken into account, caption modified to include: For all anthropogenic agents except land albedo, the timescale refers to is lifetime; for land and solar it is the time over which the change has occurred.
SPM-351	A	5:12		Fig SPM-2: Radiative forcing figure: Need to add the timeframe for this Figure. RF since 1750? Also, would be nice to have the actual RF values in the Figure caption. [Govt. of Canada (Reviewer's comment ID #: 2004-20)]	Reference year 1750 now given explicitly in definition. Actual values are in the chapter
SPM-352	A	5:13	0:13	It is universal statistical practice to indicate unvertainty by at least two standard deviations. All the error bars on the Figure should therefore be doubled [VINCENT GRAY (Reviewer's comment ID #: 88-2155)]	Error bars now correspond to 5%-95% range. See response to comment SPM-305.
SPM-353	A	5:13	5:18	No timescale is given for CO2 but some indication should be present that it is likely to be very long-time scale and perhaps on timescales of centuries being the best estimate. Policy makers have awareness of long-lifetime (eg GWP with nominal 100 year horizon). [Govt. of Australia (Reviewer's comment ID #: 2001-14)]	See SPM-284
SPM-354	A	5:13	5:13	The frequency of occurrence covered by +/-1 sigma depends on the shape of the probability distribution function. Is the 65% assumed (for a typical distribution	Taken into account, error bars now correspond to 5%-95% range. See response to comment SPM-305.

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				function such as a normal distribution, in which case it is not 65%) or estimated from the actual PDF? [Olivier Boucher (Reviewer's comment ID #: 27-4)]	
SPM-355	A	5:13	5:13	65% (1 sigma), suggest to remove (1 sigma). They are the same and 1 sigma is too technical  [Govt. of Canada (Reviewer's comment ID #: 2004-21)]	Taken into account, error bars now correspond to 5%-95% range. See response to comment SPM-305.
SPM-356	A	5:13	5:13	There are major forcing agents excluded from this diagram. They are water vapour and clouds.. It is absurd to regard these as "feedbacks" to carbon dioxide. They have forcing effects in their own right and these may or may not be depend on global temperature, since they include natural changes, both from geological and biological factors and those brought about by human activities which are unrelated to greenhouse gas emissions, such as agricultural practices, water management, land-use changes and urbanisation.. The only reason you omit them from this diagram is that their effects are so little known that the uncertainties would swamp every other forcing agent and render the whole exercise superfluous. [VINCENT GRAY (Reviewer's comment ID #: 88-2165)]	Rejected. As discussed in text, these are feedbacks not forcings.
SPM-357	A	5:13	5:28	The caption of Fig SPM-2 states that 65% (1-sigma) uncertainties are shown for the radiative forcings. In line 21 it is then stated that "Aerosols produce a net negative direct radiative forcing (-0.5 +/- 0.4 W m-2), i.e., a cooling effect...". Given the 1-sigma uncertainty is 0.4 W m-2, and given that this "1-sigma uncertainty" will by almost every reader be interpreted as 1st-moment std.error statistic of an underlying Gaussian p.d.f., we have the problem here that the stated "cooling effect" with an estimated mean of -0.5 W m-2 is formally not statistically significant at any reasonable significance level (say a 90% or 95% level). The terms "net negative" or "cooling" are therefore apparently in contradiction with the accuracy of the evidence. I say apparently, since of course we have strong evidence that the direct aerosol forcing is very likely smaller than the indirect one, but also that it is definitely negative for strong physical reasons. Thus the real problem is the presentation of the present knowledge of uncertainty by a symmetric 1-sigma statistic (implying Gaussian p.d.f.), while the more realistic p.d.f. of the direct aerosol forcing uncertainty will be a non-Gaussian and skewed p.d.f., i.e. a 1-sigma uncertainty of -0.5 - 0.4 W m-2 is reasonable to the negative side but to the positive side it is more like a -0.5 + 0.2 W m-2 behavior. In using such an a-symmetric 1st-moment statistic - be it then interpreted Bayesian type ("degree of belief") or objectively - the contradiction would be resolved and the (correct) physical argument would be re-conciled with	Taken into account, error bars now correspond to 5%-95% range. See response to comment SPM-305.

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				the (corrected) measures of uncertainty. (Maybe a log-normal p.d.f. helps.) I strongly recommend to improve on this point at this very prominent place in the SPM, as well as in the underlying report section 2.4. In a weaker way, but still to some degree, the same critique could also be applied to the sentence in lines 27-28 on the indirect aerosol forcing with quoted estimate of mean and 1-sigma uncertainty -0.9 +/- 0.5 W m <sup>-2</sup> . [Gottfried Kirchengast (Reviewer's comment ID #: 129-1)]	
SPM-358	A	5:13	5:13	The year for which these forcings apply should be specified. [Govt. of Netherlands (Reviewer's comment ID #: 2016-2)]	Accepted year is added.
SPM-359	A	5:13	5:13	I suggest to change "uncertainty range" to "uncertainty interval". [Christoph, C. Raible (Reviewer's comment ID #: 207-2)]	Text now refers to 5% - 95% uncertainty range which is standard terminology.
SPM-360	A	5:13		Suggest for clarity that a note be added to the caption: "The contrail cirrus value accounts only for the linear cirrus of persistent contrails.' This same note needs to appear with the figure and text in Chap. 2. [David Fahey (Reviewer's comment ID #: 66-2)]	Too detailed for SPM – rejected.
SPM-362	A	5:13		Change "1 sigma" to "1 standard deviation" [Govt. of United States of America (Reviewer's comment ID #: 2023-740)]	Taken into account, error bars now correspond to 5%-95% range. See response to comment SPM-305.
SPM-363	A	5:13		Add after "global mean radiative forcings" the phrase "in 2004 relative to 1750" [Govt. of United States of America (Reviewer's comment ID #: 2023-741)]	Accepted.
SPM-364	A	5:13	:18	Please be explicit in the text and caption about why volcanoes are omitted from the graphic. [Govt. of United States of America (Reviewer's comment ID #: 2023-742)]	Accepted now taken into account in caption.
SPM-365	A	5:16	5:18	Can you add to the figure caption something similar to what you state on page 15 (lines 1-2) of the SPM, "The lifetime of atmospheric carbon dioxide implies climate change commitments that persist for centuries." [Melinda Marquis (Reviewer's comment ID #: 162-98)]	See SPM-284
SPM-366	A	5:16	5:18	The lack of a figure for the timescale for CO <sub>2</sub> in Fig SPM-2 and the deeply uninformative comment in the caption are very unhelpful. Surely the figure could say "Centuries (see caption)" and the caption could include a more informative comment, e.g. that from TS, page TS-5, lines 50 to 53 [Govt. of United Kingdom (Reviewer's comment ID #: 2022-111)]	See SPM-284
SPM-367	A	5:16		I wonder whether a time scale for addition of antropogenic CO <sub>2</sub> can be put in (100 s d years?) this table. May provide fuel to sceptics as currently written. [Stephen J. Hawkins (Reviewer's comment ID #: 102-11)]	See SPM-284
SPM-368	A	5:16	:17	Can a range or lower minimum number of years for CO <sub>2</sub> be inserted? [Govt. of United States of America (Reviewer's comment ID #: 2023-743)]	See SPM-284

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SPM-369	A	5:18	5:18	It is unclear whether methane forcing includes the indirect effect. This should be clarified [Govt. of Australia (Reviewer's comment ID #: 2001-15)]	Methane RF is based on observed concentrations so accounts for changes due to all processes.
SPM-370	A	5:21	5:25	What proportion of the aerosol forcing reported here in the AR4 is a result of this better understanding and what is due to change in aerosol concentrations?  [Govt. of Canada (Reviewer's comment ID #: 2004-22)]	Taken into account.
SPM-371	A	5:21	5:25	Insert after Aerosols in line 21 "both of natural and anthropogenic origin" Is it possible to give a rough number of the share of the anthropogenic aerosols on total aerosols? [Govt. of Germany (Reviewer's comment ID #: 2011-66)]	Taken into account, text added.
SPM-372	A	5:21	5:21	The confidence limits should be two standard deviations [VINCENT GRAY (Reviewer's comment ID #: 88-2158)]	Accepted see SPM-305.
SPM-373	A	5:21	5:25	A little more detail here may be useful to the reader who never looks any further, e.g. what the sources of some of the more significant aerosols (in net contribution and anthropogenic control) are and why sulfate has been decreasing (coal scrubbing/pollution control) [Joanna House (Reviewer's comment ID #: 109-21)]	Some more detail provided but list of sources is beyond scope of SPM.
SPM-374	A	5:21	5:30	Summed together these aerosol effects are significantly more negative than estimated by inverse methods in Chapter 9.2.1.(Forest et al ,etc.). I suggest adding a statement on the inverse results for total net aerosol forcing here. [Ronald Prinn (Reviewer's comment ID #: 202-5)]	It is questionable whether the estimates from top-down can be compared with the bottom-up. Robustness of this exercise is in doubt.
SPM-375	A	5:21	5:30	The numbers given here for the aerosol forcing, which are based on forward calculations, give a misleading impression about the uncertainty in the total aerosol forcing. One might think from the numbers given here that a total aerosol cooling of 2.3 W/m <sup>2</sup> or more has about a 10 % probability. In fact the inverse calculations, which are a better method for constraining the total aerosol forcing because they are based on observed temperature changes, give stronger constraints. Even the weakest of the constraints summarized in Table 9.2.1 and section 9.2.1.2 leads to the conclusion that there is no more than a 5 % chance that the total aerosol cooling is more than 1.7 W/m <sup>2</sup> . I recommend that another bullet be added after these two, summarizing the constraints on the total aerosol forcing based on the inverse calculations. [Peter Stone (Reviewer's comment ID #: 257-2)]	See comment 202-5.
SPM-376	A	5:21		Suggest for completeness that one of the bullets include a descriptive reference to the contrail cirrus term in Fig. SPM-2 [David Fahey (Reviewer's comment ID #: 66-3)]	Insufficient space in SPM for tutorial information.

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SPM-377	A	5:21	:30	two bullets: It is a problem that forcing from anthropogenic aerosols is poorly defined in the report. It is OK to mention both direct and indirect effect, but I find the sentence in line 28 about additional effect very confusing. It should be deleted. We suggest the two bullets are merged to one. Start with "Anthropogenic aerosols produce both a direct forcing because they reflect solar radiation and an indirect forcing because they might change radiation properties and the extent of clouds." [Govt. of Norway (Reviewer's comment ID #: 2018-11)]	Partly taken into account, text re-worded
SPM-378	A	5:21		Put a comma after "i.e." [Richard Soulen (Reviewer's comment ID #: 248-5)]	OK
SPM-379	A	5:21		"Aerosols" needs to be defined for the policymaker. Add a footnote defining "aerosols" at first callout (page SPM-3, line 19). [Govt. of United States of America (Reviewer's comment ID #: 2023-744)]	Aerosols are defined in the Glossary
SPM-380	A	5:24	5:25	Should note that anthropogenic sulphate emissions have regionally disparate patterns [Govt. of Australia (Reviewer's comment ID #: 2001-16)]	This is true but not relevant to the points being discussed
SPM-381	A	5:24	5:25	Suggest changing "sulfate emissions" to "emissions leading to sulfate aerosols". Sulfate aerosols do not come from sulfate emissions. Furthermore, I had difficulty finding the basis for decline in sulfate abundance that is stated as a fact in this sentence. It was not, for example in the executive summary of chapter 2? References in 2.4 deal primarily with higher latitude emissions of SO <sub>x</sub> and not those from S and SE Asia where the majority of growth has occurred. [Haroon Kheshgi (Reviewer's comment ID #: 125-5)]	Accepted. Text no longer talks of aerosol emissions
SPM-382	A	5:24	5:24	Change "sulfate" into "sulphur dioxide". [Govt. of Netherlands (Reviewer's comment ID #: 2016-3)]	Sentence has been removed for other reasons
SPM-383	A	5:24	5:25	It would be useful to give readers an estimate of the relative importance of anthropogenic sulphate emissions relative to the total aerosol contribution. E.g. rephrase last sentence to say: "Anthropogenic sulphate emissions, which models estimated to contribute $-0.4 \pm 0.2 \text{ Wm}^{-2}$ to this forcing, have very likely been decreasing over the past two decades." [Andy Reisinger (Reviewer's comment ID #: 210-10)]	Sentence has been removed for other reasons
SPM-384	A	5:25		I would recommend providing the reason for the decline in anthropogenic sulfate emissions so that policymakers to know how to assess trends. [Timothy H. Profeta (Reviewer's comment ID #: 203-7)]	Sentence has been removed for other reasons
SPM-385	A	5:27	5:27	The confidence limits should be two standard deviations [VINCENT GRAY (Reviewer's comment ID #: 88-2159)]	Accepted see SPM-305.
SPM-	A	5:27	5:28	While in this assessment, aerosol effects on cloud prevalence (e.g. the Albrecht	Accepted – text is more specific and no longer talks of

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386				effect) are accounted for in estimates of radiative forcing, but rather are considered part of the modeled climate response, but this does not mean that such phenomena do not exist. Suggest changing “properties” to “albedo” (since the existence of a cloud is a property), and adding at the end of the sentence “, and effects of aerosols on cloud prevalence further modify the climate system.” Such effects remain one of least understood and potentially important sources of uncertainty. [Haroon Kheshgi (Reviewer’s comment ID #: 125-6)]	cloud properties
SPM-387	A	5:27	5:30	It would be useful to say that despite the remaining uncertainties about the different ways in which aerosols can affect the climate system, the overall radiative forcing of aerosols is virtually certain/very likely (?) to be negative, and hence aerosols cannot account for the observed increases in global temperatures but will always lead to a net cooling of the climate system. [Andy Reisinger (Reviewer’s comment ID #: 210-11)]	The SPM focuses on the net anthropogenic value and too many subtotals would be confusing - rejected
SPM-388	A	5:27		Replace “cloud properties” with “cloud albedo” [Govt. of United States of America (Reviewer’s comment ID #: 2023-745)]	Accepted – see SPM-386
SPM-389	A	5:28	5:30	It's just wording, but this para doesn't really mention non cloud albedo indirect effects. And it is not clear if the Uncertainty sentence refers to the 1st or second sentence or both. I suggest that the second and third sentences read "Aerosols also have additional effects on CLOUDS, precipitation and the hydrological cycle. These additional effects are poorly....". Hey you're not letting me copy and paste selected text from the pdf files, meany TSU! [Piers Forster (Reviewer’s comment ID #: 73-2)]	Taken into account, text edited.
SPM-390	A	5:29	5:30	I would urge changing the phrasing from "causing substantial uncertainty in estimates of the overall impact of aerosols on climate change" to "causing a substantial range in estimates of the overall effects of aerosols on climate change." I think great care needs to be taken in using the word "uncertainty" because there are many who interpret this as an indication that we do not understand anything about this issue, whereas the real situation is that we have a pretty well-bounded range of estimates of the value of this term--and we need to say it that way. [Michael MacCracken (Reviewer’s comment ID #: 152-14)]	Rejected – although the text mentions great uncertainty it provides ranges for the corresponding effects – this is now clearer – see SPM-391.
SPM-391	A	5:29	5:29	Are these uncertainties included in the estimate $-0.9 \pm 0.5 \text{ Wm}^{-2}$ or do they further add to the uncertainty? If they are not included, why give such definite uncertainty limits for the indirect cloud-aerosol effect? [Govt. of Sweden (Reviewer’s comment ID #: 2020-3)]	Accepted – text changed to indicate that uncertainties are included in the ranges given
SPM-	A	5:31	5:31	I think it would be useful to have a bullet on ozone, pointing out the link between	Trop Ozone was mentioned above – but has now been

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392				ozone production and pollution perhaps, but also giving the same quantitative info there should be for the other gases, actually I know at the time of the Tar it was hard to quantify the change in trop O3, so the same may be true now. [Joanna House (Reviewer's comment ID #: 109-22)]	put in a separate bullet.
SPM-393	A	5:31		Add a bullet that covers aerosol (or total) surface forcing based on the discussion in Chapter 2 (e.g., page 7, lines 17-26 as basis). If this is done, you need to define what the surface radiative forcing means. [Govt. of United States of America (Reviewer's comment ID #: 2023-746)]	This was considered but it is felt that introducing surface forcing in addition to the more important radiative forcing would more likely lead to confusion than it would to useful policy relevant messages in the SPM
SPM-394	A	6:1	6:2	The range given is for land surface changes (including BC effects on snow), not just for changes in land cover. See page 2.5 of chapter 2. [Olivier Boucher (Reviewer's comment ID #: 27-28)]	Accepted, text re-worded.
SPM-395	A	6:1	6:3	Given the huge uncertainty range that spans zero and the LOSU of "very low", I think this bullet can be dropped. LOSUs are not quoted for other forcings here (especially indirect aerosol), so it looks a little strange and out of place to quote it for this one. Also "smaller forcings in Figure SPM-2" short of covers land use and solar in the first bullet of the section [Piers Forster (Reviewer's comment ID #: 73-3)]	Rejected. There is significant interest in this value.
SPM-396	A	6:1	6:3	add after [2.5], "[7.2.2.2] [Govt. of Germany (Reviewer's comment ID #: 2011-68)]	Accepted
SPM-397	A	6:1	6:1	I would suggest to add the word "(albedo)" in brackets after "sunlight" as this term is used in fig SPM 2 [Joanna House (Reviewer's comment ID #: 109-23)]	Albedo now used in preference to reflection of sunlight
SPM-398	A	6:1	6:2	The cooling effect of vegetaiton changes is a net effect (say over what time period) as some changes have cause decreased albedo (warming), also for readers new to this concept it might be good to link a vegetation change with an albbedo change eg.g deforestation in snow-covered regions. Suggest: The NET effect of global land cover changes since 1750??? (e.g. deforestation of snow covered regions) has increased ....." [Joanna House (Reviewer's comment ID #: 109-24)]	Considered but insufficient space in SPM to cover this – refer to TS and chapters
SPM-399	A	6:1	6:3	The caption of Fig SPM-2 states that 65% (1-sigma) uncertainties are shown for the radiative forcings. And on page 6, line 1 to 2 it is then stated that "Global land-cover changes..., exerting a cooling effect on climate estimated to be -0.1 +/- 0.3 W m-2...". Given the 1-sigma uncertainty is 0.3 W m-2, and given that this "1-sigma uncertainty" will by almost every reader be interpreted as 1st-moment std.error statistic of an underlying Gaussian p.d.f., we have the problem here that the stated "cooling effect" with an estimated mean of -0.1 W m-2 is	Taken into account. See response to comment SPM-305.

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				formally not statistically significant at any reasonable significance level (say a 90% or 95% level). Using the term "cooling" is therefore in contradiction with the accuracy of the evidence. Physically it is clear that increased reflection of sunlight would lead to a negative effect, i.e., cooling, and not to a positive one. So the problem here is the presentation of the present knowledge of uncertainty by a symmetric 1-sigma statistic (implying Gaussian p.d.f.), while a more realistic p.d.f. of this forcing uncertainty is largely unknown, but is probably a skewed p.d.f., i.e. a 1-sigma uncertainty of -0.1 - 0.3 W m <sup>-2</sup> may be reasonable to the negative side but to the positive side the uncertainty measure needs be quite smaller according to the evidence available that the past global land-cover changes tentatively did *not* decrease surface SW reflection. In using an a-symmetric 1st-moment statistic - be it then interpreted Bayesian type ("degree of belief") or objectively - the contradiction would be resolved, or just drop to say "cooling" given the very low level of current scientific understanding on this forcing term. I strongly recommend to improve on this point at this very prominent place in the SPM, as well as in the underlying report section 2.5. [Gottfried Kirchengast (Reviewer's comment ID #: 129-2)]	
SPM-400	A	6:1	6:2	I would urge that this statement be modified to indicate that we have some confidence that changes in land cover can affect regional scale changes (or finer scale as regional for IPCC means a continent size domain--maybe say subcontinental scale areas) and that we have low confidence in how effects of varying sign add up across the world, both in terms of the magnitude and the couplings to atmospheric circulation, etc., but that the estimate provided appears to roughly bound the possible effects. [Michael MacCracken (Reviewer's comment ID #: 152-15)]	See SPM-398
SPM-401	A	6:1	6:1	delete the word "cooling" because if you consider the uncertainties in the next line, it could be a warming. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-112)]	Accepted, cooling removed.
SPM-402	A	6:2	6:2	The confidence limits should be two standard deviations [VINCENT GRAY (Reviewer's comment ID #: 88-2160)]	Accepted see SPM-305.
SPM-403	A	6:2	6:2	Need to be clearer about what has a low level of scientific understanding. From what I understand, the process is well understood scientifically, what is of high uncertainty is what the net effect over time has been [Joanna House (Reviewer's comment ID #: 109-25)]	Taken into account
SPM-404	A	6:2	6:2	What kind of "land-cover changes" the text refers to should be stated with an example(s) to help the reader understand the extent of change. [Govt. of Japan (Reviewer's comment ID #: 2014-8)]	See SPM-398

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SPM-405	A	6:2	6:2	"LIKELY exerting a cooling effect" [Keith Shine (Reviewer's comment ID #: 236-3)]	Text re-written
SPM-406	A	6:2	6:2	Is it the understanding or the quantification that is very low? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-113)]	Accepted. Phrase has been dropped
SPM-407	A	6:5	6:7	"Language is rather technical here for an SPM. Rephrase along lines of ""Changes in solar forcing since 1750 have had a small warming effect..."" [Govt. of Canada (Reviewer's comment ID #: 2004-23)]	Accepted
SPM-408	A	6:5	6:7	This bullet doesn't make sense as written - what does "improved" refer to? This is strange as I think I may be responsible for writing this in the first place, sorry. People can see the solar forcing on the figure, and if you don't want to make a big thing about it being smaller than TAR (there is no need to in my mind), I think this bullet could also be dropped [Piers Forster (Reviewer's comment ID #: 73-4)]	Bullet re-worded.
SPM-409	A	6:5	6:7	clarify what is meant by "a factor 2 uncertainty", does it mean the RF varies from -1.88 to 2.12Wm <sup>2</sup> ? [Govt. of Germany (Reviewer's comment ID #: 2011-69)]	That text now dropped
SPM-410	A	6:5	6:7	The statement re. RF due to solar changes requires an introductory statement. Why are solar changes significant in the discussion of GWP(Global Warming Potential)? Rationale: It is likely that this topic is not well understood by policymakers and thus a brief background would be of use. [Govt. of Japan (Reviewer's comment ID #: 2014-9)]	This is not a discussion of GWPs and the introductory chapeau makes clear that solar changes affect RF.
SPM-411	A	6:5	6:7	It is notable (surprising?) that the expert opinion of level of scientific understanding for pre-satellite-era solar forcing which is based on proxies and models has jumped from "Very Low" in the TAR, to "Medium" in the AR4. This should either be explained and highlighted here, or corrected including in Figure SPM-2. In addition, this contradicts Chapter 2, page 6, lines 27-28! [Haroon Kheshgi (Reviewer's comment ID #: 125-7)]	Accepted. Oversight – should have been Low
SPM-412	A	6:5	6:6	I think it would be helpful to give a range to better help indicate what the phrase "factor of 2 uncertainty" means--is it really 0.06 to 0.24? [Michael MacCracken (Reviewer's comment ID #: 152-16)]	Uncertainty range dropped for the SPM
SPM-413	A	6:5	6:7	For consistency, it would be useful if the uncertainty were given as 0.12(±0.12) Wm <sup>-2</sup> (what is meant by "factor of 2 uncertainty" - figure SPM-2 shows only ±0.06Wm <sup>-2</sup> as uncertainty?) [Andy Reisinger (Reviewer's comment ID #: 210-12)]	Uncertainty range dropped for the SPM
SPM-414	A	6:6	6:6	"Factor of 2 uncertainty" does this mean one standard deviation? If so it should be doubled	Uncertainty range dropped for the SPM

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				[VINCENT GRAY (Reviewer's comment ID #: 88-2161)]	
SPM-415	A	6:6	6:6	What is a factor of 2 uncertainty? Actually I am pretty sure I know what it means, but this terminology is not used elsewhere and it might confuse. [Joanna House (Reviewer's comment ID #: 109-26)]	Uncertainty range dropped for the SPM
SPM-416	A	6:6	6:6	Improved compared to what? [Govt. of Sweden (Reviewer's comment ID #: 2020-4)]	Wording improved now
SPM-417	A	6:6	6:6	instead of "with a factor of 2 uncertainty" express the uncertainty in the same way as in the other bullet points, for consistency [Govt. of United Kingdom (Reviewer's comment ID #: 2022-114)]	Uncertainty range dropped for the SPM
SPM-418	A	6:6		Do not use a new measure of uncertainty. "...with a factor of 2 uncertainty..." as a way of describing uncertainty appears here without any previous explanation. In this particular case, give the range in parentheses: 0.12 Wm <sup>-2</sup> (+0.12, -0.06). Readers need to be able to compare directly with the ± ranges used throughout the SPM. Within Chapter 2 this "factor of" language is used throughout, but not within other chapters. Authors should make the method of describing uncertainty consistent across chapters and preface the uncertainty methodology(ies) used in the SPM. Be consistent. [Govt. of United States of America (Reviewer's comment ID #: 2023-747)]	Uncertainty range dropped for the SPM
SPM-419	A	6:7	6:7	SPM Comment: To avoid being completely misleading, append the following sentence to the paragraph; "These studies ignore effects of changes to cloud cover induced by variations to solar magnetic and plasma discharge effects that may provide large changes to radiative forcing but are poorly understood."  [Richard Courtney (Reviewer's comment ID #: 49-8)]	Rejected – these issues are considered in chapter 2 and the suggested text would be incorrect.
SPM-420	A	6:9	6:9	"It should be clearer what is meant by "a wide range of past climates". How many past climates are there? Is it intended to mean studies of past climate on a wide range of timescales? Or more studies at different spatial scales? Suggest simply "and of past climates"." [Govt. of Canada (Reviewer's comment ID #: 2004-24)]	Accepted, this has been deleted
SPM-421	A	6:9	6:22	Good section [Piers Forster (Reviewer's comment ID #: 73-8)]	Thanks.
SPM-422	A	6:9	6:10	This statement is surely obvious. Hardly a great new discovery? [VINCENT GRAY (Reviewer's comment ID #: 88-2162)]	Text revised
SPM-423	A	6:9	6:10	Wow was this really doubted? Surely changing temperature is part of the definition of radiative forcing? Should this not be that "new studies of the 20th century and a wide range of past climates have increased confidence in	Text edited

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				attributing changes in past global mean temperatures to various forcing factors" As it is now, the bullet points below don't seem to fit the bullets [Joanna House (Reviewer's comment ID #: 109-27)]	
SPM-424	A	6:9	6:22	The section SPM-6 line 9 to line 22 could be moved to the section on "Understanding and Attributing Climate Change". These bullets are about understanding climate change. There may be some efficiency, too. For example, "cooling associated with many large transitory volcanic eruptions are evident in the instrumental and paleoclimate records, demonstrating that forcing leads to climate responses" is on [SPM-6]. A rather similar statement that "variability in temperature reconstructions... is very likely attributable to natural external forcing, particularly to known volcanic eruptions..." is on [SPM-11]. I understand that there are slightly different roles for the two statements, but given the pressure to keep the SPM short they might be combined. [Daniel Murphy (Reviewer's comment ID #: 183-9)]	Edited along the lines suggested
SPM-425	A	6:9	6:9	Could this sentence be strengthened to read: "New studies of the 20th century and of a wide range of past climates have increased confidence in our understanding of how radiative forcing changes affect global mean temperatures"? The current sentence only reaffirms a very basic qualitative principle, but doesn't offer confidence to any quantitative estimates of how much a given radiative forcing would affect global mean temperatures. [Andy Reisinger (Reviewer's comment ID #: 210-13)]	Text has been edited and confusing material removed
SPM-426	A	6:9		The connection between the bolded point and the bullets below is not well spelled out. [Timothy H. Profeta (Reviewer's comment ID #: 203-8)]	Text has been edited and is clearer now.
SPM-427	A	6:9	:10	Replace "radiative forcing changes" with "changes in radiative forcing". Drop "a wide range". [Govt. of United States of America (Reviewer's comment ID #: 2023-748)]	Text has been edited
SPM-428	A	6:11		Insert figure 6.10 c) that illustrate the temperature trend during the last 1300 years. [Govt. of Germany (Reviewer's comment ID #: 2011-70)]	Rejected, no basis given
SPM-429	A	6:12	6:16	We could not find the terms that Water vapor increases lead to a strong positive feedback that amplifies the global mean temperature response to increases in radiative forcing. New observational and modeling evidence confirms the importance of the expected feedbacks linked to water vapor, estimated to be approximately 1 W m <sup>-2</sup> per of global average temperature increase, or a 40-50% amplification of global mean warming. [2.3, 3.4, 8.6, 9.4]± mentioned in SPM (P6, line 12-16) in related parts (2.3, 3.4, 8.6, 9.4), but we strongly suggest to	Text has been edited. This material is in chapter 8

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				add the following words in the related paragraphs. Shi (1992) have found that the amplifying factor of water vapor feedback could be up to 1.51 by use of a radiative-convective model (RCM) with a refinement radiative transfer model of k-distribution. (Shi, G.-Y., 1992,: Radiative Forcing And Greenhouse Effect Due to the Atmospheric Trace Gases, Science in China (Series B), Vol.35 p217-229) [Govt. of China (Reviewer's comment ID #: 2006-10)]	
0-68	A	0:0		<p>We could not find the terms that “Water vapor increases lead to a strong positive feedback that amplifies the global mean temperature response to increases in radiative forcing. New observational and modeling evidence confirms the importance of the expected feedbacks linked to water vapor, estimated to be approximately 1 W m<sup>-2</sup> per ° of global average temperature increase, or a 40-50% amplification of global mean warming. [2.3, 3.4, 8.6, 9.4]” mentioned in SPM (P6, line 12-16) in related parts (section 2.3, 3.4, 8.6, 9.4), but we strongly suggest to add the following words in the related paragraphs.</p> <p>Shi (1992) have found that the amplifying factor of water vapor feedback could be up to 1.51 by use of a radiative-convective model (RCM) with a refinement radiative transfer model of k-distribution. (Shi, G.-Y., 1992,: Radiative Forcing And Greenhouse Effect Due to the Atmospheric Trace Gases, Science in China (Series B), Vol.35?No.2?, p217-229.</p> <p>[Guangyu Shi (Reviewer's comment ID #: 234-3)]</p>	See SPM-429
SPM-430	A	6:12	6:12	vapour [Piers Forster (Reviewer's comment ID #: 73-7)]	Copy-editing to be completed later.
SPM-431	A	6:12	6:16	Clarify what water vapour is meant: tropospheric, stratospheric, total? As there is some confusion arising from Figure SPM-2 where a number of RF for stratospheric water vapour is given which differs substantially from the 1 Wm <sup>2</sup> mentioned in this para [Govt. of Germany (Reviewer's comment ID #: 2011-71)]	Text has been edited.
SPM-432	A	6:12	6:23	I would recommend changing the order of text in these bullets.: Start with lines 20 to 22 on the orbital forcing as this is pretty fundamental and big-scale stuff. This should be a bullet on its own as it operates on different scales altogether than the volcanic forcings. Also by having this first it leads in to the water vapour feedback bullet. This bullet can then come second, and should perhaps talk about some of the other really big feedback mechanisms e.g. dust, land/soil carbon. Finally have the point about volcanoes. [Joanna House (Reviewer's comment ID #: 109-28)]	Section has been reorganized significantly and does follow these recommendations to some extent.
SPM-	A	6:12	6:16	It may be worth making the mechanism more clear to non-scientist readers e.g.	Insufficient space for this level of tutorial here.

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433				you could start by explaining how the feedback works e.g. warming leads to increased evaporation and transpiration of water from the earth's surface which leads to increased water vapour in the atmosphere which has a greenhouse warming effect (presumably), this leads to a strong positive feedback that ..... [Joanna House (Reviewer's comment ID #: 109-29)]	
SPM-434	A	6:12	6:16	This point does not seem to fit well with the "affect global mean temperatures" heading. Discussion of direct evidence for feedbacks would be better placed near the discussion of climate sensitivity, and also near a discussion of uncertainty in cloud feedbacks. [Haroon Kheshgi (Reviewer's comment ID #: 125-8)]	Taken into account. Text moved and edited.
SPM-435	A	6:12	6:16	The quantitative estimate of the amplification of global mean warming is in contradiction with TS 52, lines 17-24 where it is stated that a wide range of equilibrium sensitivity is found between models. There is no disagreement that water vapour is a feedback mechanism but it is surprising that such a specific estimate can be isolated from the uncertainties associated with water vapour evolution, especially as water vapour is not a well-mixed gas in the atmosphere. Suggest delete the words after "..... feedbacks linked to water vapour." [William Kininmonth (Reviewer's comment ID #: 128-93)]	Text has been edited.
SPM-436	A	6:12	6:16	This is very useful and helpful information--a very good addition to the types of summary information provided. [Michael MacCracken (Reviewer's comment ID #: 152-17)]	Thanks. This material is in the TS now.
SPM-437	A	6:12	6:16	This paragraph seems to me to be out of place, as it refers to feedbacks rather than forcings - I think the reader could easily get confused with the stratospheric water vapour column on Figure SPM-2 [Keith Shine (Reviewer's comment ID #: 236-4)]	Taken into account. Text moved.
SPM-438	A	6:12	6:12	The positive feedback must be due to a temperature increase leading to a water vapour concentration increase which in turn leads to a further increase in radiative forcing. The radiative forcing does not directly give a water vapour concentration increase. [Govt. of Sweden (Reviewer's comment ID #: 2020-5)]	Text has been edited
SPM-439	A	6:12		It is not clear how stratospheric water vapor is included here, particularly since it is decreasing in recent years. For clarity, I suggest explicitly including a sentence stating role of stratospheric water changes in relation to larger tropospheric changes or a statement excluding stratospheric water vapor. [David Fahey (Reviewer's comment ID #: 66-1)]	Text has been edited
SPM-440	A	6:13	6:15	Did the feedback of water vapor with ~a 40~50% amplification of global mean warming occur in past 140 years? Please clarify time scale.	Text has been edited

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				[Govt. of China (Reviewer's comment ID #: 2006-11)]	
SPM-441	A	6:13	6:13	Insert after "forcing" "Water vapor changes might also be taking place from natural causes or from human activities unrelated to greenhouse gas emissions" [VINCENT GRAY (Reviewer's comment ID #: 88-2163)]	Rejected. Not relevant to the point being made.
SPM-442	A	6:13	6:14	"New" information on "water vapour feedback" could not possible indicate how ALL sources of water vapour have changed over the past century [VINCENT GRAY (Reviewer's comment ID #: 88-2166)]	Rejected. Not relevant to the point being made.
SPM-443	A	6:13	6:16	Let me just check, the values given refer to possible future warming, or are the values the same for past warming? Is it necessary to have the first part of that sentence "new observational...linked to water vapour" [Joanna House (Reviewer's comment ID #: 109-30)]	Text has been edited
SPM-444	A	6:14	6:14	Replace "importance of the expected" by "likely presence of" [VINCENT GRAY (Reviewer's comment ID #: 88-2164)]	Text has been edited
SPM-445	A	6:14	6:14	in footnote 3 what is meant by "feedback in the vertical profile of temperature"? [Joanna House (Reviewer's comment ID #: 109-31)]	Text has been edited
SPM-446	A	6:14	6:16	FAQ 1.3 (Ch. 1, page 41, lines 44-46) states that the positive water vapour feedback may be strong enough to *approximately double* the change in the greenhouse effect due to the added carbon dioxide alone. However, the SPM (page 6, lines 14-16) describe this positive feedback as a "40-50% amplification of global mean warming." However, Box 8.1 (Chapter 8, page 47, lines 3-6) states, as FAQ 1.3 does, that this feedback "doubles" the warming: " In General Circulation Models (GCMs) water vapour provides the largest positive radiative feedback (see Section 8.6.2.3): alone it roughly doubles the warming in response to forcing (such as from greenhouse gas increases), while when it is combined with other positive feedbacks (such as from surface albedo) they amplify one another's effects." Probably would be better to present this positive feedback in the same way in all three parts of the report, e.g., for sake of consistency. [Melinda Marquis (Reviewer's comment ID #: 162-63)]	Text has been edited
SPM-447	A	6:14		Eliminate the footnote or revise it to provide a better explanation [Richard Soulen (Reviewer's comment ID #: 248-6)]	The footnote is no longer in the text.
SPM-448	A	6:14		Consider putting a period after "water vapour" and starting the following sentence "These are estimated . . ." [Richard Soulen (Reviewer's comment ID #: 248-7)]	Text has been edited
SPM-449	A	6:15	6:16	Suggest deleting "1 W m <sup>-2</sup> per degC of global average temperature increase, or" and adding to the end of the sentence "and contributes to climate sensitivity". The feedback forcing sensitivity could easily be confused with radiative forcing without further explanation.	Agreed. Text edited

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				[Haroon Kheshgi (Reviewer's comment ID #: 125-9)]	
SPM-450	A	6:15		The idea of quoting a water vapor feedback here as 1 Wm <sup>-2</sup> per °C might be valid, but there is no precedent for units like this or such a quantitative measure of feedback. Come up with a better way to express this. [Govt. of United States of America (Reviewer's comment ID #: 2023-749)]	Text edited
SPM-451	A	6:15		Regarding the range "40-50%," it is important to clearly indicate whether this range refers to transient runs (i.e., 1% yr <sup>-1</sup> increase in CO <sub>2</sub> ) or to steady-state doubled CO <sub>2</sub> . The amplification will be different in both cases. Define carefully or delete this attribute of the estimation clause. [Govt. of United States of America (Reviewer's comment ID #: 2023-750)]	Text edited
SPM-452	A	6:16	6:16	"What is the confidence level around the water vapour conclusions?" [Govt. of Canada (Reviewer's comment ID #: 2004-25)]	Text edited
SPM-453	A	6:18	6:19	""...Instrumental and paleoclimate TEMPERATURE records"" (add word)" [Govt. of Canada (Reviewer's comment ID #: 2004-26)]	Rejected. Paleoclimate records are indirect, not direct measures of temperature
SPM-454	A	6:18	6:19	This needs to say "Surface cooling" as elsewhere in the assessment it is indicated that the stratosphere warms due to volcanic eruptions. I think it also should be mentioned that volcanic eruptions can, in particular regions and seasons, cause changes in atmospheric circulation that can lead to transient warming. [Michael MacCracken (Reviewer's comment ID #: 152-18)]	Rejected. Length constraints of SPM. See the TS and the chapters.
SPM-455	A	6:18		Add "Surface" before "Cooling". Consider dropping the second and third sentences because this is dated material covered in the First Assessment Report. At minimum, change sentence structure to state "There remains high confidence that..." [Govt. of United States of America (Reviewer's comment ID #: 2023-751)]	Text has been deleted
SPM-456	A	6:19	6:19	Drafting of last part of sentence (beginning 'demonstrating') is confusing. [Govt. of Australia (Reviewer's comment ID #: 2001-17)]	Text has been deleted
SPM-457	A	6:19	6:19	Change sentence to read "..., demonstrating that radiative forcing leads to...". [Andy Reisinger (Reviewer's comment ID #: 210-14)]	Text changed
SPM-458	A	6:20	6:22	The use of the terms "forcing" and "orbital forcing" do not convey well the mechanism by which orbital variations are thought to have influenced climate, and could additionally be wrongly confused with annual-mean global radiative forcing which is how the term forcing has been predominately used in the IPCC context. Seasonal insolation changes (not annual) were thought to have led to the slow feedbacks of ice cover thus causing a radiative forcing change and a change in climate. Suggest rewriting these sentences avoiding the word	Text changed

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				“forcing” unless it is to mean annual-mean global radiative forcing, and to better convey the mechanism. And given that we do not know the mechanism that caused CO2 to change (the unspecified “biogeochemical feedback” in the draft) brings into question the high confidence expressed. [Haroon Kheshgi (Reviewer’s comment ID #: 125-10)]	
SPM-459	A	6:20	6:22	This is not consistent with TS-page 13, lines 10-13, where it is acknowledged that there is no comprehensive mechanistic explanation for all the observed climate and biogeochemical changes that take place over the orbital timescales, and that debate continues over important aspects. 'High confidence' is misplaced and 'medium' or 'low' confidence are more appropriate. [William Kininmonth (Reviewer’s comment ID #: 128-94)]	Text changed
SPM-460	A	6:20	6:22	I think the second part of this bullet point is a very important statement. Because it refers to quite different time scales and forcing agents compared to volcanic eruptions, it should not be merged with volcanic eruptions. The statement about ice ages deserves a stand-alone bullet point. It is also a very frequent public question and hence warrants being more visible. Also add "radiative" before "forcing". [Andy Reisinger (Reviewer’s comment ID #: 210-15)]	Text changed
SPM-461	A	6:20	6:20	Is there any global mean forcing associated with Earth orbital changes?? [Keith Shine (Reviewer’s comment ID #: 236-5)]	Text changed
SPM-462	A	6:20	:22	These lines should be deleted, at least the last sentence which introduce too complicated expressions. [Govt. of Norway (Reviewer’s comment ID #: 2018-12)]	Agreed, deleted from SPM
SPM-463	A	6:21	6:22	Whether all biogeochemical and biogeophysical feedbacks are positive ones, amplifying the initial forcing? How about thermohaline circulation? (MOC). It seems to be negative feedback as a geophysical process. [Govt. of China (Reviewer’s comment ID #: 2006-12)]	Text deleted
SPM-464	A	6:21	6:22	Why are ice-albedo feedbacks neglected when ice-ages are discussed? Furthermore it is a bit odd that volcanic eruptions leading to radiative forcing changes on time scales of a few years are mentioned under the same bullet as ice-ages extending over 100 000 years. [Govt. of Sweden (Reviewer’s comment ID #: 2020-6)]	SEE COMMENT SPM-463
SPM-465	A	6:22	6:22	""...amplified the TEMPERATURE response to orbital forcing""." [Govt. of Canada (Reviewer’s comment ID #: 2004-27)]	Text deleted
SPM-466	A	6:22	6:22	Add at end "but the mechanisms are not yet well understood" [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-115)]	Text deleted, see TS

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SPM-467	A	6:23	6:23	We would appreciate to have a statement saying whether, according to the Milankovitch cycles, we are in a cooling or a warming phase, and at which speed. [Govt. of France (Reviewer’s comment ID #: 2010-101)]	Text deleted. A statement regarding the Milankovitch cycles has been added to the TS.
SPM-468	A	6:25	6:25	Changes the title in "Observations of Changes in Climate" [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-116)]	Rejected. ‘direct’ is here due to separate section on indirect paleo data later
SPM-469	A	6:27	6:29	To shorten the SPM, delete SPM-6 lines 27-29. This is a general statement and there are specifics about some of the advances in understanding below, for example on line 44. [Daniel Murphy (Reviewer’s comment ID #: 183-10)]	Rejected. Opening sentences are needed to set broad context in each section, with detailed backup to occur later.
SPM-470	A	6:28		The qualifiers “improvements and extensions” implies that there has been no new data, no discoveries, no different types of data! Consider rephrasing. [Govt. of United States of America (Reviewer’s comment ID #: 2023-752)]	Wider variety of measurements covers new data types. Data analysis has been added. Deletion of the word ‘providing’ probably clarifies this language.
SPM-471	A	6:31	6:32	Sentence is slightly incorrect - snow/ice have not yet warmed. Replace with "Observations of coherent warming in the global atmosphere and in the ocean, and changes in snow and ice, now provide stronger joint evidence of climate change (See Figure SPM-3) [3.2,4.2,5.2]" [Govt. of Australia (Reviewer’s comment ID #: 2001-18)]	Accepted. Text has been edited.
SPM-472	A	6:31	6:31	"Suggest ""...and changes in snow and ice..."" [Govt. of Canada (Reviewer’s comment ID #: 2004-28)]	SEE COMMENT SPM-471 Text has been edited.
SPM-473	A	6:31	6:32	SPM Comment: This sentence must be amended because it is a blatant falsehood. For example, most of Antarctica is cooling and many glaciers are advancing, so the assertion of “coherent warming” is simply untrue. Delete the word “coherent” from the sentence and append “in some places” to its end. [Richard Courtney (Reviewer’s comment ID #: 49-9)]	Rejected. Antarctica is warming in most seasons and on average. Language used refers to the global mean and does not imply warming at every place at every time.
SPM-474	A	6:31	6:31	Warming..... In snow and ice: This sounds as if the temperture in ice sheets has risen; this is probably not what is meant here [Rolf Müller (Reviewer’s comment ID #: 181-3)]	Sentence now reads “melting snow and ice.” Accepted, text edited.
SPM-475	A	6:31	6:32	The current sentence says that there was warming in snow and ice, which is probably not what is meant. Also need to add sea-level. Suggestion: "Observations of coherent warming in the global atmosphere and ocean, and of changes in sea-level and snow and ice cover, now provide strong joint evidence of a warming world." If you want to stick with "stronger" rather than "strong" evidence, perhaps you need to say "even stronger" (than in the TAR) to explain what you mean - stronger than what? [Andy Reisinger (Reviewer’s comment ID #: 210-16)]	Sentence now reads “melting snow and ice.” Text has been edited to clarify.

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SPM-476	A	6:31		The syntax of the sentence conveys that the temperature of “snow and ice” has warmed, rather than the extent/duration of snow/ice cover has retreated consistent with warming. Please express the results better. [Govt. of United States of America (Reviewer’s comment ID #: 2023-753)]	now reads “melting snow and ice.” Accepted, text edited.
SPM-477	A	6:32	6:32	do you need to mention a time frame for the warming? [Piers Forster (Reviewer’s comment ID #: 73-9)]	Time frames are given in later paragraph that elaborate this headline.
SPM-478	A	6:32		Substitute “combined” for “joint”. [Govt. of United States of America (Reviewer’s comment ID #: 2023-754)]	The sentence has been re-worded such that this comment is no longer relevant.
SPM-479	A	6:34	6:36	The update to include 2006 data is very much appreciated. [Govt. of Austria (Reviewer’s comment ID #: 2002-6)]	Thank you
SPM-480	A	6:34	6:35	It is better to put this sentence in line 41 before "Urban heat island ...". It is not necessary to separately list temperature in 2001-2005 at the beginning of this part. [Govt. of China (Reviewer’s comment ID #: 2006-13)]	Edited. Current order shows the additional warm years that are needed to understand the change in trend from TAR.
SPM-481	A	6:34	6:34	When did the record begin? [Govt. of France (Reviewer’s comment ID #: 2010-102)]	Accepted, text has been edited.
SPM-482	A	6:34	6:34	Add after."record", "from the upwardly biased surface temperature record" ( for evidence of this see McKittrick and Michaels 2004 "A test of corrections for extraneous signals in gridded surface temperature data " "Climate Research" Vol 26 pages 159-173. [VINCENT GRAY (Reviewer’s comment ID #: 88-2167)]	Rejected. Inconsistent with a large body of literature assessed in detail in chapter 3. Technical points addressed in chapter 3.
0-78	A	0:0		SPM, TS, 3,4,5: The AR4 should articulate the fact that the majority of observational results presented are based on data not designed for climate monitoring and not meeting the UNFCCC-adopted climate monitoring principles. It is important for governments and the public to be reminded of this major problem in climate science. [Govt. of United States of America (Reviewer’s comment ID #: 2023-691)]	The fact that the data are not designed for a given purpose doesn’t necessarily mean they are not useful and that this is necessarily a major problem. Rejected as an unbalanced suggestion.
SPM-483	A	6:34	6:34	"2005 and 1998 were the warmest years on record" - which record? Presumably this is the instrumental record since 1861 so this should be stated otherwise some will assume something else. [David Griggs (Reviewer’s comment ID #: 90-2)]	Accepted, text has been changed.
SPM-484	A	6:34	6:34	how far back to records go? [Joanna House (Reviewer’s comment ID #: 109-32)]	Accepted, text has been changed.
SPM-485	A	6:34	6:34	Change "on record" into "since 1850". [Govt. of Netherlands (Reviewer’s comment ID #: 2016-4)]	Accepted, text has been changed.
SPM-	A	6:34	6:36	The update to include 2006 data is very much appreciated.	Thank you

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486				[Klaus Radunsky (Reviewer’s comment ID #: 204-6)]	
SPM-487	A	6:34	6:34	"warmest" what? I think that "global-mean surface (air) temperature" needs spelling out here and elsewhere [Keith Shine (Reviewer’s comment ID #: 236-6)]	Text has been edited
SPM-488	A	6:34	:35	Instead of “five of the six warmest years..”, we prefer the following from the text on Robust Findings in the TS, which is a stronger evidence in our view: “Ten of the eleven warmest years since 1850 have occurred since 1995.” [Govt. of Norway (Reviewer’s comment ID #: 2018-13)]	Agreed.
SPM-489	A	6:34	:42	two bullets: The second bullet is the most important one and should be the first. [Govt. of Norway (Reviewer’s comment ID #: 2018-14)]	Now one bullet. Current order shows the additional warm years that are needed to understand the change in trend from TAR.
SPM-490	A	6:34		It would benefit to give the length of the historical record [Timothy H. Profeta (Reviewer’s comment ID #: 203-9)]	Accepted
SPM-491	A	6:34		Be more specific in terms of year(s) regarding what is meant by “on record”—that is, “since ___” specifying a year. Explicitly state what record and the length of the record. The current form is too vague. [Govt. of United States of America (Reviewer’s comment ID #: 2023-755)]	Accepted
SPM-492	A	6:35	6:35	Add after .“(2001-2005)”, “because the biases from socioeconomic factors have been the greatest” [VINCENT GRAY (Reviewer’s comment ID #: 88-2168)]	Rejected. Comment provides no basis for the assertion
SPM-493	A	6:35		Avoid statements such as “last five years” and bound the time frame; otherwise, you risk having language that is dated before publication or at minimum a confusing window. Give the exact time period. [Govt. of United States of America (Reviewer’s comment ID #: 2023-756)]	Accepted
SPM-494	A	6:35	:36	Bracketed language indicates that data will be updated. Is the intention to carry this tack throughout the SPM themes/bullets as appropriate in order to provide the policymaker with the most current data before going into production? [Govt. of United States of America (Reviewer’s comment ID #: 2023-757)]	Updating to be considered where practical.
SPM-495	A	6:36	6:36	SPM Comment: Delete this paragraph because it is simply not true. It relies on the surface temperature records that contain spurious trends (where ‘spurious’ means a difference from reality), as is demonstrated by the statistically significantly different trends between them. The record begins ~1860. For the period 1860 to 2004, the most cited of these data sets have good coherence (which is not surprising because they were compiled from the same available source data), but their trends (in degrees C/decade) and 2SD trend errors are GHCN: 0.076 ± 0.010	This reviewer’s comments are in error. 1) The Jones et al. analysis is now updated by Brohan et al. (2006) and begins in 1850. The NOAA/NCDC analysis, which uses GHCN over land, and the GISS analysis both begin in 1880. 2) This paragraph of the SPM refers to land and ocean combined. 3) For land and ocean combined and for the longest common period, 1880-2005, trends are:

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			<p>Jones et al.: <math>0.064 \pm 0.007</math>                      GISS: <math>0.048 \pm 0.006</math>                      The Jones trend is significantly different from the GISS trend (<math>p &lt; 0.05</math>), and the GHCN trend is very significantly different from the GISS trend (<math>p &lt; 0.01</math>). So, “coolest” and “warmest” years near the ends of the data sets are generated by trends that are known to be spurious.                      A change in the indicated temperature with time is only an indication of climate change when the change exceeds the intrinsic measurement errors (otherwise the change could be intrinsic measurement error). The intrinsic errors of GHCN, Jones et al. and GISS data sets are not known, but at least two of the data sets provide wrong results because they differ in annual change by more than double their stated 95% confidence limits in each of several years.                      Furthermore, the GHCN and Jones et al. data sets also have different trends for the most recent 30 years. For the period 1976-2004, the GHCN trend is 0.3032 degrees C / decade and the Jones et al. trend is 0.2166 degrees C / decade. And this also demonstrates that at least one of them is indicating a spurious trend generated by the methods used to create the means.                      Importantly, over the most recent 30 years the errors accumulating between the GHCN and Jones et al. data sets are of the order of 0.2 degrees C (because the changes to temperature indicated by their trends differ by that much). And the errors in each of these data sets may be more than 0.2 degrees C (because they are not known).                      Indeed, the compilers of these data sets admit their methods create spurious trends (ref. Vose et al., 2004). Their methods include integrating measurements into mean values for regions over the Earth’s surface called ‘grid boxes’. And the integrations cause trend problems for individual grid boxes. Vose et al. state that when the GHCN and Jones et al. trends are compared at the grid-box level then 9.3% of grid cells display “discrepant trends”. In other words, the integration of measurements into grid boxes causes 9.3% of grid boxes to have trends with opposite sign. (!)                      Simply,                      (a) temperature measurements cannot indicate climate change in the absence of knowledge of their accuracy and precision,                      (b) coherence between data sets does not indicate their accuracy or precision, and                      (c) the accuracy and the precision are not known for mean global temperatures indicated by the GISS, GHCN and Jones et al. data sets.                      So claims such as “year x was the warmest year” cannot be substantiated from</p>	<p>Brohan et al. <math>0.053 \pm 0.019</math> °C/decade;                      NCDC, <math>0.051 \pm 0.018</math> °C/decade;                      GISS, <math>0.051 \pm 0.014</math> °C/decade.                      These are highly consistent.</p> <p>For land alone, 1880-2005, trends are:                      Brohan et al. <math>0.074 \pm 0.023</math> °C/decade;                      NCDC, <math>0.072 \pm 0.021</math> °C/decade;                      GISS, <math>0.059 \pm 0.016</math> °C/decade.                      Brohan et al and NCDC are highly consistent. GISS is not inconsistent with the other analyses, and its smaller trend is because its “1200km radius of influence” technique favours coastal stations where the trend is smaller than in the inner continents.</p> <p>Similar considerations imply consistency also for 1901-2005 and 1979-2005 (Chapter 3, Tables 3.2 and 3.3)</p> <p>Brohan et al. (2006) take measurement and sampling errors into account to estimate errors in gridded values, and take these errors into account, along with the effects of data-void areas and measurement biases, to estimate errors in the global averages. We therefore have an error-bar on each year and now state that 1998 was “probably” the warmest year because the estimate for 1998 is within the error bars for the nominally second warmest year, 2005.</p>

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				<p>the data sets. All years are the same temperature within the (unknown) measurement errors. So, no year can be said to be warmer than any other in the absence of knowledge of the inherent error of each indication of each year's temperature.</p> <p>Furthermore, over the most recent 30 years the errors accumulating between the GHCN and Jones et al. data sets for mean global temperature are known to be of the order of 0.2 degrees C, and the errors accumulating in each data set are possibly more than this. If their accumulated errors were at the rate of [0.2 degrees C / 30 years] throughout the twentieth century, then this would account for all the ~0.6 degrees C rise they indicate occurred throughout that century.</p> <p>[Richard Courtney (Reviewer's comment ID #: 49-10)]</p>	
SPM-496	A	6:38	6:38	<p>Replace "linear" with "average". It may be worth explicitly noting the cooling trend between 1945 and 1979 because, as the statement stands the numbers for the sub-periods do not add up to the total period and a discerning reader may wonder what happened in the other decades.</p> <p>[Govt. of Australia (Reviewer's comment ID #: 2001-19)]</p>	Text has been edited
SPM-497	A	6:38	6:42	<p>While the default option for uncertainty is stated in Box TS 1.1, it is not discussed in the SPM. The SPM should clearly state that these uncertainty values are +/- two standard deviations.</p> <p>[Lenny Bernstein (Reviewer's comment ID #: 20-4)]</p>	Uncertainty approach has been clarified
SPM-498	A	6:38	6:39	<p>A unit of °C/century would be more appropriate than °C to describe the linear trend over the 20th century.</p> <p>[Olivier Boucher (Reviewer's comment ID #: 27-5)]</p>	Text has been edited
SPM-499	A	6:38	6:42	<p>SPM Comment: Delete this paragraph because it is simply not true. It relies on the surface temperature records that contain spurious trends (where 'spurious' means a difference from reality), as is demonstrated by the statistically significantly different trends between them. And its assertion that urban heat island effect does "not influence these large scale values" is a (deliberate?) falsehood.</p> <p>The surface temperature record begins ~1860. For the period 1860 to 2004, the most cited of these data sets have good coherence (which is not surprising because they were compiled from the same available source data), but their trends (in degrees C/decade) and 2SD trend errors are                      GHCN: 0.076 ± 0.010                      Jones et al.: 0.064 ± 0.007                      GISS: 0.048 ± 0.006</p>	<p>This reviewer's comments are in error.</p> <p>1) The Jones et al. analysis is now updated by Brohan et al. (2006) and begins in 1850. The NOAA/NCDC analysis, which uses GHCN over land, and the GISS analysis both begin in 1880.</p> <p>2) This paragraph of the SPM refers to land and ocean combined.</p> <p>3) For land and ocean combined and for the longest common period, 1880-2005, trends are:                      Brohan et al. 0.053±/-0.019 °C/decade;                      NCDC, 0.051 +/- 0.018 °C/decade;                      GISS, 0.051 +/- 0.014 °C/decade.                      These are highly consistent.</p>

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			<p>The Jones trend is significantly different from the GISS trend (<math>p &lt; 0.05</math>), and the GHCN trend is very significantly different from the GISS trend (<math>p &lt; 0.01</math>). So, “coolest” and “warmest” years near the ends of the data sets are generated by trends that are known to be spurious.</p> <p>A change in the indicated temperature with time is only an indication of climate change when the change exceeds the intrinsic measurement errors (otherwise the change could be intrinsic measurement error). The intrinsic errors of GHCN, Jones et al. and GISS data sets are not known, but at least two of the data sets provide wrong results because they differ in annual change by more than double their stated 95% confidence limits in each of several years.</p> <p>Furthermore, the GHCN and Jones et al. data sets also have different trends for the most recent 30 years. For the period 1976-2004, the GHCN trend is 0.3032 degrees C / decade and the Jones et al. trend is 0.2166 degrees C / decade. And this also demonstrates that at least one of them is indicating a spurious trend generated by the methods used to create the means.</p> <p>Importantly, over the most recent 30 years the errors accumulating between the GHCN and Jones et al. data sets are of the order of 0.2 degrees C (because the changes to temperature indicated by their trends differ by that much). And the errors in each of these data sets may be more than 0.2 degrees C (because they are not known).</p> <p>Indeed, the compilers of these data sets admit their methods create spurious trends (ref. Vose et al., 2004). Their methods include integrating measurements into mean values for regions over the Earth’s surface called ‘grid boxes’. And the integrations cause trend problems for individual grid boxes. Vose et al. state that when the GHCN and Jones et al. trends are compared at the grid-box level then 9.3% of grid cells display “discrepant trends”. In other words, the integration of measurements into grid boxes causes 9.3% of grid boxes to have trends with opposite sign. (!)</p> <p>Simply,</p> <ul style="list-style-type: none"> <li>(a) temperature measurements cannot indicate climate change in the absence of knowledge of their accuracy and precision,</li> <li>(b) coherence between data sets does not indicate their accuracy or precision, and</li> <li>(c) the accuracy and the precision are not known for mean global temperatures indicated by the GISS, GHCN and Jones et al. data sets.</li> </ul> <p>So claims such as “year x was the warmest year” cannot be substantiated from the data sets. All years are the same temperature within the (unknown) measurement errors. So, no year can be said to be warmer than any other in the</p>	<p>For land alone, 1880-2005, trends are:                      Brohan et al. 0.074+/- 0.023 °C/decade;                      NCDC, 0.072 +/-0.021 °C/decade;                      GISS, 0.059 +/-0.016 °C/decade.</p> <p>Brohan et al and NCDC are highly consistent. GISS is not inconsistent with the other analyses, and its smaller trend is because its “1200km radius of influence” technique favours coastal stations where the trend is smaller than in the inner continents.</p> <p>Similar considerations imply consistency also for 1901-2005 and 1979-2005 (Chapter 3, Tables 3.2 and 3.3)</p> <p>Brohan et al. (2006) take measurement and sampling errors into account to estimate errors in gridded values, and take these errors into account, along with the effects of data-void areas and measurement biases, to estimate errors in the global averages. We therefore have an error-bar on each year and now state that 1998 was “probably” the warmest year because the estimate for 1998 is within the error bars for the nominally second warmest year, 2005.</p> <p>Urban heat island is not a significant factor in this discussion, as substantiated in chapter 3.</p>

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				<p>absence of knowledge of the inherent error of each indication of each year's temperature.</p> <p>Furthermore, over the most recent 30 years the errors accumulating between the GHCN and Jones et al. data sets for mean global temperature are known to be of the order of 0.2 degrees C, and the errors accumulating in each data set are possibly more than this. If their accumulated errors were at the rate of [0.2 degrees C / 30 years] throughout the twentieth century, then this would account for all the ~0.6 degrees C rise they indicate occurred throughout that century.</p> <p>Also, several studies indicate that the urban heat island effect is a substantial contributor to the apparent warming trend in these data sets. For example, Kalnay and Ming determine that land-use change and urbanisation account for a significant portion of the global surface temperature increase of the last century. They determine an effect that is at least twice as great as has been previously estimated for the United States (Kalnay and Cai (2003)) (ref. Kalnay E, and M Cai, Nature, vol. 423, 528–531 (2003)). And Brandsma et al. have demonstrated that urban heat island biases in surface temperature data are not confined to cities but may spread to surrounding rural locations thus causing urban heat island effects much larger in magnitude than was previously thought (Brandsma et al. (2003)). (ref. Brandsma, T., G. P. Konnen, and H. R. A. Wessels, 2003. International Journal of Climatology, vol. 23, 829–845 (2003))</p> <p>[Richard Courtney (Reviewer's comment ID #: 49-11)]</p>	
SPM-500	A	6:38	6:38	<p>Insert after "temperature", "anomaly, as estimated from weather stations and ship measurements"</p> <p>[VINCENT GRAY (Reviewer's comment ID #: 88-2169)]</p>	Rejected. For details of measurements, see chapter.
SPM-501	A	6:38	6:40	<p>.Delete from the amended first sentence on line 38 to "warm years" on line 40</p> <p>[VINCENT GRAY (Reviewer's comment ID #: 88-2170)]</p>	Rejected, no basis given for suggestion.
SPM-502	A	6:38	6:39	<p>The word "trend" is confusing especially when compounded with the word "linear" and where the result is in deg. C. I would replace "linear warming trend" with "temperature increase".</p> <p>[John Hunter (Reviewer's comment ID #: 112-28)]</p>	Text has been edited and shows the time behavior of changes
0-20	A	0:0		<p>the question whether there is an --accelerated-- trend in temperature etc. observations should be explicitly discussed in a separate, dedicated sub-chapter somewhere in the SPM</p> <p>[Govt. of Germany (Reviewer's comment ID #: 2011-96)]</p>	Believe that the edited text is now clear regarding this.
0-94	A	6:39	6:39	<p>Change 0.2 (before due) for 0.20</p> <p>[Govt. of Spain (Reviewer's comment ID #: 2019-17)]</p>	Accepted

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SPM-503	A	6:38	6:40	In that the record shows substantial variability, I would not phrase the second sentence as indicating a "linear warming trend"--rather this should say that the increase in temperature over the 20th century was 0.6 plus or minus 0.2--so give the change, not the trend. This will then also fit better with the third sentence. [Michael MacCracken (Reviewer's comment ID #: 152-19)]	Text has been edited We do not say it is linear, quite the contrary, but the linear trend is given for reference.
SPM-504	A	6:38	6:39	There is a small inconsistency between how the 20th century and 1901-2005 warmings are expressed here compared to the statements in Chapter 3 (3.3, lines 3-8). SPM gives a warming of "0.6°C over the 20th century" and "0.65°C over 1901-2005". However, Chapter 3 gives "0.65°C over the 20th century" (3-3, line 8), and later (3-14, line 26) "slightly more than 0.65°C" over 1901-2005. If you average the three estimated warming rates (3.3, line 6), you get 0.65°C per 100 years and 0.68°C per 105 years. Admittedly, these differences are within the rather large standard error ranges, but it is desirable for the SPM to be consistent with the main report. [A. Brett Mullan (Reviewer's comment ID #: 182-1)]	Text has ben edited and believe it is consistent
SPM-505	A	6:38	6:41	Change the text in line with the summary of chapter 3 at page 15 and add some text about the land-based trend of the Northern hemisphere: "Generally temperatures at the surface have risen, but with important variations regionally and with time. For the global average, warming has occurred in two phases, from 1920–1940 (0.3°C) and more strongly from around 1970 (0.55°C), with the strongest increase over land of the Northern hemisphere, (0.9°C). Expressed as a global average, surface temperatures have increased by about 0.75°C since the late-19th century." [Govt. of Netherlands (Reviewer's comment ID #: 2016-5)]	Text has been edited
SPM-506	A	6:38	7:38	"If the ""linear warming trend over the 20th century"" is being discussed, should there be some clarification of whether or not the 20th century warming trend actually was linear?"  [Govt. of Canada (Reviewer's comment ID #: 2004-29)]	Figure shows the data and the non-linearity. See chapter 3 for more details.
SPM-507	A	6:38	:42	The use of different temperature changes over different periods is confusing. It is difficult to compare $0.6 \pm 0.2$ with $0.65 \pm 0.2$ for 5 additional years. Are they significantly different given the uncertainties? Also, define what the range means. The current text states that temperature has increased since 1850, but only gives a number from 1900. [Govt. of United States of America (Reviewer's comment ID #: 2023-758)]	Text has been edited
SPM-508	A	6:39	6:39	Add degrees C after the 0.2 on this line [Govt. of Australia (Reviewer's comment ID #: 2001-20)]	Copy-editing will be done at a later stage.

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SPM-509	A	6:39	6:39	missed out 2nd degree C symbol [Joanna House (Reviewer's comment ID #: 109-33)]	Copy-edit to catch such things
SPM-510	A	6:39	6:39	Here and elsewhere, all estimates of linear trends should be stated per unit of time (I.e per year or per decade or (as here) per century, as appropriate), otherwise there can be serious confusion created... [Govt. of United Kingdom (Reviewer's comment ID #: 2022-117)]	Accepted
SPM-511	A	6:39	6:39	It would be better to say "was estimated in the TAR to be 0.6..." [Govt. of United Kingdom (Reviewer's comment ID #: 2022-118)]	Accepted
SPM-513	A	6:39		Be precise in the time spans referenced. Be consistent with the time rate of change for temperature. [Govt. of United States of America (Reviewer's comment ID #: 2023-759)]	Text has been edited
SPM-514	A	6:40	6:40	"Most of the warming" is untrue. ALL of the warming took place from 1910 to 1942 and from 1979 to 2005 [VINCENT GRAY (Reviewer's comment ID #: 88-2171)]	Text has been edited
SPM-515	A	6:40	6:42	Delete from "variability" on line 40 to end on line 42. Insert "there was a small cooling period from 1850 to 1910 followed by a warming period from 1910 to 1942, of 0.4°C ( 0.125°C per decade) which was probably due to urban development around the early weather stations, although IPCC 1990 ("Climate Change") attributed it to a "recovery from the ice age" There was then a cooling period from 1942 to 1979 of about 0.05°C which was likely caused by a move of weather stations to airports, The second warming period, from 1979 to 2005 of 0.42°C (0.16°C per decade) was partly caused by the unusually strong El Niño ocean event of 1998, but otherwise mainly by urban influences and land-use changes. A possible influence from increases in greenhouse gases is difficult to justify without an evident influence over the previous periods." [VINCENT GRAY (Reviewer's comment ID #: 88-2172)]	Rejected. No basis provided for suggestions, which are inconsistent with a large body of literature as assessed in chapter 3
SPM-516	A	6:40		The record shows substantial variability with some cooler periods (say when). It might help to say that there was some cooler periods (60s and 70s) [Stephen J. Hawkins (Reviewer's comment ID #: 102-12)]	Text has been edited. Ups and downs are shown in the figure but space precludes a listing
SPM-517	A	6:40	:41	I believe the reader should be told what happened between 1945 and 1979 also. This would give him or her the complete story and further illustrate the "substantial variability" mentioned earlier. [Richard Soulen (Reviewer's comment ID #: 248-9)]	Shown in the figure.
SPM-518	A	6:41	6:42	"Suggest that the sentence regarding the influence of urban heat island effects be changed to read ""Urban heat island effects are real but local, and have a negligible impact on these large-scale values.""	Accepted

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				[Govt. of Canada (Reviewer's comment ID #: 2004-30)]	
SPM-519	A	6:41	6:42	stress the increasing trend of global average surface temperature in the last decade; add the trend for thr last 10 years [Govt. of Germany (Reviewer's comment ID #: 2011-72)]	Text has been edited to show trends over different periods that are meaningful. Ten years however is much too short given volcanic and solar influences.
SPM-520	A	6:41	6:42	The statement "Urban heat island effects are real but local, and do not influence these large scale values" is, to start with, illogical. Temperature variability is also "real but local" but this does not prevent you from calculating an average. Then, the statement is untrue. McKittrick and Michaels 2004 "Climate Research" Vol 26 pages 1159-173 have shown that urban effects DO influence these "large scale values", whereas it is impossible, at present, to show how increases in greenhouse gases, which occurred in a regular fashion over the period, could possibly explain the irregular character of the surface temperature record. It could not have been a factor in the early warming, and it is difficult to use it explain why there should be a cooling between 1942 and 1979 when emissions of greenhouse gases increased steadily. [VINCENT GRAY (Reviewer's comment ID #: 88-2173)]	Aerosols also affect decadal temperature trend and probably played a role between 1942 and 1979, see ch. 9 and 2.  Urban heat island statement has been edited and is correct as it stands.
SPM-521	A	6:41	6:41	I think it is very problematic to be dividing the record up at 1945. First, the raw data during the war years are really quite suspect, and large adjustments (e.g., a degree or two for nighttime marine air temperature) have, as I understand it, often been made; there have also been rather significant changes in spatial coverage of the data--it is really a bit surprising that the error bounds on the data are not larger during the war years, and having confidence that things are right to a tenth of a degree or two seems quite problematic to me. Second, I would think that the calculation of these trends should be based on the time-averaged curves, not one year results--and 1945 was a really unusual year--that right after the war, things turned around seems to me likely more than coincidence. I also believe that in looking at long-term climate change, one should be able to get the same sense of the changes by blocking out any short section of the record--interestingly, blotting out the years covering WWII, when data were most suspect and are most adjusted, actually rather dramatically changes one's impression of the 20th century record--this is not the case for any similar period except perhaps well back in the 19th century when we know coverage was quite poor. Starting with about 1910 also seems to me to potentially introduce bias due to the strong volcanic eruptions during the first decade of the 20th century. So, I think that this first warming period is really being over dramatized as the time history is quite different than for the later warming, which could as accurately as for the first period, be said to extend from about 1950 to the present (see Figure SPM-3), accelerating over this time. Note also that here the rate of warming is	Text to be edited to show trends past century, versus past 50 yrs.  1).The error bounds on SST are not as high in 1942-5 as previously because we did not need to make bucket-corrections so we avoided their uncertainties. There was an increase in data-sparsity related uncertainty but this was moderated on a global scale because there were nonetheless data from most regions except Antarctica, and global temperature anomalies have fewer than 100 degrees of freedom. 2)There was a prolonged El Niño in the early 1940s and the peak in global temperature is very likely to have been real. 3) Point is to show that the early 20th Century warming, even when seen in a most favourable light, was not as strong as the most recent warming. 4) Agreed that we should show the amount of warming over the period.

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				given per decade--whereas elsewhere rates are given per century or per year--in the cases here I would urge instead indicating the amount of warming over the period. [Michael MacCracken (Reviewer's comment ID #: 152-20)]	
SPM-522	A	6:41	6:42	Like sentence on urban heat islands but suggest rewording to "Urban heat island effects are a real but local additional effect, and are not the cause of these wider temperature rises" [Robert Nicholls (Reviewer's comment ID #: 191-3)]	Text has been edited and covers this
SPM-523	A	6:41	6:42	clarify the point about urban warming effects - is it saying they don't influence large-scale values because they're very small, or because they've been removed? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-119)]	See Ch 3. Urban stations have been removed.
SPM-524	A	6:41		Should "0.17" be "0.27"? See 3-8, line 25 [Richard Soulen (Reviewer's comment ID #: 248-8)]	Text edited
SPM-525	A	6:41		The large trend in warming that was observed in the 1910-1945 period needs to be explained relative to the more recent trend. If the former was natural variability, why not the latter, which is of similar magnitude? This distinction is particularly important for policymakers. This discussion might be more suitable for the Attribution section of the SPM (page SPM-10), but warrants mention here. Can we say why "most of the warming" occurred during the referenced periods vs. other periods in the 20th century? Or what the cooling factors might have been? Would it be desirable to indicate/reference why the rate of increase has been larger for the 1979-2005 period? [Govt. of United States of America (Reviewer's comment ID #: 2023-760)]	No attribution to be done here; this is the observations section.
SPM-526	A	6:41	:42	This is a significant change from the TAR. Please elaborate and add uncertainty qualifiers to the urban heat island discussion. Why can we discount the urban heat island effect? [Govt. of United States of America (Reviewer's comment ID #: 2023-761)]	Papers cited in Chapter 3 indicate that urban warming has not affected estimates of large-scale warming by a statistically significant amount. However the Brohan et al. (2006) analysis, which we use in Chapter 3, is conservative in retaining an urbanisation-uncertainty term of 0.0055°C/decade since 1900, the same as that used in the TAR.
SPM-527	A	6:42	6:42	This needs a careful wording. Heat release (not just urban heat) is far too small to influence the global-mean surface temperature. It can affect our estimate of the land surface temperature from in-situ measurements (because thermometers are located in regions prone to the urban heat island effect) but this effect is corrected for. Does this make more sense? [Olivier Boucher (Reviewer's comment ID #: 27-6)]	Urban effects do not influence the large-scale values because they are corrected for or avoided.

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SPM-528	A	6:42	6:42	Change "do not influence" into "have negligible influence on". [Govt. of Netherlands (Reviewer's comment ID #: 2016-6)]	Accepted
SPM-529	A	6:42		Replace "large-scale values" with "results". The hyphenated modifier is jargon. [Govt. of United States of America (Reviewer's comment ID #: 2023-762)]	Large-scale is standard English.
SPM-530	A	6:44	6:46	SPM Comment: This paragraph is simply not true. And, if it were true then it would indicate that the unknown magnitude of the intrinsic errors in the surface data sets are so large that these data sets are incapable of indicating global temperature changes. The paragraph should be replaced by one saying; "New analyses of balloon-borne and satellite measurements of lower-tropospheric temperature show much lower warming trends than occur in the surface temperature records. For the late 20th century warming period between 1976 and 2004, the temperature trends for the Jones et al., GISS, and GHCN surface temperature time-series are +0.215 degrees C/decade, +0.204 degrees C/decade and +0.274 degrees C/decade, respectively. These rates of change are significantly higher than the rate of +0.059 degrees C/decade for the lower atmosphere measured by weather balloon radiosondes for the same period and the rate of +0.079 degrees C/decade and satellite MSUs for the period 1979 to 2004 (the satellite record begins in 1979)."  [Richard Courtney (Reviewer's comment ID #: 49-12)]	Rejected. Data cited by the reviewer is known to contain large errors and is superceded by improved analyses discussed in detail in ch 3, where references are given to more accurate work that has superseded this.
SPM-531	A	6:44	6:46	This whole paragraph is untrue. The satellite and weather balloon records are significantly different from the surface record, and you have concealed this by plotting them all together. [VINCENT GRAY (Reviewer's comment ID #: 88-2174)]	Rejected. Data cited contains large errors and is superceded by new analyses discussed in detail in ch 3
SPM-532	A	6:44	6:46	Replace whole paragraph by the following. "The satellite and weather balloon records of lower-tropospheric temperature showed significant differences from the surface record. The satellite record showed no overall warming between 1979 and 1997. Then there was a large peak for 1998 associated with the strong El Niño event of that year. From 2002 to 2005 higher temperatures were observed, at present unexplained, but four years is insufficient to establish a possible trend. The weather balloon record showed no overall temperature change between 1958 and 2004.. There was a cooler period in this record between 1964 and 1978 which has been attributed to a change in ocean circulation". [VINCENT GRAY (Reviewer's comment ID #: 88-2175)]	Rejected. No backup for assertions made.
SPM-533	A	6:44	6:46	This phrasing is likely a bit over-stated as, while the global trends are now non-inconsistent, this is a result of the changes in the tropics and in the rest of the world being off by roughly equal and opposite amounts. I should add, however, a	Text has been edited; reflects conclusions of the chapter.

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				personal view that given the presence of inversions over much of the globe that locally disconnect the surface and the troposphere and the different thermal inertias of the oceans and land (so of the seasonal phasing of their temperature changes), it is not at all obvious that the surface and tropospheric temperature changes should be as closely related as is often suggested. [Michael MacCracken (Reviewer's comment ID #: 152-21)]	
SPM-534	A	6:44	6:46	Does the results include consideration of the latest NOAA report? Full citation-Temperature Trends in the Lower Atmosphere: Steps for Understanding and Reconciling Differences. Thomas R. Karl, Susan J. Hassol, Christopher D. Miller, and William L. Murray, editors, 2006. A Report by the Climate Change Science Program and the Subcommittee on Global Change Research, Washington, DC. [Franklin SCHWING (Reviewer's comment ID #: 230-1)]	Yes, see citations in chapter.
SPM-535	A	6:44	:46	The time period of these observations should be mentioned. [Govt. of Norway (Reviewer's comment ID #: 2018-15)]	For details see chapter. Length constraints in SPM
SPM-536	A	6:44	:46	Should remaining discrepancies in the tropics (per CCSP S&A Product 1.1) be mentioned or at least referred to (as not everything is perfectly reconciled)? A possible way of handling this would be adding "in most areas" after "...that are consistent with the surface temperature record within their respective uncertainties" or adding "mostly" before consistent. [Govt. of United States of America (Reviewer's comment ID #: 2023-763)]	SEE COMMENT SPM-533.
SPM-537	A	6:45	6:46	suggest delete "within their respective uncertainties" as surely this is of course true or you wouldn't have the statement [Joanna House (Reviewer's comment ID #: 109-34)]	SEE COMMENT SPM-533.
SPM-538	A	6:46		Change the last phrase (starting "representing") to "eliminating a discrepancy present at the time of the TAR.", or something similar. It is a clearer statement of the finding's significance. [Timothy H. Profeta (Reviewer's comment ID #: 203-10)]	accepted
SPM-539	A	6:48	6:48	Replace "air temperatures" with "near surface temperatures" [Govt. of Australia (Reviewer's comment ID #: 2001-21)]	Now footnoted at first occurrence
SPM-540	A	6:48	6:49	"Perhaps qualify ""air temperatures over land"" and ""those over the ocean""; are these average values, and if so, should they reflect the fact that air temperatures have not changed uniformly?" [Govt. of Canada (Reviewer's comment ID #: 2004-31)]	Accepted
SPM-541	A	6:48	6:49	SPM Comment: Delete this paragraph because it reports trends of the surface temperature records, but those trends are spurious (where 'spurious' means a difference from reality). The existence of the spurious trends is demonstrated by	SEE COMMENT SPM-495

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			<p>the statistically significantly different trends between the global values of the surface temperature data sets.</p> <p>Indeed, the compilers of these data sets admit their methods create spurious trends (ref. Vose et al., 2004). Their methods include integrating measurements into mean values for regions over the Earth’s surface called ‘grid boxes’. And the integrations cause trend problems for individual grid boxes. Vose et al. state that when the GHCN and Jones et al. trends are compared at the grid-box level then 9.3% of grid cells display “discrepant trends”. In other words, the integration of measurements into grid boxes causes 9.3% of grid boxes to have trends with opposite sign. (!)</p> <p>It cannot be reasonable to isolate geographical regions (e.g. land and oceans) and to assume that differences between grid boxes in those regions indicate real effects when errors at the grid box level are so common that 9.3% of grid cells display “discrepant trends”.</p> <p>Furthermore, the surface temperature records begin ~1860. For the period 1860 to 2004, the most cited of these data sets have good global coherence (which is not surprising because they were compiled from the same available source data), but their global trends (in degrees C/decade) and 2SD trend errors are                      GHCN: <math>0.076 \pm 0.010</math>                      Jones et al.: <math>0.064 \pm 0.007</math>                      GISS: <math>0.048 \pm 0.006</math></p> <p>The Jones trend is significantly different from the GISS trend (<math>p &lt; 0.05</math>), and the GHCN trend is very significantly different from the GISS trend (<math>p &lt; 0.01</math>). So, “coolest” and “warmest” years near the ends of the data sets are generated by trends that are known to be spurious.</p> <p>A change in the indicated temperature with time is only an indication of climate change when the change exceeds the intrinsic measurement errors (otherwise the change could be intrinsic measurement error). The intrinsic errors of GHCN, Jones et al. and GISS data sets are not known, but at least two of the data sets provide wrong results because they differ in annual change by more than double their stated 95% confidence limits in each of several years.</p> <p>Furthermore, the GHCN and Jones et al. data sets also have different trends for the most recent 30 years. For the period 1976-2004, the GHCN trend is 0.3032 degrees C / decade and the Jones et al. trend is 0.2166 degrees C / decade. And this also demonstrates that at least one of them is indicating a spurious trend generated by the methods used to create the means.</p> <p>Importantly, over the most recent 30 years the errors accumulating between the GHCN and Jones et al. data sets are of the order of 0.2 degrees C (because the</p>	

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				<p>changes to temperature indicated by their trends differ by that much). And the errors in each of these data sets may be more than 0.2 degrees C (because they are not known).</p> <p>Simply,</p> <p>(a) temperature measurements cannot indicate climate change in the absence of knowledge of their accuracy and precision,</p> <p>(b) coherence between data sets does not indicate their accuracy or precision, and</p> <p>(c) the accuracy and the precision are not known for mean global temperatures indicated by the GISS, GHCN and Jones et al. data sets, but</p> <p>(d) the known errors at grid box level indicate that it is not possible to use the GISS, GHCN and Jones et al. data sets for comparison of temperature trends over different geographical regions.</p> <p>[Richard Courtney (Reviewer's comment ID #: 49-13)]</p>	
SPM-542	A	6:48	6:49	<p>could give the totals to compare with the earlier bullet line 38</p> <p>[Joanna House (Reviewer's comment ID #: 109-35)]</p>	Rejected. SPM length constraints
SPM-543	A	6:48	6:48	<p>For clarity, replace 'air temperatures by 'near surface air temperatures'.</p> <p>[William Kininmonth (Reviewer's comment ID #: 128-95)]</p>	Agreed. SEE COMMENT SPM-540.
SPM-544	A	6:48	6:48	<p>This needs to say "near-surface air temperatures" so as not to be confused with tropospheric temperatures. And are the values over the ocean from SST or air temperature?</p> <p>[Michael MacCracken (Reviewer's comment ID #: 152-22)]</p>	Footnoted at first occurrence  SEE COMMENT SPM-540
SPM-545	A	6:49	6:49	<p>Add at end "This discrepancy exists also in the satellite measurements in the lower troposphere.</p> <p>[VINCENT GRAY (Reviewer's comment ID #: 88-2176)]</p>	Rejected. As shown in SPM-4 and attribution section, this is expected, not a disparity.
SPM-546	A	6:49	6:49	<p>Suggest including uncertainties in land/sea temperature trends.</p> <p>[Haroon Kheshgi (Reviewer's comment ID #: 125-11)]</p>	Statement moved; now covered later where it is broader
SPM-547	A	6:51	6:52	<p>"Suggest ""in a manner consistent with observed warming"""</p> <p>[Govt. of Canada (Reviewer's comment ID #: 2004-32)]</p>	Edited
SPM-548	A	6:51	6:52	<p>I do not believe this statement. Chapter 3 gives no evidence of a plausible "globally averaged" value, and because of the difficulties of measurement over land most acceptable measurements have been over the sea. Obtaining a scientifically based "average" is almost impossible with a quantity which varies so much over time, space and height. Most reliable measurements are very recent, so do not establish a trend, or a confirmation that they can be related to</p>	Rejected. Discussed in detail in chapter 3, where a range of measurements is given, including satellite data with global coverage.

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				other greenhouse gases. [VINCENT GRAY (Reviewer's comment ID #: 88-2177)]	
SPM-549	A	6:51	6:51	Delete "Globally averaged": replace "is" by "seems to be" [VINCENT GRAY (Reviewer's comment ID #: 88-2178)]	Rejected. No basis for comment presented by the reviewer
SPM-550	A	6:51	6:52	This statement should be changed to read, "in a manner consistent with observed warming." Rationale: Current statement is ambiguous (what kind of warming?). [Govt. of Japan (Reviewer's comment ID #: 2014-10)]	Accepted, clarified
SPM-551	A	6:51	6:51	Change "as well as in the" to "in both the lower and" [Michael MacCracken (Reviewer's comment ID #: 152-23)]	Rejected. Available data is total column and upper trop, so 'lower' is not really correct.
SPM-552	A	6:51	6:51	A trend cannot be established observationally in the present tense, only in the past tense. "is increasing" should instead read "has increased" A global search and replace for other instances throughout the report seems advisable. [P.C.D. Milly (Reviewer's comment ID #: 179-1)]	Accepted in this instance. To be considered where it may occur elsewhere as appropriate.
SPM-553	A	6:51	6:52	This should be moved to the next section on "broad range of climate variables". All the other bullets about precipitation patterns, droughts, and "acceleration of the water cycle" are in that section. [Daniel Murphy (Reviewer's comment ID #: 183-11)]	Rejected. This section covers variables of global scale. Next section deals with latitude-dependent and regional effects.
SPM-554	A	6:51		"Global average water vapor" is a single value. It does not have land and ocean and upper and lower troposphere values. Consider rephrasing to state "On average, the atmospheric water vapor content is increasing..." In addition to global increases, there are changes over land and oceans. [Govt. of United States of America (Reviewer's comment ID #: 2023-764)]	Clarified
SPM-555	A	6:51	:52	This statement is not consistent with the Chapter 9 Executive Summary. Break this apart so that you have a period after the word "ocean." As it's own bullet, discuss the upper troposphere, including the caveats given in the chapter. Remove "in a manner consistent with warming" which is an attribution statement and inappropriate for the Observations subsection. [Govt. of United States of America (Reviewer's comment ID #: 2023-765)]	There is nothing in the chapter 9 SOD ES on this. Reference is to chapter 3. Chapter 9 is not inconsistent. Will retain here material on simple physical reasoning, which is not an attribution statement.
SPM-556	A	6:52	6:52	Insert "possibly" after "consistent" [VINCENT GRAY (Reviewer's comment ID #: 88-2179)]	Rejected. No basis for comment presented by the reviewer
SPM-557	A	6:52		Consider adding explanatory language, if appropriate, like "because of increased rates of evaporation caused by observed warming." Can you say anything about changes in evaporation rates? [Govt. of United States of America (Reviewer's comment ID #: 2023-766)]	Rejected. We say it is consistent with observed warming, implying an obvious connection to evaporation but other factors play a role and such elaboration isn't practical in the summary.
SPM-558	A	6:53	6:53	Could add points here: are nighttime temperatures still increasing more than daytime (very relevant to ecosystem function and health) ? Are polar	DTR to be addressed later. Polar temperatures are also addressed later.

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				temperatures icncreasing more than non-polar (very improtant point for decision amkers to take on board)? [Joanna House (Reviewer's comment ID #: 109-36)]	
SPM-559	A	7:0	7:0	Fig SPM-3: I am not convinced that the snow cover is the best quantity to be brought up to the SPM level. I suggest precipitation as an alternative quantity. A figure for precipitation could be derived from Treydte et al., 2006, Nature, p. 1179, doi=10.1038/nature04743. The supporting discussion of this issue would probably best done in chapter 6. [Rolf Müller (Reviewer's comment ID #: 181-5)]	Rejected. Here we are explicitly showing independent evidnence for warming in the atmosphere, ocean, and cryosphere.
SPM-560	A	7:0		Figure SPM 3: Panel a and b , why show the difference from 1961 to 1990 and not from pre-industrial or 1850 or 1900 since these are the periods used in the text (e.g. total rise over the 20th century), it si also different from the period to which panel (b) is zeroed which seems to be around 1940 to 1950 but is not stated inteh axis label [Joanna House (Reviewer's comment ID #: 109-39)]	Have made material more consistent where practical.
SPM-561	A	7:0		Figures SPM 3: Why smooth and then have uncertainty in smopothd curve but still show original values. Also smoothed curve quite a technical term, cound you say trend line? Or running mean?. Also uncertainty can mean something quite different - is this uncertainty or one standard deviation around the smoothed line. [Joanna House (Reviewer's comment ID #: 109-40)]	Uncertainty is now better explained. Smooth curve shows the low frequency behavior but high frequency is also of interest.
SPM-562	A	7:0		Figure SPM 3: line 15. This increase is larger than that used in the bullets, perhaps this value and time period should eb sued isntead? [Joanna House (Reviewer's comment ID #: 109-41)]	Text has been edited
SPM-563	A	7:0		Fig. SPM3: Vertical labels inconsistent, middle panel gives the quantity, the others only the unit. [Reto Knutti (Reviewer's comment ID #: 133-43)]	Panels to be made consistent
SPM-564	A	7:0		Figure SPM-3: "Replace 1961-90" with "1961-1990" just to be consistent wth the rest of the years that appear in the text [Govt. of Spain (Reviewer's comment ID #: 2019-87)]	Copy-editing and figures to be finalized later.
SPM-567	A	7:1	7:2	""""Add the following line from the TS to the end of this bullet to stress the significance of this finding about ocean heat content to Policymakers. """"Whereas strong regional warming and cooling could represent redistribution of energy, a sustained global mean ocean warming reflects a change in the earth's total energy budget."""" from TS-25, line 51-52.""""  [Govt. of Canada (Reviewer's comment ID #: 2004-34)]	The desired statement is not consistent with revised material in chapters. Need to avoid the implication that we know such a change could not have occurred due to e.g., changes in global mean cloudiness, which can't be ruled out.

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SPM-568	A	7:1	7:2	This conclusion is very uncertain. Because the average temperature of the ocean to a depth of 3000m is due to the change on thousand year scale, not due to the change on century scale. [Govt. of China (Reviewer's comment ID #: 2006-14)]	Revised text. Focus is on upper 700m
SPM-569	A	7:1	7:2	This paragraph is misleading. Figure 5.2.1 shows that the heat content of the ocean fluctuates in a possibly cyclic manner. A peak value occurred in 1980 and the current value (2005) is no higher than it was then, if inaccuracies are considered. It is impossible to speak of an "average increase" over a period when it fluctuates up and down.. [VINCENT GRAY (Reviewer's comment ID #: 88-2180)]	Rejected. There is decadal variability but there also is a long-term trend.
SPM-570	A	7:1	7:2	Delete the present paragraph and replace it with " The heat content of the ocean varies in a possibly cyclic fashion. Measurements since 1955 show a steady figure until 1968, an increase to a peak in 1980, a fall to 1987, and a rise since then to a value in 2004 which may be slightly higher than 1980. [VINCENT GRAY (Reviewer's comment ID #: 88-2181)]	Rejected. There is decadal variability but there also is a long-term trend.
SPM-571	A	7:1	7:2	Can you relate this value to something a little more tangeable to policy makers e.g. the temeprature increase of the coean and/or the temeprature reduction int eh atmosphere [Joanna House (Reviewer's comment ID #: 109-37)]	Text has been edited
SPM-572	A	7:1	7:1	I am surprised to learn that there is a reliable assessment about the global temperature trend in the ocean down to 3000 m. In any event, this statement seems to be in contradiction with the statement in the SPM on p. 10, l. 20, which talks about the "upper several 100m" I have also not found any statement supporting the "down to 3000m" statement in the TS. [Rolf Müller (Reviewer's comment ID #: 181-4)]	See point above. Emphasise the 0-700 metre layer results and we then have accord with the attribution results refered to above.
SPM-573	A	7:1	7:1	The 3000m limit is not mentioned in the TS, but is directly base on the ES of Ch5. [Govt. of Netherlands (Reviewer's comment ID #: 2016-7)]	Accepted. SPM and TS will be made consistent
SPM-574	A	7:1	7:2	Ocean heat uptake and warming: add values for the measured ocean T-increase (plus error bars) down to 3000m. In addition, add estimates for the top 700m temperature increase and point out the penetration of heat from surface to depth. This seems an important piece of information for the attribution of the observed warming signal. [Gian-Kasper Plattner (Reviewer's comment ID #: 200-4)]	Rejected. Consistency with units elsewhere in SPM
SPM-575	A	7:1	7:2	Please add to theis remark that the temperature increases from top to bottom as discribed in 5.2. [Christoph, C. Raible (Reviewer's comment ID #: 207-3)]	Rejected, length constraints.

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SPM-576	A	7:1	7:2	It is not clear what the point of this sentence is, needs to be clearer for an SPM [Govt. of United Kingdom (Reviewer's comment ID #: 2022-120)]	Text revised SEE COMMENT SPM-568.
SPM-577	A	7:1	7:15	Somewhere in the text it should be stated that the global average sea level rise is 17 cm since 1850 [Govt. of United Kingdom (Reviewer's comment ID #: 2022-123)]	Rejected. No data prior to 1870. Chose 1961 for consistency with glacier data (page SPM-6).
SPM-578	A	7:1		Say by how much the temperature was risen [Stephen J. Hawkins (Reviewer's comment ID #: 102-13)]	Text edited. See ch 5 for temperature change.
SPM-579	A	7:1		Do you mean the global average temperature to a depth of 3,000 meters? Can you put real trends on it? [Govt. of United States of America (Reviewer's comment ID #: 2023-767)]	Text revised SEE COMMENT SPM-568.
SPM-580	A	7:1		For ocean temperature, avoid the term "risen" because it sounds like a sea level qualifier. Substitute "increased". [Govt. of United States of America (Reviewer's comment ID #: 2023-768)]	accepted
SPM-581	A	7:2	7:2	"rate of 0.2 Watts per metre squared...over what time period? 1955 to present (i.e. 50 years?). Also- this sentence seems rather technical for a Policymaker? What will this number mean to them? Can you explain by saying something about how the ocean responds more slowly than the atmosphere to any positive forcing?" [Govt. of Canada (Reviewer's comment ID #: 2004-33)]	Agreed, text revised SEE COMMENT SPM-568.
SPM-582	A	7:2	7:2	Suggest that some level of uncertainty on ocean heat uptake should be assessed and stated here, or else the quantitative information deleted. [Haroon Kheshgi (Reviewer's comment ID #: 125-12)]	Text has been edited
SPM-583	A	7:2	7:2	Here W/m**2 is used, while the ES of Ch5 uses Joules. [Govt. of Netherlands (Reviewer's comment ID #: 2016-8)]	Text has been edited
SPM-584	A	7:2	7:2	It might be helpful to add "entire" before "Earth's surface", otherwise it's not clear whether the statement refers only to land masses or the whole surface of the globe. [Andy Reisinger (Reviewer's comment ID #: 210-17)]	Text edited
SPM-585	A	7:2		Does 2 Wm-2 apply to the "entire Earth's surface" or just over the ocean fraction? Be explicit. [Govt. of United States of America (Reviewer's comment ID #: 2023-769)]	Clarified
SPM-586	A	7:4	7:8	Should include the trend in sea level over the whole period from 1850 - 2003 [Govt. of Australia (Reviewer's comment ID #: 2001-22)]	Rejected. No data prior to 1870. Chose 1961 for consistency with glaciers (page SPM-6). Revised text to reflect increased rate in 20th century.
SPM-	A	7:4	7:8	We suggest adding table TS-3, as it gives a very good overview of highly	Rejected. Material is reflected in text, so it is not clear

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587				relevant recent findings (new since TAR) with regard to different factors contributing to observed SLR. [Govt. of Germany (Reviewer's comment ID #: 2011-229)]	what would be value-added. Length constraints preclude in SPM.
SPM-588	A	7:4	7:4	Insert after "level" "from unrepresentative tide-gauge measurements" [VINCENT GRAY (Reviewer's comment ID #: 88-2182)]	Rejected. No basis given.
SPM-589	A	7:4	7:4	Are the confidence limuts for TWO standard deviatiuons? If they are only ONE they must be doubled. [VINCENT GRAY (Reviewer's comment ID #: 88-2183)]	Uncertainty has been clarified
SPM-590	A	7:4	7:4	Add at end "measured by satellite-based altimetry" [VINCENT GRAY (Reviewer's comment ID #: 88-2185)]	Rejected. Other data are also used here. See chapter for details.
SPM-591	A	7:4	7:5	suggest giving the total rise over the 20th century as you do for temeprature [Joanna House (Reviewer's comment ID #: 109-38)]	Rejected. Chose 1961 for consistency with glaciers (page SPM-6). Revise text to reflect increase rate in 20th century (Bindoff). Consistent with discussion of global temperature, and ch 5 ES.
SPM-592	A	7:4	7:5	I would urge giving an indication of the sources of the data by starting the first sentence as "Coastal tide gage stations indicate that global average ..." and then starting the second sentences with "Satellite data with global coverage indicate that the rate ..." [Michael MacCracken (Reviewer's comment ID #: 152-24)]	Rejected. Too detailed. Information in Chapter 5. Altimetry data tied to gauge data and give excellent agreement with tide gauge data.
SPM-593	A	7:4	8:7	Suggest consolidating the text from Page 8 lines 33-37 on ice sheet observations into this sub section on SL observations and its causes. Would also recommend adding Table 5.5.2 to the SPM as this reflects a significant advance since the TAR and adds clarity and real content to the SPM [William Hare (Reviewer's comment ID #: 99-105)]	Rejected. Global scale variables are presented here. Re. table: see response to SPM-587
SPM-594	A	7:4	:8	We request that information on the total sea-level rise from 1961 until 2003 be added (global average), if possible. [Govt. of Norway (Reviewer's comment ID #: 2018-16)]	Rejected, length constraints. Information on rate per year can be used to multiply by the reader.
SPM-595	A	7:5	7:5	Are the confidence limuts for TWO standard deviatiuons? If they are only ONE they must be doubled.. If so, both measurements are within error bands. [VINCENT GRAY (Reviewer's comment ID #: 88-2184)]	Clarified
SPM-596	A	7:5	7:5	Insert after "to", "inaccuracy, instrument calibration" [VINCENT GRAY (Reviewer's comment ID #: 88-2186)]	No basis given for suggestion.
SPM-597	A	7:5	7:6	Is this acceleration so uncertain (see Church and White, 2006)? [William Hare (Reviewer's comment ID #: 99-106)]	See ch 5. Apparent increase since 1993 thought to be natural variability. Statement has been added regarding 20th versus 19th centuries.
SPM-	A	7:5	7:6	Change "It is unclear" to "It has not yet been fully determined" and then start the	Text has been edited

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598				next sentence with "However, consistency" [Michael MacCracken (Reviewer's comment ID #: 152-25)]	
SPM-599	A	7:5	7:5	Why is it unclear? We know that glaciers are melting faster, see page 8 lines 3 to 7. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-121)]	It is not clear that recent increased rate of past ten years is a long-term acceleration. See ch 5
SPM-600	A	7:6	7:8	"strengthens evidence that ... is contributing to sea level rise" is not mentioned in the ES of Chapter 5. Perhaps better change into "The observed patterns ...and changes in ocean heat content ... are consistent." [Gerrit Burgers (Reviewer's comment ID #: 34-5)]	Taken into account. Paragraph rewritten.
SPM-601	A	7:6	7:8	The primary evidence for sea level rise contribution from ocean warming is the ocean warming data, and the equation of state (which needs no further evidence). Unless the ocean warming data are controversial (in which case this should be stated), I suggest that this sentence be deleted. [Haroon Kheshgi (Reviewer's comment ID #: 125-13)]	Text has been edited
SPM-602	A	7:6	7:8	"strengthens evidence that ... is contributing to sea level rise" is not mentioned in the ES of Chapter 5. So better change into "The observed patterns ...and changes in ocean heat content ... are consistent." [Govt. of Netherlands (Reviewer's comment ID #: 2016-9)]	Text has been edited
SPM-603	A	7:6		this amount can be attributed with some confidence to ghg increase as noise-free ebm calculations predict a post 1993 increase of about $0.1 \times 10^{22} \text{ J / yr}$ - see crowley et al 2003 grl doi:10.1029/2003GL017801, 2003, para. 14 [Thomas Crowley (Reviewer's comment ID #: 51-6)]	Inappropriate here – this is the observations section, not the attribution section.
SPM-604	A	7:7	7:8	Can this statement be made stronger? I thought most of the sea level rise to date was due to thermal expansion. [Olivier Boucher (Reviewer's comment ID #: 27-7)]	<b>Numbers given.</b>
SPM-605	A	7:7	7:7	Replace "strengthens evidence" by "indicates" [VINCENT GRAY (Reviewer's comment ID #: 88-2187)]	Rejected. No basis given for change.
SPM-606	A	7:7		Be more specific with regard to what the "changes in heat content" have been. Regarding time frame, why start in 1961? [Govt. of United States of America (Reviewer's comment ID #: 2023-770)]	Text edited. 1961 start of glacier data availability given in text.
SPM-607	A	7:8	7:8	Replace "is" by "may be" [VINCENT GRAY (Reviewer's comment ID #: 88-2188)]	Rejected. No basis given for change.
SPM-608	A	7:8	7:8	I would urge changing "is" to "caused by human-induced climate change is a primary factor " as it is not clear that, in reading the SPM, readers will associate "thermal expansion" with human-induced global warming. [Michael MacCracken (Reviewer's comment ID #: 152-26)]	Rejected. Not appropriate in the observations section.

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SPM-609	A	7:10	7:11	"Figure SPM-3, panel (a) Add linear trend as in Figure TS-7 to agree with the text on page 6."  [Govt. of Canada (Reviewer's comment ID #: 2004-35)]	Rejected. Trend provided in text. Trend line removed from TS figure.
SPM-610	A	7:10	7:11	"Figure SPM-3: It is difficult to interpret the time frames being compared (1850-1919 to 2001-2005) and how this compares to the conclusion in TAR that global mean temperatures rose by 0.6+/- over the last century. What is the point here? The contribution of the recent warm years to a further increase the global mean temperature? "  [Govt. of Canada (Reviewer's comment ID #: 2004-36)]	Text has been edited
SPM-611	A	7:10	7:11	I like the three panel figure, but I still prefer the old bar chart presentation for the top figure. I just think it is clearer and, if nothing else, people are used to it. [David Griggs (Reviewer's comment ID #: 90-3)]	Rejected for clarity and consistency.
SPM-612	A	7:10	7:11	Figure SPM-3 labelling - se level should be sea level [Meric Srokosz (Reviewer's comment ID #: 250-1)]	copy-editing will be done at a later stage.
SPM-613	A	7:10	8:2	add the separate average surface temperature for the NH and the SH [Govt. of Germany (Reviewer's comment ID #: 2011-73)]	Rejected. Length constraints
SPM-614	A	7:10	8:3	It is our view that part (c) of the figure can be deleted (Northern hemisphere snow cover), since we do not find this information as important as the information on global average temperature and sea level respectively. In addition, information on the reduction in NH snow cover of 5 % from 1966-2004 is included in the text. [Govt. of Norway (Reviewer's comment ID #: 2018-17)]	Disagree. Figure shows evidence that all three components of the earth system are changing (air, water, ice) in a manner consistent with a warming world
SPM-615	A	7:10		The labels on the vertical axis are confusing. The top plot should be something like "Difference from 1961-1990 Mean (°C)". Middle plot should be consistent with this: "Difference from XXXX-YYYY Mean (mm)" (caption does not say what reference period is used to calculate sea-level anomalies). Normally the unit of sea-level rise should be height/time, but present plot shows anomalies in mm. Caption should be changed accordingly. [Govt. of United States of America (Reviewer's comment ID #: 2023-771)]	Clarify labels and caption. Taken into account. Figure being revised. TSU to revise labels.
SPM-616	A	7:10		There appear to be inconsistencies between the global land surface temperature trends in Figure SPM-3 panel (a) and in Chapter 3; specifically, global mean temperature is in disagreement with that depicted in Figure 3.2.1, which shows the global land temperature for the period 1850-1919. One might argue that it is the difference between global and land temperature trends, but after 1910 they are in good agreement. Perhaps the land temperature record was too sparse prior	Rejected. Panel a is not just land temperature. Revise caption to clarify. Figure from which these data came is clearly given in the caption.

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				<p>to 1910 (but how extensive was the ocean temperature record for that period)? This disagreement has implication for the temperature trends since it is the mean value for this earlier period that is being compared against in the SPM (the average in Figure 3.2.1 appears considerably lower). Is this chosen for effect? Cite exactly which figures were distilled to construct the SPM figures so that readers don't have to guess.</p> <p>[Govt. of United States of America (Reviewer's comment ID #: 2023-772)]</p>	
SPM-617	A	7:10		<p>In Figure SPM-3 panel (a), the uncertainty depicted is inconsistent with the intention of the figure—which is, presumably, to show overall temperature changes since 1850. The uncertainty shading must be added to the structural uncertainty of the differences among the three (at least three) global data sets, including the NOAA and NASA data sets. The differences between all three data sets represent an estimate of the structural uncertainty in using various approaches to adjust for data biases. The shaded band, although not stated, only represents the parametric uncertainty for one data set. To fix this, plot a band bounded by all three global data sets.</p> <p>[Govt. of United States of America (Reviewer's comment ID #: 2023-773)]</p>	<p>The three global data sets give highly consistent global trends so this is not needed. See response to SPM-495.</p>
SPM-618	A	7:10		<p>In Figure SPM-3 panel (c), some estimate of decadal uncertainty should be provided or indicated in the legend, if no estimates are available. Construct needs to be reevaluated to reflect multi-decadal uncertainty in the trends, particularly because two data sets are being used.</p> <p>[Govt. of United States of America (Reviewer's comment ID #: 2023-774)]</p>	<p>Comment unclear. Uncertainty and smoothing will be consistent across 3 panels.</p>
SPM-619	A	7:10		<p>The figure caption talks about the change from the first 70 years of instrumental record (1850-1919). Make the figure correspond to the caption (which is done well) by making the zero delta-T for this 70-yr average.</p> <p>[Govt. of United States of America (Reviewer's comment ID #: 2023-775)]</p>	<p>Text has been edited</p>
SPM-620	A	7:11	7:	<p>Figure SPM-3: "While panel 1 of this figure explains that the y-axis is with respect to the 1961-90 average, panel 2 should more clearly indicate what the 0 represents (the 1961-90 average sea level?). Also, the y-axis label for Panel 1 should read ""difference from 1961-90 (°C) average""."</p> <p>[Govt. of Canada (Reviewer's comment ID #: 2004-38)]</p>	<p>Agreed. Clarify labels, caption.</p>
SPM-621	A	7:12	7:12	<p>Are the "uncertainty ranges" in the graphs based on one or two standard deviations. If only one, they should be doubled.</p> <p>[VINCENT GRAY (Reviewer's comment ID #: 88-2189)]</p>	<p>Clarified</p>
SPM-622	A	7:12	7:12	<p>"mean SURFACE temperature"</p> <p>[Keith Shine (Reviewer's comment ID #: 236-7)]</p>	<p>Agreed.</p>

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SPM-623	A	7:12	8:2	This is a new period for calculating the trend (1850-2005). This is different from the Third Assessment Report. Why? Give a rationale why the first 70 years are used as a baseline (why 1850-1919?). Make sure that all figures in this conglomerate are traceable. Where do the uncertainty bars come from? [Govt. of United States of America (Reviewer's comment ID #: 2023-776)]	Chapter 3 is now substantially based on Brohan et al. (2006) who extended the land and ocean record back to 1850. The uncertainty bars come from Brohan et al. The use of the first 70 years as a baseline is to sample pre-anthropogenic-warming climate. See ch 3. Figure tracing and uncertainty clarified.
SPM-624	A	7:14	7:14	Are the confidence limits for TWO standard deviations? If they are only ONE they must be doubled. If so, both measurements are within error bands [VINCENT GRAY (Reviewer's comment ID #: 88-2190)]	Clarified
SPM-625	A	7:14	7:15	I think that indicating the change over time is just the right way to indicate how much change has been occurring. [Michael MacCracken (Reviewer's comment ID #: 152-27)]	Thank you
SPM-626	A	7:15	7:17	"Panel b) discussion should include a mention of the uncertainty/ error bars (eg prior to 1950)" [Govt. of Canada (Reviewer's comment ID #: 2004-37)]	Taken into account. Caption revised
SPM-627	A	7:15	7:15	It looks a bit odd to compare a 5-year period with a 70-year period. If you do so, you should explain why you have chosen these time periods. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-122)]	Text has been edited
SPM-628	A	7:17	8:1	It is unclear why snow cover area in April is given here. Generally, snow cover in winter (DJF) or spring (MAM) is used. Please state the reason why "April" snow cover is used. [Govt. of China (Reviewer's comment ID #: 2006-15)]	Reasons for choice are left to the chapter. Now shows March-April average.
SPM-629	A	7:17		Add the words in all caps to the existing caption "...MEAN sea level, and NORTHERN snow cover area IN APRIL." [Govt. of United States of America (Reviewer's comment ID #: 2023-777)]	Accepted
SPM-630	A	7:17		The sea-level rise is deceptive/misleading here, since the implication is that the entire rise is due to warming. Adjust the caption to make more precise. [Govt. of United States of America (Reviewer's comment ID #: 2023-778)]	Rejected. Text (not caption) discusses analysis of thermal expansion and ice melt contributions.
SPM-631	A	8:1	8:2	The references in the Caption to the three Figures are probably inappropriate here (??) [Govt. of United Kingdom (Reviewer's comment ID #: 2022-124)]	Do not understand the comment. References allow reader to find original figures in report.
SPM-632	A	8:3	8:3	replace "in some locations" by "in a few locations", or give a quantitative idea of the percentage [Govt. of France (Reviewer's comment ID #: 2010-103)]	Statement has been confined to a broader one stating the average behavior. There are always some areas with different local behavior for every variable but the

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					average is what matters here.
SPM-633	A	8:3	8:6	Quantify by how much, what proportion are decreasing and what are increasing, this is critical given this is one of the current strong scientific arguments that some glaciers are increasing in size. [Joanna House (Reviewer's comment ID #: 109-42)]	Taken into account in revised bullet point. Delete remainder of sentence starting "There has been...". Length constraints. Detailed discussion in chapter.
SPM-634	A	8:3	8:3	State over which period the loss of land ice occurred [Rolf Müller (Reviewer's comment ID #: 181-6)]	Text has been edited
SPM-635	A	8:3		"land ice" - what is this? is it snow, glaciers only? ALSO, drop the parenthetical (particularly those that ...precip) it may be a random factoid here, but it is really is confusing here and does not add to the understanding. [Govt. of United States of America (Reviewer's comment ID #: 2023-779)]	Taken into account in revised text.
SPM-636	A	8:4	8:5	SPM Comment: The sentence is misleading to the degree of being untrue. It should be amended from "Mountain glaciers are declining in area and volume averaged over both hemispheres." to be "Mountain glaciers are estimated to be declining in area and volume averaged over both hemispheres but this estimate is highly uncertain because very few glaciers have been measured (only 30 are contained in the standard convenience sample assessed by the World Glacier Monitoring Service and National Snow and Ice Data Center/World Data Center for Glaciology. Boulder, CO. Digital media ( <a href="http://nsidc.org/data/glacier_inventory/">http://nsidc.org/data/glacier_inventory/</a> ), that can be accessed at <a href="http://www.wgms.ch/mbb/mb04/sum04.html">http://www.wgms.ch/mbb/mb04/sum04.html</a> )." <p>[Richard Courtney (Reviewer's comment ID #: 49-14)]</p>	Rejected. Chapter 4 documents the large number of glaciers that have been sampled and the many different regions these pertain to. Consistency across many different glaciers in each region and across regions strengthens confidence here.
SPM-637	A	8:5	8:7	I don't think it is very helpful for the reader to be giving trends here in terms of rate per year--and remember that on page 6 rates were given variously as per century and per decade, so really confusing. I think it would be more helpful to give the change in sea level over a period and then perhaps give a rate per decade in no--no one comes close to believing a rate per year). [Michael MacCracken (Reviewer's comment ID #: 152-28)]	Reject. mm/yr used in many places in SPM.
SPM-638	A	8:6	8:6	Are the confidence limits for TWO standard deviations? If they are only ONE they must be doubled.. [VINCENT GRAY (Reviewer's comment ID #: 88-2191)]	Clarified
SPM-639	A	8:7	8:7	Are the confidence limits for TWO standard deviations? If they are only ONE they must be doubled...	Clarified

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				[VINCENT GRAY (Reviewer's comment ID #: 88-2192)]	
SPM-640	A	8:9	8:9	Add, after "April", "(a sensitive month)" [Govt. of France (Reviewer's comment ID #: 2010-104)]	See SPM-628.
SPM-641	A	8:9	:10	Why is April the only month mentioned? Recommend including a generalization of trends at other times of the year lest there be accusations of "cherry-picking." [Govt. of United States of America (Reviewer's comment ID #: 2023-780)]	Rejected. Not cherry picked. Largest change in July. April chosen because of close relationship to temperature and important for impacts. Discussion in chapter 4. Length constraints here preclude further discussion. See SPM-628
SPM-642	A	8:10	8:11	"The sentence ""Permafrost temperatures have increased on average and the maximum area covered by seasonally frozen ground has decreased by about 7%..." could be better written as it implies permafrost temperatures have increased on average 7% which is not the case. Regarding permafrost temperatures, it would be better to say that an increase in permafrost temperatures has been observed (give appropriate time period) through out the permafrost region in the northern hemisphere, however the magnitude of the increase has varied."  [Govt. of Canada (Reviewer's comment ID #: 2004-39)]	Rejected. Text is specific and clear and does not imply temperature has increased everywhere by 7%. It says that on average the temperatures have increased.
SPM-643	A	8:10	8:10	replace "increased on average". By "generally increased" Table 4.7.1. gives only "ranges", not "averages". [VINCENT GRAY (Reviewer's comment ID #: 88-2193)]	Rejected. Table makes clear that changes are positive in nearly every region, so it follows that the average has increased irrespective of the exact value.
SPM-644	A	8:10	8:11	The sentence "Permafrost temperatures have increased on average and the maximum area covered by seasonally frozen ground has decreased by about 7%..." could be better written as it implies permafrost temperatures have increased on average 7% which is not the case. Regarding permafrost temperatures, it would be better to say that an increase in permafrost temperatures has been observed (give appropriate time period) through out the permafrost region in the northern hemisphere, however the magnitude of the increase has varied. [Sharon Smith (Reviewer's comment ID #: 244-77)]	SEE COMMENT SPM-642
SPM-645	A	8:14	8:15	"This title is very weak and confusing - doesn't convey much."  [Govt. of Canada (Reviewer's comment ID #: 2004-40)]	Clarified in revised headline
SPM-646	A	8:14	8:56	the range of indices of climate changes should be added by some more variables and their trends in the last decades:e.g. snow cover, permafrost, changes in the ocean, salinity [Govt. of Germany (Reviewer's comment ID #: 2011-74)]	Global variables covered in the previous section; regional variables here. Clarified in revised headline.

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SPM-647	A	8:14	8:14	Delete "systematic" There is no evidence that recent changes are due to some sort of "system" [VINCENT GRAY (Reviewer's comment ID #: 88-2194)]	Text has been edited
SPM-648	A	8:14	8:14	Change "shows evidence of systematic changes" into "change" (A trend can always be part of a slower "cycle"). [Govt. of Netherlands (Reviewer's comment ID #: 2016-10)]	Text has been edited
SPM-649	A	8:14	:15	All the material under this header does not reflect a finding in Chapter 4 that the Antarctic and Greenland ice sheets are shrinking. Why is that not repeated here? Also the last two bullets seem to fit better under an Extremes heading than Systematic Change. [Govt. of United States of America (Reviewer's comment ID #: 2023-781)]	Headline notes extremes along with systematic change. Statements about Greenland and Antarctica are separated for clarity and consistent with error bars in chapter.
SPM-650	A	8:15	8:15	I think all these bullet points need to say explicitly the period over which the trends are referring too. [Keith Shine (Reviewer's comment ID #: 236-8)]	Agreed where practicable.
SPM-651	A	8:17	8:17	Start Page SPM-8 Start Line 17 End Page SPM-8 End Line 17 SPM Comment: Delete the word "Widespread" because the table does not show that (it only says they happened), and the word "widespread" is not defined or justified in the table or the bulk of the document.  [Richard Courtney (Reviewer's comment ID #: 49-15)]	Rejected. SPM uses standard language found in any dictionary, such as "widespread." Chapters present a great deal of evidence regarding the extent of changes referred to.
SPM-652	A	8:17	8:18	Slightly picky comment, but speaking of "warm" and "cold" temperatures is like speaking of "wet" and "dry" precipitation. The correct phrase would be "high" and "low" temperatures. [Andy Reisinger (Reviewer's comment ID #: 210-18)]	Rejected. The adjective 'warm' is modifying 'extremes', not temperature here.
SPM-653	A	8:17	8:17	Have cold temperature extremes really decreased? Wouldn't they increase in a warming climate! Be clear about frequencies vs. temperature changes. [Govt. of Sweden (Reviewer's comment ID #: 2020-7)]	Agreed. Text edited
SPM-654	A	8:17	8:18	This is a very short sentence on a very critical issue. Please expand and add trends in extremes. Suggest bring all the extremes together in one section. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-125)]	Rejected. Table covers the range of extremes and is referred to.
SPM-655	A	8:17	8:56	This section is not structured and is confusing. It would be helpful to replace it with a set of diagrams showing changes from 1850 to present and then projections to 2100 or beyond on precipitation, extremes, ice cover, etc. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-126)]	Rejected. Headline now clarifies reason for structure. Projections occur later. Diagrams mixing projections and observations could be misleading.

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SPM-656	A	8:17		Explain "Widespread increases in warm..." What is this? An increase in the warmth, the extreme, the extent? Please rewrite in plain terms. [Govt. of United States of America (Reviewer's comment ID #: 2023-782)]	Text edited
SPM-657	A	8:20	8:23	Changes in what direction? [Joanna House (Reviewer's comment ID #: 109-43)]	Rejected. Change is in the circulation pattern, as stated.
SPM-658	A	8:20	8:23	There may have been changes in these variables, but as long as it cannot be associated with global warming it does not deserve to be mentioned in the SPM. [Govt. of Sweden (Reviewer's comment ID #: 2020-8)]	Rejected. Association made clear shortly on page 11, lines 15-16.
SPM-659	A	8:21		What sorts of "Changes"...in mid-latitude westerly winds, etc. [Govt. of United States of America (Reviewer's comment ID #: 2023-783)]	Rejected. Change is in the pattern, as stated.
SPM-660	A	8:22	8:22	"For clarity, suggest "... as well as ocean wave height..." [Govt. of Canada (Reviewer's comment ID #: 2004-42)]	Accepted
SPM-661	A	8:22	8:23	I think it would be helpful to define "high sea level events" in a caption, as they can vary in type and persistence (storm surges, El Nino, etc.) and this is not an often used term. [Michael MacCracken (Reviewer's comment ID #: 152-29)]	Text edited
SPM-662	A	8:25	8:26	Rates per decade aren't very meaningful for policy - can you give an overall x% since whatever number? [Govt. of Canada (Reviewer's comment ID #: 2004-41)]	Rejected. Timeframe is given so the reader can compute this but trend is consistent with use elsewhere
SPM-663	A	8:25	8:25	Are the confidence limits for TWO standard deviations? If they are only ONE they must be doubled... [VINCENT GRAY (Reviewer's comment ID #: 88-2195)]	Clarified
SPM-664	A	8:25	8:31	The first and last sentences belong more to previous section here ice changes discussed. [Joanna House (Reviewer's comment ID #: 109-44)]	Rejected. Previous section dealt with global and hemispheric scales. Here regionally specific issues are noted. Headline has been changed to clarify.
SPM-665	A	8:25	8:26	Here trends are given in per cent per decade (shifting sea level rise to per decade would also make sense). I would also urge giving the total percent change in parentheses. [Michael MacCracken (Reviewer's comment ID #: 152-30)]	SEE COMMENT SPM-662
SPM-666	A	8:25	8:37	Conversely, page SPM-8 lines 25 to 37 should be moved into the section starting on page SPM-6 that includes "snow and ice". That would put the statement about changes in sea ice next to the statement about changes in land ice. [Daniel Murphy (Reviewer's comment ID #: 183-12)]	Rejected. One is regional, the other global. See SPM-664
SPM-	A	8:25	8:31	Will this be updated to take 2006 into account? On page 6, line 35, there is a note	

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667				that a similar type of statement will be updated. And there are several places later in the report where reference is made (sometimes inconsistently - see later comments) to specific recent years as the warmest one or few on record. These too will need changing for consistency. [Adrian Simmons (Reviewer's comment ID #: 242-3)]	Will try. But the September value will not be available before mid-October.
SPM-668	A	8:26	8:31	I recommend to drop the sentences "The smallest extent of ... and circulations there". They cannot be found in the ES of Ch4, I could not find them in section 4.4, and it is rather differently phrased in the TS. [Gerrit Burgers (Reviewer's comment ID #: 34-6)]	The statement on Antarctic sea ice is in ch 4 and the temperatures are in ch 3. Smallest extent of Arctic ice is shown in ch 4 figures
SPM-669	A	8:26	8:26	Are the confidence limits for TWO standard deviations? If they are only ONE they must be doubled... [VINCENT GRAY (Reviewer's comment ID #: 88-2196)]	Clarified
SPM-670	A	8:26	8:31	Drop the sentences "The smallest extent of ... and circulations there". They cannot be found in the ES of Ch4, I could not find them in section 4.4, and it is rather differently phrased in the TS. [Govt. of Netherlands (Reviewer's comment ID #: 2016-11)]	SEE SPM-668
SPM-671	A	8:27	8:27	Inset after "Average" , "land-based" [VINCENT GRAY (Reviewer's comment ID #: 88-2197)]	Rejected. Statement includes information from satellites where appropriate
SPM-672	A	8:27	8:28	To shorten the SPM, delete "Average Arctic...1945" [Daniel Murphy (Reviewer's comment ID #: 183-13)]	Rejected. Believe this is needed for balance.
SPM-673	A	8:27	:31	We had expected the most important finding(s) of the ACIA – Arctic Climate Impact Assessment to be included in the SPM. This assessment was prepared over five years by an international team of over 300 scientists, and we consider it a major achievement. The report was called for by the Arctic Council as a response to the gaps in knowledge regarding regional climate change expressed in the TAR. We would like to comment on the two sentences included in this paragraph: "Average Arctic temperatures have been rising since the 1960s, and 2005 was the warmest Arctic year. However, Arctic temperatures are variable, and a warm period was also observed from 1920-1945." Compared to the findings of the ACIA, this is a major understatement of what is happening in the Arctic. We propose the following sentence from the ACIA-report to replace the ones cited: "The annual average arctic temperature has increased at almost twice as the rate as that of the rest of the world over the past few decades, with some variations across the region. 2005 was the warmest Arctic year."	Accept the ACIA sentences but also retain the mention of the mid 20th Century Arctic warmth, qualifying it following SPM-676 below.

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				[Govt. of Norway (Reviewer's comment ID #: 2018-18)]	
SPM-674	A	8:28	8:29	"These two sentences about Arctic temp change seem misleading - why the 'however'? What is being implied - that the warming is not expected to continue? Why not say that the Arctic temp trend matches the global trend, with a warming period in the first part of the century, then a cooling, then a warming again?"  [Govt. of Canada (Reviewer's comment ID #: 2004-43)]	Text has been edited.  The Arctic is different from the global in the ratio of the early 20th century to recent warming is much larger, so the suggestion is not appropriate to state.
SPM-675	A	8:28	8:28	"In saying that ""Arctic temperatures are variable"", the text implies that other temperatures are not. Perhaps qualify this with some comparison to other parts of the world?"  [Govt. of Canada (Reviewer's comment ID #: 2004-44)]	Rejected. Context makes clear that the variability is high compared to the trend.
SPM-676	A	8:28	8:29	It really needs to be made clear (and the results are indicated in the Arctic Climate Impacts Assessment) that only parts of the Arctic were warm from 1920-1945. Certainly, there are many contraindications that this was the case in northwestern North America and eastern Siberia--it is just that there were not many measuring stations there. So, it is important to add the phrase "in some parts of the Arctic" after "observed" or maybe say "in the North Atlantic sector" or something similar--but it really needs to be made clear that the present Arctic wide warm period is very different than the one in the early 20th century. [Michael MacCracken (Reviewer's comment ID #: 152-31)]	Accepted, text has been clarified
SPM-677	A	8:28	8:29	Wasn't the warmth between 1920 and 1945 less sustained and spatially extensive than that during the past few decades? Please state. [James S. Wang (Reviewer's comment ID #: 281-5)]	See response to SPM-676.
SPM-679	A	8:29	8:31	Change last sentence to "Antarctic mean sea ice extent since 1978 shows inter-annual variability but no consistent trends that are statistically significant and consistent with both atmospheric and oceanic temperatures and circulations" [Govt. of Australia (Reviewer's comment ID #: 2001-23)]	Text has been edited along the lines suggested
SPM-680	A	8:29	8:29	"How does the current warm period in the Arctic compare to the one cited from 1920-1945?"  [Govt. of Canada (Reviewer's comment ID #: 2004-45)]	See response to SPM-676.
SPM-681	A	8:29	8:31	"In the sentence ""Antarctic mean sea ice extent continues to show inter-annual variability but no consistent trends, consistent with temperatures and circulation there"", are the temperatures and circulation being referred to in the air, in the surrounding ocean, or both? A few explanatory words would help here."	Refers to the atmosphere. Text clarified

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				[Govt. of Canada (Reviewer's comment ID #: 2004-46)]	
SPM-682	A	8:29	8:29	It might be helpful to compare the 1920-1945 period to the current one. Suggestion: "However, Arctic temperatures are variable, and a warm period, with lower temperatures but longer duration than the present warming, was also observed from 1920-1945." Add [3.2] as reference chapter. [Andy Reisinger (Reviewer's comment ID #: 210-19)]	See response to SPM-676.
SPM-683	A	8:29		What is not noted here that is very significant is that the 1940s showed a very different latitudinal pattern from that of the 1990s, and what might be expected from greenhouse gases. This is a key result when comparing these two very different warm periods and is missed entirely by this summary. [Govt. of United States of America (Reviewer's comment ID #: 2023-784)]	See response to SPM-676.
SPM-684	A	8:29	:35	Move the last sentence of the third bullet to its own bullet. Remove the reference to Antarctic in line 34. Greenland and the Antarctic should be their own bullets. Then clarify message. The implication in the current text is that there are no trends when in reality there are (e.g., the warming of the Antarctic peninsula). [Govt. of United States of America (Reviewer's comment ID #: 2023-785)]	Accepted in part. Antarctic sea ice has been moved. Text has been edited for clarity. Greenland and Antarctica kept together for ice sheet statement because of common limitation of uncertainties.
0-95	A	8:30		SPM: repeat of "consistent"; suggest changing sentence to: Antarctic mean sea ice extent continues to show inter-annual variability, consistent with temperatures and circulation changes, but no coherent trends." 5 0-5 58 [Richard Allan (Reviewer's comment ID #: 3-17)]	Text has been edited
SPM-685	A	8:30		Rework sentence for clarity. [Govt. of United States of America (Reviewer's comment ID #: 2023-786)]	Text has been edited
SPM-686	A	8:30	:31	"consistent with temperatures and circulation there" needs clarification, as sea ice is affected by, and effects, both atmosphere and ocean. Suggest changing to "consistent with both atmospheric and oceanic temperatures and circulations." [Andrew Watkins (Reviewer's comment ID #: 283-2)]	Rejected, cannot substantiate the many specifics suggested here with material in the chapter
SPM-687	A	8:33	8:37	This para could be moved to add to the sub section pages 7 line 4 - page 8 line 7. In addition the dynamic character of these ice sheet changes needs to be brought out in the SPM (eg issue of fast ice dynamics and the link to surface and or deeper ocean warming, See eg the likely cause of the acceleration of Amundsen sea ice streams eg on Chapter 4 page 27 lines 11-28 the issue is related to basal melting and increased ocean temperatures. Given the significance of this issue in the context of questions of the stability of the WAIS (See Vaughan 2006 in press) it would be very relevant. [William Hare (Reviewer's comment ID #: 99-109)]	Rejected. Paragraph has regional flavour. Sharpen last part of paragraph, by noting that ice flow speed has accelerated in some coastal regions.

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SPM-688	A	8:33	8:37	Belongs more to earlier section on sea level rise [Joanna House (Reviewer's comment ID #: 109-45)]	Rejected. This is here because it depends on regional processes.
SPM-689	A	8:33		We suggest to delete or rewrite the first period of this bullet, it is too complicated. Write like this: "Identification of trends in patterns of precipitation is still limited by the length and consistency of records." [Govt. of Norway (Reviewer's comment ID #: 2018-23)]	This applies to line 43 not 33.  Text has been edited
SPM-690	A	8:34	8:34	"Mass loss from the Greenland ice sheet" This is untrue. Zwally et al 2005 Journal of Glaciology Vol 51, No 175 Vol 51, page 509 to 527 found that between 1992-2002 there was a loss of ice at the margins but a greater increase in the centre, a small net increase which produced an estimated decline of 0.03±0.01mm/yr. [VINCENT GRAY (Reviewer's comment ID #: 88-2198)]	Rejected. See assesment of the range of papers in ch 4
SPM-691	A	8:34	8:34	Insert "margins of" after "from", Insert "was more than compensated by an increase in the centre which" after "Sheet" [VINCENT GRAY (Reviewer's comment ID #: 88-2199)]	Rejected. See assessment of both effects in chapter 4
SPM-692	A	8:35	8:35	Replace "raising" by "lowering". Replace "0.21±0.7" by "0.03±0.01" [VINCENT GRAY (Reviewer's comment ID #: 88-2200)]	Rejected. No basis given for change
SPM-693	A	8:35	8:35	Greater uncertainty exists is a clumsy form of words. Better to say "Estimates from earlier periods are more uncertain" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-127)]	Rejected. Current wording is clear and accurate and allows inclusion of Antarctica, which would become longet with this proposal.
SPM-694	A	8:35		Can you indicate what % of sea level rise the Greenland Ice Sheet loss represents? [Timothy H. Profeta (Reviewer's comment ID #: 203-11)]	Rejected. Too much detail for SPM.
SPM-695	A	8:36	8:36	Suggest adding "glacier" after "ice sheet" as the ice sheets are heterogeneous and it is the glacier flows that have been measured. [Haroon Kheshgi (Reviewer's comment ID #: 125-14)]	Rejected. Ice streams as well as glaciers have accelerated. See glossary.
SPM-696	A	8:36	8:36	The last sentence is a bit cryptic to the lay person. It might be helpful to expand this a little to say "Recent observations also show rapid changes in ice sheet flows following thinning or disintegration of nearby ice shelves." [Andy Reisinger (Reviewer's comment ID #: 210-20)]	Not all such flows are following disintegration. Text has been edited.
SPM - 697	A	8:36		We suggest adding the following relevant sentence from the TS: " The volume of the Greenland and Antarctic ice sheets are equivalent to approximately 7 m and 57 m of sea level rise, respectively." [Govt. of Norway (Reviewer's comment ID #: 2018-19)]	Rejected. Would be misleading since evidence does not support the idea that all of either ice sheet is likely to melt in the coming century. Neither has completely melted in many thousands of years including periods of warm climates (Eemian).
SPM-698	A	8:36		The sentence starting "Recent observations. . ." is not clear in its significance for	Text has been edited.

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				policy makers. [Timothy H. Profeta (Reviewer's comment ID #: 203-12)]	
SPM-699	A	8:36		Change "in Antarctica" to "for Antarctica" [Govt. of United States of America (Reviewer's comment ID #: 2023-787)]	accepted
SPM-700	A	8:39	8:47	The paragraph on precipitation trends should precede the paragraph on drought. The first sentence of the current line 39 would better read "Droughts have become widespread in various parts of the world since the 1970s and greater continental evapotranspiration is noted. The paragraph on precipitation tries to generalise to the extent that it is wrong. The statement that there is less precipitation over land from 10N to 30N after about 1970, while increased precipitation has been observed poleward of about 30 degrees in both hemispheres is actually quite incorrect when applied to the Australian situation. Note Ch 3-4, line 21 refers only to South America. [Govt. of Australia (Reviewer's comment ID #: 2001-24)]	Shift drought bullet to after latitudinal changes. Text edited to account for second point.
SPM-701	A	8:39	8:40	I do not think that this statement is supported by the material in chapter 3. This bullet falls under the "Direct observations of changes in current climate" section. I do not think there are evidences of an acceleration of the water cycle. The greater continental evapotranspiration has been reconstructed from other climate variables in a couple of regions but it certainly does not hold on a global scale. With an overall decrease in land precipitation and an overall increase in runoff over the last 4 decades (Labat et al., 2004; Gedney et al., Nature, 2006), it is very unlikely that there has been an increase in land evapotranspiration. This bullet needs to be made consistent with both chapter 3 and chapter 9 (detection and attribution). [Olivier Boucher (Reviewer's comment ID #: 27-8)]	Text edited to account for this
SPM-702	A	8:39	8:39	There is not likely an acceleration of the hydrological cycle but an intensification of this cycle. Also, droughts have not increased everywhere; please be more specific. [Govt. of France (Reviewer's comment ID #: 2010-105)]	Text edited
SPM-703	A	8:39	8:41	clarify where droughts have increased [Govt. of Germany (Reviewer's comment ID #: 2011-75)]	Text edited.
SPM-704	A	8:39	8:41	I can find no evidence for the statement "Droughts have increased" in Paragraph 3.3.4 of Chapter 3. [VINCENT GRAY (Reviewer's comment ID #: 88-2201)]	Section 3.3.4 gives a region by region summary consistent with the SPM statement and this is also reflected in the chapter Exec Summary
SPM-705	A	8:39	8:39	Replace "have increased" with "are" [VINCENT GRAY (Reviewer's comment ID #: 88-2202)]	Rejected. Inconsistent with chapter and basis is not given.
SPM-	A	8:39	8:40	explain "acceleration of the water cycle' and "greater continental ET"	Text edited

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706				[Joanna House (Reviewer's comment ID #: 109-46)]	
SPM-707	A	8:39	8:41	These statements appear illogical as they read. An accelerated hydrological cycle would lead to greater precipitation and, without further explanation, less drought. Suggest the paragraph be amplified to read: "Rainfall over tropical and subtropical land areas has decreased consistent with a shift in preferred precipitation to favour ocean regions. Generally higher temperatures and increased evapotranspiration have exacerbated drought episodes. Observed changes in tropical sea surface temperature (SST) can also be linked to changed atmospheric circulation patterns and to decreased snowpack and snow cover in some regions". [William Kininmonth (Reviewer's comment ID #: 128-96)]	Text edited
SPM-708	A	8:39	8:40	I expect this sentence will not be clear or obvious to a PM. Indeed the statement that 'droughts have increased' is not a valid conclusion from chapter 3. [Michael Manton (Reviewer's comment ID #: 157-25)]	Text edited
SPM-709	A	8:39	8:40	The meaning of "acceleration" of the water cycle is ambiguous and the term is perhaps better avoided, perhaps replaced by more transparent wording. Additionally, isn't the changing pattern of water fluxes more important than a global trend, given that there are regional ups and downs? [P.C.D. Milly (Reviewer's comment ID #: 179-14)]	Text edited
SPM-710	A	8:39	8:40	Is the cited "greater continental evapotranspiration" a hypothesized one or an observed one that is documented in the report? It's not clear from the text. In fact, the deduced (by water balance, precipitation minus streamflow) ET can mostly be explained by (mostly 'random') changes in precipitation (e.g., Milly and Dunne, 2001). A more concrete independent confirmation of precipitation drought inferences can be given by reference to observed global pattern of mean streamflow change identified by Milly et al (2005), not currently cited in section 3.3, but proposed in another comment for citation therein. [P.C.D. Milly (Reviewer's comment ID #: 179-15)]	Text edited
SPM-711	A	8:39	8:39	Is this sentence meant to be read "consistent with HYPOTHESIZED acceleration in the water cycle [whatever that means] and HYPOTHESIZED greater continental evapotranspiration." Or was it meant to refer to OBSERVED things. If the latter, then where is the supporting material? [P.C.D. Milly (Reviewer's comment ID #: 179-22)]	Text edited
SPM-712	A	8:39	8:39	There is not likely an acceleration of the hydrological cycle but an intensification of this cycle (see chapter 10, p4, lines 6-9 and p16, lines 46-48; see also TS p52, line 44). [Serge PLANTON (Reviewer's comment ID #: 199-8)]	Text edited

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SPM-713	A	8:39	8:40	It might be useful to add "associated with increased temperatures" at the end of the first sentence, if the authors feel this attribution is sufficiently robust and justified. [Andy Reisinger (Reviewer's comment ID #: 210-21)]	Text edited. Drought is not universally associated with warmer temperatures.
SPM-714	A	8:39	41:	Replace by: "Droughts have increased in several regions. This may be linked to changing circulation patterns, changes in sea surface temperatures, higher evaporation, and decreased snow abundance." [Govt. of Netherlands (Reviewer's comment ID #: 2016-12)]	Text edited
SPM-715	A	8:39		SPM 8, line 39. It could be clarified whether findings compiled regarding Droughts are widespread or based on data from specific areas of the world. [Govt. of Chile (Reviewer's comment ID #: 2005-4)]	Text edited. See chapter for more detail
SPM-716	A	8:39		We would expect that droughts have increased in some particular regions (not everywhere as it may seem like now), and suggest this information be added here. [Govt. of Norway (Reviewer's comment ID #: 2018-20)]	Text edited
SPM-717	A	8:39	:40	It is stated that droughts have increased but there is no indication about locations of the increase, duration, frequency, or magnitude. This assertion needs to be substantiated with additional detail. [Govt. of United States of America (Reviewer's comment ID #: 2023-788)]	Text edited
SPM-718	A	8:39	:42	This statement is not supported by the observations in Chapter 3. Overall precipitation has increased since 1900, with only recent decreases (last few decades) in the tropics. At best the statement requires temporal and spatial qualification, but more generally it seems biased not to mention that droughts may have decreased where precipitation has increased (high latitudes). There is concern about the statistical significance of any trend in droughts. Of special concern is whether this statement is based on the diagram related to the drought question in Chapter 3. There the first PC is shown with an increasing trend toward droughts, but it reflects only 6% of the total variance. A better statistic would be the percentage of land areas affected by moderate to extreme drought. The drought bullet should be dropped in its present form. It does not contribute much and is not of enough significance to be included in the SPM. [Govt. of United States of America (Reviewer's comment ID #: 2023-789)]	Increased precip does not mean fewer droughts, owing to increases in evaporation as well. See the PDSI map. The increase in drought is not based on the first EOF but on PDSI < -3 Fig 9 of Dai et al 2004. The map is quite similar. Text edited to clarify where changes have occurred, and when.
SPM-719	A	8:40	8:40	"Clarify whether ""circulation patterns"" refer to atmosphere or ocean." [Govt. of Canada (Reviewer's comment ID #: 2004-47)]	Accepted. Atmospheric added.
SPM-	A	8:40	8:40	What changes? Increase?	Rejected. Cannot use this term with reference to

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720				[Joanna House (Reviewer's comment ID #: 109-47)]	circulation
SPM - 721	A	8:40	:41	We suggest deleting the last period of this bullet, it is too general and too unclear. [Govt. of Norway (Reviewer's comment ID #: 2018-21)]	Rejected. Insufficient basis given for comment. We don't consider that a link of drought to less snowpack is unclear.
SPM-722	A	8:40		Can you add at the end of the sentence the phrase, "from warming", so that the reader understands its significance? [Timothy H. Profeta (Reviewer's comment ID #: 203-13)]	Rejected. This is the observations section, not the attribution section.
SPM-723	A	8:40		"circulation patterns" should be clearly defined as "atmospheric circulation patterns" [Andrew Watkins (Reviewer's comment ID #: 283-3)]	Agreed.
SPM-724	A	8:41	8:41	linked to drought how? Though decrease in water circulation? [Joanna House (Reviewer's comment ID #: 109-48)]	Rejected. Depends on which variable – snow, circulation etc.
SPM-725	A	8:43	8:47	For continuity, put this paragraph on precipitation before the preceding one that deals with drought. [William Kininmonth (Reviewer's comment ID #: 128-97)]	OK
SPM-726	A	8:43	8:46	On line 43, change "precipitation trends" to "changes in precipitation" as the changes are not really monotonic--nor should they be, in some regions. On line 45, change "after" to "since". On line 46, change "trends" to "changes" [Michael MacCracken (Reviewer's comment ID #: 152-32)]	Accepted in part. Some editing of text.
SPM-727	A	8:43	8:44	What does it mean to say that "trends are emerging." The wording here seems vaguely and presciently to imply a climate-change signal that is on a trajectory to rise above internal variability literally tomorrow. Is this supported by analysis from the cited sections? Or should one simply describe the trends that are observed, without the potentially misleading word "emerging." If so, is the reader to understand that these are consistent with model simulations, or are they internally generated by the climate system? Suggest hydrologic focus instead on the streamflow results, which have linked model simulations to observations. [P.C.D. Milly (Reviewer's comment ID #: 179-16)]	Text edited
SPM-728	A	8:43	8:47	New and independent support for observed patterns of precipitation trends are found not only in salinity but also in streamflow. [P.C.D. Milly (Reviewer's comment ID #: 179-17)]	Rejected. Cannot be applied to all the regions considered here although it is true in some.
SPM-729	A	8:43	:46	Restate what the "trends" are that are referenced in this bullet. [Govt. of United States of America (Reviewer's comment ID #: 2023-790)]	Text edited.
SPM-730	A	8:44	8:44	"On average". Average of what, over what period? [VINCENT GRAY (Reviewer's comment ID #: 88-2203)]	Text edited
SPM-	A	8:44	8:44	Delete "On avergae", Replace "has" by "seems to have"	Rejected. No basis given

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731				[VINCENT GRAY (Reviewer's comment ID #: 88-2204)]	
SPM-732	A	8:45	8:46	"term 'poleward of about 30 degrees' is confusing. Does this mean that precipitation has increased in the high Arctic? Does this mean 30 degrees poleward from the equator? Perhaps actual latitudinal measurements would be better."  [Govt. of Canada (Reviewer's comment ID #: 2004-48)]	Text revised
SPM-733	A	8:46	8:47	The line "Observations of trends ... support for these changes" is weaker formulated in the ES of Ch5: "... .. for changes in the hydrological cycle" reflects Ch5 better. [Gerrit Burgers (Reviewer's comment ID #: 34-7)]	Text edited
SPM-734	A	8:46	8:47	"The sentence on trends in ocean salinity should provide also a brief explanation of how salinity is related to precipitation patterns as it currently would appear as two separate and unrelated phenomena to a lay (policy) reader."  [Govt. of Canada (Reviewer's comment ID #: 2004-49)]	More precip changes the water budget of a given basin and hence salinity. See chapter for details.
SPM-735	A	8:46	8:46	Change "30° in both hemispheres" to "30°N" [see Figure 3.3.4] [David Parker (Reviewer's comment ID #: 195-121)]	Text revised, See SPM-732.
SPM-736	A	8:47	8:47	Change "support for these changes" into "for changes in the hydrological cycle" (This reflects Ch5 better). [Govt. of Netherlands (Reviewer's comment ID #: 2016-13)]	Bullet rewritten.
SPM-737	A	8:49	8:50	"Statement may be too general to be useful. Is there an observable pattern here that could be highlighted? Is there an example of heavy precip in an area that has traditionally not had high precip?"  [Govt. of Canada (Reviewer's comment ID #: 2004-50)]	Text edited
SPM-738	A	8:49	8:49	SPM Comment: Delete the word "Widespread" because the table does not show that (it only says it is "likely" that they happened), and the word "widespread" is not defined or justified in the table or the bulk of the document. [Richard Courtney (Reviewer's comment ID #: 49-16)]	Rejected. The word widespread can be found in any dictionary. The meaning of 'likely' in this assessment has been carefully described. See ch 3 figure on heavy precip for distribution of the changes
SPM-739	A	8:49	8:50	This sentence is too vague; does it apply to convective precipitation, to monsoons? What is meant by "heavy precipitation" should be explained (hourly, daily, monthly?) [Govt. of France (Reviewer's comment ID #: 2010-106)]	Chapters note that the definition of heavy precip is specific to the circumstances so a generic definition is not practical in the SPM. Reader should refer to cited section of the report.
SPM-740	A	8:49	8:49	Replace "observed" by "reported" [VINCENT GRAY (Reviewer's comment ID #: 88-2205)]	Rejected. No basis given for suggestion

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SPM-741	A	8:49	8:50	It would be useful to indicate regions/continents where this is the case. [Michael MacCracken (Reviewer's comment ID #: 152-33)]	Rejected. Widespread means what it says – many areas and listing them all by name is not needed
SPM-742	A	8:49		We propose replacing the word “widespread” with “substantial”, which is the term used in the TS. [Govt. of Norway (Reviewer's comment ID #: 2018-22)]	Taken into account. Revised text -
SPM-743	A	8:49		Rephrase to “Increases in heavy precipitation events ARE observed, even in....” [Govt. of United States of America (Reviewer's comment ID #: 2023-791)]	See SPM-742.
0-96	A	8:52	8:55	SPM-8: The paragraph should start with the second sentence: "There is evidence for ...". The first sentence: "There is is no clear trend..." should be the second sentence in the para [European Commission (Reviewer's comment ID #: 2008-75)]	Rejected. No basis given for change. The present order makes it clear that numbers have not changed before discussing intensity. The reverse order often seems to confuse non experts while this order clarifies.
SPM-744	A	8:52	8:55	"Is the term tropical cyclone a generic term that covers both Atlantic and Pacific events (typhoons and hurricanes)? If so, a footnote to this effect could help the reader understand this." [Govt. of Canada (Reviewer's comment ID #: 2004-51)]	accepted
SPM-745	A	8:52	8:55	Not only “in the satellite record”, also using instrumental observation. The quality of historical TC data is an important concern, but more importantly, some scientists argue that natural inter-decadal variation is a main cause. [Govt. of China (Reviewer's comment ID #: 2006-16)]	The satellite record is cited here due to its coverage and accuracy. Causes and attribution are addressed later.
SPM-746	A	8:52	8:52	Change "total numbers of tropical cyclones" to "average number of tropical cyclones occurring each year"--the total do fluctuate--what is the same is the time averaged number. [Michael MacCracken (Reviewer's comment ID #: 152-34)]	Rejected. The statement clearly says trend which has a clear meaning and does not imply that totals don't fluctuate.
SPM-747	A	8:52	8:55	This bullet should be combined with bullet in page 13, line 24 to 28 in order to avoid duplication and discrepancy [Louis Jose Mata (Reviewer's comment ID #: 170-1)]	Rejected in order to keep observations clear and discuss separately aspects of projections.
SPM-748	A	8:52		Tropical cyclones, hurricanes, typhoons [Stephen J. Hawkins (Reviewer's comment ID #: 102-14)]	SEE COMMENT SPM-744
SPM-749	A	8:53	8:54	why mention the correlation with SSTs, when relationships with the "many other environmental factors" (mentioned on page 3-64, line 4) are not mentioned? This bullet gives the reader an impression that rising SSTs have caused more intense tropical cyclones, but this impression is not conveyed by section 3.8.3, which is rather equivocal (unlike the executive summary of ch.3). If this statement is retained, some explanation of its significance should be added - here, and to chapter 3. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-129)]	SSTs are by the far the most important factor and relate to global warming. The other factors are less clearly related to climate change. Ch 3 is consistent with this

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SPM-750	A	8:54	:55	The last period of this bullet is confusing and should be deleted. Do historical refer to data before 1970 or the data after 1970 that gives a trend in the intensity? [Govt. of Norway (Reviewer's comment ID #: 2018-24)]	Text has been edited
SPM-751	A	8:54	:55	Are there really concerns about the tropical cyclone data from the satellite era? The numbers of storms is certainly unquestioned. Landfall data are a poor statistical sampling. Explain exactly what data set quality is of concern, because a blanket statement as currently phrased condemns all recent observations. [Govt. of United States of America (Reviewer's comment ID #: 2023-792)]	Text has been edited
SPM-752	A	8:55	8:56	Not clear how last sentence links to rest of para. Does 'historically' refer to near 1970, or some other period? [Govt. of Australia (Reviewer's comment ID #: 2001-25)]	Text has been edited
SPM-753	A	8:55	8:55	earlier would be a better word than "historical", or to be absolutely clear, say "pre-1970" (historical could mean any past data) [Govt. of United Kingdom (Reviewer's comment ID #: 2022-128)]	Text has been edited
SPM-754	A	8:55		Please be specific with the dates. [Govt. of United States of America (Reviewer's comment ID #: 2023-793)]	Text has been edited
SPM-755	A	8:56	8:56	"There is no bullet to discuss the line in Table SPM-1 about mid-latitude storms. Suggest some text be added."  [Govt. of Canada (Reviewer's comment ID #: 2004-52)]	Rejected due to need to keep SPM short particularly since already in the Table.
SPM-756	A	9:0		Table SPM-1: To gain more attention from local policymakers in this Table, it could be useful to indicate whether the different phenomena described in this Table are more likely to affect to some particular areas of the globe. [Govt. of Chile (Reviewer's comment ID #: 2005-3)]	Rejected. Not practical given SPM length constraints
SPM-757	A	9:0		Table SPM 1: Droughts row: 21st century column, what is the confidence for low-latitudes? [Joanna House (Reviewer's comment ID #: 109-49)]	Text edited
SPM-758	A	9:0		Table SPM-1, column 2: the term "more likely than not" is inconsistent with the definitions in Chapter 1, page 27. This also applies to Table TS4 in the Technical Summary. [John Hunter (Reviewer's comment ID #: 112-24)]	Text edited
SPM-759	A	9:0		Table SPM-1, column 4: the term "moderate" is inconsistent with the definitions in Chapter 1, page 27. This also applies to table TS4 in the Technical Summary. [John Hunter (Reviewer's comment ID #: 112-26)]	Text edited
SPM-760	A	9:0		Table SPM-1, column 2, last row: the likelihood of late 20th century "high sea level events: increase" is considerably higher than "more likely than not". I	Agreed. Change to "Likely".

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				would describe it as "very likely" or "virtually certain". This also applied to table TS4 in the Technical Summary. [John Hunter (Reviewer's comment ID #: 112-27)]	
SPM-761	A	9:0		Table SPM-1, column 4, header: "predicted" should read "projected". [John Hunter (Reviewer's comment ID #: 112-33)]	Accepted
SPM-762	A	9:0		Table SPM-1. Can you provide a code for the "likelihood" scale? Is it <5% for alternative hypothesis? [Timothy H. Profeta (Reviewer's comment ID #: 203-14)]	See TS table on likelihood
SPM-763	A	9:0		Table SPM-1. The table is missing a column that states the projected trend for the 21st century. Currently it only gives the confidence in the predicted trend, but not what the trend is expected to be. We wouldn't want to assume that by default, whatever was observed during the late 20th century is necessarily the predicted trend for the 21st century. [Andy Reisinger (Reviewer's comment ID #: 210-22)]	Table has been revised to indicate that it is the sign of the projection that is being evaluated here, not the magnitude
SPM-764	A	9:0		Table SPM-1. 1st row. Wording is inconsistent with Chapter 3, which refers to 'cold' extremes (not 'cool'). [Blair Trewin (Reviewer's comment ID #: 266-4)]	Text in SPM and chapter are now consistent
SPM-765	A	9:0		Table SPM-1: For the two mentions of "human influence," perhaps specify that this refers mostly to greenhouse gases, to make it clear that GHGs are the dominant influence. [James S. Wang (Reviewer's comment ID #: 281-2)]	Rejected. Aerosols are also very important, see ch 9
SPM-766	A	9:1	5:9	There is no evidence or link to the underlying report presented in the SPM providing support for the assessment of high confidence that warm temperature extremes will increase and cold temperature extremes decrease in the 21st century. Either provide this information or delete the assessment. The SPM should be a stand alone document because it is often the only part of the report that is read. [Jeff Kueter (Reviewer's comment ID #: 137-9)]	Rejected. See chapter 10. This is discussed in detail and several figures are presented
SPM-767	A	9:1	9:10	Give definitions for the terms "very likely, likely, more likely than not" in a footnote [Govt. of Germany (Reviewer's comment ID #: 2011-76)]	Accepted
SPM-768	A	9:1	9:9	This Table is pure speculative guesswork without scientific foundation, and with very slight evidence from observations. It serves no useful purpose, and should be deleted. [VINCENT GRAY (Reviewer's comment ID #: 88-2206)]	Rejected. See chapters.
SPM-769	A	9:1	9:9	The SPM refers the reader to Box TS 1.1 for the definitions of uncertainty terms, but the definition of "More likely than not" i.e., more than 50% likelihood, is	Clarified. See footnote and revised box.

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				vague, since it gives no indication of an upper bound. WG I needs to either provide an upper bound for the term "more likely than not" or not use it. [Jeff Kueter (Reviewer's comment ID #: 137-5)]	
SPM-770	A	9:1	9:9	Change the evaluation of human influence on mid to high latitude cyclones to "unknown." The current evaluation "more likely than not (>50% likelihood) but with low confidence (2 out of 10 chances of being correct)" indicates a high level of uncertainty about the conclusion. Especially in the SPM, WG I has an obligation to be precise about what is known and what is not known. The low level of confidence assigned to this conclusion indicates that it is still unknown. [Jeff Kueter (Reviewer's comment ID #: 137-6)]	Rejected. See chapter 3 and 9. Confidence level is indicated and the reasons for it are discussed in the chapters.
SPM-771	A	9:1	9:9	The definitions of confidence levels given on lines 7-9 only add to the confusion over WG I's treatment of uncertainty. High confidence is defined differently from the definition provided in Box TS 1.1, and a new term, moderate confidence, is introduced. Should moderate confidence be considered equivalent to medium confidence, which is defined in Box TS 1.1? Either find different terminology for these terms or use the definitions provided in Box TS 1.1. [Jeff Kueter (Reviewer's comment ID #: 137-7)]	SEE COMMENT SPM-758
SPM-772	A	9:1	9:9	What is the basis for the findings that it is likely (66 to 90% probability) that warm temperature extremes have increased and cold temperature extremes have decreased? The text refers the reader to Section 9.4, which, on Pg. 9-34, lines 3-5, states that including anthropogenic effects improves the simulation of extreme events. No indication of how much improvement occurs or why such improvement should be taken as a strong indicator of human influence. Either make a more compelling argument supporting this finding, or change the assessment to unknown. [Jeff Kueter (Reviewer's comment ID #: 137-8)]	Rejected. The observations can be found in chapter 3. The attribution discussion can be found in ch 9. See also the box on extremes in the TS, which shows how a shift in the mean shifts the extremes
SPM-773	A	9:1		Table SPM-1 The "confidence" terms as defined in the footnote to this table are not the same as stated in the Technical Summary (TS-4, line 24). Maybe "high confidence" could be interpreted the same as defined in TS-4 table, but SPM uses "Moderate" whereas TM uses "Medium" confidence.  [A. Brett Mullan (Reviewer's comment ID #: 182-2)]	Text edited
SPM-774	A	9:1	:9	Table SPM-1: We suggest the column on "Likelihood that observed trend is due to human influence" be deleted. The rationale is that most of the text in this column is in italics, indicating that no formal detection and attribution study has been completed (according to the heading of the table). A solution could be to	Rejected. Information on what is and is not currently attributable based on available studies is a component in providing a balanced report. The table in the TS indicates what is based on expert judgement and what

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			<p>add the information for the three cases where studies have been completed in a separate paragraph below the table. Deleting this column would in our view, increase the readability of the table. If the column is deleted, the explanation on what Italics indicate in the heading could also be removed (lines 3-4).</p> <p>[Govt. of Norway (Reviewer's comment ID #: 2018-25)]</p>	is based on formal detection studies and there is more information in the chapter.
SPM-775	A	9:1	<p>Table SPM-1 seems unbalanced in terms of suggesting negative outcomes. For example, decreases in cool nights and frosts might be accompanied by “longer growing season” just to add some balance. The statement on increase in summer mid-latitude droughts seems to rely too heavily on PDSI estimates, and even here the trends are not universal since the United States and China do not show strong evidence of increases. Moreover, some references in Section 3.3 (e.g., Robuck, 2000; Hirabayashi et al., 2005) do not suggest negative trends in soil moisture.</p> <p>[Govt. of United States of America (Reviewer's comment ID #: 2023-794)]</p>	Rejected. Table is not attempting to cover all climate phenomena. WG1 does not cover impacts such as growing season lengths. See WG2 for that. Drought statement has been clarified.
SPM-776	A	9:1	<p>Table SPM-1 has serious problems. In the previous summaries (e.g., 2001 SYR Table SPM-1), there were at least 25 phenomena and now just 8. How did the authors pare back to this number? Perhaps the writing team should consider updating and augmenting the last table as a starting point.</p> <p>[Govt. of United States of America (Reviewer's comment ID #: 2023-795)]</p>	Rejected. This is an extremes table, as in the WG1 TAR and includes nearly the same variables. Wording of the title of the table has been changed to clarify. Table is not covering all phenomena that are not extremes, and not covering WG2 material as in SYR.
SPM-777	A	9:1	<p>With regard to specific fixes for this rendition, either include quantification or explain why not.</p> <p>[Govt. of United States of America (Reviewer's comment ID #: 2023-796)]</p>	Rejected due to length constraints of SPM. Quantification is beyond the scope here.
SPM-778	A	9:1	<p>Eliminate confusion about how the phenomena were chosen. State clearly what the objective criteria are for inclusion or exclusion in this table. In the table legend, link the “Phenomenon” column header with “Projected Trends for the 21st Century”. Is the rationale that the contents in the table are items that have high confidence? This would allow labels of moderate to high confidence, thereby excluding lower confidence events? Confidence based on statistical correlation of trends above the noise of natural variability? A basis for the selections eliminates contentions of bias. There are obvious things that are missing (growing season, Arctic sea ice, temperature, water vapor, sea level, ...). Note that the absence of trend or confidence in trend is valuable information for policymakers. In sum, the authors need to include a very clear statement about why these particular items are here.</p> <p>[Govt. of United States of America (Reviewer's comment ID #: 2023-797)]</p>	Rejected. This is an extremes table, as in the WG1 TAR; it is not covering phenomena that are not extremes. The variables follow those observed and projected, see chapters.
SPM-	A	9:1	In reference to “Warm days/warm nights increase over mid-and high-latitude	Drought statement has been revised.

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779				<p>land areas,” does this language suggest no increase over low-latitude land areas? Remove the parenthetical in the “Droughts” entry. There is no evidence provided in Chapter 3 that droughts have increased in intensity in mid-latitude summer, other than some inference that when it gets warmer in summer there is generally less precipitation. At minimum, the “increase” in droughts needs to be explained in terms of extent or duration. The third column entry for the drought row should be in italics.</p> <p>[Govt. of United States of America (Reviewer’s comment ID #: 2023-798)]</p>	Warm extremes clarified.
SPM-780	A	9:1		<p>Telling a policymaker that something is more likely to have happened, but with only low confidence—which is defined as about 2 out of 10 chances of being correct—is a highly mixed message. Given the low level of confidence, the correct assessment is that it is unknown whether the increase in tropical cyclone intensity can be attributed to human activities. Change the evaluation of human influence on mid- to high-latitude cyclones to “unknown.”</p> <p>[Govt. of United States of America (Reviewer’s comment ID #: 2023-799)]</p>	Rejected. The evidence points to a human influence, although it is agreed that all the lines of evidence have uncertainties
SPM-781	A	9:1		<p>Table SPM-1 is awkward and not well balanced: (1) It really needs to have temperature and water vapor listed first (and included), including temperature patterns as these are the first of the attribution studies and still the most important; and (2) the subtle difference between a “formal” attribution study (roman) and “other” (italics) is quite vague, since similar conclusions on likelihood appear for both. The header “Confidence in Trend Predicted for 21st Century” is not helpful, since there could be a reverse trend. Perhaps rephrase as “Confidence that the 20th Century Trend will Continue” or else explain what the 21st century projections are. Italics stand out, so why use it for the marginal cases?</p> <p>[Govt. of United States of America (Reviewer’s comment ID #: 2023-800)]</p>	Rejected. Table is on extreme events, not all phenomena
SPM-782	A	9:1		<p>With regard to the bottom two entries in Table SPM-1, “Not assessed” seems arbitrary since undoubtedly some research has been performed on the subject. Perhaps “Not assessed by this report” is more accurate. Also what constitutes a high sea-level event? Would “storm surge” be more apt?</p> <p>[Govt. of United States of America (Reviewer’s comment ID #: 2023-801)]</p>	<p>Wording changed to clarify these points.</p> <p>Agreed for sea level. Changed to “More likely than not”, but still no formal studies. No known attribution studies for storms.</p>
SPM-783	A	9:3	9:6	<p>Table SPM-1. The italics should be removed from the column headers and reserved for indicating those outcomes where no formal studies have been completed.</p>	Accepted

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				[Govt. of Australia (Reviewer's comment ID #: 2001-26)]	
SPM-784	A	9:5	9:6	<p>This table needs revision. Suggestions include removal of the fourth column as up until this stage in the SPM, models have not been considered. Unless text is added, this column is out of context. If the fourth column is retained, the final element of confidence relating to high sea level events should be upgraded to high. It is considered that sea level is rising in nearly all places and a high level of confidence can be given to high sea level specific events as well. In addition, suggest changing column 1 heading from "Phenomenon" to "Unusual Event" which more fully describes the elements listed.</p> <p>[Govt. of Australia (Reviewer's comment ID #: 2001-27)]</p>	<p>Approval process will need to be ordered appropriately, as it was during the approval of the similar table in the TAR WG1 SPM.</p> <p>Assessment on sea level was revised after further discussions with ch 5. The observations of sea level rise are not everywhere, because limited number of stations and their distribution.</p>
SPM-785	A	9:5	9:9	<p>Box TS 1.1 provides the obvious definition for "More likely than not," i.e., more than 50% likelihood, but this is too nebulous a definition to be useful. Since "likely" is defined as &gt;66% probability, the implication is that "more likely than not" covers the range 50-66%. If this is the case, it should be clearly stated. If not, some additional information should be provided as to what "more likely than not" means.</p> <p>[Lenny Bernstein (Reviewer's comment ID #: 20-5)]</p>	SEE COMMENT SPM-758
SPM-786	A	9:5	9:9	<p>Telling a policymaker that something is more likely to have happened, but with only low confidence, which is defined as about 2 out of 10 chances of being correct, is a highly mixed message. Given the low level of confidence, the correct assessment is that it is unknown whether the increase in tropical cyclone intensity can be attributed to human activities.</p> <p>[Lenny Bernstein (Reviewer's comment ID #: 20-6)]</p>	Rejected. More likely than not is not the same as unknown.
SPM-787	A	9:5	9:9	<p>The basis for the assessment that it is likely that the increase in warm temperature extremes and decrease in cold temperature extremes presented in Table SPM-1, and again on Pg. 11, lines 20-22, is unclear. The underlying text for this topic is section 9.4.3.2, which presents the results of modeling studies which show that including anthropogenic effects "improves the simulation of these changing temperature extremes", but stops well short of attributing those changes to anthropogenic effects. Unless a clear logic can be provided for attributing changes in temperature extremes to human activities, the assessment should be that it is unknown whether these changes can be attributed to human activities.</p> <p>[Lenny Bernstein (Reviewer's comment ID #: 20-8)]</p>	Rejected. Chapter 9, page 34, lines 11-22 does attribute (except for warm days – which are excluded from table column).
SPM-788	A	9:5	9:9	<p>The SPM does not provide a basis, either in Table SPM-1 or in subsequent text, for assigning high confidence to the trends in warm and cold temperature extremes projected for the 21st century.</p>	REJECTED: The basis an evidence is given in full in Ch10 and contained in Figs 10.3.16 and 10.3.17. No space or need for further explanation in SPM; reader is

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				[Lenny Bernstein (Reviewer's comment ID #: 20-9)]	referred to the chapter.
SPM-789	A	9:5	9:5	"In table SPM-1, the terms very likely, likely, high, etc., should be quantified to make them less ambiguous and more comparable. Also keep consistent terminology with Technical Summary." [Govt. of Canada (Reviewer's comment ID #: 2004-53)]	SEE COMMENT SPM-758
SPM-790	A	9:5	9:9	regarding tropical cyclones, both assessments of "more likely than not" are misleading and should be changed to "about as likely as not." This would be consistent with the terminology defined on TS-4. The underlying chapter states, "Nonetheless, detection and attribution of observed changes in hurricane intensity or frequency to external influences remains difficult given deficiencies in theoretical understanding of tropical cyclones, their modelling, and their long-term monitoring." Telling a policymaker that something is more likely to have happened, but with only low confidence, which is defined as about 2 out of 10 chances of being correct, is inappropriate. Mid and high level cyclones should also be changed. [Howard Feldman (Reviewer's comment ID #: 70-1)]	Rejected. Chapter gives physical arguments why the hurricane statement is more likely than not.  Mid-latitude cyclone statement has been edited
SPM-791	A	9:5	9:6	Same as above: what is meant by "heavy precipitation" should be specified. [Govt. of France (Reviewer's comment ID #: 2010-107)]	See SPM-739
SPM-792	A	9:5	9:5	In Table SPM-1 and the entry for "Tropical cyclones", in the third column the phrase "(but with low confidence)" should be dropped--there is strong theoretical evidence that this should be the trend, and it is occurring. The confusion comes, perhaps in that the rate of change is faster than the models (doing very limited and idealized studies) suggest. But, climate change has to be contributing to this change--no one is suggesting it operates in the opposite sense. Then in the fourth column and in lines 7-9, it seems overly cautious to say "Moderate." One has theory on one's side in addition to the model simulations--with much more moisture in the air and with the additional CO2 tending to slightly stabilize the troposphere, it seems very clear that more energy will be needed--and much more is available, so one has to get intensification--and this is being seen. [Michael MacCracken (Reviewer's comment ID #: 152-35)]	Rejected. Uncertainties in understanding, modelling, and historical data preclude stronger statements at this stage.
SPM-793	A	9:5	9:5	Again, it would be useful to have the term "High sea level events" defined, perhaps in a footnote. [Michael MacCracken (Reviewer's comment ID #: 152-36)]	Glossary
SPM-794	A	9:5	9:6	Table SPM-1, Header of third column: replace "Likelihood that observed trend is due to human influence" by "Anthropogenic contribution to observed trend". Rationale: in most cases natural contributions to the observed trend cannot be excluded.	Agreed

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				[Govt. of Netherlands (Reviewer's comment ID #: 2016-14)]	
SPM-795	A	9:5	9:6	Table SPM-1, Second Column, 6th row: Replace "likely" by "more likely than not". Rationale: Published analyses of trends in drought show conflicting results, and objective observations are scarce.  [Govt. of Netherlands (Reviewer's comment ID #: 2016-15)]	See ch 3 for discussion. Believe the text is clearer now regarding what kind of trends and data are used.
SPM-796	A	9:5		Table SPM-1: "The phenomena listed in the ""Phenomenon"" column of the table are inadequately defined. Increases/decreases in cool days, cool night, frosts, warm days, warm nights, warm spells, haet waves, droughts, ""most intense storms"" and high sea level events should be qualified: are these changes in the frequency of such events? In the severity? In the extent or geographic range?"  [Govt. of Canada (Reviewer's comment ID #: 2004-54)]	Text clarified
SPM-797	A	9:5		Table SPM-1. It should be clear that the last column refers to confidence in the magnitude of the trend (rather than just the sign). [Michael Manton (Reviewer's comment ID #: 157-26)]	Accepted. Title of columns changed.
SPM-798	A	9:7	9:9	Box TS 1.1. defines "High confidence" as about 8 out of 10 chances of being correct, and "medium confidence" as about 5 out of 10 chances of being correct. Table SPM-1 introduces a different definition of high confidence and a new term -- moderate confidence. The treatment of uncertainty in this report is difficult enough to follow without changing terminology. The terminology defined in Box TS 1.1 should be used. Amplification of the definitions, i.e., "It is our judgment that this statement has 8 out of 10 chances of being correct because ..." would be useful. [Lenny Bernstein (Reviewer's comment ID #: 20-7)]	SEE COMMENT SPM-758
SPM-799	A	9:7	9:9	This attempt to quantify shear guesswork has no scientific basis. Delete it [VINCENT GRAY (Reviewer's comment ID #: 88-2207)]	Rejected. No basis offered for assertion
SPM-800	A	9:7	:9	Box TS 1.1. defines "High confidence" as about 8 out of 10 chances of being correct, and "medium confidence" as about 5 out of 10 chances of being correct. Table SPM-1 introduces a different definition of high confidence and a new term: moderate confidence. The treatment of uncertainty in this report is difficult enough to follow without changing terminology. The terminology defined in Box TS 1.1 should be used. Amplification of the definitions—i.e., "It is our judgment that this statement has 8 out of 10 chances of being correct because ..."—would	SEE COMMENT SPM-758

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				be useful. [Govt. of United States of America (Reviewer's comment ID #: 2023-802)]	
SPM-801	A	9:12	9:12	Assessment of paleoclimate is one of foci of AR4. Therefore, suggest to further add more relevant information in this part. For example, add regional paleoclimatic situation. [Govt. of China (Reviewer's comment ID #: 2006-17)]	Rejected due to space constraints and limited information in ch 6.
SPM-802	A	9:12	9:12	Replace title with "climate change on very long time scales". [Govt. of United Kingdom (Reviewer's comment ID #: 2022-130)]	Rejected. Too vague.
SPM-803	A	9:12	9:28	The whole section is missing a key message. Add key messages in bold. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-131)]	Accepted. new headline written.
SPM-804	A	9:12	10:2	Every other major section of the SPM has at least one supporting graphic. The lack of a supporting graphic in the "A Paleoclimate Perspective" section seems a slap in the face to the authors of chapter 6 as well as the paleoclimate community. It also sends a disturbing message that AR4 is somehow backing away in its support for claims made in the TAR where conclusions drawn from paleoclimate studies were highlighted in the SPM. Yet, a reading of chapter 6 in fact reveals more robust evidence in support of the key conclusions from the TAR. Chapter 6 highlights the fact that there are now a large number of different paleoclimate studies which all lead to the same key conclusion that northern hemisphere mean temperatures in recent decades are likely unprecedented in at least a millennial timeframe. Moreover, several of the newer studies extend these conclusions back to at least the past 2000 years. It was a mistake for the authors of the SPM in the TAR to show only one reconstruction (that of Mann et al, '99) when in fact there were multiple reconstructions shown in the body of the report (chapter 2) which supported the main conclusion regarding anomalous late 20th century warmth. This clearly set up one study as a straw man for attack. AR4 has an opportunity to undo the damage of that unfortunate decision, and show in the SPM Figure 6.10 from chapter 6, which indicates that the key conclusions regarding recent hemispheric warmth in a millennial context are now supported by more than a dozen different reconstructions taking into account the ensemble of uncertainties associated with the different reconstructions.  [Michael Mann (Reviewer's comment ID #: 156-42)]	Not every chapter has a figure. Figures depend upon merit and need. Text has been clarified regarding conclusions of TAR and new conclusions here, as well as uncertainties.
SPM-805	A	9:12		Section: Paleoperspective Comment 1) This section needs to include Figure 6.10. (a) or (b) as it confirms the results presented in the TAR (Records of Northern Hemisphere temperature variation during the last 1300 years). Failure to include this is likely to be seen as an admission by the IPCC that the TAR results were in	Material is covered. Reasoning presented for a figure does not appear to be based upon a scientific need to convey information.

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			error, which is not the case. [William Hare (Reviewer's comment ID #: 99-107)]	
SPM-806	A	9:12	Section: Paleoperspective Comment 2) One the important conclusions of Chapter six is "Global mean cooling and warming associated with past glacial maxima and minima are comparable in magnitude, but not in rate, to a projected global mean warming of several degrees over the 21st century. The temperature change since the Last Glacial Maximum (ca. 21,000 years ago) took place at a rate more than ten times slower than this projected future change." Consideration needs to be given to including this. [William Hare (Reviewer's comment ID #: 99-108)]	Projections come later, not here.  The time resolution of the data do not rule out a change in a century that is quite large, so this statement is misleading.
SPM-807	A	9:12	I find the terms "paleoclimate" and "proxy data" to be rather technical for an SPM. Why not be direct with the important distinction, something like [Daniel Murphy (Reviewer's comment ID #: 183-14)]	SEE SPM 808
SPM-808	A	9:12	"CHANGES IN FORCING AND CLIMATE BEFORE MODERN RECORDS"? * Changes in long-lived greenhouse gases can be measured with high confidence from ice cores. The concentrations of these gases increased at the end of the last ice age about 17,000 years ago as the plane warmed, but the rates of those changes were much slower than those in the last century. {moved from SPM-3 line 32} * Changes in past temperatures on time scales ranging from thousands to millions of years must be inferred from climatically sensitive indicators. Such proxy data may be influenced by both local temperature and other factors such as precipitation and are often representative of particular seasons rather than full years. Recent studies draw confidence from coherent behavior across multiple indicators in different parts of the world, but uncertainties generally increase with time into the past due to sparse data. {moved from italics to a bullet in order to have one bullet on past gases and one bullet on past temperatures} * {continue with SPM-9 line 21} [Daniel Murphy (Reviewer's comment ID #: 183-15)]	Reject. Retain trace gases in drivers section, not here, for clarity.  Reject. Content covered in lines 14-17, page 9.
SPM-809	A	9:12	Section "A palaeoclimatic perspective". This section seems somewhat short and weak, given the amount of public and media interest in potential large-scale Earth system changes and what we can learn from palaeoclimate research. A number of policymakers consider next steps in climate negotiations on the basis of long-term outcomes such as ice sheet stability. They may need to be told more extensively what we do and don't know - in this section in particular, statements about what we don't know are as important as statements about what we do	Rejected. Media interest is not the basis for including material. Ice sheet stability is covered elsewhere.

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				know! Some specific suggestions for additional relevant information are in separate comments. Perhaps it would also help to rephrase the title to say "Lessons from climates of the past", or something like that, to make it clear that this section is only looking back and does not make projections. [Andy Reisinger (Reviewer's comment ID #: 210-23)]	
SPM-810	A	9:15		Replace "thousands" with "hundreds" [Govt. of United States of America (Reviewer's comment ID #: 2023-803)]	Accepted
SPM-811	A	9:15	:17	Replace sentence with: "Individual proxy measurements (e.g., tree-ring width) can be influenced by single (e.g., temperature) or multiple environmental factors (e.g., both temperature and precipitation) operating during one or more seasons, in some cases integrated over the temporal resolution of the particular record." The authors are encouraged to try to convey this information in a shorter distilled form. [Govt. of United States of America (Reviewer's comment ID #: 2023-804)]	Rejected. This proposed formulation seems less clear to the authors.
SPM-812	A	9:17	9:19	The SPM could use some rewriting by consultants who are expert in presenting technical concepts to lay audiences. Sentences like the following from page 9, to wit, "Recent studies draw confidence from coherent behaviour across multiple indicators in different parts of the world, but uncertainties generally increase with time into the past due to the sparsity of relevant data." are PRACTICALLY INCOMPREHENSIBLE to me, and I consider myself to be an educated person in the climate sciences. I can hardly imagine what a policymaker would take home from the "message" in this sentence. [Chuck Hakkarinen (Reviewer's comment ID #: 96-5)]	No alternative suggested nor any specifics regarding what could help this reader.
SPM-813	A	9:18	9:18	Change "due" to "owing". [David Parker (Reviewer's comment ID #: 195-122)]	Copy editing will be done at the end of the process
SPM-814	A	9:20	9:20	"There is no bolded summary statement to lead into the bullets that present the results from the paleoclimate data. This is the only section without such a header. Such headers are very useful for summarizing the main findings of each section, and one should be added to this section." [Govt. of Canada (Reviewer's comment ID #: 2004-55)]	Accepted, added
SPM-815	A	9:21	9:24	"This section needs a figure showing the NH temp trend over the past 1000 years." [Govt. of Canada (Reviewer's comment ID #: 2004-56)]	Rejected. Space considerations; lack of clarity regarding reason for suggestion
SPM-816	A	9:21	9:24	SPM Comment: This paragraph is grossly misleading and must be replaced. The following replacement is the minimum required to correct the error in the	Rejected. See ch 6 for discussion of these issues. Text suggested is not appropriate for an SPM either in

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				<p>TAR.            “The SAR had reported that temperatures of the late twentieth century are similar to or lower than temperatures 1000 years ago. The TAR placed emphasis on the work of Mann et al. that indicated very little variability in NH temperatures over the last 1000 years with consistently low temperature until a temperature rise began around 1900. This finding of Mann et al. seemed to refute the large climate variability previously reported in many places including the SAR. However, since the TAR several studies have provided doubt to that work of Mann et al.. Many studies provide data that conflict with the findings of that work of Mann et al. (e.g. Beltrami et al) (ref. Beltrami et al "Long-term tracking of climate change by underground temperatures", Geophysical Research Letters v.12 (2005) ) and indicate that the report of climate variability in the SAR was correct. In 2005 McIntyre and McKittrick published two papers that together provide a complete refutation of that work of Mann et al. (ref. McIntyre S &amp; McKittrick R, Energy &amp; Environment, v 16, no.1 (2005)) (2005), Geophysical Research Letters Vol. 32, No. 3, (2005)). But, perhaps the most important of their studies of that work of Mann et al. was their publication in 2003 (ref. McIntyre S &amp; McKittrick R, Energy &amp; Environment, v 24, pp 751-771 (2003)) that showed it is not possible to replicate the work of Mann et al. There are several reasons for the inability to replicate this work of Mann et al.; not least that Mann refuses to reveal his source codes. The inability to replicate this work of Mann et al. means it has no scientific worth: i.e. this work of Mann et al. is anecdote of similar kind to a report of a ghost sighting. Hence, the IPCC now apologises for including it in the TAR. The IPCC will now disregard this work of Mann et al. and recommends that all others should also disregard it until it can be – and has been – independently replicated.”</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-17)]</p>	length or level of clarity. Text makes explicitly clear that this assessment supports greater variability, but that the 20 <sup>th</sup> century warming is likely outside that of the last 1300 years.
SPM-817	A	9:21	9:24	<p>suggest including figure TS-23, as this gives an update of the information given in graphical form in the TAR, which has been used and discussed widely. This figure helps policymakers understanding progress in understanding.            [Govt. of Germany (Reviewer’s comment ID #: 2011-230)]</p>	See SPM 808
SPM-818	A	9:21	9:24	<p>is there a figure to go with this?            [Joanna House (Reviewer’s comment ID #: 109-50)]</p>	See SPM 808
SPM-819	A	9:21	9:22	<p>It is not at all clear that this sentence is needed--as it is not really very clear what its significance is. What is meant by "greater variability"? Does this mean there really is more variability (is this in annual or seasonal values), or just less agreement among different proxies, or more externally driven variations--what is</p>	Rejected. Believe text is clear as it stands.

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				purpose of this sentence? [Michael MacCracken (Reviewer's comment ID #: 152-37)]	
SPM-820	A	9:21	9:22	This statement is misleading. Some studies (such as Rutherford et al '05 and Oerlemans '05) show very similar levels of variability to those shown in the TAR, and virtually all of the reconstructions that have been produced agree with those shown in the TAR within the uncertainty range that was also shown in the TAR. The statement should be corrected to accurately reflect these considerations. [Michael Mann (Reviewer's comment ID #: 156-43)]	Rejected. See detailed discussion in chapter
SPM-821	A	9:21	9:24	Figure 6.10b should be added here, in view of the world-wide discussions on this issue.  [Govt. of Netherlands (Reviewer's comment ID #: 2016-16)]	See SPM 808
SPM-822	A	9:21	9:22	Suggested re-phrase to improve clarity: "Some recent studies indicate greater variability in REGIONAL NH temperatures over the last 1000 years than reported in the TAR, BUT those studies ALSO imply a larger warming since the early 19th century." I think the constraint that the CO2 concentrations imply for hemispheric and global temperature variations is an important one; while it may not be necessary to bring this into the SPM, emphasis on regional vs hemispheric and global temperature variations is probably important. [Andy Reisinger (Reviewer's comment ID #: 210-24)]	Rejected. Chapter 6 makes clear that regional is not what is referred to but fully hemispheric. Prefer current wording.
SPM-823	A	9:21	9:24	It would be helpful to include a graphic here illustrating the estimated temperature trends over the past millennium. In fact, the lack of a figure may inadvertently suggest a decrease in confidence in our understanding of trends for the past millennium, considering that the well known "hockey stick" curve was displayed in the SPM for the TAR. [James S. Wang (Reviewer's comment ID #: 281-3)]	SEE COMMENT SPM-808
SPM-824	A	9:21		Recommend changing "NH" to "Northern Hemisphere (NH)" [Timothy H. Profeta (Reviewer's comment ID #: 203-15)]	copy-editing to be completed at a later stage
SPM-825	A	9:22		The phrase "a larger warming since the early 19th century" seems a non sequitor. Delete the whole sentence. [Govt. of United States of America (Reviewer's comment ID #: 2023-805)]	Accepted
SPM-826	A	9:23	9:24	"This sentence should read ""...temperatures during the second half of the 20th century were very likely warmer than during any other 50-year period...""  [Govt. of Canada (Reviewer's comment ID #: 2004-57)]	Accepted
SPM-	A	9:24	9:24	Insert after "500 years" , "because they were measured close to human	Rejected. Incorrect; see chapter 3

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827				habitation" [VINCENT GRAY (Reviewer's comment ID #: 88-2208)]	
SPM-828	A	9:24	9:24	Add at end "for the same reason; although there is persuasikve evidence for a "medieval warm period" around the 15th century which was even warmer" [VINCENT GRAY (Reviewer's comment ID #: 88-2209)]	Rejected. See chapter 6
SPM-829	A	9:24	9:24	At end of sentence, change to "past 1000 years, or even more." While the evidence is weak, there is no indication that this trend ends at that point. [Michael MacCracken (Reviewer's comment ID #: 152-38)]	Changed to 1300, which is based on the assessment in ch 6
SPM-830	A	9:24	9:24	The TS (page 32, line 18) says that NH temps of the second half of the 20th century ... and likely the warmest in the past 1,000 years, as does the SPM (page 9, line 24). But Ch. 6 (RF&KU, page 41, line 41) cites past 1,300 years. Please make consistent in all (three) places. [Melinda Marquis (Reviewer's comment ID #: 162-129)]	Accepted.
SPM-831	A	9:26	10:2	Suggest to adapt the bullet: put the focus on sea level rise, not on the period in time. For example, move the first part of the sentence to the end: "It is likely that...above current levels during the last interglacial period, about 125'000 years ago. This is associated ..." [Gian-Kasper Plattner (Reviewer's comment ID #: 200-5)]	Accepted
SPM-832	A	9:26	10:2	Provide some explanation of how the interglacial events 125,000 years ago are relevant to the present-day situation, e.g. unmitigated greenhouse gas emissions over the next century could trigger a similar level of sea-level rise. [James S. Wang (Reviewer's comment ID #: 281-4)]	Covered under projections section as appropriate
SPM-833	A	9:26	:28	Add after "125,000 years ago" the phrase "it is virtually certain that sea level was 4-6 meters higher than present and". Replace "to a sea-level rise above current levels" with "of that total." Add a sentence: "Paleoclimate observations also suggest that the Antarctic ice sheet likely also contributed to the last Interglacial sea level increase." (from page 6.2, line 55 to page 6.3, line 1) [Govt. of United States of America (Reviewer's comment ID #: 2023-806)]	Taken into account. Bullet revised.
SPM-834	A	9:27	8:27	The SPM here says Greenland plus other Arctic ice fields contributed between 2 and 3.5 m to sea level rise (as does Chapter 6, page 21, line 39), but the TS (page 33, lines 15-16) refers to a contribution of between 2.2 and 3.5 m. Please make consistent. [Melinda Marquis (Reviewer's comment ID #: 162-99)]	SEE COMMENT SPM-835
SPM-835	A	9:27	9:27	The underlying chapter says that Arctic ice sheet retreat contributed between 2.2 and 3.5m, not 2 and 3.5m. [Andy Reisinger (Reviewer's comment ID #: 210-25)]	Taken into account. Bullet revised.  The revised text reads 2-4 m; this is rounded appropriately.

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SPM-836	A	9:28	10:1	The Holocene temperature (3500-8000 years B.P.) should be given. Is current warming approaching the temperature of that period? A great concern exists in policy makers. [Govt. of China (Reviewer's comment ID #: 2006-18)]	See chapter 6. Not included here because of limited information and length constraints
SPM-837	A	9:28	10:2	Would the authors be prepared to rephrase and slightly extend this sentence: "This retreat occurred over a time span of about xxx thousand years [the time scale is important - there are so many different popular claims and expectations of how quickly Greenland could disappear; if the time scale cannot be given with sufficient robustness, then it would be policy-relevant to say that we do not know the time scale for previous retreats], was associated with estimated Arctic summer temperatures about 2-4°C higher than at present [does this mean 1990s, or 2006?]. The higher Arctic temperatures during this period can be linked to radiative forcing resulting from changes in the Earth's orbit around the sun." [Andy Reisinger (Reviewer's comment ID #: 210-26)]	Taken into account. Bullet revised. Unable to constrain rate. Rate removed from ES of ch 6.
SPM-838	A	9:28	10:1	"This is associated with" and "linked to" are vague verbs. Also, reference to "forcing due to changes in the Earth's orbit around the Sun" is ambiguous. Are these positive or negative forcings? Rewrite sentence as follows: "This melting can be explained by model simulated Arctic summer temperatures about..." [Govt. of United States of America (Reviewer's comment ID #: 2023-807)]	Taken into account. Bullet revised.
SPM-839	A	10:0		figure SPM-4: This figure is very much welcomed. Really impressive! [Govt. of Austria (Reviewer's comment ID #: 2002-9)]	Thank you
SPM-840	A	10:0		Figure SPM-4: Due to the importance of Figure SPM-4, it may be described and interpreted more widely than in the current version of this SPM [Govt. of Chile (Reviewer's comment ID #: 2005-2)]	Rejected. SPM is constrained in length.
SPM-841	A	10:0		Figure SPM-4 Comment: Delete Figure SPM-4 and its caption because it is a set of assertions that cannot be true. The assertions of "natural" and "anthropogenic" forcings creating agreement with the "surface air temperature" record (the caption does not say which one) requires accurate knowledge of "the decadal air temperature anomalies for 1906-2005" but such knowledge does not exist. The surface temperature record begins ~1860. For the period 1860 to 2004, the most cited of these data sets have good coherence (which is not surprising because they were compiled from the same available source data), but their trends (in degrees C/decade) and 2SD trend errors are GHCN: $0.076 \pm 0.010$ Jones et al.: $0.064 \pm 0.007$ GISS: $0.048 \pm 0.006$	Rejected due to errors of fact.

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			<p>The Jones trend is significantly different from the GISS trend (<math>p &lt; 0.05</math>), and the GHCN trend is very significantly different from the GISS trend (<math>p &lt; 0.01</math>). So, “coolest” and “warmest” years near the ends of the data sets are generated by trends that are known to be spurious.</p> <p>A change in the indicated temperature with time is only an indication of climate change when the change exceeds the intrinsic measurement errors (otherwise the change could be intrinsic measurement error). The intrinsic errors of GHCN, Jones et al. and GISS data sets are not known, but at least two of the data sets provide wrong results because they differ in annual change by more than double their stated 95% confidence limits in each of several years.</p> <p>Furthermore, the GHCN and Jones et al. data sets also have different trends for the warming period of the most recent 30 years. For the period 1976-2004, the temperature trends for the Jones et al., GISS, and GHCN surface temperature time-series are +0.215 degrees C/decade, +0.204 degrees C/decade and +0.274 degrees C/decade, respectively. And this also demonstrates that at least two of them are indicating spurious trends generated by the methods used to create the means.</p> <p>Data of this kind cannot be used to justify any assertion concerning probabilities of the causes of warming they suggest.</p> <p>Indeed, other recent data suggest that the trends suggested by these data sets for recent decades are very wrong. For the late 20th century warming period between 1976 and 2004, the rates of change indicated by these data sets are significantly higher than the rate of +0.059 degrees C/decade for the lower atmosphere measured by weather balloon radiosondes for the same period and the rate of +0.079 degrees C/decade and satellite MSUs for the period 1979 to 2004 (the satellite record begins in 1979).</p> <p>Furthermore, the compilers of these data sets admit their methods create spurious trends (ref. Vose et al., 2004). Their methods include integrating measurements into mean values for regions over the Earth’s surface called ‘grid boxes’. And the integrations cause trend problems for individual grid boxes. Vose et al. state that when the GHCN and Jones et al. trends are compared at the grid-box level then 9.3% of grid cells display “discrepant trends”. In other words, the integration of measurements into grid boxes causes 9.3% of grid boxes to have trends with opposite sign. (!)</p> <p>Simply,</p> <ul style="list-style-type: none"> <li>(a) temperature measurements cannot indicate climate change in the absence of knowledge of their accuracy and precision,</li> <li>(b) coherence between data sets does not indicate their accuracy or precision,</li> </ul>	

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			<p>and  (c) the accuracy and the precision are not known for mean global temperatures indicated by the GISS, GHCN and Jones et al. data sets.  So claims such as “year x was the warmest year” cannot be substantiated from the data sets. All years are the same temperature within the (unknown) measurement errors. So, no year can be said to be warmer than any other in the absence of knowledge of the inherent error of each indication of each year’s temperature.  Furthermore, over the most recent 30 years the errors accumulating between the GHCN and Jones et al. data sets for mean global temperature are known to be of the order of 0.2 degrees C (because their different trends demonstrate that), and the errors accumulating in each data set are possibly more than this (because they are not known). If their accumulated errors were at the rate of [0.2 degrees C / 30 years] throughout the twentieth century, then this would account for all the ~0.6 degrees C rise they indicate occurred throughout that century.  Hence, it is invalid to attribute any causes – “natural” or otherwise – to “the decadal air temperature anomalies for 1906-2005”.</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-21)]</p>	
SPM-842	A	10:0	<p>We suggest adding table TS-3, as it gives a very good overview of highly relevant recent findings (new since TAR) with regard to different factors contributing to observed SLR.  [Govt. of Germany (Reviewer’s comment ID #: 2011-231)]</p>	Not appropriate here; this is the attribution section
SPM-843	A	10:0	<p>Figures SPM-4: Good figures.. I have a suggestion for a change of title to make it more readable: "Observed changes in temeprature comapred to model simulations of natural and anthropogenic forcing. These show change in decadal temeprature from 1906 to 2005 relative to the average of the 1901 to 1997 period" Should it be relative to the 1901-1997 period , can it not be relative to the mean over the whole period show i.e. 1906 to 2005?  [Joanna House (Reviewer’s comment ID #: 109-52)]</p>	<p>Caption suggestion accepted in part.  Scaling of these figures to be checked by chapter 9 and the text in the caption revised.</p>
SPM-844	A	10:0	<p>figure SPM-4: This figure is very much welcomed. Really impressive!  [Klaus Radunsky (Reviewer’s comment ID #: 204-9)]</p>	Thank you
SPM-845	A	10:0	<p>Figure SPM-4. This figure is very powerful and speaks volumes without words. Perhaps the bottom three figures could be enlarged to fill the full width of the page, given that they represent averages and therefore more robust trends than the continental records.  [Andy Reisinger (Reviewer’s comment ID #: 210-32)]</p>	Figure layout to be completed by TSU

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SPM-847	A	10:1	10:1	Suggest deleting “to forcing” as this forcing does not refer to mean-annual global radiative forcing as has been the use in IPCC, but rather to the theory that changes in seasonality of insolation led to slow changes in ice cover and GHGs. [Haroon Kheshgi (Reviewer’s comment ID #: 125-15)]	Accepted
SPM-848	A	10:2		The significance of the bullet point for policy makers is not clear. [Timothy H. Profeta (Reviewer’s comment ID #: 203-16)]	Rejected. Govt comments suggest this was understood by them.
SPM-849	A	10:3	10:3	I would add a very important generic bullet point, before the one that refers specifically to Arctic ice sheets, to say that: "There is now improved understanding and ability by models to reproduce large-scale temperature variations during ice ages and interglacial periods over the past several hundred thousand years. These large-scale temperature variations were caused primarily by variations in Earth's orbit around the sun, and bio-geochemical feedback mechanisms that further amplified this radiative forcing. [6.4; 6.6; 9.3]" This is a very important message. A lot of lay people still believe that previous ice ages are proof that the climate system simply has "mood swings" that have no good reason whatsoever, and that hence it is entirely futile to think we can influence the current "mood swing" in one way or another. To save space, the relevant part of the similar bullet point on page 6 lines 20-22 could be deleted since it will get lost especially to the lay reader there. [Andy Reisinger (Reviewer’s comment ID #: 210-27)]	Rejected (but will be in TS)
SPM-850	A	10:3	10:3	Please consider a further bullet point talking about rapid climate changes of the past, especially MOC shut-down events. The TS contains important information on this, such as that a complete and rapid shut-down is unlikely to be triggered by global warming alone, but appears to have been linked with inter-hemispheric energy shifts. There is so much public interest in "rapid" climate change, trigger points etc, that it may seem a bit strange if the SPM is silent on this issue. If you believe that knowledge is not sufficiently robust to allow clear statements in the SPM about past rapid changes, then it would be equally policy-relevant to actually say so, rather than to say nothing. [Andy Reisinger (Reviewer’s comment ID #: 210-28)]	Rejected (but will be in TS). See FAQ on abrupt change. Length constraints here.
SPM-851	A	10:3	10:3	add "and increased CO2/CH4 emissions. [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-132)]	Text has been edited
SPM-852	A	10:5	10:5	Replace title with "Reasons for climate change". [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-133)]	Rejected. Attribution and statistical approach are important here
SPM-853	A	10:8		The sentence starting "Confidence in the assessment . . ." is a very important sentence, and probably should be featured more prominently. [Timothy H. Profeta (Reviewer’s comment ID #: 203-17)]	Accepted, see revised headline.

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SPM-854	A	10:8	:12	The second sentence of the italicized paragraph says that there's an increase in confidence since the TAR, when in line 14-15 the very same confidence level as in the TAR has been used. In lines 9-10, does "stronger signals emerging in longer records" merely mean that the last 5 years have extended the series of warmest years on record? This is unclear to the reader. [Govt. of United States of America (Reviewer's comment ID #: 2023-808)]	First point: Reviewer is incorrect. The TAR used the word 'likely'; here we have very likely. This has been added to the italicized paragraph.  Second point: This is one of the reasons for higher confidence.
SPM-855	A	10:9		Delete "in part" [Richard Soulen (Reviewer's comment ID #: 248-10)]	Text has been edited
SPM-856	A	10:10		Remove the comma [Richard Soulen (Reviewer's comment ID #: 248-11)]	Accepted
SPM-857	A	10:11	10:12	"Replace ""seasonal, interdecadal, and paleoclimate timescales"" with ""seasonal, interdecadal, and paleoclimatic timescales""."  [Govt. of Canada (Reviewer's comment ID #: 2004-58)]	Text has been edited
SPM-858	A	10:12	10:12	It is proposed to delete the last part of the sentence ("although uncertainties remain"). This is because uncertainties will always remain. An alternative might be to say: Uncertainties might even be further reduced. [Klaus Radunsky (Reviewer's comment ID #: 204-7)]	Accepted
SPM-859	A	10:12	10:12	The mention of uncertainties seems obvious and unnecessary, since there will always remain uncertainties. [James S. Wang (Reviewer's comment ID #: 281-6)]	accepted
SPM-860	A	10:12		It is proposed to delete the last part of the sentence ("although uncertainties remain"). This is because uncertainties will always remain. An alternative might be to say: Uncertainties might even be further reduced. [Govt. of Austria (Reviewer's comment ID #: 2002-7)]	accepted
SPM-861	A	10:12		Add "in the simulations" after "although uncertainties remain" [Govt. of United States of America (Reviewer's comment ID #: 2023-809)]	Text has been edited.
SPM-862	A	10:14	10:14	"The heading is very weak compared to the bullet that follows that makes it very clear that it is anthropogenic forcing. Change to, for example, "anthropogenic forcing, particularly that due to greenhouse gas forcing".  [Govt. of Canada (Reviewer's comment ID #: 2004-59)]	Accepted, text edited.
SPM-863	A	10:14	10:15	SPM Comment: Delete this sentence because it is an assertion that cannot be true. The assertion requires accurate knowledge of "the observed warming of globally averaged temperatures in the last 50 years" but such knowledge does not exist. The surface temperature record begins ~1860. For the period 1860 to 2004, the	Rejected due to errors of fact in the comment; see chapter 3

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			<p>most cited of these data sets have good coherence (which is not surprising because they were compiled from the same available source data), but their trends (in degrees C/decade) and 2SD trend errors are</p> <p>GHCN: <math>0.076 \pm 0.010</math>                      Jones et al.: <math>0.064 \pm 0.007</math>                      GISS: <math>0.048 \pm 0.006</math></p> <p>The Jones trend is significantly different from the GISS trend (<math>p &lt; 0.05</math>), and the GHCN trend is very significantly different from the GISS trend (<math>p &lt; 0.01</math>). So, “coolest” and “warmest” years near the ends of the data sets are generated by trends that are known to be spurious.</p> <p>A change in the indicated temperature with time is only an indication of climate change when the change exceeds the intrinsic measurement errors (otherwise the change could be intrinsic measurement error). The intrinsic errors of GHCN, Jones et al. and GISS data sets are not known, but at least two of the data sets provide wrong results because they differ in annual change by more than double their stated 95% confidence limits in each of several years.</p> <p>Furthermore, the GHCN and Jones et al. data sets also have different trends for the warming period of the most recent 30 years. For the period 1976-2004, the temperature trends for the Jones et al., GISS, and GHCN surface temperature time-series are +0.215 degrees C/decade, +0.204 degrees C/decade and +0.274 degrees C/decade, respectively. And this also demonstrates that at least two of them are indicating spurious trends generated by the methods used to create the means.</p> <p>Data of this kind cannot be used to justify any assertion concerning probabilities of the causes of warming they suggest.</p> <p>Indeed, other recent data suggest that the trends suggested by these data sets for recent decades are very wrong. For the late 20th century warming period between 1976 and 2004, the rates of change indicated by these data sets are significantly higher than the rate of +0.059 degrees C/decade for the lower atmosphere measured by weather balloon radiosondes for the same period and the rate of +0.079 degrees C/decade and satellite MSUs for the period 1979 to 2004 (the satellite record begins in 1979).</p> <p>Furthermore, the compilers of these data sets admit their methods create spurious trends (ref. Vose et al., 2004). Their methods include integrating measurements into mean values for regions over the Earth’s surface called ‘grid boxes’. And the integrations cause trend problems for individual grid boxes. Vose et al. state that when the GHCN and Jones et al. trends are compared at the grid-box level then 9.3% of grid cells display “discrepant trends”. In other words, the</p>	

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				<p>integration of measurements into grid boxes causes 9.3% of grid boxes to have trends with opposite sign. (!) Simply,</p> <p>(a) temperature measurements cannot indicate climate change in the absence of knowledge of their accuracy and precision, (b) coherence between data sets does not indicate their accuracy or precision, and (c) the accuracy and the precision are not known for mean global temperatures indicated by the GISS, GHCN and Jones et al. data sets.</p> <p>So claims such as “year x was the warmest year” cannot be substantiated from the data sets. All years are the same temperature within the (unknown) measurement errors. So, no year can be said to be warmer than any other in the absence of knowledge of the inherent error of each indication of each year’s temperature.</p> <p>Hence, it is invalid to attribute any causes – anthropogenic or otherwise – to “the observed warming of globally averaged temperatures over the last 50 years. Also, other causes could be attributed and are more likely to be cause for any recent warming that may have occurred; for example, changes to cloud cover. Clouds reflect solar heat and a mere 2% increase to cloud cover would more than compensate for the maximum possible predicted warming due to a doubling of carbon dioxide in the air. Good records of cloud cover are very short; but it appears that cloudiness decreased markedly between the mid 80s and late 90s. Over that period, the Earth’s reflectivity decreased to the extent that if there were a constant solar irradiance then the reduced cloudiness provided an extra surface warming of 5 to 10 Watts/sq metre. This is a lot. The TAR said that since the industrial revolution, the build-up of human-caused greenhouse gases in the atmosphere has had a warming effect of only 2.4 W/sq m.</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-18)]</p>	
SPM-864	A	10:1 4	10:1 4	<p>Replace "greenhouse gas forcing" with "increases in human population, prosperity, energy usage and land-use changes". McKittrick and Michaels 2004 Climate Change Vol 26 pages 159-173 have shown this to be so for the surface temperature record</p> <p>[VINCENT GRAY (Reviewer’s comment ID #: 88-2210)]</p>	Rejected, see chapter 3 McKittrick & Michaels has itself been discredited. See e.g. Benestad (2004). Climate Research 27: 171 – 173.
SPM-865	A	10:1 4	10:1 5	<p>It is not clear what is the difference between this attribution judgement, and the attribution judgement of the TAR other than the change from likely to very likely. Suggest using identical wording as in the TAR (except for the likelihood judgement) including the footnote definition of likelihood, or describe what</p>	Text has been edited and this point clarified

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				exactly is the difference in this conclusion. [Haroon Kheshgi (Reviewer's comment ID #: 125-16)]	
SPM-866	A	10:1 4	10:1 5	This bullet seems weaker than a previous one in TAR(i.e, "There is new a stronger evidence that most of the warming observed over the last 50 years is attributable to human activities") is that the idea wanted to convey? [Louis Jose Mata (Reviewer's comment ID #: 170-2)]	Taken into account. Sentence reworded.
SPM-867	A	10:1 4	10:1 5	I find the words "very likely" a bit timid! Considering that there is absolutely no other scientific explanation for the observed warming (except the vague "internal variability") except greenhouse gas forcing, you should use more definitive words! [J. Shukla (Reviewer's comment ID #: 237-3)]	Rejected. Not supported by Chapter 9.
SPM-868	A	10:1 4	11:6	There is very little emphasis given to what Australia considers to be the key result of this section (as evident from Figure SPM-4) - namely that the observed changes in temperature, both at a global and regional scale, are consistent with model simulations which include both anthropogenic and known natural forcings, but are not consistent those including known natural forcings alone. This point should be the opening point of this section, instead of being buried in the third dot point (page 11 lines 1-6). [Govt. of Australia (Reviewer's comment ID #: 2001-28)]	Partly taken into account by revised opening statement.
SPM-869	A	10:1 4	11:3 3	Comparing these two statements may confuse readers: (1) on SPM-10, line 14, "It is very likely that greenhouse gas forcing has been the dominant cause of the observed warming of globally averaged temperatures in the last 50 years."; and (2) on SPM-11, line 33, " Attribution studies considering the entire record of the past 700 years support the conclusion that it is likely that greenhouse gas forcing has been the dominant cause of the observed warming of the northern hemisphere over the last 50 years." Why is one statement "very likely" and the other only "likely"? It takes a careful reader to see that the "very likely" refers to the globe and the "likely" refers to a hemisphere rather than the 700-yr record. [Govt. of United States of America (Reviewer's comment ID #: 2023-811)]	Taken into account. Text has been edited.
SPM-870	A	10:1 4	11:3 3	Double-check that you mean the global temperature change due to humans is very likely, while the northern hemisphere change due to humans is only likely and the southern hemisphere change is uncertain. [Govt. of United States of America (Reviewer's comment ID #: 2023-812)]	Taken into account. Text has been edited.
SPM - 871	A	10:1 4	:15	Here the word "anthropogenic" must be inserted, i.e. "anthropogenic greenhouse gas forcing".	Accepted

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				[Govt. of Norway (Reviewer's comment ID #: 2018-26)]	
SPM-872	A	10:1 4		Since "highly unlikely" is defined at line 26, make clear if it corresponds with a definition for "very likely" and for the terms in Table SPM-1 [Timothy H. Profeta (Reviewer's comment ID #: 203-18)]	SEE COMMENT SPM-890
SPM-873	A	10:1 4		Add some discussion—as a new first bullet—of the likelihood of anthropogenic forcing as related to the warming of the first half of the 20th century, as shown in the figures. Attribution for at least the whole 20th century needs to be addressed, including periods of warming and level periods. [Govt. of United States of America (Reviewer's comment ID #: 2023-810)]	Rejected due to length constraints. Covered in the chapter.
SPM-874	A	10:1 5	10:1 5	Add at end "from weather stations and ships" [VINCENT GRAY (Reviewer's comment ID #: 88-2211)]	Rejected. Text and chapter make clear that other types of warming are relevant, such as ocean heat content.
SPM-875	A	10:1 5		The phrase "last 50 years" should be "since 1950" [Govt. of United States of America (Reviewer's comment ID #: 2023-813)]	Text now reads "for the last half century."
SPM-876	A	10:1 7	10:2 3	SPM Comment: This paragraph is grossly misleading because it omits an important clarification. It is necessary to append an additional final sentence to it that says; "Ability to attribute a cause to an observed change demonstrates that the suggested cause is a possible explanation for the change, and it is important to note that the ability to attribute is not evidence that the attributed cause is responsible for the change in part or in whole."  [Richard Courtney (Reviewer's comment ID #: 49-19)]	Rejected. See glossary for definitions
SPM-877	A	10:1 7	10:1 7	This whole paragraph is without foundation. The "observed" temperature increase is most likely due to increases in human habitation and land-use, The increase in ocean temperature is probably cyclic. All these "attribution" studies are without any scientific basis. [VINCENT GRAY (Reviewer's comment ID #: 88-2212)]	Rejected, see chapters for extensive literature providing supporting evidence.
SPM-878	A	10:1 7	10:2 3	This bullet point contains two distinct different statements that should be separated into two separate bullet points. The first bullet point should be the generic attribution statement (lines 18-23), and the second bullet point should be the masking effect of aerosols (lines 17-18). [Andy Reisinger (Reviewer's comment ID #: 210-30)]	Taken into account.
SPM-879	A	10:1 7	10:2 3	Start the bullet point with the second sentence "Anthropogenic warming of the climate system ...". [Govt. of United Kingdom (Reviewer's comment ID #: 2022-134)]	Accepted.
SPM-880	A	10:1 7		I would think that one could characterize this with some confidence as "very" likely	Rejected. Not supported by chapter 9

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				[Thomas Crowley (Reviewer's comment ID #: 51-7)]	
SPM-881	A	10:17	:18	The message of the first sentence of this paragraph is very important and may be made even clearer by replacing it with the following sentence from the Robust Findings in the TS: "It is likely that greenhouse gases would have caused more warming than observed over the last 50 years with some warming offset by net cooling cause by natural and other anthropogenic factors." [Govt. of Norway (Reviewer's comment ID #: 2018-27)]	Taken into account. A slightly edited version of the TS text has been inserted.
SPM-882	A	10:17		The first sentence should really be last, it would be much better to begin with the strong statement "Anthropogenic warming..." and finish with the qualifiers. [Govt. of United States of America (Reviewer's comment ID #: 2023-814)]	Taken into account, text edited.
SPM-883	A	10:18	10:19	"Term 'anthropogenic warming' may be too vague to be understood. Recommend changing sentence to 'Warming of the climate system, due to anthropogenic emissions of greenhouse gases, can be detected and attributed...'" [Govt. of Canada (Reviewer's comment ID #: 2004-60)]	Accepted, text edited similar as suggested.
SPM-884	A	10:20	10:20	the several hundred meters mentioned here are not consistent with the 3000m stated on p. 7, line 1. [Rolf Müller (Reviewer's comment ID #: 181-7)]	Accepted, now consistent
SPM-885	A	10:22	10:22	It is proposed to insert after greenhouse gas increases: "in the atmosphere" because ozone depletion relates to the stratosphere as correctly indicated. [Klaus Radunsky (Reviewer's comment ID #: 204-8)]	Rejected. Not needed.
SPM-886	A	10:22		It is proposed to insert after greenhouse gas increases: "in the atmosphere" because ozone depletion relates to the stratosphere as correctly indicated. [Govt. of Austria (Reviewer's comment ID #: 2002-8)]	SEE COMMENT SPM-885
SPM-887	A	10:24		Insert figure 6.13 e) that illustrate the attribution by different causes. [Govt. of Germany (Reviewer's comment ID #: 2011-77)]	Rejected. Here we are considering the modern record.
SPM-888	A	10:24		I would like to suggest that the essence of a sentence from chapter 9, page 38 (lines 11-14), reproduced here: ("The fact that climate models are only able to reproduce observed surface temperature changes over the 20th century when they include anthropogenic forcings and their failure to do so when they exclude anthropogenic forcings in strong evidence for the influence of humans on global climate.") should also appear in PM-10, line 24. In my opinion, this is a very important statement of the report, and the strongest model based evidence to say that the observed changes cannot be explained as natural variations. An essence of this statement should appear in SPM. A suggested language for a new bullet on line 24 of SPM-10:	Text has been edited to provide greater clarity. However, the suggestion provided is not accepted since the material provided examines both models and data, and thereby makes a clearer and more consistent statement than the suggested text based on models. Some additional material has been added to the TS regarding the inability of models to reproduce observed global and continental trends.

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				* There is not a single model in the world that can simulate the warming observed over the past 50 years without anthropogenic forcings, which suggests that it is highly unlikely that observed warming was caused by natural internal variability. [J. Shukla (Reviewer's comment ID #: 237-1)]	
SPM-889	A	10:2 5	10:2 7	I think that what you want to rule out here is that recent global climate change was caused only by a combination of natural internal variability and natural radiative forcing, not just natural internal variability. [Olivier Boucher (Reviewer's comment ID #: 27-9)]	Rejected. The bullet is about internal variability.
SPM-890	A	10:2 5	10:2 7	"Use of confidence term 'highly unlikely' could be misread and is misplaced among other confidence terms in this section. Recommend rewording sentence '...jointly supports the conclusion that it is [highly likely, likely, etc.] that recent global climate change was not caused by natural internal variability.' [Govt. of Canada (Reviewer's comment ID #: 2004-61)]	Accepted
SPM-891	A	10:2 5	10:2 7	SPM Comment: This sentence is grossly misleading because it omits an important clarification. It is necessary to append an additional final clause to it that says; "on the basis of understandings built into model studies." [Richard Courtney (Reviewer's comment ID #: 49-20)]	Rejected because more than modelling is involved. See chapter 9 for discussion of the basis.
SPM-892	A	10:2 5	10:2 5	Is it just that it was widespread, or also that it is long-term/persistent warming? [Joanna House (Reviewer's comment ID #: 109-51)]	Rejected. Time frame is clear from context.
SPM-893	A	10:2 5	10:2 7	The basis for this statement is modelling studies. There is a plausible argument that reduced upwelling in the tropical ocean surface layer has resulted in increased tropical SST (McPhaden and Zhang, 2002); that the increased SST have led to a warming of the tropical troposphere by convective overturning (satellite observed tropical troposphere temperature response to ENSO variations, theory of Riehl and Malkus, 1958) and that the increased tropical troposphere heat content is transported by the atmospheric circulation to middle and high latitudes (Trenberth and Stepaniak, 2004). The probability of atmosphere and ocean warming as a result of internal variability (wind stress driving ocean circulation modes and aperiodically modifying tropical SST patterns; changing ocean SST patterns regulating poleward heat transport by the atmospheric circulation) is considerably higher than 5 percent and cannot be arbitrarily dismissed in this way. The statement should be modified to read: "The observed widespread warming of the atmosphere and ocean, together with ice	Rejected, see observations section. Global ocean heat content has increased.

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				mass loss has been simulated by climate models that include natural and anthropogenic forcing, giving confidence to the attribution". [William Kininmonth (Reviewer's comment ID #: 128-98)]	
SPM-894	A	10:2 5	10:3 5	This figure is extremely helpful, and likely to be frequently reproduced subsequent to the publication of AR4. One question (perhaps for the chapter 9 authors): why are ocean regions not considered as regional panels? I would highly encourage the inclusion of a panel for the tropical North Atlantic, given its prominence in discussions of Atlantic tropical cyclone genesis. [Michael Mann (Reviewer's comment ID #: 156-44)]	Rejected. Figure is already quite complex and focus is on land areas. Also, data are limited in many ocean regions.
SPM-895	A	10:2 5	10:2 7	FAQ 9.2 (page 85, lines 16-17) says that it's " <i>very</i> unlikely that the 20th century warming can be explained by natural variability," but the SPM (page 10, lines 25-27) makes a similar statement -- about global climate change in the last 50 years -- but refers to it as " <i>highly</i> unlikely." Is the distinction between "very unlikely" and "highly unlikely" intentional? If so, is this because the former refers to the entire 20th century, whereas the latter refers only to the last 50 years? If so, this intentional distinction and greater degree of (un)likelihood should perhaps be stated explicitly. [Melinda Marquis (Reviewer's comment ID #: 162-101)]	SEE SPM-890
SPM-896	A	10:2 5	10:2 7	It would be helpful if the authors could come up with a more precise qualifier for the statement in the last line. E.g. it is highly unlikely that recent global climate change was caused "solely" or "predominantly", or something like that, by natural internal variability. It would also be helpful if the authors could add "or changes in solar intensity" to this sentence, to capture all significant natural drivers for climate change in this one single bullet point. It would also be useful to add "in both hemispheres" after "ocean" to emphasise the inability to explain the observed warming patterns by a simple inter-hemispheric redistribution of energy. [Andy Reisinger (Reviewer's comment ID #: 210-31)]	Rejected. The statement is correct without qualifier. The bullet is addressing internal variability not external forcing. It is true that interhemispheric heat exchange can not explain the observed warming patteern, but this is too detailed for the SPM.
SPM-897	A	10:2 5	:26	Clarify "observed" (over what time period or since what year)? Similarly, what is meant by "recent"? [Govt. of United States of America (Reviewer's comment ID #: 2023-815)]	Clarified
SPM-898	A	10:2 5	:27	The conclusion here applies either to "natural forcing and internal variability" or this <5% likelihood applies only to internal variability. If the latter, this conclusion is very misleading and the word "natural" should be dropped, as most will read this as all "natural" forces (vs. anthropogenic). If it is only internal variability, then this is not important enough for an SPM bullet. No matter what construct remains (if any), if retained in the text, authors need to clarify what it	Text has been clarified. Disagree that it is not important enough for SPM.

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				means by natural INTERNAL variability and natural EXTERNAL variability. [Govt. of United States of America (Reviewer's comment ID #: 2023-816)]	
SPM-899	A	10:2 6	10:2 7	It is better to add "only" at the end of the sentence, i.e., "...it is highly unlikely that recent global climate change was caused by natural internal variability only". [Govt. of China (Reviewer's comment ID #: 2006-19)]	Text has been edited
SPM-900	A	10:2 7	10:2 7	Suggest that this line read "was caused solely by natural variability" [Govt. of Australia (Reviewer's comment ID #: 2001-29)]	SEE COMMENT SPM-899
SPM-901	A	10:2 7	10:2 7	Do you mean "natura variability". Doesn't "internal" imply unforced varaibility - where here you also mean solar and volcanic? [Piers Forster (Reviewer's comment ID #: 73-10)]	Accepted. Text has been edited.
SPM-902	A	10:2 7	10:2 7	"natural variuability" is estimated from models which deny the existence of "natural change". Most of the changes discussed in this section could readily be "attributed" to a mixture of changes in the sun, volcanoes, El Niño,cosmic rays and increases in human prosperity. [VINCENT GRAY (Reviewer's comment ID #: 88-2213)]	Rejected, see figure 9.4.1. See also extensive discussion of modelling and internal variability in chapters 8, 9, and 10.
SPM-903	A	10:2 7	10:2 7	Replace "was caused by" with "is a symptom of". [Govt. of United Kingdom (Reviewer's comment ID #: 2022-135)]	Rejected. Suggested change seems less clear.
SPM-904	A	10:2 9	10:3 0	"Figure SPM-4 has a lot in it and not that easy to read or understand. Some purple bands in here are not defined in the figure caption either. The key point that the red band is seperating from the blue band and is indicative of the climate signal, is not expressed very clearly."  [Govt. of Canada (Reviewer's comment ID #: 2004-62)]	Figure has been redrawn
SPM-905	A	10:2 9	10:3 0	Figure SPM-4. The figure is perfect from a scientific point of view. Is the concept of anomalies clear to policy makers ? I wonder if the variable displayed may not be difficult to understand for some readers. It should be checked. [Philippe Tulkens (Reviewer's comment ID #: 271-1)]	Rejected. Anomalies have been used in WG1 figures since the first report in 1990 and policymakers have found them useful.
SPM-906	A	10:3 2	10:3 3	First sentence in caption is confusing. Why are two periods specified (which are close to the same)? [Govt. of Australia (Reviewer's comment ID #: 2001-30)]	Has been clarified
SPM-907	A	10:3 2	10:3 5	"For clarity, the discussion of figure SPM-4 should indicate the colour where red and blue areas overlap (eg, mauve)"  [Govt. of Canada (Reviewer's comment ID #: 2004-63)]	SEE SPM 904
SPM-908	A	10:3 2	10:3 3	"Time periods for anomalies (1906-2005) and baseline (1901-1997) seem too	SEE COMMENT SPM-906. Dates clarified

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				similar to be correct. As a minimum, the closeness of the dates is confusing." [Govt. of Canada (Reviewer's comment ID #: 2004-64)]	
SPM-909	A	10:3 2	10:3 2	"RE: 5-95%. Uncertainty range used in SPM is not consistent. Why 90% in this case and 65% elsewhere?" [Govt. of Canada (Reviewer's comment ID #: 2004-65)]	Uncertainties clarified
SPM-910	A	10:3 2	10:3 2	All these graphs are spurious. The figures for the USA ( Figure 3.2.3) and in China (see Zhao, Ding,Luo,Wang, 2005 Acta Metroologica Sinica Vol 19 pages 389-400) have been subjected to a procedure called "homogeneity adjustment" to remove the bias. When this is done "global warming" all but disappears. You should replace ALL of the graphs in this diagram with "homogeneity adjusted" graphs, and your contentions about a consistent "warming trend" will disappear [VINCENT GRAY (Reviewer's comment ID #: 88-2214)]	Rejected. The datasets have been discussed in detail in chapter 3.
SPM-911	A	10:3 2	10:3 2	At least this graph shows that the models are completely worthless to forecast future trends, since we have no knowledge of future "natural" changes. [VINCENT GRAY (Reviewer's comment ID #: 88-2216)]	Noted that this is the reviewer's view. No action has been suggested.
SPM-912	A	10:3 2	:33	I do not understand what is meant by the sentence beginning "Continental- and . . .", especially what is implied by the two time periods given. It may be OK, but I don't understand it. [Richard Soulen (Reviewer's comment ID #: 248-12)]	See comment SPM -906
0-97	A	10:3 2	19:3 3	SPM: why are anomalies calculated relative to 1901-1997 relative to 1906-2005? [Richard Allan (Reviewer's comment ID #: 3-59)]	SEE COMMENT SPM-906
SPM-914	A	10:3 3	10:3 3	Would be clearer if added "mean temperature during" before "the 1901-1997 period." [Melinda Marquis (Reviewer's comment ID #: 162-102)]	Accepted
SPM-915	A	10:3 4	10:3 4	I question your ability to determine "natural forcings. "Climate Change 1990" attributed the rise in temperature from 1910 to 1942 to "recovery from the little ice age" which you now try to deny. You refuse to even consider that most of the observed temperature changes are caused by improvements in human habitation, despite all the evidence. [VINCENT GRAY (Reviewer's comment ID #: 88-2215)]	View noted. No action has been suggested.
0-98	A	11:1		SPM: why is the difference between land and ocean temperatures an anthropogenic signal? I'd assume it relates to the differences in heat capacity. [Richard Allan (Reviewer's comment ID #: 3-60)]	Text has been edited
SPM-916	A	11:1	11:2	"The first sentence is too technical and not clear enough. Suggest not just saying ""contrasts between"", but DESCRIBING what the contrast is and then	Text has been edited

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				<p>EXPLAINING why this is an anthropogenic signal. "</p> <p>[Govt. of Canada (Reviewer’s comment ID #: 2004-66)]</p>	
SPM-917	A	11:1	11:6	<p>SPM Comment: Delete this paragraph because it is a set of assertions that cannot be true. The assertions require accurate knowledge of “the surface air temperature changes” over “land” and “ocean” but such knowledge does not exist.</p> <p>The surface temperature record begins ~1860. For the period 1860 to 2004, the most cited of these data sets have good coherence (which is not surprising because they were compiled from the same available source data), but their global trends (in degrees C/decade) and 2SD trend errors are</p> <p>GHCN: <math>0.076 \pm 0.010</math>                      Jones et al.: <math>0.064 \pm 0.007</math>                      GISS: <math>0.048 \pm 0.006</math></p> <p>The Jones trend is significantly different from the GISS trend (<math>p &lt; 0.05</math>), and the GHCN trend is very significantly different from the GISS trend (<math>p &lt; 0.01</math>). So, “coolest” and “warmest” years near the ends of the data sets are generated by trends that are known to be spurious.</p> <p>A change in the indicated temperature with time is only an indication of climate change when the change exceeds the intrinsic measurement errors (otherwise the change could be intrinsic measurement error). The intrinsic errors of GHCN, Jones et al. and GISS data sets are not known, but at least two of the data sets provide wrong results because they differ in annual change by more than double their stated 95% confidence limits in each of several years.</p> <p>Furthermore, the GHCN and Jones et al. data sets also have different trends for the warming period of the most recent 30 years. For the period 1976-2004, the temperature trends for the Jones et al., GISS, and GHCN surface temperature time-series are +0.215 degrees C/decade, +0.204 degrees C/decade and +0.274 degrees C/decade, respectively. And this also demonstrates that at least two of them are indicating spurious trends generated by the methods used to create the means.</p> <p>Indeed, other recent data suggest that the trends suggested by these data sets for recent decades are very wrong. For the late 20th century warming period between 1976 and 2004, the rates of change indicated by these data sets are significantly higher than the rate of +0.059 degrees C/decade for the lower atmosphere measured by weather balloon radiosondes for the same period and the rate of +0.079 degrees C/decade and satellite MSUs for the period 1979 to 2004 (the satellite record begins in 1979).</p>	<p>Rejected due to errors of fact in the comment. See Chapter 3.</p>

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				<p>Furthermore, the compilers of these data sets admit their methods create spurious trends (ref. Vose et al., 2004). Their methods include integrating measurements into mean values for regions over the Earth's surface called 'grid boxes'. And the integrations cause trend problems for individual grid boxes. Vose et al. state that when the GHCN and Jones et al. trends are compared at the grid-box level then 9.3% of grid cells display "discrepant trends". In other words, the integration of measurements into grid boxes causes 9.3% of grid boxes to have trends with opposite sign. (!)</p> <p>Hence, comparing regions (e.g. "land" and "ocean" regions) using this data is invalid. The regional data are so unreliable that 9.3% of grid boxes to have trends with opposite sign on an annual basis.</p> <p>Simply,</p> <p>(a) temperature measurements cannot indicate climate change in the absence of knowledge of their accuracy and precision,</p> <p>(b) coherence between data sets does not indicate their accuracy or precision,</p> <p>(c) the accuracy and the precision are not known for mean global temperatures indicated by the GISS, GHCN and Jones et al. data sets, and</p> <p>(d) regional temperatures indicated by the GISS, GHCN and Jones et al. data sets are known to be wrong (i.e. they have large unquantified errors).</p> <p>Hence, it is invalid to attribute any "signal" – anthropogenic or otherwise – to comparison of the parts of these data sets that represent specific regions (e.g. "land" and "ocean").</p> <p>[Richard Courtney (Reviewer's comment ID #: 49-22)]</p>	
SPM-918	A	11:1	11:6	<p>Replace whole paragraph by "The contrast between land and sea is one of the best proofs of the influence of local heating on the surface record. Higher temperatures are found in the winter and at night, both indications of improved comfort in human dwellings"</p> <p>[VINCENT GRAY (Reviewer's comment ID #: 88-2217)]</p>	Rejected. Assertion is not supported by literature. See Ch 3.
SPM-919	A	11:1	11:	<p>Surely models do not postulate greater radiative forcing over land than over the ocean. Radiative forcing is supposed to be "global" not selective ( See Question 1, Figure 1) The explanation from the extra warming from cities is far more plausible</p> <p>[VINCENT GRAY (Reviewer's comment ID #: 88-2218)]</p>	Text edited. Urban heat island effects are discussed in chapter 3. They do not affect conclusions. Oceans are cooler because of thermal inertia in a warming world.
SPM-920	A	11:1	11:2	<p>why is it indicated by this?</p> <p>[Joanna House (Reviewer's comment ID #: 109-53)]</p>	Text has been edited.
SPM-921	A	11:1	11:6	<p>The statements are ambiguous and misleading. The fact that warming has been</p>	Text has been edited.

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				<p>detected on all continents, and land warming is more than ocean warming, does not necessarily point to an anthropogenic cause. It is the modelling simulation that supports the view of an anthropogenic signal. Climate variability linked to internal processes becomes very important on longer timescales, especially wind-driven excitation of internal modes of the ocean circulations, such as the ocean gyres and overturning circulations with decadal to multi-centennial natural periods. ENSO type internal variability has relatively limited duration. However excitation of ocean modes with natural periods of decades to centennial and millennial periods are likely to have greater magnitudes, especially for polar ice sheet variation involving prolonged latent energy exchanges depending on the magnitude of poleward transport of energy. Suggest the paragraph read: "Warming has been likely detected on all inhabited continents. The different surface temperature changes between global land and ocean areas are also simulated by computer models and indicate an anthropogenic signal (see Figure SPM-4). The chance that these patterns are reproduced by chance or are spurious is small. Attribution studies suggest a physical limit to climate variability due to internal processes that at smaller scales (such as ENSO) become more important relative to changes due to external forcing".</p> <p>[William Kininmonth (Reviewer's comment ID #: 128-99)]</p>	
SPM-922	A	11:1	11:6	<p>This is an important paragraph, but it is likely to be incomprehensible to a PM. A PM probably understands the concept of signal to noise ratio.</p> <p>[Michael Manton (Reviewer's comment ID #: 157-27)]</p>	Not clear what the reviewer is suggesting as an action item. Text has been edited for clarity.
SPM-923	A	11:1	11:6	<p>This bullet point contains two distinct statements that should be separated into two separate bullets. The first bullet is lines 1-3 (to "...very small"), the second bullet is lines 3-6. In line 3, it would be helpful to replace the phrase "very small" with "very unlikely" or another appropriate such phrase. In line 5, it would help clarify what you are trying to say by adding "regional" before "climate processes". The sentence is also correct as it stands of course, but the main thrust of this sentence is nonetheless focused on difficulties at sub-hemispheric scales.</p> <p>[Andy Reisinger (Reviewer's comment ID #: 210-33)]</p>	Taken into account. Text has been edited.
SPM-924	A	11:1	:6	<p>The content in this bullet is hard to understand and must be rewritten. For instance, I do not understand what is meant by "these patterns". The decadal time scale should be mentioned. To me the content of this bullet is connected to practical predictability of regional decadal climate change obtained in climate models, a predictability which still seems to be low.</p> <p>[Govt. of Norway (Reviewer's comment ID #: 2018-28)]</p>	Taken into account. Text has been edited.
SPM-925	A	11:1	:2	<p>"The signal of greenhouse-gas forcing" phrase needs to explicitly include GHGs</p>	Text has been edited

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				+ aerosols. In line 2, add the all caps item to the existing sentence "...temperature CHANGES." [Govt. of United States of America (Reviewer's comment ID #: 2023-817)]	
SPM-926	A	11:2	11:2	this seems a separate point to the first part of this sentence [Joanna House (Reviewer's comment ID #: 109-54)]	Taken into account. Text has been edited.
SPM-927	A	11:3	11:4	"Sentence 'Difficulties remain in simulating temperature changes in specific parts of the world' begs the question, why? Suggest that at least one reason be provided for why this remains difficult (e.g. '...mainly because of poor data sets for these regions, or, ...mainly due to limitations on regional specificity of climate models)."  [Govt. of Canada (Reviewer's comment ID #: 2004-67)]	See rest of paragraph.
SPM-929	A	11:3		"very small" is not a standard term. Edit to use a standard term. [Govt. of United States of America (Reviewer's comment ID #: 2023-818)]	Taken into account. Text has been edited.
SPM-930	A	11:4	11:6	This information about climate variability seems to be very important and should not be deleted. [Govt. of Austria (Reviewer's comment ID #: 2002-10)]	Taken into account. Text has been edited. Thank you
SPM-931	A	11:4	11:6	"Last sentence in this paragraph is way too technical again. Explain in simpler terms."  [Govt. of Canada (Reviewer's comment ID #: 2004-68)]	Taken into account. Text has been edited.
SPM-932	A	11:4	11:6	Rephrase, not readily understandable [Stephen J. Hawkins (Reviewer's comment ID #: 102-15)]	Taken into account. Text has been edited.
SPM-933	A	11:4	11:6	This information about climate variability seems to be very important and should not be deleted. [Klaus Radunsky (Reviewer's comment ID #: 204-10)]	Taken into account. Text has been edited. Thank you
SPM-934	A	11:4	11:4	How specific? Explain the role of local variability. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-136)]	Taken into account. Text has been edited.
SPM-935	A	11:4	11:6	What does this sentence mean? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-137)]	Taken into account. Text has been edited.
SPM-936	A	11:4		What does spurious mean? A result of natural variability or of bad measurements? Probably can be more specific. [Dennis Hartmann (Reviewer's comment ID #: 100-7)]	Taken into account. Text has been edited.
SPM-937	A	11:6		Delete "predictable" and replace "due" with "attributable" [Govt. of United States of America (Reviewer's comment ID #: 2023-819)]	Text has been edited such that this comment is no longer relevant
SPM-	A	11:8	11:8	We could say "confirms" instead of "suggests"; it is more than suggestion.	Rejected. Uncertainty language seems appropriate as

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938				[Govt. of France (Reviewer's comment ID #: 2010-108)]	is.
SPM-939	A	11:8	11:9	It is unclear in "heat waves and other extremes" whether "extremes" indicates temperature extremes, precipitation extremes or both. Clarification required. [Govt. of Japan (Reviewer's comment ID #: 2014-11)]	Headline statement is clarified in the bullets that follow.
SPM-940	A	11:8		The word "other" stands out. It suggests reference to preceding bolded entry on SPM-10, lines 14-15. Delete "other aspects of climate including". [Govt. of United States of America (Reviewer's comment ID #: 2023-820)]	Text has been edited.
SPM-941	A	11:9	11:9	""the additional aspects of climate could also be usefully included in Table SPM-1. This section would likely benefit from a bullet on cyclones. ""  [Govt. of Canada (Reviewer's comment ID #: 2004-69)]	The relevant extremes are in SPM-1. Material is discussed here for those with appropriate confidence levels.
SPM-942	A	11:9	11:9	Add at end "but this is not necessarily related to emissions of greenhouse gases" [VINCENT GRAY (Reviewer's comment ID #: 88-2219)]	Rejected, no basis provided for suggestion.
SPM-943	A	11:9	11:9	is sea ice cliamte? Is changing sea ice not an impact of changing cliamte? [Joanna House (Reviewer's comment ID #: 109-55)]	Rejected. Sea ice is part of the climate system.
SPM-944	A	11:1 1	11:1 1	"Is term 'anthropogenic forcing' defined? Is it well understood? If not, recommend using full term 'Radiative forcing due to anthropogenic emissions of greenhouse gases'. "  [Govt. of Canada (Reviewer's comment ID #: 2004-70)]	Rejected. Text has covered anthropogenic forcing, which includes aerosols.
SPM-945	A	11:1 1	11:1 1	Replace "anthropogenic" by "human-induced" [VINCENT GRAY (Reviewer's comment ID #: 88-2220)]	SEE COMMENT SPM-949
SPM-946	A	11:1 1	11:1 1	Replace "has likely" with "may have" [VINCENT GRAY (Reviewer's comment ID #: 88-2221)]	Rejected, no basis provided for suggestion
SPM-947	A	11:1 1	11:1 3	why is the change in arctic sea ice attributed directly to anthropogenic forcing and not oteher ice or sea-level change observations? [Joanna House (Reviewer's comment ID #: 109-56)]	Taken into account. Sea level now mentioned above.
SPM-948	A	11:1 1	11:1 1	At end of sentences, change "sea ice extent" to "sea ice extent and thickness" as this is really a key change as well. [Michael MacCracken (Reviewer's comment ID #: 152-39)]	Rejected. No attribution studies on ice thickness.
SPM-949	A	11:1 1		"Anthropogenic forcing" is a vague term and is mixed here with "human influence". Avoid using "anthropogenic forcing" unless it is explicitly and carefully defined in the SPM. Also include definition in the glossary. [Govt. of United States of America (Reviewer's comment ID #: 2023-821)]	Wording changed to build in definition
SPM-	A	11:1	11:1	Replace "strengthen the confidence in this conclusion" with "indicate this"	Rejected, no basis given for suggestion

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950		2	3	"influences", "not necessarily related to greenhouse gas emissions" [VINCENT GRAY (Reviewer's comment ID #: 88-2222)]	
SPM-951	A	11:1 2	11:1 3	Delete. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-138)]	Rejected, no basis given for suggestion
SPM-952	A	11:1 5	11:1 8	Suggest deletion of this dot point as it is too watered down for the SPM [Govt. of Australia (Reviewer's comment ID #: 2001-31)]	Rejected. Text edited to improve clarity.
SPM-953	A	11:1 5	11:1 8	Bullet SPM-11-[15-18] is very vague. Moreover, the ES of Chapter 10 does not have a heading "circulation patterns". I recommend to replace "circulation patterns" by "sea level pressure patterns". [Gerrit Burgers (Reviewer's comment ID #: 34-8)]	Rejected. This is the attribution section – not the projections section. Please see the section numbers that are appropriate, indicated in brackets. Sea level pressure patterns not clear to non-specialists.
SPM-954	A	11:1 5	11:1 8	SPM Comment: Delete this paragraph because it cannot be true. If the "differences in the magnitudes of model simulations and observed changes are not understood" then there cannot be any valid basis for the assertion that "human influences are likely to have contributed to" the modelled effects. [Richard Courtney (Reviewer's comment ID #: 49-23)]	Rejected. Text says there is an effect, not that it accounts for all changes observed
SPM-955	A	11:1 5	11:1 5	Insert after [VINCENT GRAY (Reviewer's comment ID #: 88-2223)]	Do not understand comment
SPM-956	A	11:1 5	11:1 8	The first sentence is difficult to sustain in light of thermodynamic principles and the qualifier of the second sentence. Greater warming over polar regions is expected to reduce the meridional temperature gradient and the strength of the middle latitude westerly winds. Certainly the model simulations do not identify the intensification of the mean surface pressure systems and their associated wind fields (Figure TS-27 and Figure 9.5.2). Suggest paragraph be deleted. [William Kininmonth (Reviewer's comment ID #: 128-100)]	Rejected, see underlying chapter for details. Model simulations certainly do identify these signals.
SPM-957	A	11:1 5	11:1 6	This sentence seems stronger than those in chapter 9, which talks of 'suggestive' changes. We can barely measure precip and wind changes. It is also early days for the circulation changes. Does a PM know of the NAO and NAM? What about the SAM? [Michael Manton (Reviewer's comment ID #: 157-28)]	Rejected. The statement represents our scientific understanding and is consistent with chapter 9. No country complained about NAO/NAM so apparently they do understand what is meant.
SPM-958	A	11:1 5	:18	The last period should be deleted or rewritten. What is the magnitude of a simulation? I would rather say that the climate models fail to predict the amplitudes of the variations. I have read the relevant papers forming the basis for this bullet. It might be considered to leave out this bullet. [Govt. of Norway (Reviewer's comment ID #: 2018-29)]	Sentence rewritten.
SPM-959	A	11:1 5		"Human influence" also needs definition in the glossary. [Govt. of United States of America (Reviewer's comment ID #: 2023-822)]	Influence and human are standard English words and we believe their meaning is clear here.

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SPM-960	A	11:1 5	:18	This bullet is misleading about the attribution of human influences on changes in precipitation patterns. Replace “related variables” with “related storm tracks”. Also in line 9, remove “and precipitation” because circulation effects on precipitation are not sufficient to determine changes in total precipitation. [Govt. of United States of America (Reviewer’s comment ID #: 2023-823)]	Text has been edited for clarity.
0-99	A	11:1 6		SPM: the footnote (4) is obscure and should be removed, instead replacing with a reference to the chapters [3.6, 9.5] [Richard Allan (Reviewer’s comment ID #: 3-61)]	Footnote has been retained but reference to 3.6 added.
SPM-961	A	11:1 6	11:1 6	Replace “circulation patterns” by “sea level pressure patterns”. (The ES of Chapter 10 does not have a heading “circulation patterns”.) [Govt. of Netherlands (Reviewer’s comment ID #: 2016-17)]	Rejected. This is the attribution section – not the projections section. Please see the section numbers that are appropriate, indicated in brackets. Sea level pressure patterns not clear to non-specialists.
SPM-962	A	11:1 7	11:1 7	It is proposed to insert "yet" after "changes are not". [Govt. of Austria (Reviewer’s comment ID #: 2002-11)]	Text has been edited
SPM-963	A	11:1 7	11:1 8	"What is not understood about differences in the magnitudes of model simulations and observed changes? Are the differences 'between' model simulations and observed changes? If so, recommend rephrasing to 'Differences between the magnitudes of model simulations and observed changes are not understood'." [Govt. of Canada (Reviewer’s comment ID #: 2004-71)]	Taken into account. Text edited.
SPM-964	A	11:1 7	11:1 8	Replace "not understood" with "to be expected, since models cannot simulate natural changes or human influences unrelated to greenhouse gas emissions" [VINCENT GRAY (Reviewer’s comment ID #: 88-2224)]	Rejected. No basis for comment, which ignores a large body of literature as assessed in this report.
SPM-965	A	11:1 7	11:1 7	It is proposed to insert "yet" after "changes are not". [Klaus Radunsky (Reviewer’s comment ID #: 204-11)]	Text has been edited
SPM-966	A	11:1 7	11:1 7	What does magnitude of model simulations mean? Explain or rephrase the sentence. [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-139)]	Text has been edited.
SPM-967	A	11:1 7	:18	This last “however” sentence does not enhance understanding of the first or serve as a suitable caveat. Change to “Differences between model simulations and observed changes are...” Given this new more accurate statement is it strong enough to be included in the SPM? [Govt. of United States of America (Reviewer’s comment ID #: 2023-824)]	Editing change accepted. Attribution confidence is assessed in ch 9 and retained here.
SPM-968	A	11:1 8		"suggest change to: “on these short time scales” as it is not made clear enough that it is only on the short time scale that there is little difference."	Rejected. The changes are multidecadal

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				[Govt. of Canada (Reviewer's comment ID #: 2004-72)]	
SPM-969	A	11:20	11:20	Word 'trends' is confusing. Suggest redraft: 'Frequencies of occurrence of surface....' [Govt. of Australia (Reviewer's comment ID #: 2001-32)]	SEE COMMENT SPM-977 Taken into account. Statement reworded
SPM-970	A	11:20	11:22	The basis for the assessment that it is likely that the increase in warm temperature extremes and decrease in cold temperature extremes can be attributed to human activities is unclear. The underlying text for this topic is section 9.4.3.2, which presents the results of modeling studies which show that including anthropogenic effects "improves the simulation of these changing temperature extremes", but stops well short of attributing those changes to anthropogenic effects. Unless a clear logic can be provided for attributing changes in temperature extremes to human activities, the assessment should be that its is unknown whether these changes can be attributed to human activities. [Lenny Bernstein (Reviewer's comment ID #: 20-10)]	SEE COMMENT SPM-975 Appropriate attribution methods have been used, see chapter 9 and table SPM-1.
SPM-971	A	11:20	11:20	Delete "Trends in" and capitalise "Surface" [VINCENT GRAY (Reviewer's comment ID #: 88-2225)]	Text has been edited
SPM-972	A	11:20	11:21	Replace "are likely to" by "might" [VINCENT GRAY (Reviewer's comment ID #: 88-2226)]	Rejected, no basis for suggestion
SPM-973	A	11:20	11:20	presumably decreasing frost days, decreasing cold nights and decreasing cold days? [Joanna House (Reviewer's comment ID #: 109-57)]	Taken into account. Statement reworded
SPM-974	A	11:20	11:22	The use of the term 'anthropogenic forcing' cannot be sustained. The trends in frost night, cold nights and cold days are real and an indicator of global warming, but not necessarily due to anthropogenic influences. Suggest replace 'anthropogenic forcing' with 'global warming'. The cause of the global warming is then linked back to attribution on the larger scale. [William Kininmonth (Reviewer's comment ID #: 128-101)]	Rejected. Specific attribution studies are assessed in the underlying chapter 9 sections.
SPM-975	A	11:20	11:22	What is the basis for the findings that it is likely (66 to 90% probability) that warm temperature extremes have increased and cold temperature extremes have decreased? The text refers the reader to Section 9.4, which, on Pg. 9-34, lines 3-5, states that including anthropogenic effects improves the simulation of extreme events. No indication of how much improvement is provided on how much improvement occurs or why such improvement should be taken as a strong indicator of human influence. Either make a more compelling argument supporting this finding, or change the assessment to unknown . [Jeff Kueter (Reviewer's comment ID #: 137-10)]	Rejected. Referencing is correct. See section 9.4.3.
SPM-	A	11:2	11:2	On line 20, change "including" to "including the frequency of occurrence of". On	Rejected. Not consistent with language used in chapter.

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976		0	1	line 21, change "may have increased" to "likely has increased" in order to make sure using the lexicon. [Michael MacCracken (Reviewer's comment ID #: 152-40)]	
SPM-977	A	11:2 0	11:2 1	What kind of trends? Clarify. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-140)]	Taken into account. Statement reworded.
SPM-978	A	11:2 0		Add the word "fewer" before "frost". What is the time frame of the trend? This comment rendered moot if time frames elucidated in Table SPM-1. [Govt. of United States of America (Reviewer's comment ID #: 2023-825)]	Refer to Table SPM-1
SPM-979	A	11:2 1	11:2 2	The language is not precise with regard to the likelihood. Therefore the following wording is proposed: It is likely that anthropogenic forcing has increased the risk of heat waves. [Govt. of Austria (Reviewer's comment ID #: 2002-12)]	Rejected. Not consistent with language used in chapter.
SPM-980	A	11:2 1	11:2 2	SPM Comment: Delete the sentence saying; "Anthropogenic forcing may have increased the risk of heat waves. (See Table SPM-1 [9.4])" because it is not science. Either something is proven or it is not, and what "may" have happened is spin. If this sentence is not to be deleted then I suggest addition of the equally valid sentence that says; "Anthropogenic forcing may have increased the risk of flying pigs".  [Richard Courtney (Reviewer's comment ID #: 49-24)]	Rejected. Evidence for possible change in heat waves is presented and described carefully in chapter 9. Language here including the word 'may' avoids overstatement or misinterpretation.
SPM-981	A	11:2 1	11:2 1	Insert after "anthropogenic forcing" "which might not imply an influence of greenhouse gas emissions" [VINCENT GRAY (Reviewer's comment ID #: 88-2227)]	Rejected, not the meaning of anthropogenic forcing
SPM-982	A	11:2 1	11:2 1	Insert after "Anthropogenic forcing" "which might not imply an influence of greenhouse gas emissions" [VINCENT GRAY (Reviewer's comment ID #: 88-2228)]	See SPM 981
SPM-983	A	11:2 1	11:2 2	The language is not precise with regard to the likelihood. Therefore the following wording is proposed: It is likely that anthropogenic forcing has increased the risk of heat waves. [Klaus Radunsky (Reviewer's comment ID #: 204-12)]	Rejected. Not consistent with language used in chapter.
SPM-984	A	11:2 1	11:2 2	Delete "may" as it is likely to very likely that the risk of heat waves is a result of anthropogenic forcing. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-141)]	Rejected. Not consistent with language used in chapter.
SPM-985	A	11:2 1	:22	The IPCC lexicon of likelihood has not been applied in this statement about possible attribution of the increase in heat waves to human-induced warming.	Rejected. Not consistent with language used in chapter.

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				Rather than saying human-induced warming “may” have increased the risk of heat waves, shouldn’t it say it is “likely” to have increased the risk of heat waves or something similar? The word “may” does not provide any information regarding likelihood. In addition, change “risk” to “frequency”. [Govt. of United States of America (Reviewer’s comment ID #: 2023-826)]	
SPM-986	A	11:2 4	11:2 5	The sentence is trivial and do not contribute to the attributing of climate change. Therefore replace this sentence with the information given in line 33 and 34: In bold letters: "It is likely that greenhouse gas forcing has been the dominant cause of the observed warming of the northern hemisphere over the last 50 years." [Govt. of Germany (Reviewer’s comment ID #: 2011-78)]	Rejected. Not appropriate to repeat the same language used above here.
SPM-987	A	11:2 4	11:2 5	Given that we have not just improved our understanding of palaeoclimates but we are also increasingly able to model past changes, this sentence could be strengthened to read: "Proxy climate data and palaeoclimate models have increased our confidence in understanding past and present influences on climate and our ability to reproduce historical changes." [Andy Reisinger (Reviewer’s comment ID #: 210-34)]	Rejected. Modelling is part of the understanding.
SPM-988	A	11:2 7	11:3 0	Questioning whether the strength of attribution of interdecadal variability over the last seven centuries is "very likely". Some reconsideration of this level of surety may be warranted. Any changes would obviously need to be consistent with Chapter 10. [Govt. of Australia (Reviewer’s comment ID #: 2001-33)]	Taken into account. The assessment by chapter 9 remains unchanged.
SPM-989	A	11:2 7	11:3 0	Can you say anything on the role of natural variability for the variations in climate over the past millenium? [Olivier Boucher (Reviewer’s comment ID #: 27-10)]	Taken into account. Text edited.
SPM-990	A	11:2 7	11:3 0	There is no basis to attribute interdecadal variability prior to the 20th century to any cause because the magnitude of the variability is poorly known. It is as likely as not that the earlier variability was caused by internal variability associated with ocean-atmosphere interactions, especially those that excite internal modes of variability of the ocean circulations. Suggest replace 'very likely' with 'as likely as not'. [William Kininmonth (Reviewer’s comment ID #: 128-102)]	Rejected. While magnitude of changes has been questioned, thee timing is what is important here and has links to eruptions identified in e.g. sulafate in ice cores. Attribution to past volcanic activity rests on a large number of studies.
SPM-991	A	11:2 7		Quantify “a large fraction” or merely change to “some”. Go back to the chapter and make consistent. [Govt. of United States of America (Reviewer’s comment ID #: 2023-827)]	Taken into account. Text has been edited.
SPM-992	A	11:2 8	11:2 8	you say changes for previous seven centuries very likely to be attributed to natural forcing - but what else would they be attributable to? Surely the point here is that the changes have been attributed to particular changes in the natural	Rejected. Internal variability could also have been a factor

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				forcings [Joanna House (Reviewer's comment ID #: 109-58)]	
SPM-993	A	11:2 9	11:2 9	Volcanic eruptions do not in themselves cause climate change and I would suggest "stratospheric volcanic aerosols" would be better [Keith Shine (Reviewer's comment ID #: 236-9)]	Explosive volcanic eruptions covers this point in a simpler manner.
SPM-994	A	11:2 9		"possible" solar irradiance variations - it has quite simply not been established with any degree of reliable statistical significance that solar variability has really played a role yet - the Hegerl et al 2003 grl paper is the most complete published assessment of solar using anything like real statistics and the results are very equivocal. before I forget any usage of the term late Maunder Minimum should be restricted from the record books - it is half of one realization of a solar oscillation - the statistical significance of such a perspective must be less than zero [Thomas Crowley (Reviewer's comment ID #: 51-8)]	Taken into account. Text has been edited.
SPM-995	A	11:2 9		Change "causing" to "which cause" [Govt. of United States of America (Reviewer's comment ID #: 2023-828)]	Text has been edited
SPM-996	A	11:3 2	11:3 5	"This evidence may be better placed near the attribution statement on SPM-10 or in the paleoclimatic section. Seems to be somewhat out of place here." [Govt. of Canada (Reviewer's comment ID #: 2004-73)]	Rejected. Does not belong with the paleo data section. Paleo attribution is being kept separate from the instrumental studies.
SPM-997	A	11:3 2	11:3 5	SPM Comment: Delete this paragraph because it is an assertion that cannot be true. The assertion requires accurate knowledge of "the observed warming of northern hemisphere over the last 50 years" but such knowledge does not exist. The surface temperature record begins ~1860. For the period 1860 to 2004, the most cited of these data sets have good coherence (which is not surprising because they were compiled from the same available source data), but their global trends (in degrees C/decade) and 2SD trend errors are GHCN: $0.076 \pm 0.010$ Jones et al.: $0.064 \pm 0.007$ GISS: $0.048 \pm 0.006$ The Jones trend is significantly different from the GISS trend ( $p < 0.05$ ), and the GHCN trend is very significantly different from the GISS trend ( $p < 0.01$ ). So, "coolest" and "warmest" years near the ends of the data sets are generated by trends that are known to be spurious. A change in the indicated temperature with time is only an indication of climate change when the change exceeds the intrinsic measurement errors (otherwise the change could be intrinsic measurement error). The intrinsic errors of GHCN,	Rejected, due to errors of fact, see chapter 3

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			<p>Jones et al. and GISS data sets are not known, but at least two of the data sets provide wrong results because they differ in annual change by more than double their stated 95% confidence limits in each of several years.</p> <p>Furthermore, the GHCN and Jones et al. data sets also have different trends for the warming period of the most recent 30 years. For the period 1976-2004, the temperature trends for the Jones et al., GISS, and GHCN surface temperature time-series are +0.215 degrees C/decade, +0.204 degrees C/decade and +0.274 degrees C/decade, respectively. And this also demonstrates that at least two of them are indicating spurious trends generated by the methods used to create the means.</p> <p>Data of this kind cannot be used to justify any assertion concerning probabilities of the causes of warming they suggest.</p> <p>Indeed, other recent data suggest that the trends suggested by these data sets for recent decades are very wrong. For the late 20th century warming period between 1976 and 2004, the rates of change indicated by these data sets are significantly higher than the rate of +0.059 degrees C/decade for the lower atmosphere measured by weather balloon radiosondes for the same period and the rate of +0.079 degrees C/decade and satellite MSUs for the period 1979 to 2004 (the satellite record begins in 1979).</p> <p>Furthermore, the compilers of these data sets admit their methods create spurious trends (ref. Vose et al., 2004). Their methods include integrating measurements into mean values for regions over the Earth’s surface called ‘grid boxes’. And the integrations cause trend problems for individual grid boxes. Vose et al. state that when the GHCN and Jones et al. trends are compared at the grid-box level then 9.3% of grid cells display “discrepant trends”. In other words, the integration of measurements into grid boxes causes 9.3% of grid boxes to have trends with opposite sign. (!)</p> <p>Simply,</p> <ul style="list-style-type: none"> <li>(a) temperature measurements cannot indicate climate change in the absence of knowledge of their accuracy and precision,</li> <li>(b) coherence between data sets does not indicate their accuracy or precision,</li> <li>(c) the accuracy and the precision are not known for mean global temperatures indicated by the GISS, GHCN and Jones et al. data sets, and</li> <li>(d) regional temperatures (e.g. for the northern hemisphere) indicated by the GISS, GHCN and Jones et al. data sets are known to be wrong (i.e. they have large unquantified errors).</li> </ul> <p>GISS, GHCN and Jones et al. data sets.</p> <p>So claims such as “year x was the warmest year” cannot be substantiated from</p>	

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				<p>the data sets. All years are the same temperature within the (unknown) measurement errors. So, no year can be said to be warmer than any other in the absence of knowledge of the inherent error of each indication of each year's temperature. This is true for all regions (e.g. for the northern hemisphere) and for the entire globe.</p> <p>Hence, it is invalid to attribute any causes – anthropogenic or otherwise – to “the observed warming of northern hemisphere over the last 50 years”</p> <p>[Richard Courtney (Reviewer's comment ID #: 49-25)]</p>	
SPM-998	A	11:3 2	11:3 5	<p>After the inclusion of the new chapeau (last comment) delete the text in line 32 to 35 completely.</p> <p>[Govt. of Germany (Reviewer's comment ID #: 2011-79)]</p>	Text has been clarified
SPM-999	A	11:3 2	11:3 4	<p>Rephrase: By considering the entire record of the past 700 years the dominant cause of the observed warming of the northern hemisphere over the last 50 years can be attributed to greenhouse gas forcing.</p> <p>[Stephen J. Hawkins (Reviewer's comment ID #: 102-16)]</p>	Text has been edited
SPM-1000	A	11:3 2	11:3 5	<p>The methods associated with paleoclimate reconstructions are not sufficiently precise, the available data are limited, and the differences between individual reconstructions are so large that it is not possible to have confidence in other than the broad envelope of Medieval Warm Period, Little Ice Age, Contemporary Warm Period. The claim from modelling studies, based on the paleoclimate reconstructions and best estimates of external forcing, that the warming of the past 50 years is unusual cannot be sustained. Suggest the paragraph be deleted.</p> <p>[William Kininmonth (Reviewer's comment ID #: 128-103)]</p>	Rejected. No literature cited and not consistent with the assessments of chapter 6 and 9. There is a large amount of evidence supporting the role of volcanoes, which are evident in these records.
SPM-1001	A	11:3 2	11:3 5	<p>I'm not sure how the statement here that it is "likely" that GHGs were the dominant cause of warming of the NH for the past 50 years, fits with the earlier chapeau statement (p10 114-15) that it is "very likely" that GHG were the dominant cause of global warming for the past 50 years. I guess you are basing this bullet purely on the basis of what 700-year palaeo model-runs have contributed to attribution rather than GCM runs for only the 20th century, but it still seems inconsistent with the stronger attribution statement. Please consider and clarify.</p> <p>[Andy Reisinger (Reviewer's comment ID #: 210-29)]</p>	Taken into account. Text has been edited.
SPM-1002	A	11:3 2	:35	<p>Since last sentence of the paragraph indicates that “insufficient data are available to make a similar southern hemisphere evaluation” it could be worth to point out that efforts with the aim of improving Southern Hemisphere data would be</p>	Text has been edited

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				needed in the future. [Govt. of Chile (Reviewer's comment ID #: 2005-12)]	
SPM-1003	A	11:3 2	:35	"Likely" is too weak, I think (or hope). It should be "very likely". With "likely" you damp considerably the conclusions already drawn, e.g. SPM-10, line 14-15. [Govt. of Norway (Reviewer's comment ID #: 2018-30)]	Text has been edited.
SPM-1004	A	11:3 3	11:3 3	We should say "very ikely" instead of "likely" to be consistent with the whole report, and wth page 10, line 14. [Govt. of France (Reviewer's comment ID #: 2010-109)]	Taken into account. Text has been edited. SEE COMMENT SPM-1003
SPM-1005	A	11:3 3	11:3 3	Replace "greenhouse gas forcing" with "increases in human population, prosperity, energy usage and land-use changes". McKittrick and Michaels 2004 Climate Change Vol 26 pages 159-173 have shown this to be so for the surface temperature record [VINCENT GRAY (Reviewer's comment ID #: 88-2229)]	Rejected. Inconsistent with large body of literature assessed in ch 3
SPM-1006	A	11:3 3		"and a fraction of the mid-20th c. warming" - cf hegerl et al 2003 grl. If hegerl et al jclim paper gets accepted and can be quoted then the attribution of about 1/3 of the mid-20th c. warming can be added [Thomas Crowley (Reviewer's comment ID #: 51-9)]	Rejected. Too much detail for SPM. Inappropriate to be this specific here based on a single study.
SPM-1007	A	11:3 3		Another forcing qualifier is now introduced: "greenhouse gas forcing". Two similar terms have been used prior (e.g. "anthropogenic forcing"). Limit the use of terms or be sure to clearly define the differences. Greenhouse gas forcing is the correct term but need to clarify in this particular instance by "anthropogenic forcing from greenhouse gases". [Govt. of United States of America (Reviewer's comment ID #: 2023-829)]	Taken into account. Text has been edited.
SPM-1008	A	11:3 4	11:3 5	As a note to the editors, noun phrases are often too long. I would suggest saying "evaluation for the Southern Hemisphere." And, just to note, the text needs to consistently capitalize Northern Hemisphere, Southern Hemisphere, etc. [Michael MacCracken (Reviewer's comment ID #: 152-41)]	Accepted
SPM-1009	A	11:3 4	11:3 5	This sentence is misleading, rephrase. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-142)]	Text has been edited
SPM-1010	A	11:3 4	:35	Capitalize proper nouns Northern Hemisphere and Southern Hemisphere [Govt. of United States of America (Reviewer's comment ID #: 2023-830)]	SEE COMMENT SPM-1008
SPM-1011	A	11:3 6		Include the robust finding from [6.7] as it contributes to the attribution: "There is no evidence for a natural interglacial climate cycle that could explain recent global warming, or that the current warming will be mitigated by a natural cooling trend." [Govt. of Germany (Reviewer's comment ID #: 2011-80)]	Rejected. Attribution statement takes this into account. This material has been put in the TS.

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SPM - 1012	A	11:3 8	11:3 8	SECTION Projections: Add figure Figure TS-33. Projected global mean temperature change for the year 2100 for three of the six illustrative [William Hare (Reviewer’s comment ID #: 99-130)]	Rejected: Values in text. Other figures more important. Length constraints
SPM- 1013	A	11:3 8	15:	The section "Projections of Future Changes of Climate" is arguably the most crucial in the whole of the SPM, but it lacks any compelling Figures to illustrate the projections. Surely the SPM should include (as a minimum) Figures TS-30 and TS-33, and appropriate text from the TS to go with them ??? [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-143)]	Rejected: Values in text. Other figures more important. Length constraints Figure 30 does not include all SRES marker scenarios.
SPM- 1014	A	11:3 8		Section: Projections: Three points 1) This section should also include mitigation scenarios. 2) The full range of SLR projections should be shown and 3) the use of the term "commitment" needs to be introduced so that confusion is minimized. [William Hare (Reviewer’s comment ID #: 99-111)]	Ch 10 WG1 does not assess scenario science (WG3 does); see chapter 10 for discussion of approach. Word “commitment” will be removed to avoid confusion.
SPM- 1015	A	11:3 8		Section "Projections of future changes in climate". The idea of climate "commitment" is very important, but we need to make sure that policymakers don't confuse this "commitment" with the same word that is used under the UNFCCC, where it refers to commitments by countries to reduce GHG emissions. It's simply a problem of the same word appearing in two completely different contexts, and where the sensitivity of policymakers to mitigation commitments, especially where English is not their first language, could cause problems. Unless the authors really want to "hammer home" the specific word "commitment" to introduce it to the scientific vocabulary of the policy community, it might on some occasions be more effective to talk about the same principle but without necessarily using the word "commitment" in each instance. [Andy Reisinger (Reviewer’s comment ID #: 210-35)]	Word “commitment” will be removed to avoid confusion.
SPM- 1016	A	11:3 8		Section "Projections in future changes in climate". This section is currently organised into near-term projections, 21st century projections, and "context for policy options linked to stabilisation". I'm not sure this is the best way to present this very rich information. The authors may wish to consider alternatively to arrange this section into near-term outcomes as at present, into scientific projections of climate change to 2100 AND BEYOND, and into a third section that focuses more generally on uncertainties, irreversibility, commitment, limits to current knowledge, and implied risk over all those time scales. I think this would better match how policymakers may think about these issues ("what's likely to happen", and "how sure are you about this"). I don't think it's helpful to make the an explicit link to policy options only in the context of stabilisation issues - Fig SPM-5 shows that policy options can influence climate change also during the 21st century, regardless of where we ultimately stabilise long-term.	Rejected. The order used provides a better map to the underlying chapters.

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			[Andy Reisinger (Reviewer's comment ID #: 210-41)]	
SPM-1017	A	11:38	Section "Projections of future changes in climate". Many policymakers don't understand the difference between transient climate responses and equilibrium warming, the different time scales involved, and the different types of uncertainties. It would be extremely helpful to have a lay-person's box (text plus figure) of about half a page that explains these differences and shows the relevant time scales and uncertainties of transient and equilibrium warming, meaning of "climate sensitivity", why the pdf of climate outcomes spreads over time, role of CO <sub>2</sub> -equivalent stabilisation levels etc. A lot of policy interest has been shown in questions such as "what is the risk of exceeding a certain temperature target for a given CO <sub>2</sub> -equivalent concentration level?" Such a text box and figure could present a valuable framework for presenting uncertainties and projections over different time scales in answer to such questions, not just in WG-I but also including its potential use in the Synthesis Report. [Andy Reisinger (Reviewer's comment ID #: 210-53)]	Rejected. Length issues. Issue discussed in chapter 10 (figure 10.7.5 in SOD).
SPM-1018	A	11:40	11:52 Why have no mitigation scenarios been assessed in WG I? From the point of view of policymakers, this is a serious gap, in particular as plausible low mitigation scenarios would give a better indication to policymakers of what level of warming and related climate impacts can be avoided. It is strongly suggested adding information in this regard in the WG I report and in the SPM. [Govt. of Germany (Reviewer's comment ID #: 2011-233)]	WG1 scientists do not have the expertise to assess new scenarios, which is the purview of WG3 as is noted.
SPM-1019	A	11:40	11:40 "fully coupled" means nothing to most policy makers, delete, probably too technical for here. [Joanna House (Reviewer's comment ID #: 109-59)]	Accepted – See SPM-1021
SPM-1020	A	11:40	:52 This is too much detail and not really useful in the SPM, and provides opportunities for internal inconsistencies. Emphasize briefly what is new since the TAR (for this and all other preambles). [Govt. of United States of America (Reviewer's comment ID #: 2023-831)]	As in the TAR, italic sections are used for explanation and background. What is new will be in the headline statements.
0-52	A	0:0	SPM-13 & figure TS-32: I have some doubt about the validity of probabilistic analysis. Is there any guarantee that the existing models cover whole range of future projections? Can we treat equally all the projections of different models? Without clear-cut explanation of reasons for the different values of projections made by different models, I cannot understand whether this type of probabilistic treatment is right or not. [Shigeki KOBAYASHI (Reviewer's comment ID #: 134-4)]	Partly accepted – "probabilistic" has been removed. The basis for combining different models is covered in chapters 8 and 10 and is too detailed to include in the SPM.
0-53	A	0:0	SPM-13: Statement on the tropical cyclones. This is very interesting, because the phenomenon is existing so this is a good example for people to believe that global	Rejected. No specific suggestion offered. Text in chapter discusses level of understanding.

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				warming is real. However, how certain is this? [Shigeki KOBAYASHI (Reviewer's comment ID #: 134-5)]	
SPM-1021	A	11:4 0		Delete "fully". Also consider deleting "coupled". The first is suspect, and the second does not really convey anything to a policymaker. [Govt. of United States of America (Reviewer's comment ID #: 2023-832)]	SEE COMMENT SPM-1019
SPM-1022	A	11:4 2	11:4 3	Methods of probabilistic predictions, likelihood estimation and observational constraints are still in their infancy. The second two sentences in this section might read "The use of different types of ensembles of models and the formal application of observational constraints has, for the first time, allowed the likelihood of future warming to be quantified in the form of probabilities. This represents a major advance since the TAR, recognising that diverse methods have been applied and the predictions need still to be refined and extended to other variables." [Matthew Collins (Reviewer's comment ID #: 44-39)]	Partly accepted "Probabilistic" removed.
SPM-1023	A	11:4 2	11:4 2	SPM Comment: Replace "quantitative" with "qualitative". The inherent errors of the simulations are not known so the outputs of the simulations cannot be "quantitative" (a result is not quantitative merely because it is presented as a number). [Richard Courtney (Reviewer's comment ID #: 49-26)]	Rejected, see chapters 8 and 10.
SPM-1024	A	11:4 2	11:4 4	Model intercomparisons may not represent the full range of structural uncertainty in climate response. This sentence gives the false conclusion that a full, non-conditional estimate of uncertainty is constructed by running many models many times. Suggest adding "partial" before "basis". [Haroon Kheshgi (Reviewer's comment ID #: 125-17)]	Reject comment. The text makes it clear this provides a basis for estimates – not that it is a full description of uncertainties. The text also explicitly mentions model shortcomings, and "probabilistic" has now been removed which should partly deal with the concern.
SPM-1025	A	11:4 2	11:4 3	On line 43, I would delete "probabilistic" as repetitive given that likelihoods are being estimated--or are model results being weighted in some probabilistic fashion--and if this is the case, it should be mentioned more directly? Also, this is a major advance only to the extent that the models are valid. [Michael MacCracken (Reviewer's comment ID #: 152-42)]	Accepted. Probabilistic deleted. Rest of comment rejected.
SPM-1026	A	11:4 2	11:4 5	Change "The large number...shortcomings" into "The large number of simulations provides a basis for estimating likelihoods of expected global warming. Observations have been used to identify shortcomings of models. Because these shortcomings were rather diverse, there was no consensus on a quality ranking of models. For this reason, projections of global change patterns reported below, are based on multi-model ensembles in which each model has an equal weight."	Taken into account. Text reworded but unable to cover all points due to length constraint

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				[Govt. of Netherlands (Reviewer’s comment ID #: 2016-18)]	
SPM-1027	A	11:4 2	11:4 4	<p>There are serious conceptual problems with the approach that is being taken here to quantify the uncertainties in the projections of climate change. The problems are described briefly in Chapter 10, on page 73, in the paragraph from lines 32 to 44. There it is stated that the AOGCMs being used for the projections "are an 'ensemble of opportunity' not designed to sample modelling uncertainties systematically or randomly". The approach being used assumes that different models are independent, that they are all equally likely, and that they cover the full range of uncertainty. None of these assumptions are valid. For example, models have not been developed independently -- the modelers all go to the same conferences and read the same literature. Even worse, different models developed at the same centers are being treated as independent, when they obviously have a common heritage. That they do not cover the full range of uncertainty is obvious from the fact that the current SPM is now saying that there is a 10 to 34 % chance that the climate sensitivity lies outside the 2.0 to 4.5 C range, yet none of the 23 AOGCMs being used in the projections have climate sensitivities that lie outside this range. Indeed in the same paragraph in Chapter 10 referred to above it states: "...the spread of model(s) is unable to span the full possible range of uncertainty...". Another potential problem is that the models could have systematic errors because of common errors in parameterizations of poorly understood subgrid scale processes. In fact Forest et al., (2006) found such a systematic error in all the AOGCMs they analyzed. That analysis did not include most of the 23 AOGCMs being used in the projections, but the point is that such tests have not been applied to these models. This sentence in the SPM (lines 42 to 44 on page 11) gives a very misleading view of the quality of the projections and to call it "a major advance over the TAR" grossly misrepresents their quality. A possible replacement for this sentence would be the following two sentences: "These AOGCMs are an 'ensemble of opportunity', and are therefore not designed to sample modelling uncertainties systematically or randomly. However the large number of simulations does provide a more comprehensive basis for estimating likelihoods of expected warming than was available in the TAR."</p> <p>[Peter Stone (Reviewer’s comment ID #: 257-4)]</p>	<p>Rejected. Confidence levels discussed in bullets and in supporting chapter text. Issue too detailed. “probablistic” deleted from text.</p>
SPM-1028	A	11:4 3	11:4 3	<p>SPM Comment: Delete “expected”. Scientists do not expect a result before they get it (but I suppose climate modelers may).</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-27)]</p>	<p>Rejected. Expected from known physical principles is meant here, and that is standard practice in all science.</p>
SPM-1029	A	11:4 3	11:4 4	<p>SPM Comment: Delete “, representing a major advance over the TAR”.</p> <p>The ability to conduct statistical analyses of many model simulations cannot be</p>	<p>Rejected. See chapter 10 for details of how probabilistic studies improve information versus single</p>

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				<p>claimed to be an “advance” (except in the generation of financial incomes for the modelers). Demonstration that the models have improved accuracy would be an advance in the modeling, and demonstration that the models accurately simulate reality would be an advance in climate science, but merely having more simulations (that may all be wrong) cannot be a “major advance”. Indeed, this paragraph concludes with a sentence that the “plausibility or likelihood” of simulations is not considered.</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-28)]</p>	simulations that do not sample internal variability.
SPM-1030	A	11:4 3	11:4 3	<p>Please replace "probabilistic" by "statistical". In any case, this affirmation is questionable, as nothing proves that the models encompass the mechanisms of the real world. We would prefer a more neutral and strict presentation.</p> <p>[Govt. of France (Reviewer’s comment ID #: 2010-110)]</p>	Taken into account. Wording changed.
SPM-1031	A	11:4 4	0:44	<p>Insert after "TAR" "but since they cannot simulate natural climate changes, or those caused by humans which do not involve greenhouse gas emissions, the models are of little use in future forecasting"</p> <p>[VINCENT GRAY (Reviewer’s comment ID #: 88-2230)]</p>	Rejected. See chapter 8 for evaluation of these issues.
SPM-1032	A	11:4 4	11:4 4	<p>Phrase 'important new constraints' is not meaningful to policy maker.</p> <p>[Govt. of Australia (Reviewer’s comment ID #: 2001-34)]</p>	Accepted – wording changed
SPM-1033	A	11:4 4	11:4 5	<p>you could simply say instead that observations have been used to improve models</p> <p>[Joanna House (Reviewer’s comment ID #: 109-60)]</p>	Accepted
SPM-1034	A	11:4 5	11:4 8	<p>Insert after "doubling", " (requiring over 200 years at the present rate)"</p> <p>[VINCENT GRAY (Reviewer’s comment ID #: 88-2231)]</p>	Rejected, incorrect and not relevant to the point being made
SPM-1035	A	11:4 5		<p>The phrase “possible future conditions” is problematic. The word “projected” is generally used. And what is meant by “condition”? Do the authors mean atmospheric composition?</p> <p>[Govt. of United States of America (Reviewer’s comment ID #: 2023-833)]</p>	partly accepted – word “conditions” removed
SPM-1036	A	11:4 6	11:4 6	<p>what are "idealised emissions"</p> <p>[Joanna House (Reviewer’s comment ID #: 109-61)]</p>	“idealised” meant to qualify “assumptions” –text improved
SPM-1037	A	11:4 7	11:4 7	<p>For greater clarity it is proposed to insert "emissions of" before greenhouse gases and aerosols ...</p> <p>[Govt. of Austria (Reviewer’s comment ID #: 2002-13)]</p>	Rejected, incorrect.
SPM-1038	A	11:4 7	11:4 7	<p>"Suggest ""experiments with concentrations of greenhouse gases"""</p> <p>[Govt. of Canada (Reviewer’s comment ID #: 2004-74)]</p>	Agree but SEE COMMENT SPM-1041

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SPM-1039	A	11:4 7	11:4 7	Suggest to delete the description about "commitment experiments" because it is already year 2006 now, while "commitment" experiments are held constant at year 2000 levels. [Govt. of China (Reviewer's comment ID #: 2006-20)]	Accepted.
SPM-1040	A	11:4 7	11:4 7	avoid the term "commitment" in the Spm, as without careful explanation it is highly misleading for policymakers. Refer to "constant forcing at present levels" experiments. [Govt. of Germany (Reviewer's comment ID #: 2011-232)]	Agreed. Commitment will not be used.
SPM-1041	A	11:4 7	11:4 7	replace "greenhouse gases and aerosols" with "greenhouse gas and aerosol concentration" to clarify that what is held constant is concentrations (not emissions). [Govt. of Germany (Reviewer's comment ID #: 2011-234)]	accepted
SPM-1042	A	11:4 7	11:5 0	I believe the Summary for Policymakers misrepresents the purpose of the experiments in which atmospheric greenhouse gases concentrations are instantaneously held constant at year 2100 levels as "stabilisation experiments", which would probably imply to the lay reader that atmospheric concentrations, climate responses, and ecological responses modelled in these scenarios after 2100 reflect the expected outcomes from the various SRES scenarios (A1B, B1, and B2 are presented throughout the report as examples) in the years beyond 2100. The text of the relevant chapter 10 notes, however, that this instantaneous stabilization of GG concentrations at 2100 levels would effectively require an equally instantaneous reduction of GG EMISSIONS to ZERO -- an impossible outcome barring a nuclear or astronomical annihilation of all human life on Earth. In fact, the Chapter 10 text notes that such an instantaneous stabilization is applied as a mathematical means of estimated the magnitude and timing of future commitments to additional climate changes from all cumulative emissions that occurred in the 21st century (and as well in earlier centuries, depending upon the specific initialization and spin-up conditions applied in the model simulations.) The ACTUAL greenhouse gas concentrations and resulting climate responses will be MUCH, MUCH LARGER from simulations that truly follow the A1B, A2 and B2 scenarios to their logical endpoints. These endpoints are not 2100, but more likely to be 2300 or 2400. A major shortcoming of existing AOGCM runs is that NONE of them have been carried out to date, AFAIK, to the logical endpoints of SRES scenarios. The implications of this shortcoming have numerous effects through Chapter 10 and the Technical Summary and SPM, some of which I will elaborate on in later comments. [Chuck Hakkarinen (Reviewer's comment ID #: 96-2)]	Rejected. The reviewer is wrong on a number of points. However, the text has been revised to use more careful language to make clear what is referred to. Stabilisation experiments are no longer mentioned.

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SPM-1043	A	11:4 7	11:4 7	Delete the word "commitment" as this has ambiguous meanings (At least four (constant forcing, constant emissions, zero emissions and plausible mitigation scenarios) (Hare and Meinshausen 2005 accepted) and three are used in Chapter 10. [William Hare (Reviewer's comment ID #: 99-110)]	Agreed. Commitment will not be used.
SPM-1044	A	11:4 7	11:4 7	For greater clarity it is proposed to insert "emissions of" before greenhouse gases and aerosols ... [Klaus Radunsky (Reviewer's comment ID #: 204-13)]	Rejected, incorrect. Concentrations are held constant not emissions.
SPM-1045	A	11:4 7		The term "commitment" is used for the first time here in the SPM. We think it deserves a bit more explanation since it is mentioned several times in the following. In our view, this information could be included in a small box. Another possibility could be to make a reference to Box TS.5.2 where the term is explained. [Govt. of Norway (Reviewer's comment ID #: 2018-31)]	Taken into account. "Commitment" has been deleted .
SPM-1046	A	11:4 7		The use of the term "commitment" is a serious mistake in the SPM. The idea that freezing greenhouse gases at today's levels is a "commitment" to future warming is a decades-old concept, originally (and still should be) called "unrealized warming" (Hansen). More importantly, the term "committed warming" has a standard dictionary definition, and should not be used for a specific mind-experiment that will not happen. Governments are committed to much more warming, since the increase in CO2 cannot be stopped cold—as shown by the IPCC TAR stabilization scenarios, which have CO2 rise before a reduction in fossil carbon dependency and an eventual leveling off at values well above those today. These are the true "commitment" studies. The SPM authors should not misuse the term to describe a purely gedanken experiment. [Govt. of United States of America (Reviewer's comment ID #: 2023-834)]	See SPM-1045.
SPM-1047	A	11:4 8	11:4 8	There are two stabilization scenarios after 2100, not three. [Reto Knutti (Reviewer's comment ID #: 133-44)]	accepted
SPM-1048	A	11:4 8		There is reference to "SRES marker scenarios". Provide a footnote that steers readers to this IPCC Special Report (i.e., a complete cite). [Govt. of United States of America (Reviewer's comment ID #: 2023-835)]	Accepted. Footnote and box explaining SRES added
SPM-1049	A	11:4 9	11:4 9	For greater clarity it is proposed to insert "concentrations of" before "greenhouse gases and aerosols .. [Govt. of Austria (Reviewer's comment ID #: 2002-14)]	accepted
SPM-1050	A	11:4 9	11:4 9	"Suggest ""experiments with concentrations of greenhouse gases"" [Govt. of Canada (Reviewer's comment ID #: 2004-75)]	SEE COMMENT SPM-1049

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SPM-1051	A	11:4 9	11:4 9	It would be nice to see assessment results of stabilization scenarios also included in the Figures.  [Govt. of Canada (Reviewer's comment ID #: 2004-76)]	Agreed but these were not available. Point will be covered in Preface.
SPM-1052	A	11:4 9	11:4 9	For greater clarity it is proposed to insert "concentrations of" before "greenhouse gases and aerosols .. [Klaus Radunsky (Reviewer's comment ID #: 204-14)]	SEE COMMENT SPM-1049
SPM-1053	A	11:4 9	:50	Does this really provide new info on the physical aspects of climate change that we did not know before? [Govt. of United States of America (Reviewer's comment ID #: 2023-836)]	Yes. Time scales and commitment are now able to be presented in much more detail than in the TAR
SPM-1054	A	11:5 0	11:5 2	The last sentence is welcome. However, it is proposed to be more specific with regard to which new research on emission scenarios has been assessed by WG III. [Govt. of Austria (Reviewer's comment ID #: 2002-15)]	That would require taking material from the WG3 report before it was finalized in breach of IPCC rules.
SPM-1055	A	11:5 0	11:5 2	The last sentence is welcome. However, it is proposed to be more specific with regard to which new research on emission scenarios has been assessed by WG III. [Klaus Radunsky (Reviewer's comment ID #: 204-15)]	SEE COMMENT SPM-1054
SPM-1056	A	11:5 0	11:5 2	I would suggest replacing the phrase "does not consider the plausibility" with "does not make any judgements on the plausibility" - just to make sure the reader understands that it's not like WG-I didn't want to get into the question of plausibility and probabilities, but that it requires judgements that are simply outside the domain of WG-I. [Andy Reisinger (Reviewer's comment ID #: 210-44)]	As text makes clear this is an assessment by WG1 we think the point is already covered
SPM-1057	A	11:5 1	11:5 1	Insert afer "scenario" "even when some of them are patently ridiculous" [VINCENT GRAY (Reviewer's comment ID #: 88-2232)]	Rejected, no basis provided
SPM-1058	A	11:5 1		"plausibility or likelihood" pick one or the other, not both. [Govt. of United States of America (Reviewer's comment ID #: 2023-837)]	Rejected – different plausible scenarios may have different likelihoods in the eyes of many
SPM-1059	A	11:5 2	11:5 2	Add at end "which are not circulated for approval to climate scientists" [VINCENT GRAY (Reviewer's comment ID #: 88-2233)]	Rejected, the WG3 report is open to all experts to review.
SPM-1060	A	11:5 2	11:5 2	Delete "new research on" as some of the WGIII assessment is not on what might be termed "new" eg the post SRES scenarios are not really new... [William Hare (Reviewer's comment ID #: 99-112)]	Accepted.
SPM-1061	A	12:0		figure SPM-5: Again a very impressive figure. However, it might be better to use in all three panels the same 20 years periods and avoid mixing of 10 and 20 year periods.	Accepted. The same decadal averages will be used throughout Figure.

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				[Govt. of Austria (Reviewer's comment ID #: 2002-17)]	
0-100	A	12:0		SPM: 3 bullet points: bullet points 1 and 3 do not seem distinct and could be combined. Alternatively bullet 1 could be removed since it suggests that if we stabilise radiative forcing agents it will have no effect on temperature rises (in fact the temperature rises will be smaller than if concentrations were allowed to rise) [Richard Allan (Reviewer's comment ID #: 3-62)]	Accepted
SPM-1062	A	12:0		Figure SPM-5: Good powerful picture. Could probably do with some more explanation for the SPM, particularly the last sentence. [Joanna House (Reviewer's comment ID #: 109-66)]	No explicit suggestion given and other reviewers seem happy – no change made.
SPM-1063	A	12:0		figure SPM-5: Again a very impressive figure. However, it might be better to use in all three panels the same 20 years periods and avoid mixing of 10 and 20 year periods. [Klaus Radunsky (Reviewer's comment ID #: 204-17)]	SEE COMMENT SPM-1061
SPM-1064	A	12:1	12:2 5	There is loose language around the different timeframes here compared with those clearly defined in Chapter 10. What does near-term mean? Out to 2030 or the next several decades? [Govt. of Australia (Reviewer's comment ID #: 2001-35)]	Reject. Time frame identified in following bullets.
SPM-1065	A	12:1	12:2	SPM Comment: Delete "improved climate models". The body of the report and its summaries contain no information that indicates there have been improvements to climate models. The report does say that more complex models have been constructed but the assumption that additional complexity is an improvement has not been demonstrated. And this assumption is very, very unlikely to be correct because additional complexity does not remove parametrisations of poorly understood climate effects (e.g. cloud formation processes). Indeed, additional complexity may reduce the accuracy of a model because it may exacerbate the effects of an erroneous parametrisation.  [Richard Courtney (Reviewer's comment ID #: 49-29)]	Rejected, see detailed discussion in ch 8 showing many areas of improvement
SPM-1066	A	12:1	12:3	provide more substantive information in this highlightes /bold letter sentence [Govt. of Germany (Reviewer's comment ID #: 2011-237)]	Accepted
SPM-1067	A	12:1	12:1 9	add information on expected warming for mitigation scenarios, in particular very low stabilisation scenarios. [Govt. of Germany (Reviewer's comment ID #: 2011-238)]	WG1 has no basis to evaluate which scenarios are appropriate since it is outside our expertise.
SPM-1068	A	12:1	12:1	Insert after "confidence" "" :based entirely on self-assessment" [VINCENT GRAY (Reviewer's comment ID #: 88-2235)]	Rejected. See chapters for extensive literature by many scientists.
SPM-	A	12:1	12:3	This seems to be the most important message of the section?	See SPM-1066

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1069				[Govt. of United Kingdom (Reviewer's comment ID #: 2022-144)]	
SPM-1070	A	12:1	:3	It should be defined what is meant with "near-term climate changes". I think you mean "next several decades". [Govt. of Norway (Reviewer's comment ID #: 2018-32)]	Reject. Details on time frame in bullets.
SPM-1071	A	12:1		The improvement also involves the ability to marry ensembles. That is as important as model improvements. Also define "near-term". [Govt. of United States of America (Reviewer's comment ID #: 2023-838)]	Reject. Ensembles are minor importance here. Time frame defined in bullets.
SPM-1072	A	12:2	12:2	replace "better understanding of commitments to further warming" by "better understanding of mid- and long-term projections" or similar wording, to avoid the word "commitment" which is highly misleading, see comment number 232 (on page 11 line 47). [Govt. of Germany (Reviewer's comment ID #: 2011-236)]	Partly accepted – the term "commitment" has been removed to avoid confusion. But this bullet remains about near term warming in contrast to the next bullet which deals with the longer term.
SPM-1073	A	12:3	12:3	Add at end "However, no such projection has ever been shown to agree with a future change" [VINCENT GRAY (Reviewer's comment ID #: 88-2234)]	Rejected. See chapter 1 among many other examples.
SPM-1074	A	12:5	11:6	This point seems to be redundant to that one lines 14-16 and hence I suggest deletion of it here [William Hare (Reviewer's comment ID #: 99-113)]	Accepted
SPM-1075	A	12:5	12:6	Suggest deletion of this dot point as it is covered better in the third dot point on this page [Govt. of Australia (Reviewer's comment ID #: 2001-36)]	Accepted
SPM-1076	A	12:5	12:6	It is proposed to move these two lines into the text written in bold letters in lines 1 to 3 on the same page. [Govt. of Austria (Reviewer's comment ID #: 2002-16)]	Accepted
SPM-1077	A	12:5	12:7	"An explanation of why average temperatures would still increase if the concentrations of all radiative forcing agents were to be stabilized would be useful. Include a sentence making the point that the climate system takes time to respond to changes in external forcings, so that stabilization of atmospheric concs of GHGs and aerosols does not lead to stabilization of temp in the near term."  [Govt. of Canada (Reviewer's comment ID #: 2004-77)]	Taken into account. Wording added.
SPM-1078	A	12:5	12:6	In order to give the full scientific background please add: "... would still increase somewhat. The lower the stabilization level of greenhouse gas concentration the smaller the temperature change would be." [Govt. of Germany (Reviewer's comment ID #: 2011-81)]	Stabilization is dealt with later.

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SPM-1079	A	12:5	12:6	Some kind of quantification of magnitude and timescales would be useful here. OK the globally averaged temperatures would increase, by 0.0001C or 10C, for 10 minutes or 1000 years. Without the quantification the statement is a bit meaningless. At the moment you have to wait until the third bullet for some quantification, but even this only covers the next several decades. At the very least this should be moved to be the second bullet, but I would combine into a single bullet. [David Griggs (Reviewer's comment ID #: 90-4)]	Bullet deleted and point dealt with in more detail below
SPM-1080	A	12:5	12:6	This is a fact beyond model results surely? it is without doubt due to inertia in the system, in fact this would be a good chance to talk about inertia in the system (see TAR synthesis report figure on this). Suggest bullet should start "If the concentrations of all ..... would still increase due to inertia in the system - time taken for the system to respond including feedbacks. [Joanna House (Reviewer's comment ID #: 109-62)]	Taken into account. Wording added
SPM-1081	A	12:5	12:6	combine this bullet with the third bullet and have the new combined bullet after what is currently the second bullet [Joanna House (Reviewer's comment ID #: 109-63)]	Accepted. Deleted first bullet.
SPM-1082	A	12:5	12:19	The first and the third bullet point make the same point and need to be combined. [Rolf Müller (Reviewer's comment ID #: 181-8)]	Accepted
SPM-1083	A	12:5	12:6	Move lines 5-6 to line 14. [Govt. of Netherlands (Reviewer's comment ID #: 2016-19)]	Accepted
SPM-1084	A	12:5	12:6	It is proposed to move these two lines into the text written in bold letters in lines 1 to 3 on the same page. [Klaus Radunsky (Reviewer's comment ID #: 204-16)]	Accepted
SPM-1085	A	12:5	12:6	I believe the phrase "the concentrations of" can be deleted, since it doesn't add anything - and by doing so, you also capture solar radiation and regional aerosols as a forcing agents. [Andy Reisinger (Reviewer's comment ID #: 210-36)]	Bullet deleted
SPM-1086	A	12:5	12:6	It would be helpful to insert "at current levels" in line 6, and also add a statement of the time period over which temperatures would increase, i.e. "for several decades to centuries". [Andy Reisinger (Reviewer's comment ID #: 210-37)]	Bullet deleted
SPM-1087	A	12:5	12:6	Is this bullet point needed? The one in lines 14-19 makes the same point. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-145)]	Accepted
SPM-1088	A	12:5	12:6	A time scale for stabilization should be given. An increase in global average temperatures after stabilization depends on the time scale considered. [Guus Velders (Reviewer's comment ID #: 276-3)]	Bullet deleted and dealt with in more detail below now. Stabilisation though is dealt with separately

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SPM-1089	A	12:5		Psychologically this is a devastating message: " we can't do anything anyway". Please amend to: "...would still increase somewhat, but much less than if the concentrations continue to increase." [Stefan Rahmstorf (Reviewer's comment ID #: 206-35)]	Taken into account. Wording changed to note values are for no policy intervention
SPM-1090	A	12:5		The question is "stabilized WHEN and at WHAT levels"? CO2 cannot be stabilized at current levels. This is an obvious conclusion and should be dropped. [Govt. of United States of America (Reviewer's comment ID #: 2023-839)]	For clarity the word "stabilization" will not be used.
SPM-1091	A	12:6	12:6	Please replace "increase" by "continue to increase during generations". [Govt. of France (Reviewer's comment ID #: 2010-111)]	Bullet deleted
SPM-1092	A	12:8	12:1 2	Sorry for playing the devil's advocate here. But I would assume that also at the time of the FAR and SAR, WGI did not consider the plausibility or likelihood of the emissions scenarios used at that time. The argument that the realisation of past projections of global warming increases our confidence is therefore in apparent contradiction with SPM-11 lines 50-52. This contradiction can simply be circumvented by saying that, irrespective of the plausibility of the old emissions scenarios, the CO2 concentration has followed the estimated pathway over the last one or two decades because of the inertia in the carbon cycle, while aerosol forcing has probably stabilised. [Olivier Boucher (Reviewer's comment ID #: 27-11)]	Noted but tracking of GHG concentrations is only part of the story – inertia in the climate system is the other part and we do not have space to explain all that – no change made.
SPM-1093	A	12:8	12:1 2	SPM Comment: Delete the final two sentences of this paragraph; i.e. everything from "This can now be compared ..." to "... those earlier assessments. [1.2, 3.2]" because they cannot be true. Confidence in such short term projections could only be gained if there were any confidence in the "observed values of about 0.2 degrees C per decade". Also, this warming cannot be due to "commitments" or anything else if it did not exist. The surface temperature record begins ~1860. For the period 1860 to 2004, the most cited of these data sets have good coherence (which is not surprising because they were compiled from the same available source data), but their global trends (in degrees C/decade) and 2SD trend errors are GHCN: $0.076 \pm 0.010$ Jones et al.: $0.064 \pm 0.007$ GISS: $0.048 \pm 0.006$ The Jones trend is significantly different from the GISS trend ( $p < 0.05$ ), and the GHCN trend is very significantly different from the GISS trend ( $p < 0.01$ ). So, "coolest" and "warmest" years near the ends of the data sets are generated by trends that are known to be spurious. A change in the indicated temperature with time is only an indication of climate	Rejected due to errors of fact, see chapter 3

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				<p>change when the change exceeds the intrinsic measurement errors (otherwise the change could be intrinsic measurement error). The intrinsic errors of GHCN, Jones et al. and GISS data sets are not known, but at least two of the data sets provide wrong results because they differ in annual change by more than double their stated 95% confidence limits in each of several years.</p> <p>Furthermore, the GHCN and Jones et al. data sets also have different trends for the warming period of the most recent 30 years. For the period 1976-2004, the temperature trends for the Jones et al., GISS, and GHCN surface temperature time-series are +0.215 degrees C/decade, +0.204 degrees C/decade and +0.274 degrees C/decade, respectively. And this also demonstrates that at least two of them are indicating spurious trends generated by the methods used to create the means.</p> <p>Data of this kind cannot be used to justify any assertion concerning magnitudes of warming they suggest.</p> <p>Indeed, other recent data suggest that the trends suggested by these data sets for recent decades are very wrong. For the late 20th century warming period between 1976 and 2004, the rates of change indicated by these data sets are significantly higher than the rate of +0.059 degrees C/decade for the lower atmosphere measured by weather balloon radiosondes for the same period and the rate of +0.079 degrees C/decade and satellite MSUs for the period 1979 to 2004 (the satellite record begins in 1979).</p> <p>Furthermore, the compilers of these data sets admit their methods create spurious trends (ref. Vose et al., 2004). Their methods include integrating measurements into mean values for regions over the Earth’s surface called ‘grid boxes’. And the integrations cause trend problems for individual grid boxes. Vose et al. state that when the GHCN and Jones et al. trends are compared at the grid-box level then 9.3% of grid cells display “discrepant trends”. In other words, the integration of measurements into grid boxes causes 9.3% of grid boxes to have trends with opposite sign. (!)</p> <p>Hence, assertion that there was warming of about 0.2 degrees C per decade from 1990-2005 cannot be substantiated from available data. Indeed, the actual warming is not known but – on the basis of the radiosonde and MSU data – it seems most likely to have been less than 0.1 degrees C per decade from 1990-2005</p> <p>[Richard Courtney (Reviewer’s comment ID #: 49-30)]</p>	
SPM-1094	A	12:8	12:1 2	This paragraph ignores the fact that most of the warming took place prior to 1998 and there has been no warming trend in the years since. This pattern of	Rejected, see chapter 3 for discussion of temperature increases in the past decade and the 1998 ENSO. The

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				initial warming followed by stabilisation of global temperatures was not anticipated in the first and second assessments. The pattern underscores that we cannot have confidence in the short term projections because of internal variability of the climate system. Suggest that this paragraph be deleted. [William Kininmonth (Reviewer's comment ID #: 128-104)]	comparison between projected and observed warming is clearly indicated to be at the decadal and longer time scales.
SPM-1095	A	12:8		The report has only a casual reference to Charney et al. (1979) in Chapter 1 (page 1-26, line 3). I would like to suggest that the fact that the complex models projected warming as early as 1979 should be highlighted early in the report. One possible place could be page 12 of SPM (SPM-12, line 8). A suggested language for page SPM-12, starting line 8: * Climate models estimated the sensitivity in the range 1.5°- 4.5°C as early as 1979. [J. Shukla (Reviewer's comment ID #: 237-2)]	Rejected due to length constraints in the SPM which don't allow enough space to put such a statement into context. See chapter 1.
SPM-1096	A	12:9	12:9	Add at end "But the higher figures were based on scenarios which assumed impossible trends" [VINCENT GRAY (Reviewer's comment ID #: 88-2236)]	Rejected. No basis given for assertion.
SPM-1097	A	12:10	12:11	Replace from "providing confidence" on line 10 to "projections" on line 11 with "But the observed temperature increase was not uniform. It was minus 0.02°C per decade from 1942 to 1878, followed by 0.20°C per decade from 1978 to 1998 and a fall since then. These sudden changes could not have been caused by a steady increase in greenhouse gases" [VINCENT GRAY (Reviewer's comment ID #: 88-2237)]	Rejected see comment 1094 response
SPM-1098	A	12:10	12:10	The 0.2C/decade figure should be given, if possible, an uncertainty range. On page SPM-6, L38 to 42, the temperature change uncertainties are given for the century scale changes but not for the change per decade. If available the uncertainties on temperature change/decade should be given. [Philippe Tulkens (Reviewer's comment ID #: 271-2)]	Reject. Space limitations. 0.2C is "about".
SPM-1099	A	12:10	:11	Confirmed projections made in earlier IPCC reports cannot be used to argue for greater confidence in today's assertions. We have no short-term prediction skill. Further, what is "short-term"? [Govt. of United States of America (Reviewer's comment ID #: 2023-840)]	Taken into account. Short-term deleted. Evaluation of earlier projections have provided additional confidence.
0-101	A	12:11		I don't understand this sentence. [Richard Allan (Reviewer's comment ID #: 3-63)]	Text simplified.
SPM-1100	A	12:11	12:11	Insert after "projections" "Provided it can be assumed that the observed change was entirely due to increases in greenhouse gases; a highly unlikely assumption" [VINCENT GRAY (Reviewer's comment ID #: 88-2238)]	Rejected, see chapter 10 for discussion of the physical basis for commitment.

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SPM-1101	A	12:1 1	12:1 2	To avoid any confusion with commitments that have been made under the UNFCCC in the 1990s, the sentence could be rephrased to read: "Some of this recent warming was due to the continued warming effect of greenhouse gases already in the atmosphere at the time of those earlier assessments." [Andy Reisinger (Reviewer's comment ID #: 210-38)]	"commitment" has been removed
SPM-1102	A	12:1 2		This sentence should add "...assessments, and the limited ability to change the trajectory of greenhouse gas increases." Or something to that effect. Clarification needed. [Govt. of United States of America (Reviewer's comment ID #: 2023-841)]	Rejected, not the point being made
SPM-1103	A	12:1 4	12:1 9	This point is expressed in a confusing fashion. It should more clearly back up the figure SPM-5 that a much higher percentage of the temperature response at 2030 (or near-term?) is commitment compared to 2100 where a smaller fraction of the temperature response is commitment but rather driven by the emission pathway the world takes. Reference in lines 15-16 to oceans does not seem to connect with earlier part of first sentence. [Govt. of Australia (Reviewer's comment ID #: 2001-37)]	Partly accepted –text nos makes clearer the diminishing role of the past. Role of ocean clarified.
0-102	A	12:1 4	12:1 9	SPM-12 ; line 14-19; The second half of the paragraph is not very clearly worded - message?? Please rephrase [ European Commission (Reviewer's comment ID #: 2008-76)]	Text revised
SPM-1104	A	12:1 4	12:1 9	I could not find this as a conclusion in the ES of Ch10. In section 10.5 I find only a reference to the TAR (!) stating that a warming in the range 0.1-0.2K/decade is expected (page 45, line 12). My recommendation is to replace this bullet by a shorter bullet that summarizes the change of 2011-2030 compared to 1980-1999 are mentioned in the ES of Ch10 (10-3-16). [Gerrit Burgers (Reviewer's comment ID #: 34-9)]	Rejected. See chapter 10 figures which explicitly show this.
SPM-1105	A	12:1 4	12:3 2	This figure in the SPM would be enhanced for the lay reader, taking into account my previous comments above, by including temperature PDFs for periods beyond 2100, including an extension in time out to at least those time periods where the high emissions scenario would generate mean temperature responses well in excess of those generated in the medium and low emissions scenarios -- I would guess that this time period would be somewhere between 2200 and 2300. A similar graph labelling concept could be used for the world map of temperature patterns in the A2 scenario for the 2080-2099 period, providing some additional labelling that this temperature pattern would closely resemble the pattern observed for the A1B scenario in, say, 2130-2149, and for the B1 scenario in, say, 2170-2189. The exact decades to apply here to the text could	Rejected. Too few model results available for the 4xCO2 case to do this reliably, unfortunately. Only a few models have run this case making it difficult to compare.

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				<p>be determined by the chapter 10 authors through a quick examination of their modelling results extended out through the 22nd century -- a time period that I know has been modelled by several of the groups already, including NCAR and UKMO.</p> <p>[Chuck Hakkarinen (Reviewer's comment ID #: 96-4)]</p>	
SPM-1106	A	12:1 4	12:1 5	<p>Modify the use of the term "committed" here to something like "the stabilized forcing commitment would lead to a warming of about...etc". It is important that policy makers understand that there are several different "commitment" concepts and these need to be used carefully.</p> <p>[William Hare (Reviewer's comment ID #: 99-114)]</p>	"Committed" no longer used
SPM-1107	A	12:1 4	12:1 4	<p>when is "today"?</p> <p>[Joanna House (Reviewer's comment ID #: 109-64)]</p>	Think readers will assume this to mean time report is finalized
SPM-1108	A	12:1 4	12:1 4	<p>there are two very policy relevant points to strongly emphasise here, the first is that you are talking about concentrations not emissions, as some will mis-read it. The second is that to stabilise concentrations, emissions have to fall a lot (particularly with respect to CO2). Suggest can do both by adding in brackets "...were stabilised today (requiring a large drop in emissions), a committed warming ....</p> <p>[Joanna House (Reviewer's comment ID #: 109-65)]</p>	Accepted. Wording added.
SPM-1109	A	12:1 4	12:1 9	<p>Statements in this paragraph are untrue. The ENSO and the interannual variations in SST, and the Pacific Ocean 'climate shift' of 1976/77 and the subsequent rise in tropical SST, highlight the ability of the ocean surface layer to respond rapidly to internal dynamic forcing. The concept of 'climate commitment' can only be sustained with respect to the very large scale and long period inertial mode circulations of the oceans that are likely excited by atmosphere-ocean momentum exchanges. The fact is that it is only modelling studies, that recognise forcing but do not recognise internal variability, that are the basis for the statements made. In addition 'would be expected' is not one of the defined probability estimates. Suggest that the paragraph be deleted.</p> <p>[William Kininmonth (Reviewer's comment ID #: 128-105)]</p>	Assertion that modelling studies do not consider in ternal variability is incorrect. See chapter 8 for discussion of model internal variability studies, as well as chapter 9. Text modified to refer to trend for further clarity.
SPM-1110	A	12:1 4	12:1 6	<p>I think the phrase "committed warming" is quite awkward and not obvious for the public. I would rephrase this sentence to read: "If the concentrations of all radiative forcing agents were stabilized today (which would require, for example, an instantaneous reduction of about 90% in CO2 emissions) and in the absence of changes in climate due to large volcanic eruptions and solar irradiance, warming of about 0.1 C per decade would be expected to continue for the next several decades as the oceans and land surface adjust to the present modifications</p>	Accepted. Wording changed.

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				to atmospheric composition. [Michael MacCracken (Reviewer's comment ID #: 152-43)]	
SPM-1111	A	12:1 4	12:1 4	The phrase "the concentrations of" can probably be deleted without loss, with the benefit of including solar radiation as a potential forcing agent. [Andy Reisinger (Reviewer's comment ID #: 210-39)]	Rejected, as several reviewers have pointed out it is essential to distinguish between constant concentrations and constant emissions.
SPM-1112	A	12:1 4	12:1 4	This first sentence is a bit weird, in my view - it implies that it is somehow possible to do this!. I would suggest rewording "Even if the concentrations of all radiative forcing agents could be stabilised today ..." [Keith Shine (Reviewer's comment ID #: 236-10)]	Partly Accepted. Text now separates the statement from any sense of an emission scenario – alternative changes made.
SPM-1113	A	12:1 4	:20	We think the main result down to line 20 is that global temperature most likely will increase 0,2 degrees per decade the nearest decades. This result, which is rather stable and independent on SRES, should come first. [Govt. of Norway (Reviewer's comment ID #: 2018-33)]	Accepted. See SPM-1066, -1069. Headline changed
SPM-1114	A	12:1 4		The stabilization scenarios have a trajectory. What is meant here is "fixed instantly", not "stabilized". [Govt. of United States of America (Reviewer's comment ID #: 2023-842)]	That is why this sentence does not refer to a scenario. It is to be considered a physics test only. To clarify the word "stabilized" is replaced
SPM-1115	A	12:1 5	12:1 5	"next several decades" would need to be explicated. [Olivier Boucher (Reviewer's comment ID #: 27-12)]	Accepted
SPM-1116	A	12:1 5	12:1 5	What does "several" mean here? Two to three, four to six or what? I think these need careful definition to ensure that it is correct and that it is consistent with the following two sentences and the contextual reference back to "several" decades. [William Hare (Reviewer's comment ID #: 99-115)]	Accepted
SPM-1117	A	12:1 5	12:1 5	The whole paragraph is not very helpful, specify what several decades actually means (35 years?). [Govt. of United Kingdom (Reviewer's comment ID #: 2022-146)]	Accepted
SPM-1118	A	12:1 6	12:1 6	"This leaves the reader trying to make connections themselves. The sentence that ends with ""...unless there are large changes in volcanic eruptions or solar forcing."" should continue with "" WHICH WOULD.....(and then explain to reader what would happen)."" [Govt. of Canada (Reviewer's comment ID #: 2004-78)]	Text rewritten for clarity
SPM-1119	A	12:1 7	12:1 9	These statements are correct if "several" refers to 2-3 decades but not longer. [William Hare (Reviewer's comment ID #: 99-116)]	Accepted
SPM-1120	A	12:1 7	:18	The sentence "About twice as much warming (0.2°C per decade) would be expected if emissions follow those of the SRES scenarios" leads to confusion.	We believe the text is clear that it applies for all SRES marker scenarios

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				SRES scenarios cover a broad range of emissions, so a further specification whether the sentence applies to a particular range of emissions (maximum, medium, minimum) would be advisable. [Govt. of Chile (Reviewer's comment ID #: 2005-13)]	
SPM-1121	A	12:1 8	12:1 9	It would be helpful to give the relevant time frame explicitly, i.e. to say "Over these time scales of a few decades, this result...". The phrase "which do not consider any policy intervention" should be deleted to avoid confusion because it is not actually relevant; the outcome would apply to a policy intervention scenario that, say, specifically aims for stabilisation at 550ppm CO <sub>2</sub> or even a little less, i.e. even if it's outside the SRES range. [Andy Reisinger (Reviewer's comment ID #: 210-40)]	Rejected – WG1 can not rule out stringent policy responses that decrease warming below 0.2 C/decade
SPM-1122	A	12:1 8		Explain SRES at first mention [Stephen J. Hawkins (Reviewer's comment ID #: 102-17)]	Accepted
SPM-1123	A	12:1 9	12:1 9	"Suggest deleting ""which do not consider any policy intervention"" (already assumed for SRES scenarios)."  [Govt. of Canada (Reviewer's comment ID #: 2004-79)]	Many others want this explicitly mentioned to contrast the 0.2C /decade warming with possibly lower rates
SPM-1124	A	12:1 9	12:1 9	Add at end "All this assumes that there are no natural changes or human influences not related to greenhouse gases" [VINCENT GRAY (Reviewer's comment ID #: 88-2239)]	Accepted. Reference to changes in natural forcing now included.
SPM-1125	A	12:1 9	12:1 9	delete "which do not consider any policy intervention" - it is unnecessary to say this. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-147)]	SEE COMMENT SPM-1123
SPM-1126	A	12:2 1	12:2 2	"This heading is weak and does not serve to make it clear that it is not necessarily FURTHER emissions, but even these very SAME emission profiles that will have a much greater impact on the change in climate over the longer-term - as reflected in the bullets that follow. Suggest change to: "Over the course of 21st century, change to the climate globally and regionally is expected to exceed/occur at a greater rate than that of the near-term and is scenario dependent."  [Govt. of Canada (Reviewer's comment ID #: 2004-80)]	Partly accepted. Depends upon scenario; revised headline will be used and include B1 as lower bound.
SPM-1127	A	12:2 1	12:2 2	Here a clear statement is needed, how further emissions will change the future climate. Therefore complete the text with detailed information from the executive summary of chapter 10: "...would be expected to increase the global mean temperature by x-y°C by 2100, increase mean precipitation in tropical regions and decrease it in subtropics as well as increase the risk of extreme	The requested detail is provided in subsequent bullets

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				events (e.g. heat waves, heavy rain and tropical cyclones)...." [Govt. of Germany (Reviewer's comment ID #: 2011-82)]	
SPM-1128	A	12:2 1	12:2 1	Need to say "Further emissions" beyond what? This phrase really does not make sense. [Michael MacCracken (Reviewer's comment ID #: 152-44)]	Accepted – text clarified.
SPM-1129	A	12:2 1	12:2 2	Consistent with my general comment on this whole section, I would suggest amending this title to read "Future emissions of greenhouse gases are expected to change the climate of the 21st century AND BEYOND both globally and regionally". This sub-section should provide projections and uncertainties not just to 2100 but, where quantitative projections are possible, within clearly stated assumptions, also for the longer term. This is particularly relevant for sea level due to its larger inertia. [Andy Reisinger (Reviewer's comment ID #: 210-42)]	Covered in the next section
SPM-1130	A	12:2 1	13:4 5	There should be a bullet point describing projected sea-level rise over the 21st century. It be best inserted at line 16 page 13. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-148)]	Accepted. Sea level bullet will be added.
SPM-1132	A	12:2 1		Clarify "Further". [Govt. of United States of America (Reviewer's comment ID #: 2023-843)]	Accepted – text clarified
SPM-1133	A	12:2 2	12:2 2	SPM Comment: To avoid being grossly misleading, after "... globally and regionally" add the clause "according to model simulations". [Richard Courtney (Reviewer's comment ID #: 49-31)]	Rejected. The words 'are expected to' is meant to imply that physical reasoning leads to this expectation.
SPM-1134	A	12:2 2	12:2 2	Add at end "But it is not known to what extent" [VINCENT GRAY (Reviewer's comment ID #: 88-2240)]	Rejected no rationale given
SPM-1135	A	12:2 4	12:3 2	There is a contradiction between the decades given on top of the plots (2011-2030 and 2080-2099) and those given in the figure caption on line 31. [Olivier Boucher (Reviewer's comment ID #: 27-13)]	Accepted. Revised figure.
SPM-1136	A	12:2 4	12:2 5	Fig. SPM-5, left panels: Each panel needs numbers along the x-axis. It is very hard to see what the scale is on the top two panels by scanning all the way down to the bottom. In the future, people may want to use individual panels in teaching and presentations, and each should have completely labeled captions. - Alan Robock, Rutgers University [Alan Robock (Reviewer's comment ID #: 217-2)]	The figure appears in separated form in the chapter.
SPM-1137	A	12:2 4		The left panel of Figure SPM-5 needs some work. What does the horizontal bar with the circle mean? Put a vertical line at the zero (1980-1999) mark. It would be very good to add a vertical dashed line showing the 1850-1919 value. Label both of these lines so that the X-axis label is then clear (i.e., relative-to-what). Expand the caption to fully explain the lefthand panel.	Accepted. Figure redrawn and caption modified. Reject idea of vertical bar at zero.

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				[Govt. of United States of America (Reviewer's comment ID #: 2023-844)]	
SPM-1138	A	12:2 5	12:2 5	Figure SPM-5: "It might be useful to highlight which of the SRES scenarios is closest to the 2X CO <sub>2</sub> scenario from the TAR. This could provide a comparative basis for readers."  [Govt. of Canada (Reviewer's comment ID #: 2004-83)]	Not clear why we should provide this focus on 2 x CO <sub>2</sub> ? No change made
SPM-1139	A	12:2 7	12:3 2	"Figure SPM-5: The use of several time periods may be confusing/too complex. Add a sentence to the end here to explain how to interpret the left hand panel. It could read something like this: ""For example, over the period 2020-2030, it is twice as likely that the global temp rise will be about 1 degree C vs. 0.5 degrees C."" Alternately, consider showing the left hand panel as a separate figure."  [Govt. of Canada (Reviewer's comment ID #: 2004-81)]	Accepted. Time periods will be changed. Reject idea of explaining relative probability – see TS and chapter 10.
SPM-1140	A	12:2 7	12:3 2	(Same comment as to TS)Caption SPM5: Please make it more clear that the two parts of the figure come from very different studies and models. Add AOGCM to description of the right part, and 'models of different complexity, partly constrained by observations' to the left part. The idea to combine the two figures is interesting, but there is a danger that the data shown will be interpreted as coming from the same set of model runs, whereas the two have very different origins, and are not even necessarily consistent with each other, because the AOGCMs span a more narrow range of projections than the observationally constrained ensembles with simpler models. Also, the time periods need to be consistent in the figure. Changing the right part to a single decade is easier since the maps are based on new data, while the PDFs are from various papers. I realize that a decade of averaging is normally a bit short for the map, but since this is a multi model average over twenty or so models and probably sixty or so ensembles, the noise is averaged out anyway, so a decade should be fine. [Reto Knutti (Reviewer's comment ID #: 133-45)]	Reject in figure caption. Time periods have been adjusted.
SPM-1141	A	12:2 7		note that the left panel implies that the entire warming sequence of the 20th century is compressed in the next 20 years. The warming by the end of the century has not occurred since the mid-Pliocene warm period of about 3 million years ago (3 Ma). I know IPCC doesn't want to sound hairy-scary about this but these numbers have been around for quite some time and at some point it is ducking an important point NOT to state them. [Thomas Crowley (Reviewer's comment ID #: 51-10)]	Reject. Length issue. Too complex for SPM, covered in chapter 6.
SPM-1142	A	12:2 8	12:1 8	B1, A1B ... these labels are a menace, in my view - they are only interpretable by a cognescenti. Couldn't more friendly names be used?	Reject. Label for SRES members established.

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				[Keith Shine (Reviewer's comment ID #: 236-11)]	
SPM-1143	A	12:29	12:29	It should be noted that scenario A2 makes a number of entirely ridiculous assumptions about the future (such as a nine times growth in coal production by 2100, nearly five times CO2 emissions and a two and a half increase in world population. It should be ignored [VINCENT GRAY (Reviewer's comment ID #: 88-2241)]	Reject. WGIII issue.
SPM-1144	A	12:29		SRES in full [Stephen J. Hawkins (Reviewer's comment ID #: 102-18)]	Acronym will be explained
SPM-1145	A	12:30	12:30	""The left panel shows corresponding uncertainties as the relative probabilities..."" Wording of the phrase is complex; the meaning needs to be clearer." [Govt. of Canada (Reviewer's comment ID #: 2004-82)]	Text will be simplified
SPM-1146	A	12:31	12:31	It would be useful to elaborate on what is meant by "several different studies"-- are these results from single model runs, from an ensemble of runs with multiple models, what? [Michael MacCracken (Reviewer's comment ID #: 152-45)]	Covered in chapter
SPM-1147	A	12:34	12:32	If the graph is still maintained here, the following should be considered: (a) there are two groups of graphs. The one, more understandable for policymakers, is the spatial graphs. However, all graphs are so small that it is impossible to interpret them. Suggest to only retain these graphs, but in an enlarged form. (b) the probability graphs have different time scales, and are too confusing because of the many curves. Suggest to replace them with a "mean" curve for both periods. (c) the references to the B1, A1B and A2 scenarios must be completed with a short explanation of these scenarios - "new" policy makers will not understand these scenarios without explanation (they will not read the full report). [Govt. of Hungary (Reviewer's comment ID #: 2012-4)]	A-Layout will consider how to make figures as readable as possible B-The time scales will be corrected. Do not accept that it is valid to merge the PDFs. C-SRES cases will be explained in box from TAR
SPM-1148	A	13:0		"Over all, this section on future projections seems weak and vague and not nearly as useful as the corresponding section in the SPM of the TAR. Also, the reader expects to find projections of SLR, Ice sheets and THC with the other projections - consider perhaps a different heading for next section to alert reader to fact that some projections are listed there. For ex. ""Commitment to long term climate change"" [Govt. of Canada (Reviewer's comment ID #: 2004-84)]	Sea level will be added and the range of issues considered will be the same as in the TAR.

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0-103	A	13:1	13:4	SPM-13; line 1-4; the wording is very unclear and difficult to understand - please rephrase: (perhaps turn the order round: first mention projected temperature increases under certain scenarios and then give the related likelihood [ European Commission (Reviewer's comment ID #: 2008-77)])	Accepted – using simpler language.
SPM-1149	A	13:1	13:4	It might be helpful to indicate the total emissions in the 21 century for the different emission scenarios. [Govt. of Austria (Reviewer's comment ID #: 2002-18)]	Rejected. Total emissions doesn't reflect the fact that a fraction of emission is also removed.
SPM-1150	A	13:1	13:10	It might be more convenient to compare the increase in temperature to the level before industrialization (as was the case in the TAR). [Govt. of Austria (Reviewer's comment ID #: 2002-21)]	The TAR presented model results in the same way as done here – so this is a better basis for comparison – no change.
SPM-1151	A	13:1	13:4	Uncertainty range should be given as +/- two standard deviations (95% range) following conventional scientific practice. [Lenny Bernstein (Reviewer's comment ID #: 20-11)]	Explicit confidence intervals now stated
SPM-1152	A	13:1	13:4	"An explanation of what some of the main differences are between various scenarios would be useful."  [Govt. of Canada (Reviewer's comment ID #: 2004-85)]	To be covered in new box on SRES
SPM-1153	A	13:1	13:1	"65% range for the projection. Would critics say again that you use 65% range to report projected uncertainty range to make the estimated uncertainty range look smaller?"  [Govt. of Canada (Reviewer's comment ID #: 2004-86)]	Taken into account – Changing to 5-95% b
SPM-1154	A	13:1	13:4	"The temp projections in this bullet should be compared to the range presented in the TAR. Policymakers will want to understand this. The range appears to be about the same (1.5 - 5.8) but what more can be said? If the lower and upper limits here are 65% probability bounds then can it be said that there is more confidence in this range - that the upper and lower limits are probable under specified emission scenarios? Also, this bullet is awkward to read as all one sentence."  [Govt. of Canada (Reviewer's comment ID #: 2004-87)]	Reject. We are not providing range across scenarios to avoid confounding model uncertainties with potential policy choices
SPM-1155	A	13:1	13:4	"Also missing from this bullet, is some statement about this rate of temp change compared to historical rates of change. Unprecedented or not?"  [Govt. of Canada (Reviewer's comment ID #: 2004-88)]	Accepted – revised headline covers this
SPM-1156	A	13:1	13:4	The inclusion of probabilistic ranges in the SPM is welcomed but a quick glance at figure SPM-5 shows that there are a range of different estimates of pdfs	Rejected. Suggested language doesn't inform the broad audience of the SPM although scientists would

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				(uncertainties on uncertainties). These numbers are based only on a simple model tuned to the multi-model archive and thus ignore other approaches. Could this statement be caveated with "Different approaches have been used to estimate probability ranges and while these give broadly consistent ranges, there are nevertheless differences in detail." [Matthew Collins (Reviewer's comment ID #: 44-40)]	understand it.
SPM-1157	A	13:1	13:4	These important findings are not presented adequately. Furthermore we see a risk of misunderstanding or misinterpreting these findings. It is worth inserting a figure in order to illustrate the range of possible future temperature trends and there dependencies from emission scenarios. We propose to present this range in connection with the already observed temperature trend in order to give an idea about the scale of future changes. [Govt. of Germany (Reviewer's comment ID #: 2011-83)]	Reject. We already have more figures than in the TAR. The relationship between 21st and 20th century warming is now covered in the headline statement
SPM-1158	A	13:1	13:4	This very relevant information is hard to digest this way by policymakers. Add figure TS-33, but please add information in figure TS-33 about expected temperature ranges for a wide range of mitigation scenarios, including, in particular, low stabilisation scenarios that have been published/analysed, in order to inform about which warming can be avoided assuming ambitious but plausible mitigation scenarios. Please also inform about changes compared to TAR and give short explanation for differences. [Govt. of Germany (Reviewer's comment ID #: 2011-239)]	Comparison with the TAR is covered. Assessment of the plausibility of mitigation scenarios is outside the scope of WG1. See SPM-1157 also.
SPM-1159	A	13:1	13:1	"65% probability" yet again. Double the figures to get the required 95% probability [VINCENT GRAY (Reviewer's comment ID #: 88-2242)]	Accepted - 90% confidence intervals now given
SPM-1160	A	13:1	13:4	I like the idea of splitting the projection into scenarios but EVERYONE will take the lowest of B1 and the highest of A1FI and say the range is 1.5 to 5.8 and will compare this to the TAR range, which was almost identical. They will do this even though the figure now includes carbon cycle feedbacks and has a confidence limit attached. Because everyone will do it anyway the range and the comparison with the TAR should be done here in the SPM so that a suitable commentary can be included to avoid the mis-use which will inevitably follow if you don't. [David Griggs (Reviewer's comment ID #: 90-5)]	Taken into account. Some comparison to TAR added.
SPM-1161	A	13:1	13:4	It would be helpful t have expressed here in simple terms what these scenarios represent - e.g. business as usual, etc. Otherwise the casual reader is not given any useful information that relates cause and effect. IPCC participants know the codes, but others don't. Some common language here would make the ES/IPCC	SEE COMMENT SPM-1152 and new box on SRES definitions

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				seem less insular. [Dennis Hartmann (Reviewer's comment ID #: 100-9)]	
SPM-1162	A	13:1	13:4	It is necessary to emphasize that accuracy of projections of temperature, CO2 concentration and sea level rise have been improved since TAR so as to avoid a wrong impression that projected warming has weakened. In case of projections of temperature, it should be pointed out probable range with maximum and minimum projected temperature among all scenarios/models, in the form of **.* , as it did at TAR. [Govt. of Japan (Reviewer's comment ID #: 2014-12)]	We believe improved confidence in projections and new aspects are covered explicitly. See SPM-1154 on why we do not coalesce the warming range across scenarios
SPM-1163	A	13:1	13:4	It is surprising how narrow the range of uncertainty is for any given scenario. I had difficulty tracing the logic for assigning a probability to such a range. Assigning a probability is a significant change from the TAR, and its basis should be discussed fully. In addition, literature that uses similar simple models to arrive at different ranges should be assessed; this is neglected in chapter 10. Reading the description on estimating probabilities on page 10-48, I do not see how a probability can be justified. Suggest that the probability be removed, and that the range be stated as a model range. [Haroon Kheshgi (Reviewer's comment ID #: 125-18)]	Reject. Basis for estimating probability discussion too long and complex for SPM. Discussion appears in chapter and TS.
SPM-1164	A	13:1	13:4	All uncertainty ranges should be +/- two standard deviations, following conventional scientific practice. [Jeff Kueter (Reviewer's comment ID #: 137-11)]	Accepted 90% confidence intervals now used.
SPM-1165	A	13:1	13:1	It would be helpful to indicate how the 65% figure was derived--is this across single model runs from multiple models, ensembles of runs across multiple models, what? [Michael MacCracken (Reviewer's comment ID #: 152-46)]	Reject. Basis for estimating probability discussion too long and complex for SPM. Discussion appears in chapter and TS.
SPM-1166	A	13:1	13:4	Perhaps too much information for a SPM. Suggestion: Projected 65% probability ranges for globally averaged surface warming in 2100 compared to 1980-2000 are scenario dependent and estimated to be between 1.5 to 5.8 °C. [Louis Jose Mata (Reviewer's comment ID #: 170-3)]	Rejected – See SPM-1154.
SPM-1167	A	13:1	13:4	It might be helpful to indicate the total emissions in the 21 century for the different emission scenarios. [Klaus Radunsky (Reviewer's comment ID #: 204-18)]	Total emissions do not tell the whole story – no change
SPM-1168	A	13:1	13:10	It might be more convenient to compare the increase in temperature to the level before industrialization (as was the case in the TAR). [Klaus Radunsky (Reviewer's comment ID #: 204-21)]	Reject. The TAR quoted model projections in warming from 1990 – this is comparable.

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SPM-1169	A	13:1	13:5	<p>I am not comfortable with these ranges expressed as probabilities since they are not the result of a formal probability analysis concatenating uncertainty. They are rather the result of a sensitivity study as explained in Ch10.5.3.1 and reiterated at the end of the section in the paragraph Ch10 p48 lines 4-10. The '(mean+/-1 standard deviation)' which appears in the Ch10 summary (page 3 line 44 ) refers only to uncertainty due to the 19 model tunings and this should no-doubt be clarified.</p> <p>I would be more comfortable with the words 'Projected likely range' but since 'likely' is formally defined as '66% probability' this may not help in the defence. Note that the ranges are based on a single estimate of the forcing magnitude as in the TAR.</p> <p>[Sarah Raper (Reviewer's comment ID #: 208-2)]</p>	Taken into account. Bullet reworded and strict probabilistic interpretation removed
SPM-1170	A	13:1	13:4	<p>I think you have found a very useful way to give the full range of warming across all SRES scenarios without giving the rather less helpful "1.5 to 5.8°C" range. It would be really helpful if you could give a similar range for SLR projections in a separate bullet point - or say that to 2100, the difference between scenarios is small (with some quantification of how small).</p> <p>[Andy Reisinger (Reviewer's comment ID #: 210-45)]</p>	Taken into account. New bullet.
SPM-1171	A	13:1	13:4 5	<p>This section needs some graphics to convey the main messages in a powerful way. In its current state it is very weak.</p> <p>[Govt. of United Kingdom (Reviewer's comment ID #: 2022-149)]</p>	Rejected - Figure SPM-5 is part of this section
SPM-1172	A	13:1	13:4 5	<p>The section lacks information on future sea level rise, NAC and</p> <p>[Govt. of United Kingdom (Reviewer's comment ID #: 2022-150)]</p>	Accepted new bullet will be added
SPM-1173	A	13:1	13:4	<p>Could we have these important conclusions illustrated by means of a diagram analagous to that used in the TAR SPM Figure 5?</p> <p>[Govt. of United Kingdom (Reviewer's comment ID #: 2022-152)]</p>	Reject. Length. See TS and chaptre 10.
SPM-1174	A	13:1	13:4	<p>Is the additional 1.2 deg C of SPM-15 line 3-5 included in these temperature ranges?</p> <p>[Guus Velders (Reviewer's comment ID #: 276-4)]</p>	Yes – text now makes this clearer
SPM-1175	A	13:1		<p>A dot point should be added which summarises the projected sea-level changes</p> <p>[Govt. of Australia (Reviewer's comment ID #: 2001-38)]</p>	Taken into account. New bullet added.
SPM-1176	A	13:1		<p>I think you need both a precipitation and P-E plot for the next two decades also. I know the changes are not as large but bringing it closer in time to people means that many of the people reading this document will actually be alive to witness the projected change in the next twenty years. it is imperative to add P-E because there is still this willy-nilly feeble-minded thinking that if ppt increases, you don't</p>	Rejected; precipitation already covered and P-E issues too complex for SPM

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				need to worry. but higher temps will wipe out the positive ppt changes in some (if not many) areas and P-E presents a sobering and much more realistic assessment as to the state of real availability of altered water balance, and this is what planners need to know. one should also contempate P-E for the end of century projection. [Thomas Crowley (Reviewer's comment ID #: 51-11)]	
SPM-1177	A	13:1		why one SD not two SDs? (95% more accepted value) [Stephen J. Hawkins (Reviewer's comment ID #: 102-19)]	Accepted – 90% confidenc eintervals will be used
SPM-1178	A	13:1	:4	The only mention in the SPM of the importance of economic growth and policy decisions is buried here in jargon about scenario dependence with intervening phrases: “probability ranges ... for globally-averaged surface warming in 2100...are scenario dependent...” How about being direct: The globally-averaged surface warming in 2100 depends on population and economic growth as well as policy decisions. The projected 65% probability ranges (mean $\pm$ 1 standard deviation) are 1.5-2.8C, 2.3-4.1C, 3.0-5.0C, and 3.5-5.8C for the B1, A1B, A2, and A1FI scenarios respectively. [Daniel Murphy (Reviewer's comment ID #: 183-17)]	Rejected, WG1 does not assess population or economic growth potential and how emissions may be linked to those. See WG3
SPM-1179	A	13:1	:4	The information in this paragraph is very important, but the message is lost in the figures. We propose expressing the temperature projections and the corresponding scenarios in a small table. [Govt. of Norway (Reviewer's comment ID #: 2018-34)]	Reject. Space limitations.
SPM-1180	A	13:1		You need to define, in lay terms, what the various scenarios are. It is highly unlikely that policy makers will read more than this document, and he or she will have no idea what B1, A1B, A2, and A1F1 are. [Timothy H. Profeta (Reviewer's comment ID #: 203-19)]	Accepted – SRES box to be added
SPM-1181	A	13:1		This bullet is very important, and very hard to decipher. It needs to be stated much more clearly for policymakers. [Timothy H. Profeta (Reviewer's comment ID #: 203-20)]	Accepted – wording has been simplified
SPM-1182	A	13:1		Is not 1-sigma 68%? [Govt. of United States of America (Reviewer's comment ID #: 2023-845)]	Accepted – 90% confidence intervals now given
SPM-1183	A	13:3	13:4	just give for lowest and highest scenarios [Joanna House (Reviewer's comment ID #: 109-67)]	Reject – see SPM-1154
SPM-1184	A	13:3		Delete “are scenario dependent” since obvious and unnecessary. [Govt. of United States of America (Reviewer's comment ID #: 2023-846)]	Reject – this is an important policy relevant point
SPM-1185	A	13:4	13:4	Add at end "Since the scenarios A1F1 and A2 are highly improbable the realistic range is 1.5°C to 4.1°C" [VINCENT GRAY (Reviewer's comment ID #: 88-2243)]	Rejected. WG1 does not ases scenario likelihood.

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0-104	A	13:4		SPM: the scenario names could be described in a footnote [Richard Allan (Reviewer's comment ID #: 3-64)]	Accepted SRES box to be added
SPM-1186	A	13:4	13:4	Some comparison with the TAR range would be important here. [William Hare (Reviewer's comment ID #: 99-117)]	Comparison with TAR p[rovided in separate bullet
SPM-1187	A	13:5	13:5	This section need to provide a bullet on projected sea-level rise across different emission scenarios to complement the temperature projections. It's one of the fundamental projections that policymakers and end-users are looking for. Given the inertia of the ocean system, it would be very useful to give specific projections not only to 2100, but also to longer-term sea-level rise and the commitment to such rise resulting from the GHG build-up during the 21st century, including the possible contribution of ice sheets, and uncertainties due to assumptions in ocean mixing. Sea-level is a crucial planning variable for urban and rural development in many regions, and policymakers want a single reference point for this rather than having to piece this information together from different bullets about sea-level from thermal expansion, for one specific scenario, and with uncertainties due to ice sheets and ocean mixing parametrisation considered somewhere else again. Since urban planning by implication has to look beyond 2100, it is important not to omit information on SLR (and its uncertainties) beyond 2100. [Andy Reisinger (Reviewer's comment ID #: 210-43)]	Taken into account. New sea level bullet.
SPM-1188	A	13:6	13:8	"Language is rather technical here again. For example, the term equilibrium climate sensitivity should be explained. The reader is not told what the climate sensitivity values are in reference to (e.g., 2 x pre-ind CO2 concs). "  [Govt. of Canada (Reviewer's comment ID #: 2004-90)]	Taken into account. The term is no longer used.
0-105	A	13:6	13:10	SPM-13; line 6-10; Key paragraph that should stay in the SPM text, however, the first sentence starting "Additional information... [ ...] ... sensitivity." seems to be not needed here. [ European Commission (Reviewer's comment ID #: 2008-78)]	Accepted
SPM-1189	A	13:6	13:10	needs much better explanation of what climate sensitivity is and why it is used/relevant to modelling studies [Joanna House (Reviewer's comment ID #: 109-68)]	SEE COMMENT SPM-1188
SPM-1190	A	13:6	13:10	Consider adding a clause to explain the term climate sensitivity as well as equilibrium climate sensitivity. We believe much of the audience, who otherwise benefit from the SPM significantly and could use this message, too, may not know what the jargon "climate sensitivity" or "equilibrium CS" refers to. We know it is very important in an SPM to balance the provision of new and	SEE COMMENT SPM-1188

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				scientifically robust information with staying relevant and clear for your audience - but I believe in this case more explanation is essential for being able to convey your message to the majority of the audience of the SPM (which probably differs from the audience of the TS or the full report). [Govt. of Hungary (Reviewer's comment ID #: 2012-5)]	
SPM-1191	A	13:6	13:10	COMMENT: add some explanation on "equilibrium climate sensitivity". REASON: ECS have been calculated using atmosphere models coupled with non-dynamic "slab" ocean, i.e., models omitting all ocean dynamics. The numbers of ECS are higher than actual temperature change. Explanation on ECS is necessary for easy understanding. RECOMMEND: add the following sentence, "It should be noted, however, that the equilibrium climate sensitivity which assumes 50m-depth ocean will give higher temperature rise than what will actually happen." [Govt. of Japan (Reviewer's comment ID #: 2014-13)]	Taken into account. Footnote added to point to glossary.
SPM-1192	A	13:6	13:10	COMMENTS: suggest describe the "transient climate response: TCR" REASON: TCR is evaluating in the more actual condition than ECS. Therefore, TCR is useful information about more real estimation of future temperature changes. RECOMMEND: add the following paragraph "On the other hand, the range of transient climate responses among models is smaller than the range in the equilibrium climate sensitivity. The 90% confidence interval of TCR is estimated to be 1.2–2.4 °C. The median for TCR is 1.8°C. TCR can better simulate what will actually happen than ECS. [9.4, 10.5, Box 10.2] [Govt. of Japan (Reviewer's comment ID #: 2014-14)]	Reject. Space limitation. Bullet will be added to TS.
SPM-1193	A	13:6	13:10	This bullet point on climate sensitivity is very important, but I feel it hangs a little bit in the air since it is the only bullet point that makes a statement about the climate system as such, rather than a specific projection over a specific time scale. A re-organisation of the entire section on "Projections of future changes in climate" (see two general comments on this section) might help provide a more useful context for this very important bullet point - i.e. what is climate sensitivity good for, but also when is it not quite as relevant? A lot of policymakers don't fully appreciate the time scales it takes for the temperatures implied in climate sensitivity to come to realisation - many actually assume that a climate sensitivity of 3°C means that if you double CO2 you will have 3°C by 2100. I make a more generic suggestion on how to address this problem in a comment on the whole "projections" section. [Andy Reisinger (Reviewer's comment ID #: 210-52)]	Accepted – material on climate sensitivity moved to previous section.

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SPM-1194	A	13:6	:10	"Values up to 4.5 are said to be possible in the text (line 9), yet Figure SPM-5 shows the high end tails to be up to 8 degrees. This needs to be better explained."  [Govt. of Canada (Reviewer's comment ID #: 2004-89)]	Reject. Confusing climate sensitivity with response. Moving climate sensitivity to preceding section should improve clarity
SPM-1195	A	13:7	13:9	The revision upward of the estimate of climate sensitivity is, perhaps, the most important conclusion of the AR4, however, it is not well supported by the underlying text. Tracing this conclusion back to chapter 10 it is found to be the conclusion of expert judgement. Searching for a more explicit basis, I find the statement in Chapter 10, page 73, line 11 to state that climate sensitivity is "very unlikely below 2C". This is apparently a primary basis for a lead conclusion of the AR4, the revision of climate sensitivity estimate upward from that in the TAR and previous assessments. This statement seems to rely on model sensitivity and ensembles calculations that should include caveats such as those outlined on chapter 10 page 48 that indicates that such calculations generate partial estimates of uncertainty. Since 2 of the 7 model results that were the basis for the TAR estimates of future warming had a climate sensitivity below 2 (see TAR table 9.A1), this basis needs better explanation. Unless a better explanation can be given, the conclusion should be reconsidered.  [Haroon Kheshgi (Reviewer's comment ID #: 125-19)]	Rejected. It is stated that climate sensitivity is unlikely to be less than 1.5, not less than 2. See box on climate sensitivity which brings together all the evidence.
SPM-1196	A	13:7	:8	The language describing the most "likely" climate sensitivity is confusing. Since likely suggests a 66-90% probability and climate sensitivity is usually described by a PDF, it's not clear how the so-called "most likely" climate sensitivity was determined. Does this reflect multiple PDFs?  [Govt. of United States of America (Reviewer's comment ID #: 2023-847)]	Not practical to explain in SPM. See sections referenced.
SPM-1197	A	13:7		Be explicit on climate sensitivity. Use "climate sensitivity to doubled CO2" everywhere where that is the intent to be expressed. The shorthand jargon can have multiple meanings.  [Govt. of United States of America (Reviewer's comment ID #: 2023-848)]	accepted
SPM-1198	A	13:8	13:8	It is not clear what "is likely to" means here when applied to a range--is this one sigma, or does this mean 67-90% chance, or what? Same for "most likely"--how can one have a most likely probability (so greater than 90% likelihood) for one number in a range that is only "likely" (so 67-90% is in this range at all? It would make sense to say that the climate sensitivity is most likely in the range 2 to 4.5 C and is likely about 3 C--but the reverse seems quite problematic.  [Michael MacCracken (Reviewer's comment ID #: 152-47)]	"Likely" is explicitly defined in a footnote now.
SPM-1199	A	13:8	13:8	I am not sure, but can one expect at the SPM level the reader to know what "the" climate sensitivity is and understand that it is measured in units of temperature?	Definition now in a footnote.

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				[Rolf Müller (Reviewer's comment ID #: 181-9)]	
SPM-1200	A	13:8	13:8	Stating climate sensitivities in C, without clarification, always strikes me as odd. Again, it is only for the cognescenti. I think it is much better to stick to the units of K/(W/sqm) as this doesn't require the rather odd unit of "C for a doubling of CO2", which is hard to interpret. [Keith Shine (Reviewer's comment ID #: 236-12)]	Rejected. Clearer to a scientist perhaps but not to a non-expert.
SPM-1201	A	13:9	13:9	The sentence "It is very unlikely to be less than 1.5C" should be qualified by the rider ", unless the warming of recent decades can be shown to be due to internal variability resulting from atmosphere-ocean interactions". The model studies are based on minimal internal variability because the computer models do not reflect internal variability, not because atmosphere-ocean interactions have been demonstrated not to be important. [William Kininmonth (Reviewer's comment ID #: 128-106)]	Rejected – basis for the statement is covered in the chapters as referenced
SPM-1202	A	13:9	13:10	"Values substantially higher than 4.5C cannot be excluded, but agreement with observations is worse for those values." Why is there no "probability" word associated with this statement? Annan et al. (2006) state that it is "very unlikely" (less than 5% chance) that the climate sensitivity is greater than 4.5°C and Hegerl et al. (2006) state that the chance that the climate sensitivity exceeds 4.5°C is 16%. Since the IPCC uses the "probability" word "very unlikely" to describe the chance that the climate sensitivity is less than 1.5°C, it should also use either "very unlikely" or at least "unlikely" to describe the chance that it exceeds 4.5°C—in accordance with the recent literature. The IPCC's hesitance to assign a low probability to an alarmist change despite strong scientific support, is a subtle, yet significant example of the IPCC allowing an alarmist sentiment to exist within the framework of its findings. [Patrick Michaels (Reviewer's comment ID #: 176-1)]	Rejected. See underlying sections referenced which explain the reasons.
SPM-1203	A	13:9	:10	Please clarify "substantially higher than 4.5C" and "cannot be excluded", the latter with reference to the standard terms for the probability of assessed outcomes defined for this purpose. Eg, do you judge it to be exceptionally unlikely that S>6C, or very unlikely, or merely unlikely? I don't think you can reasonably duck this decision. You are supposed to be informing, not obfuscating. How can a policy-maker be expected to interpret these statements? [James Annan (Reviewer's comment ID #: 6-1)]	Rejected. See underlying sections.
SPM-1204	A	13:9		Recommend indicating the time frame for the projected change of 2-4.5 degrees C. [Timothy H. Profeta (Reviewer's comment ID #: 203-21)]	There is no time frame directly associated with these numbers – see definitions
SPM-	A	13:9		The "Values substantially..." sentence should be recast as "Models with higher	Reject – the existing language is carefully considered

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1205				than 4.5°C are unlikely(?) in terms of representing current climate....” Do not exclude anything. [Govt. of United States of America (Reviewer’s comment ID #: 2023-849)]	
SPM-1206	A	13:10	13:10	<p>SPM Comment: To avoid being grossly misleading, replace the phrase “, but agreement with observations is worse for those values.” with “according to the model simulations.”.</p> <p>The observations are not capable of assessing the temperature rise that has occurred in recent decades.</p> <p>The surface temperature record begins ~1860. For the period 1860 to 2004, the most cited of these data sets have good coherence (which is not surprising because they were compiled from the same available source data), but their global trends (in degrees C/decade) and 2SD trend errors are                      GHCN: <math>0.076 \pm 0.010</math>                      Jones et al.: <math>0.064 \pm 0.007</math>                      GISS: <math>0.048 \pm 0.006</math></p> <p>The Jones trend is significantly different from the GISS trend (<math>p &lt; 0.05</math>), and the GHCN trend is very significantly different from the GISS trend (<math>p &lt; 0.01</math>). So, “coolest” and “warmest” years near the ends of the data sets are generated by trends that are known to be spurious.</p> <p>A change in the indicated temperature with time is only an indication of climate change when the change exceeds the intrinsic measurement errors (otherwise the change could be intrinsic measurement error). The intrinsic errors of GHCN, Jones et al. and GISS data sets are not known, but at least two of the data sets provide wrong results because they differ in annual change by more than double their stated 95% confidence limits in each of several years.</p> <p>Furthermore, the GHCN and Jones et al. data sets also have different trends for the warming period of the most recent 30 years. For the period 1976-2004, the temperature trends for the Jones et al., GISS, and GHCN surface temperature time-series are +0.215 degrees C/decade, +0.204 degrees C/decade and +0.274 degrees C/decade, respectively. And this also demonstrates that at least two of them are indicating spurious trends generated by the methods used to create the means.</p> <p>Data of this kind cannot be used to justify any assertion concerning magnitudes of warming they suggest.</p> <p>Indeed, other recent data suggest that the trends suggested by these data sets for recent decades are very wrong. For the late 20th century warming period between 1976 and 2004, the rates of change indicated by these data sets are significantly higher than the rate of +0.059 degrees C/decade for the lower</p>	Rejected due to errors of fact.

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				<p>atmosphere measured by weather balloon radiosondes for the same period and the rate of +0.079 degrees C/decade and satellite MSUs for the period 1979 to 2004 (the satellite record begins in 1979).</p> <p>Furthermore, the compilers of these data sets admit their methods create spurious trends (ref. Vose et al., 2004). Their methods include integrating measurements into mean values for regions over the Earth's surface called 'grid boxes'. And the integrations cause trend problems for individual grid boxes. Vose et al. state that when the GHCN and Jones et al. trends are compared at the grid-box level then 9.3% of grid cells display "discrepant trends". In other words, the integration of measurements into grid boxes causes 9.3% of grid boxes to have trends with opposite sign. (!)</p> <p>Hence, the actual warming of recent decades not known (but – on the basis of the radiosonde and MSU data – it seems most likely to have been less than 0.1 degrees C per decade ) and, therefore, "agreement with observations" is meaningless.</p> <p>[Richard Courtney (Reviewer's comment ID #: 49-32)]</p>	
SPM-1207	A	13:1 0	13:1 0	<p>Don't like the wording, or I don't know what it means. Is it that the QUMP ensembles with these high values don't fit observations (e.g.). Or does it just mean that the pdf numbers are smaller?</p> <p>[Piers Forster (Reviewer's comment ID #: 73-11)]</p>	Reject. Details see chapter 10.
SPM-1208	A	13:1 0	13:1 0	<p>Insert before "worse" "even"</p> <p>[VINCENT GRAY (Reviewer's comment ID #: 88-2244)]</p>	Reject - unnecessary
SPM-1209	A	13:1 0	13:1 0	<p>The ref. to the climate sens. part in chapter nine should probably be 9.6, not 9.3</p> <p>[Reto Knutti (Reviewer's comment ID #: 133-46)]</p>	Accepted
SPM-1210	A	13:1 0	13:1 0	<p>Add: "In the TAR the most likely value for climate sensitivity was estimated at 2.5°C."</p> <p>[Govt. of Netherlands (Reviewer's comment ID #: 2016-20)]</p>	Rejected? TAR avoided giving a most likely value
SPM-1211	A	13:1 0		<p>Should a statement to the effect that this is 'not likely' be made. The current sentence is a little vague.</p> <p>[Stephen J. Hawkins (Reviewer's comment ID #: 102-20)]</p>	We do not say "not likely" and the point that higher values can not be excluded is accurate and important.
SPM-1212	A	13:1 1	13:1 1	<p>"Should include a bullet similar to Page SPM-13 Line 6-10) to discuss the implications of the probabilities attached to the scenarios in Figure SPM-5."</p> <p>[Govt. of Canada (Reviewer's comment ID #: 2004-91)]</p>	Taken into account by the new 5-95% range in previous bullet.
SPM-1213	A	13:1 2	13:1 5	<p>What is the period over which this projected future warming shows similar geographic patterns to those observed over the past 50 years</p>	Accepted. Wording added.

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				[Govt. of Australia (Reviewer's comment ID #: 2001-39)]	
SPM-1214	A	13:1 2	13:1 2	Replace "similar to those" with "which do not show the wild variability" [VINCENT GRAY (Reviewer's comment ID #: 88-2245)]	Rejected, no basis provided for the suggested 'wild variability' which does not agree with observations.
SPM-1215	A	13:1 2	13:1 2	suggest chnign to: "Projected future warming shows COONSISTENT geographical patterns similar to those observed over the past 50 years THAT ARE INDEPENDENT OF WHICH SCENARIO IS USED." [Joanna House (Reviewer's comment ID #: 109-69)]	Reject. Wording ok.
SPM-1216	A	13:1 2		"...warming for a range of scenarios shows geog..." is not truly scenario-independent. Consider massive volcanoes. [Govt. of United States of America (Reviewer's comment ID #: 2023-850)]	Rejected. Future volcanoes are not part of emission scenarios.
SPM-1217	A	13:1 3	13:1 3	.Insert after "northern" "and southern" [VINCENT GRAY (Reviewer's comment ID #: 88-2246)]	Rejected, incorrect. See ch 10
SPM-1218	A	13:1 3	13:1 5	States, "warming is expected to be greatest "over land", however it is not clear if this means land at high northern latitudes or all land areas in general. Clarification required. [Govt. of Japan (Reviewer's comment ID #: 2014-15)]	Text clarified
SPM-1219	A	13:1 7	13:1 9	This applies to summer ice only, so I recommend to change the last part of this sentence into: "For the A2 scenarios, summer ice in the Arctic even disappears in some models by the latter part of the 21st century". [Gerrit Burgers (Reviewer's comment ID #: 34-10)]	Accepted. Late-summer added.
SPM-1220	A	13:1 7	13:1 9	"Bullet should be expanded to provide more detail and explanation. E.g. How much sea ice reduction is projected for Antartica? Why aren't feedbacks amplifying the sea ice loss in the Antarctic as they are in the Arctic? Clarify what projections say about summer ice season vs. winter sea ice. Clarify whether the result as stated, that some models project sea ice to disappear under A2 scenario, means that the simulation showed NO sea ice cover at all, not in any season, not just the disappearance of summer sea ice."  [Govt. of Canada (Reviewer's comment ID #: 2004-92)]	Text clarified on some of these points but unable to cover all within the length constraints of the SPM. Readers need to refer to chapters for that level of detail.
SPM-1221	A	13:1 7	13:1 2	The term "shrink" here seems a little too weak to characterise the loss of ice in the Arctic projected. A range of estimated losses should be given for the SRES scenarios and examples of the summer ice retreat as this is highly relevant to ecosystem and other impacts. [William Hare (Reviewer's comment ID #: 99-118)]	Rejected. Wording ok.
SPM-1222	A	13:1 7	13:1 9	Some models project sea ice to disappear: 1) add 'boreal summer' or 'seasonally', since sea ice does only disappear in summer. 2) 'Some models', in the absence of any sort of quality control of the models used, is in fact a weak statement. One	Taken into account. Wording added. Rejected point 2.

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				could argue that in a suite of good and bad models one will always find one or two that show a strange results, so that doesn't mean a lot. Or in other words, if some project disappearance, and some project an increase, then the statement doesn't mean anything. Suggest something like 'where models on average project a reduction of 80% in summer sea ice by the end of the 21st century, with some models projecting complete disappearance of summer sea ice. [Reto Knutti (Reviewer's comment ID #: 133-47)]	
SPM-1223	A	13:1 7	13:1 7	Sea ice shrinks: suggest 'sea ice is projected to shrink' [Reto Knutti (Reviewer's comment ID #: 133-48)]	Accepted
SPM-1224	A	13:1 7	13:1 9	The first sentence needs to indicate over what period--out at 2100 or what? And then this needs to say on line 18 that "project that summer sea ice will disappear"--so indicate what season. This point also might say something about changes ice thickness and seasonal duration as this will matter for impact studies. [Michael MacCracken (Reviewer's comment ID #: 152-48)]	Taken into account. Wording added. Reject point on impact studies.
SPM-1225	A	13:1 7	:19	Result is given for A2 scenario only. Why not give a range as is done for pH in lines 42-43 on the same page? [Richard Soulen (Reviewer's comment ID #: 248-13)]	Reject. Space limitation.
SPM-1226	A	13:1 8	13:1 8	the A2 scenario is not the one that produces the greatest temperature change by 2100, so is it something to do with the rate of change that this one was picked out? [Joanna House (Reviewer's comment ID #: 109-70)]	Response depends on evolution of forcing not just the endpoint in 2100. This is the way the model results come out.
SPM-1227	A	13:1 8	13:1 9	Change into: "where some models suggest that summer sea ice may disappear completely by the end of the 21st century."  [Govt. of Netherlands (Reviewer's comment ID #: 2016-21)]	OK Similar wording now used.
SPM-1228	A	13:1 8	13:1 8	You might want to replace the phrase "for the A2 scenario" with the phrase "if emissions were to follow the A2 scenario", to enhance appreciation of the fact that for a WG-I perspective, it doesn't actually matter where or why emissions occur - i.e. the scientific projections are robust against any debate about the plausibility of underlying socio-economic scenarios. [Andy Reisinger (Reviewer's comment ID #: 210-46)]	Reject. Wording is shorter and OK
SPM-1229	A	13:1 8	13:1 9	sea ice to disappear - clarify, does this mean all year round, permanently, or for part of the year only, with sea-ice reforming in winter? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-153)]	"late summer has been added to the bullet for clarification
SPM-1230	A	13:1 9	13:1 9	Add at end ."These results are all based on the assumption that greenhouse gases are the only influence on the climate" [VINCENT GRAY (Reviewer's comment ID #: 88-2247)]	Rejected, incorrect. The scenarios consider a number of other drivers as discussed in SRES and chapter 10.

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SPM-1231	A	13:2 1	13:2 1	"upper layer, need to specify depth range(3m?) [Roger Barry (Reviewer's comment ID #: 13-1)]	Text edited
SPM-1232	A	13:2 1	13:2 2	"The statement that up to 90% of the upper layer of permafrost is projected to thaw by 2100 does not make sense (doesn't use terminology that permafrost scientists would use) is misleading and based on results from models with some rather important limitations (see comments on Ch. 10). A more correct statement (which is a better interpretation of the results of the cited studies) would be that increases in thaw depth are projected to occur in response to warming over the next century. "  [Govt. of Canada (Reviewer's comment ID #: 2004-93)]	Text edited.
SPM-1233	A	13:2 1	13:2 1	Please specify the thickness of the layer considered. [Govt. of France (Reviewer's comment ID #: 2010-112)]	SEE COMMENT SPM-1231
SPM-1234	A	13:2 1	13:2 2	The statement about permafrost is difficult to understand. Is the message that melting from top will take place over 90% of the perennially frozen area? Use a better term than "upper layer" (active layer?) and make clear what "90%" refers to (area, thickness?). [Wilfried Haeberli (Reviewer's comment ID #: 94-1)]	Text edited
SPM-1235	A	13:2 1	13:2 2	Where are the sea level rise projections? There needs to be a new para with this in an explaining each of the SLR terms (thermal, glaciers, ice sheets) and comparing this to the TAR. [William Hare (Reviewer's comment ID #: 99-119)]	ACCEPTED: sea level bullet will be added
SPM-1236	A	13:2 1	13:2 2	Given the large range of uncertainty, it is not clear what is meant by "up to 90%". Suggest giving a range and likelihood, or not giving such undefined quantitative information. [Haroon Kheshgi (Reviewer's comment ID #: 125-20)]	Rejected – depends on the scenario – details in the chapter/ section cited.
SPM-1237	A	13:2 1	13:2 2	It is not clear what is meant by "up to 90% of the upper layer of permafrost"--is this in some areas so it refers to coverage, or what? [Michael MacCracken (Reviewer's comment ID #: 152-49)]	Text edited
SPM-1238	A	13:2 1	13:2 1	It is proposed to insert "further" after "are projected to". [Klaus Radunsky (Reviewer's comment ID #: 204-19)]	Reject. Wording ok
SPM-1239	A	13:2 1	13:2 2	The statement that up to 90% of the upper layer of permafrost is projected to thaw by 2100 does not make sense (doesn't use terminology that permafrost scientists would use) is misleading and based on results from models with some rather important limitations (see comments on Ch. 10, comment # 58 to 72). A more correct statement (which is a better interpretation of the results of the cited studies) would be that increases in thaw depth are projected to occur in response	Text edited

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				to warming over the next century. [Sharon Smith (Reviewer's comment ID #: 244-78)]	
SPM-1240	A	13:2 1	13:2 2	The impact of permafrost in terms of GHG emissions and albedo may not be known by policy makers. Therefore, it may be worth adding a brief explanation on the consequences of permafrost thawing. [Philippe Tulkens (Reviewer's comment ID #: 271-3)]	Rejected, little information available and not suitable for SPM
SPM-1241	A	13:2 1		It is proposed to insert "further" after "are projected to". [Govt. of Austria (Reviewer's comment ID #: 2002-19)]	SEE COMMENT SPM-1238
SPM-1242	A	13:2 1	:22	Result is given for A2 scenario only. Why not give a range as is done for pH in lines 42-43 on the same page? [Richard Soulen (Reviewer's comment ID #: 248-14)]	SEE COMMENT SPM-1225
SPM-1244	A	13:2 1		Define "upper layer". Is this 1 cm or 100 m? [Govt. of United States of America (Reviewer's comment ID #: 2023-851)]	SEE COMMENT SPM-1231
SPM-1245	A	13:2 2	13:2 2	You might want to replace the phrase "for the A2 scenario" with the phrase "if emissions were to follow the A2 scenario", to enhance appreciation of the fact that for a WG-I perspective, it doesn't actually matter where or why emissions occur - i.e. the scientific projections are robust against any debate about the plausibility of underlying socio-economic scenarios. [Andy Reisinger (Reviewer's comment ID #: 210-47)]	Reject – wording simpler and OK
SPM-1246	A	13:2 3	13:2 3	seems to be missing a bullet on sea level rise that should go about here [Joanna House (Reviewer's comment ID #: 109-71)]	Accepted. New bullet added.
SPM-1247	A	13:2 4	13:2 8	It might be helpful to indicate that this might be due to the limitations of current models, e.g. because of their limited geographical resolution compared to the area of very high wind speed in such storms. [Govt. of Austria (Reviewer's comment ID #: 2002-20)]	REJECTED: "current models" implies that this statement is based on the current structure and resolution of climate models.
SPM-1248	A	13:2 4	13:2 6	COMMENT: Replace the phrase of "do not suggest substantial increases" with "suggest a global decrease" REASON: keep consistency with Chapter 10.3.6.3.  [Govt. of Japan (Reviewer's comment ID #: 2014-16)]	Accepted. Wording modified.
SPM-1249	A	13:2 4	13:2 8	It might be helpful to indicate that this might be due to the limitations of current models, e.g. because of their limited geographical resolution compared to the area of very high wind speed in such storms. [Klaus Radunsky (Reviewer's comment ID #: 204-20)]	See SPM-1247
0-106	A	13:2 4	13:2 8	SPM-13; line 24-28; turn message round and put second half of the first sentence	Wording improved in similar way

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				in the beginning - start with what models do suggest and not with what models do not suggest [European Commission (Reviewer's comment ID #: 2008-79)]	
SPM-1250	A	13:2 4	13:2 8	To enhance the link to impacts assessments in Working Group 2, and if the authors are comfortable with this statement, it would be helpful to add a sentence in the middle of this paragraph that says "This means that the number of [very intense / category 4 and 5] cyclones would be [likely / very likely] to increase over the 21st century." Or some such sentence that makes it clearer how the statement of "no increase in numbers, but increase in intensity" is to be interpreted from a risk management perspective. [Andy Reisinger (Reviewer's comment ID #: 210-48)]	Too detailed to be well supported by chapter
SPM-1251	A	13:2 4	13:2 8	There is much more information available on cyclones than what can be inferred from this pubblet point. Expand and/or add graphics. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-151)]	Insufficient space in SPM
SPM-1252	A	13:2 4		This is a very important bullet. It should be the first in the list. Does the literature address the issue of longer lived; and if so, that may be the most important finding. The last sentence needs to have the final clauses reversed or rewritten. [Govt. of United States of America (Reviewer's comment ID #: 2023-852)]	REJECTED: insufficient basis in chapter to provide this type of information at the SPM level.
SPM-1253	A	13:2 5	13:2 6	The confidence level on the maximum wind speed change is lower than on the precipitation change. [Govt. of France (Reviewer's comment ID #: 2010-113)]	Reject. Comment incorrect.
SPM-1254	A	13:2 5	13:2 6	The results presented in chapter 10, sub-section 10.3.6.3 (pages 33-35) give a rather more complicated image of model projections. In particular, it appears that the confidence level on the maximum wind speed change is much lower than on the precipitation change. [Serge PLANTON (Reviewer's comment ID #: 199-9)]	See SPM-1253
SPM-1255	A	13:2 5		Earlier in document you left tropical cyclones undefined. Recommend defining it in every place. [Timothy H. Profeta (Reviewer's comment ID #: 203-22)]	See glossary
SPM-1256	A	13:2 6	13:2 7	This sentence is not fully consistent with the doubts about the quality of the historical cyclone data, expressed in the SPM, page 8, lines 54-55 [Govt. of France (Reviewer's comment ID #: 2010-114)]	Reject – here we make it clear that we are talking about post 1970 where data quality is better.
SPM-1257	A	13:2 6	13:2 8	Does this mean current models cannot capture the past pbservations? Suggest use "observed" instead of "reproted". Suggest delete "in the same direction but" [Joanna House (Reviewer's comment ID #: 109-72)]	Accepted
SPM-1258	A	13:2 6	13:2 8	It is not clear in the last sentence of this bullet whether the increase simulated by current models applies to the same time period (i.e. the past, 1970 to 2005), or to	Wording added

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				the future (e.g. from 1990 to 2100?) If the model simulation refers to the past, is this appropriate here or should it rather be stated in the section on observed changes (p8, 152-55)? [Andy Reisinger (Reviewer's comment ID #: 210-49)]	
SPM-1259	A	13:2 6	:27	The sentence is very awkward. Rework. [Govt. of United States of America (Reviewer's comment ID #: 2023-853)]	Text simplified
SPM-1260	A	13:3 0	13:3 5	Are these projections in winter patterns due to increasing greenhouse gases alone or do stratospheric ozone changes have some contribution? [Govt. of Australia (Reviewer's comment ID #: 2001-40)]	Insufficient material in chapter to provide response to this query.
SPM-1261	A	13:3 2		We did not find information on projected sea level rise in this sub-chapter and suggest it be added. [Govt. of Norway (Reviewer's comment ID #: 2018-35)]	Accepted. New bullet added.
SPM-1262	A	13:3 3	:35	Suggest editing sentence to read (underlined text added): "These changes are linked to projections for fewer but more intense storms..." to make it clear the link references a projection as opposed to an observed change. [Govt. of United States of America (Reviewer's comment ID #: 2023-854)]	Similar wording adopted
SPM-1263	A	13:3 4	13:3 4	Comment on "fewer but more intense mid-latitude storms" should note this applies to "both hemispheres" to be consistent with the discussion in Chapter 10 (10-5, lines38-41). However, I would like to see the statement about increasing intensity modified, as the evidence for this is far from robust in the global context from the information supplied in Chapter 10. I am just flagging the issue here (in the SPM), and have given a detailed comment in Chapter 10. [A. Brett Mullan (Reviewer's comment ID #: 182-3)]	Reject first part – text is symmetric already w.r.t hemispheres
SPM-1264	A	13:3 4	13:3 4	Change "with associated damaging winds and extreme wave heights" into "and a spatial shift of the areas with extreme winds and wave heights" [Govt. of Netherlands (Reviewer's comment ID #: 2016-22)]	Reject. Wording ok. See chapter 10.3.6.4
SPM-1265	A	13:3 4	13:3 4	The word "damaging" might be seen as unnecessarily emotive, or straying into an impacts assessment - perhaps better to say "high winds" or another relevant technical term. [Andy Reisinger (Reviewer's comment ID #: 210-50)]	Accepted
SPM-1266	A	13:3 7	13:3 9	Based on Fig SPM-6, for clarity that changes relate to amounts and not area, suggest editing of line 38 to read "...projected patterns of changes in precipitation with, in general, decreased precipitation in dry regions and increased precipitation in wet regions" [Govt. of Australia (Reviewer's comment ID #: 2001-41)]	Accepted - clarified
SPM-1267	A	13:3 7	13:3 9	"Again, this bullet seems unusually vague. Is there nothing that can be said about summer drought for ex? Or about intense precip events? "	REJECTED: based on the sign consistency across the models, illustrated in Fig SPM-6, this is the correct

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				[Govt. of Canada (Reviewer's comment ID #: 2004-94)]	statement.
SPM-1268	A	13:3 7	13:3 8	"Suggest, for readability, adding comma after 'confidence' and after 'TAR'." [Govt. of Canada (Reviewer's comment ID #: 2004-95)]	Wording changed
SPM-1269	A	13:3 7	13:3 7	SPM Comment: To avoid being grossly misleading, replace the phrase "There is now greater confidence" with "The models now show more agreement". Agreement between the models is not – and cannot be – an increase in confidence that the models are behaving correctly. Such agreement is only an indication that the models are converging towards similar behaviour, and this provides no confidence that they are converging towards the behaviour of the real climate. [Richard Courtney (Reviewer's comment ID #: 49-33)]	Rejected – basis for model evaluation given in Ch08
SPM-1270	A	13:3 7	13:3 9	Consideration could be given to including drought and heatwave effects in this para. [William Hare (Reviewer's comment ID #: 99-120)]	Accepted. Drought and heat wave in extremes table.
SPM-1271	A	13:3 7	13:3 9	This sentence is ambiguous and not consistent with earlier statements that pointed to reduced rainfall over tropical and subtropical land areas and increased precipitation over higher latitude land areas. Also, decreases in areas of reduced rainfall (dry areas?) is not obviously consistent with increased frequency of drought. The authors should be more explicit as to what they mean or the paragraph should be deleted. [William Kininmonth (Reviewer's comment ID #: 128-107)]	Rejected, underlying sections referenced provide backup and explain these issues.
SPM-1272	A	13:3 7	13:3 9	This bullet provides a very central climate change conclusion; both precipitation as well as temperature is an important issue. Suggestion: move this bullet after the first bullet on page 13 [Louis Jose Mata (Reviewer's comment ID #: 170-4)]	SPM adopting a standard style of ordering throughout and deals with temperature before precipitation consistently.
SPM-1273	A	13:3 7	13:3 9	"...changes in STREAMFLOW AND precipitation" This increased confidence in streamflow changes is derived from the demonstration by Milly et al. (2005) of skill of climate models in retrospective prediction of observed historical streamflow. This work is documented in sections 8.3 and 9.5, which are already given as justification for the draft SPM statement on precipitation. In fact, those sections do not directly support the precipitation statement, but do directly support a streamflow statement (and therefore indirectly supports the	REJECTED: point of figure SPM-6 is to show obs and model for climatology as a basis on which the response is presented.

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				precipitation statement). As far as I can tell, the cited precipitation studies did not show skill in modeling trends, but the streamflow study did. To assess the credibility of a model's sensitivity, the ability to reproduce historical mean state _and_ trends is more compelling than to reproduce mean state along. For this reason, some variation on Milly et al (2005) figures 2 and 4 might be a better choice than (or a very good complement to) the current SPM-6 figure. [P.C.D. Milly (Reviewer's comment ID #: 179-18)]	
SPM-1274	A	13:3 7	13:3 7	The projected patterns of precipitation change are still quite uncertain. For most of the globe, high latitudes excepted, the projected changes in annual mean precipitation are smaller than the inter-model standard deviation. (See figure SPM-6 and figure 10.3.9a). With respect to the sign of the projected precipitation changes, the model results are somewhat more robust, although over many land areas less than 75% of the models agree with respect to this sign. (See Box 11.1, Figure 2).  [Govt. of Netherlands (Reviewer's comment ID #: 2016-23)]	Reject. Confidence in precip does not just come from Milly paper. REJECT: no action necessary or requested. This is the current state of the art.
SPM-1275	A	13:3 8	13:3 8	"Does ""decreases in dry regions and increases in wet regions"" refer to total sizes of these regions, or to changes in precipitation amounts in them? Please clarify."  [Govt. of Canada (Reviewer's comment ID #: 2004-96)]	Text clarified
SPM-1276	A	13:3 8		Intent not clear. "In general, decreases in the number/area of regions becoming dry, and increase in those becoming wetter??" Dry and wet are associated with soil moisture, not precipitation. [Govt. of United States of America (Reviewer's comment ID #: 2023-855)]	Accepted - Text edited
SPM-1277	A	13:4 0	13:4 0	I think the SPM really should mention the biophysical effects of land use change as a further consideration for projections of future climate change in addition to emissions of greenhouse gases. I suggest "There is increasing confidence that climate change in some regions will be further modified by changes in land surface characteristics as a result of land use change. These changes are unlikely to affect global mean temperatures." [Richard Betts (Reviewer's comment ID #: 21-4)]	REJECT: the link from land use change to change in climate as shown in chapters 7, 8, 9, 10 and 11 is too complex to merit this type of summary
SPM-1278	A	13:4 1	13:4 4	"Some context for the projected change in pH would be nice. How has pH of the Ocean varied over time? Do we know this? How do the projections compare to past rates (of natural change)?"  [Govt. of Canada (Reviewer's comment ID #: 2004-97)]	Taken into account. More information added to SPM from ES of chapter 7.

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SPM-1279	A	13:4 1	13:4 5	glad to see a bullet on coean acidification here. I think this is an improtant issue. [Joanna House (Reviewer's comment ID #: 109-73)]	Thank you
SPM-1280	A	13:4 1		Add a bullet describing sea level rise in the 21st century. The projected range of 0.14 - 0.43 m given in SPM-15, line 21 is not well quantified in Chapter 10. Recent publications on the possible contributions of melting of glaciers and ice-sheets are not thoroughly assessed. It is not clear how the uncertainty in these numbers is obtained. [Govt. of Netherlands (Reviewer's comment ID #: 2016-24)]	Taken into account. Bullet added and chapter 10 text will be brought forward to SPM.
SPM-1281	A	13:4 1	:44	This bullet is confusing. The increase in pH occurs 'over' the 21st century, not sometime 'in' the middle of... Add "many" before "marine". Is the 0.1 decrease since pre-industrial times an observation or just a model calculation? "marine calcifying organisms" seems too technical for the SPM. At minimum, add to glossary. [Govt. of United States of America (Reviewer's comment ID #: 2023-856)]	pH in Glossary. Marine calcifying organisms removed
SPM-1282	A	13:4 2	13:4 3	I see different numbers in Chapter 10 (page 38, line 45). [Gerrit Burgers (Reviewer's comment ID #: 34-11)]	Taken into account. Chapter 10 and SPM will be consistent
SPM-1283	A	13:4 2	13:4 4	This paragraph states that the increasing acidification of the surface ocean is "raising concerns for marine calcifying organisms." However, it assumes that the policymaker understands the relationship between increased ocean acidification and the associated decrease in the saturation state of CaCO <sub>3</sub> in the ocean. Suggest to add further information about the corresponding effects on marine calcifying organisms due to an increase in CO <sub>2</sub> saturation in the surface ocean. [Govt. of Japan (Reviewer's comment ID #: 2014-17)]	Partly accepted. Text changed. Full effects are in scope of WG2 though
SPM-1284	A	13:4 3	13:4 5	Rephrase to: "..., adding to the presently reduced level of 0.1 units below pre-industrial times ...". [Govt. of Netherlands (Reviewer's comment ID #: 2016-25)]	Similar text change made
SPM-1285	A	13:4 4	13:4 4	SPM Comment: To avoid being grossly misleading, delete the phrase ", and raising concerns for marine calcifying organisms". If such concerns were raised then they would be mistaken.  [Richard Courtney (Reviewer's comment ID #: 49-34)]	Rejected, no basis given for assertion.
SPM-1286	A	13:4 4	13:4 4	Add to this sentence something like " and with conditions detrimental to high-latitude ecosystems within decades, not centuries as suggested previously" from page 10-38 lines 55-57 as this adds clarity and depth the conclusions reported here.	Reject. Length limitations. Too specific and WG I not qualified to discuss ecosystem responses.

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				[William Hare (Reviewer's comment ID #: 99-129)]	
SPM-1287	A	13:4 4	13:4 5	The phrase "and raising concern..." might be seen as unnecessarily emotive, or straying into an impacts assessment. Perhaps better to have a full stop after "from pre-industrial times.", followed by a new sentence: "Such changes in pH have the potential to affect marine calcifying organisms and influence [reduce / impede?] the carbon cycle in the ocean." [Andy Reisinger (Reviewer's comment ID #: 210-51)]	Accepted. Wording changed.
SPM-1288	A	13:4 4		marine calcifying organisms of major ecological and biogeochemical significance. [Stephen J. Hawkins (Reviewer's comment ID #: 102-21)]	SEE COMMENT SPM-1283
SPM-1289	A	13:4 4		Recommend providing examples of what marine calcifying organisms are, and examples of the threats to them. [Timothy H. Profeta (Reviewer's comment ID #: 203-23)]	Rejected, WG2
SPM-1291	A	13:4 6	13:4 6	There is no bullet about projections for sea level rise??? OK - found on page 15. Shouldn't it be with other projections? [Govt. of Canada (Reviewer's comment ID #: 2004-98)]	Accepted. Bullet added.
SPM-1292	A	13:4 6	13:4 6	"It would also be nice to have in this section a paragraph stating something about how or whether natural forcings may factor into climate change of the coming century. Do we expect them to have a negligible/small/large influence, relative to that of GHGs? " [Govt. of Canada (Reviewer's comment ID #: 2004-99)]	Rejected.Space limitations.
SPM-1293	A	13:4 6	13:4 6	"There is no bullet about projections for the THC? OK - found on next page. Shouldn't this bullet be with the other projections?" [Govt. of Canada (Reviewer's comment ID #: 2004-100)]	Accepted. Bullet moved.
SPM-1294	A	14:0		figure SPM-6: It is proposed to show also the impact of another emission scenario in order to show the effect of reducing emissions. Furthermore it might be helpful to transform this graph into relative changes in precipitation compared to current level in precipitation. If this is not feasible relation to yearly average precipitation should be shown and not for daily average values. [Govt. of Austria (Reviewer's comment ID #: 2002-22)]	Not practical to show more than one scenario. Accepted that relative change is better. Figure changed.
SPM-1295	A	14:0		Figure SPM-6: put panels relating to the past furst. The panes realting to the past also need a headline time period above them as for the future panel, and should also be labelled above "observed" and "Modelled" - this makes it easier for reader to pick out fast what its all about. Key for past panels need "mm/day"	Taken into account. Figure changed

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				Suggest changing title to make it a bit more readable "Mean annual precipitation in the recent past (1979-1993) and modelled change by the end of the century (2050-2099). [Joanna House (Reviewer's comment ID #: 109-74)]	
SPM-1296	A	14:0		Figure 6. I don't like the black and white panels here - it is hard to tell one end of the colour scale from the other. [Chris Jones (Reviewer's comment ID #: 120-2)]	Taken into account. Figure changed
SPM-1297	A	14:0		Fig. SPM6: Why is the bottom part in black and white? Couldn't all the panels be in the same color scheme and also the same map projection? [Reto Knutti (Reviewer's comment ID #: 133-49)]	Taken into account. Figure changed
SPM-1298	A	14:0		figure SPM-6: It is proposed to show also the impact of another emission scenario in order to show the effect of reducing emissions. Furthermore it might be helpful to transform this graph into relative changes in precipitation compared to current level in precipitation. If this is not feasible relation to yearly average precipitation should be shown and not for daily average values. [Klaus Radunsky (Reviewer's comment ID #: 204-22)]	Not practical to show more than one scenario in this format
SPM-1299	A	14:0		Figure SPM-6. It would be more helpful to provide the precipitation change in mm/year or mm/month, since this is the basis on which most popular rainfall statistics are given. It would probably be even more relevant if the figure could show precipitation changes in percent, but I understand that this may throw up additional uncertainties and data gaps. [Andy Reisinger (Reviewer's comment ID #: 210-54)]	Taken into account. Figure changed
SPM-1300	A	14:1	14:1	The text on the top plot should read "Change in the annual-mean precipitation rate between 1980-1999 and 2080-2099." [Olivier Boucher (Reviewer's comment ID #: 27-14)]	Figure has been redrawn and corrected.
SPM-1301	A	14:1	14:1	Figure SPM-6: I strongly advise to reverse colors in the top panel of this Figure, since it is more natural to show positive and negative changes by the red and blue colors, respectively (i.e., opposite to the current color scale). Two bottom panels should be made color, since their current grey-black version is confusing because their low (0-2) and high (10-11) values look practically the same. [Michael Danilin (Reviewer's comment ID #: 55-5)]	In this case more natural to use bluish colors for increased precip and reddish for decreased
SPM-1302	A	14:1	14:1 9	The lower two graphs are absolutely useless, and should be deleted. An explanation is needed why the A1B scenario is selected. Concerning the upper graph, it does not say anything in itself whether more or less precipitation is good or bad. Maybe encircling some regions where the changes can be especially good or bad could represent a message to policy makers.	Figure changed – those panels dropped

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				[Govt. of Hungary (Reviewer's comment ID #: 2012-6)]	
SPM-1303	A	14:1	14:1	I think the key should be labeled "Change in annual mean precipitation"--running too many words together can get confusing for the average reader. [Michael MacCracken (Reviewer's comment ID #: 152-50)]	Figure and caption changed
SPM-1304	A	14:1	14:1	Top figure SPM-6 The top figure does not appear in Chapters 10 and 11 and should be replaced by the original figure 10.3.9a.  [Govt. of Netherlands (Reviewer's comment ID #: 2016-26)]	Figure and caption changed
SPM-1305	A	14:1	14:1 2	The bottom two panels and scales of Fig. SPM-6 need to be in color. As it is, both high and low values are white. -Alan Robock, Rutgers University [Alan Robock (Reviewer's comment ID #: 217-3)]	Figure changed
SPM-1306	A	14:1	:19	The order of the figures should be changed. Put the 1979-1993 figures on top and add suitable captions (e.g., Annual Mean Precipitation Rate 1979-1993 covering both and Observed and Multimodel Mean identifying them individually). This establishes a degree of confidence in the models. Then put the 2080-2099 figure underneath with a suitable caption (e.g., Multimodel Mean Estimate for 2080-2099). This corresponds with the order in which they are discussed in the caption. [Richard Soulen (Reviewer's comment ID #: 248-15)]	Taken into account. Figure changed
SPM-1308	A	14:1 7	14:1 7	For decision makers who might be confused with the scenario naming, explicit that A1B is a middle range scenario. [Govt. of France (Reviewer's comment ID #: 2010-115)]	Have added a box explaining SRES
SPM-1309	A	14:2 2	14:2 3	"This section seems a conclusion and would be helpful for policy makers to draw a quick inference. Title should be changed to Conclusions or something similar to this instead of a sentence."  [Govt. of Canada (Reviewer's comment ID #: 2004-101)]	The section is not a conclusion – SPM's do not have them
SPM-1310	A	14:2 2	14:2 2	SPM Comment: Replace "an important" with "a". It is a value judgement as to whether this is an important context, and many – including me – do not think it is. Indeed, it is hard to comprehend how "current understanding of climate processes" could be an important context for "climate stabilization for considering policy options that might lead to climate stabilization" when climate has never been stable and stabilization of climate is – to put it very mildly – ambitious.  [Richard Courtney (Reviewer's comment ID #: 49-35)]	Rejected. Text presents physical principles, not plausibility, as already explained.

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SPM-1311	A	14:2 2	14:2 3	Again, this sentence in bold letters is too general and does not give any relevant information. Replace by main messages with regard to long-term projections. [Govt. of Germany (Reviewer's comment ID #: 2011-240)]	See spm 1315 for a possible approach
SPM-1312	A	14:2 2	14:2 3	This headline is quite wishy-washy, no real information and doesn't seem to fit well with bullet points below. Also doesn't read well for IPCC to tell policy makers what is important for them, we present the facts, they decide what is important. [Joanna House (Reviewer's comment ID #: 109-75)]	Accepted – text changed
SPM-1313	A	14:2 2	14:2 3	I'm not sure that reference to "policy options" is necessary or helpful here. Perhaps an alternative might be: "Current understanding of climate processes provides important information for any decisions related to future stabilisation of greenhouse gas concentrations at a range of possible levels". The authors could also consider whether they might be comfortable, instead of talking about "future stabilisation of greenhouse gas concentrations at a range of possible levels", to say "...meeting the objectives and provisions of the UNFCCC" - which is wording that the IPCC Panel chose to refer to long-term perspectives in a chapter heading for the Synthesis Report - but then again this wording may cause more problems here than it tries to solve. [Andy Reisinger (Reviewer's comment ID #: 210-55)]	Accepted – text changed
SPM -1314	A	14:2 2	15:3 9	This last part should be rewritten. In our view it should reflect the key findings in the WGI report related to the risk for dangerous global warming in a more understandable language. It should also be said that the magnitude of climate change during the next 100 years related to temperature etc. will depend on the level of GHG emissions during this period. [Govt. of Norway (Reviewer's comment ID #: 2018-36)]	See spm1315
SPM-1315	A	14:2 2		Section on "context for policy options". The TS contains a very important statement that should be included in the "policy context" section of the SPM. This statement is "Model results suggest that 21st century emissions will provide a long-lasting commitment to climate change for multiple centuries, irrespective of later emissions." This could be further elaborated by using the first sentence of bullet page 15 lines 1-2 as part of this new bullet point. [Andy Reisinger (Reviewer's comment ID #: 210-71)]	Taken into account.
SPM-1316	A	14:2 2		Section on "context for policy options". I do feel the need for this section to repeat some information that was already stated in the TAR, namely that to stabilise concentrations, emissions would have to decline and eventually fall to very low levels (and that to reduce concentrations, emissions would have to fall even further and even more rapidly). I realise that this isn't new scientific	Accepted. Added new bullet from chapter 10. Information from figure 10.4.3.

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				information per se. I'm making this comment mainly from the perspective of the Synthesis Report, where we will have to pull together information about the climate system, and how this matches with emission and policy options that may or may not lead to stabilisation at a range of levels. So from this perspective, it would be very helpful to have a brief bullet point, substantiated of course by relevant information in the TS and underlying chapters, that makes this point and that can act as a reference point in discussions on long-term issues. [Andy Reisinger (Reviewer's comment ID #: 210-72)]	
SPM-1318	A	14:2 2		Do not see how process knowledge has much to do with "policy options." It does not apply to all the sub-bullets here. The processes do not necessarily lead to better understanding of climate stabilization either. The simple statement that "Stabilization of radiative forcing is a prerequisite for climate stabilization" should be a separate bullet, perhaps immediately after line 23. [Govt. of United States of America (Reviewer's comment ID #: 2023-857)]	Material being reordered and should deal with this concenter. The stabilization of RF will be pulled out as a separate bullet
SPM-1319	A	14:2 2	:23	This section should include mention of the level of certainty of expected climate change on regional scales relative to global scales. [Govt. of United States of America (Reviewer's comment ID #: 2023-858)]	Reject. Insufficient information in chapter 11 or papers.
SPM-1320	A	14:2 5	14:2 5	What is meant by "very long-term"? [Govt. of Canada (Reviewer's comment ID #: 2004-102)]	Accepted. Text changed.
SPM-1321	A	14:2 5	14:3 1	Since I deeply understand some people are very nervous about description of stabilization level from viewpoint of political issues, I think the second draft of SPM sounds good because we can find understatement related to the stabilization level. However, I wonder too many new findings about "the dangerous anthropogenic interference with climate system" might give strong negative information for pessimistic people and young students. Therefore, I recommend the following positive information might be added in the sentence; "Stabilization of radiative forcing is a prerequisite for climate stabilization. Changes in sea level, ocean and ice sheets will continue for centuries or longer. However, if radiative forcing will be reduced, there is possibility of mitigation of these climate changes." [Koki Maruyama (Reviewer's comment ID #: 169-3)]	Reject. The balance between SRES and possible mitigation in the 21st century is covered in the previous section. This section deals with timescales and inertia so the comment seems misplaced.
SPM-1322	A	14:2 5	14:3 1	It is not clear to me what this bullet point really wants to say. Is it inertia, or the fact that despite uncertainties there is also robust knowledge, or that changes are already happening? It might be useful ensure that each bullet carries only one key message, especially in this last section.	Taken into account. Text modified.

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				[Andy Reisinger (Reviewer’s comment ID #: 210-56)]	
SPM-1323	A	14:2 5	14:3 1	It really needs an illustration to reinforce this very important point, and get beyond the erroneous idea that "it'll all be fine after 2100" which many people mistakenly got from the TAR SPM. Fig TS-31 from the TS would be suitable. [Govt. of United Kingdom (Reviewer’s comment ID #: 2022-154)]	Unfortunately there is insufficient space to add another figure.
SPM-1324	A	14:2 5		First sentence is wrong. The major issue is the projection of greenhouse gases (more important than climate feedbacks). [Govt. of United States of America (Reviewer’s comment ID #: 2023-859)]	Taken into account. Text modified.
SPM-1325	A	14:2 5	:27	“Very long-term...”? How long is that? “more uncertainty” than what? Delete “of slow feedbacks” because it is not correct. Fast feedbacks are also not well observed or understood. “processes” alone likely captures intent. Inertia needs a new section or bullets, and should not to be tacked onto this bullet as a throwaway. There are too many different points being made, some of which even conflict (e.g., the observations sentence and the stabilization one). [Govt. of United States of America (Reviewer’s comment ID #: 2023-860)]	Partly accepted. Text will be made more specific on time scale and uncertainty. Reject- slow feedbacks – ice sheets and etc.
SPM-1326	A	14:2 7	14:2 8	Whether present understanding provides useful input for policy is at best a value judgement for policymakers which IPCC should not be making and at worst a policy statement that policy action is justified, which IPCC should certainly not be making. [David Griggs (Reviewer’s comment ID #: 90-6)]	Accepted. Text deleted.
SPM-1327	A	14:2 7	14:2 7	it is for policy makers to decide what is useful not IPCC to tell them [Joanna House (Reviewer’s comment ID #: 109-76)]	Accepted. Text deleted.
SPM-1329	A	14:2 8	14:3 0	""Observations and models show that climate is changing and is expected to continue to change."" This kind of sentence is not very useful, and is another example of the vague language in use in this draft. Need to put human forcing into context with natural climate change. Similarly, the next sentence that says ""stabilization of radiative forcing"" is too vague - surely it needs to be specified that it is RF from GHGs that needs to be stabilized - we can't do much about the other climate forcings."  [Govt. of Canada (Reviewer’s comment ID #: 2004-103)]	Accepted. Text deleted.
SPM-1330	A	14:2 8	14:2 9	delete sentence "observational models....continue to change" - already said this [Joanna House (Reviewer’s comment ID #: 109-77)]	Accepted
SPM-1331	A	14:2 9	14:3 0	SPM Comment: Delete the sentence “Stabilisation of radiative forcing is a prerequisite for climate stabilization.” because it is ridiculous. Climate has never been stable and stabilization of climate is – to put it mildly –	Rejected. Clearly other reviewers as well as the authors see this as fundamental in relation to anthropogenic climate change.

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				ludicrously ambitious.  [Richard Courtney (Reviewer's comment ID #: 49-36)]	
SPM-1332	A	14:29	14:30	This seems to me a little more obvious and missing out the more important point to make I.e. stabilising concentrations as a pre-requisite for stabilising climate, and a pre-requisite for this is substantial drop in emissions - for CO2 to almost nothing! [Joanna House (Reviewer's comment ID #: 109-78)]	Partly accepted. Text will draw the linkage to stabilizing ghg concentrations.
SPM-1333	A	14:29	14:31	The first sentence is only true if recent climate change has been as a result of forcing. If the observed climate change has been a consequence of internal variability then climate stabilization is a mirage - we should expect variability at least of the magnitude that has occurred from the Little Ice Age to the present. The second sentence is inconsistent with high quality observations over recent decades that show rapid changes in global sea level (25 mm between 1994 and 1998 with a subsequent 15 mm fall over the next two years - Topex/Poseidon), in ocean circulation (ENSO and the subtropical ocean gyres, including the Gulf Stream), and of glaciers with significant advance and retreat over just a few years. Suggest the sentence be deleted. [William Kininmonth (Reviewer's comment ID #: 128-108)]	Accepted that the text needs to distinguish natural variability from forced change. But the statements here are explicitly about long term change so the reviewers examples are not relevant.
SPM-1334	A	14:29	14:31	Add a statement indicating the need to reduce emissions in order to stabilize atm. CO2 concentrations, radiative forcing and climate. For example write "Stabilization of radiative forcing is a prerequisite for climate stabilization, and requires that emissions are reduced below present levels in the future". [Gian-Kasper Plattner (Reviewer's comment ID #: 200-6)]	Rejected due to length constraints and as is dealt with in more detail by WG3
SPM-1335	A	14:30	14:31	"For clarity say, ""will continue for centuries or longer even with stabilisation of radiative forcing"". " [Govt. of Canada (Reviewer's comment ID #: 2004-104)]	Text will be edited
SPM-1336	A	14:31	14:31	Add "after forcing is stabilized" to this sentence. [William Hare (Reviewer's comment ID #: 99-121)]	Text will be edited
SPM-1338	A	15:0	15:0	An overall comment about the projections is that they should incorporate the best available scientific evidence, not just model results. At present the reader gets the impression that they are just model results. In many cases, model results simply are not credible at this point; for example, ice flow models. [Jeffrey Severinghaus (Reviewer's comment ID #: 232-4)]	No specific suggestion, but this section is explicitly intended to diminish focus on AOGCMs and point to basic understanding of climate – so should meet the concern
SPM-1339	A	15:1	15:25	the several uses of word 'commitments' on this page seems to have very different meanings.	Accepted. Word deleted.

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				[Govt. of Australia (Reviewer's comment ID #: 2001-42)]	
SPM-1340	A	15:1	15:1	The first sentence should be expanded in an ad-hoc bullet, eg taken from the TS, page 41, line 15 to 19 : "Model results suggest that 21st century emissions will provide a long-lasting commitment to climate change for multiple centuries, irrespective of later emissions. If emissions were to cease in 2100, emissions that occurred in the 21st century are expected to continue to have an impact even at year 3000, when both surface temperature and sea level rise due to thermal expansion are still projected to be substantially higher than preindustrial." This is policy relevant and deserves being included in this SPM. [Govt. of France (Reviewer's comment ID #: 2010-116)]	Taken into account. Text modified.
SPM-1341	A	15:1	15:7	The CO2 life time issue is separated from the coupled climate-carbon cycle issue. I would rather see the climate-carbon results on page 13 line 40 (just above the ocean acidification result). [Pierre Friedlingstein (Reviewer's comment ID #: 77-43)]	Partly accepted. Text in earlier section notes CC – CC feedbacks in relation to 21st c warming. However, need text here to note role of feedbacks on emissions consistent with a stabilization level.
SPM-1342	A	15:1	15:2	A sentence or two needs to be added after this one to qualify it to deal with different concepts of commitment. Something like "When emissions of CO2 are reduced to zero, radiative forcing drops and soon after global mean temperature, but there is still a long term commitment to sea level rise." [William Hare (Reviewer's comment ID #: 99-122)]	"Commitment" deleted but other issues will be covered
SPM-1343	A	15:1	15:1	The term "commitment" is not normally used to define the response of the carbon cycle, it is used to define the temperature lag due to heat uptake by the ocean that is modeled to diminish with time. Suggest using another term rather than commitment. What this does appear to be is an evaluation of the so-called overshoot stabilization scenarios. If so, then overshoot scenarios should be clearly assessed, and the potential for engineered carbon sinks with technologies developed over the next several centuries evaluated. [Haroon Kheshgi (Reviewer's comment ID #: 125-21)]	Accepted. Commitment deleted..
SPM-1344	A	15:1	15:7	Carbon dioxide does not have a long lifetime in the atmosphere. The mean residence time is measured in years. Elevated concentrations due to human emissions are expected to persist longer (possibly for centuries) because of the apparent slow time over which the biological and ocean trajectories will respond to enhanced carbon dioxide concentration and increase their respective uptakes. Better wording for the first sentence would be: "The slow response time of the oceans and biosphere to increased atmospheric carbon dioxide emissions means that elevated concentrations of atmospheric carbon dioxide may persist for centuries. Increases in global temperature are expected to progressively reduce the ability of the oceans and biosphere to absorb anthropogenic carbon dioxide	Rejected. There is a large body of literature showing the very long timescales associated with CO2 removal and this is covered in Chapter 7. It is necessary to separate the long timescales in the carbon cycle from those in the climate system – and the suggested text would confuse those.

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				emissions." The rest of the paragraph is conjecture and should be deleted. [William Kininmonth (Reviewer's comment ID #: 128-110)]	
SPM-1345	A	15:1	15:1	I would suggest indicating here that this applies to not only carbon dioxide, so would change this to say "carbon dioxide and several other important greenhouse gases (and then give a list)"--to really accomplish anything, we are going to have to do much more than just limit CO2. [Michael MacCracken (Reviewer's comment ID #: 152-51)]	Partly accepted. Text will refer to all GHGs but no space to give a list
SPM-1346	A	15:1	15:7	This bullet point seems to contain two distinct messages, the first being in line 1/2 (long lifetime of CO2), and the other being in the rest (system feedback leading to reduced CO2 uptake). It might be helpful to see if the message from the first 2 lines can't be included in another general bullet point on inertia elsewhere. I'm also a little concerned that the way the word "commitments" is used here could be misunderstood by some people to mean a commitment to having to reduce emissions. Perhaps an alternative phrase could be "...implies a commitment to further climate change..." [Andy Reisinger (Reviewer's comment ID #: 210-57)]	Taken into account. Bullet seperated.
SPM-1347	A	15:2	15:7	It's not entirely clear from this bullet point whether the 1.2°C additional warming is already incorporated in GCMs and the results of Figure SPM-5 and the bullet point on page 13 lines 1-4. Does page 13 lines 1-4 give the current best estimate of future warming, or is this estimate already known to be a systematic underestimate because of these unaccounted for feedbacks? [Andy Reisinger (Reviewer's comment ID #: 210-58)]	Accepted. Line moved. Text for carbon feedback is now clear.
SPM-1348	A	15:3	15:4	The current language is not clear enough weather whether or not this positive feedback effect has been included in the figures presented so far. [Govt. of Austria (Reviewer's comment ID #: 2002-23)]	Accepted. Text modified.
SPM-1349	A	15:3	15:5	compared to the SRES seanrio projections given, or compared to re-doing the projections without feedbacks? I thought these feedbacks were already in the models, so should make it clear whether or not this effect has been accounted for. [Joanna House (Reviewer's comment ID #: 109-79)]	Accepted. Text modified.
SPM-1350	A	15:3	15:5	Suggest reporting the range of model results for added warming due to carbon/climate feedback, and stating that this range is an included contributor to the SRES temperature ranges reported on SPM page 13, lines 6-10 (unless it is not, then this should be added as a caveat to that statement on page 13). Also suggest that this statement be moved to page 13 following that statement since it is on SRES, and not on stabilization. [Haroon Kheshgi (Reviewer's comment ID #: 125-22)]	Accepted. Text moved.

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SPM-1351	A	15:3	15:3	Delete: “anthropogenic” and “emissions”. [Govt. of Netherlands (Reviewer’s comment ID #: 2016-27)]	Text revised for other reasons
SPM-1352	A	15:3	15:4	The current language is not clear enough whether or not this positive feedback effect has been included in the figures presented so far. [Klaus Radunsky (Reviewer’s comment ID #: 204-23)]	Accepted. Text modified.
SPM-1353	A	15:4	15:4	This sentence needs to be made more clear and consistent with the rest of the SPM. The added warming by 2100 would be for an estimate that does not include the carbon-climate feedback. But estimates given on page 13 do include the carbon cycle feedback. [Olivier Boucher (Reviewer’s comment ID #: 27-15)]	Accepted. Text modified.
SPM-1354	A	15:4	15:4	"Why no range on the 1.2 degree Celcius number? Suggest, for consistency of presentation, that sentence be reworked to show a temperature range." [Govt. of Canada (Reviewer’s comment ID #: 2004-105)]	Text will make clearer that 1.2C is the upper limit for cases run
SPM-1355	A	15:4	15:5	"In reference to the ""added warming"", there should be a brief explanation of what this warming is added to." [Govt. of Canada (Reviewer’s comment ID #: 2004-106)]	Text clarified
SPM-1356	A	15:4	15:4	1.2° is inconsistent with the 1.5° figure mentioned chapter 10, page 37, line 15 [Govt. of France (Reviewer’s comment ID #: 2010-117)]	Text has been corrected
SPM-1357	A	15:4		How likely is this added 1.2°C? Uncertainty qualifiers must accompany numbers like this in the SPM. [Govt. of United States of America (Reviewer’s comment ID #: 2023-861)]	Text edited.
SPM-1358	A	15:5	15:7	The following language is proposed: This feedback effect also reduces the total emissions .... [Govt. of Austria (Reviewer’s comment ID #: 2002-24)]	Accepted that text needs to be improved
SPM-1359	A	15:5	15:5	"For clarity, say ""Alternatively, this feedback reduces the total emissions associated with""" [Govt. of Canada (Reviewer’s comment ID #: 2004-107)]	Accepted that text needs to be improved
SPM-1360	A	15:5	15:7	The following language is proposed: This feedback effect also reduces the total emissions .... [Klaus Radunsky (Reviewer’s comment ID #: 204-24)]	SEE COMMENT SPM-1358
SPM-1361	A	15:5	:7	Difficult to discern that “Alternatively” did not refer to an alternative to the positive feedback effect but instead to an alternative emission scenario (stabilization instead of SRES). Since carbon dioxide stabilization is not	Accepted that text needs to be improved – but suggested text not accurate

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				mentioned elsewhere in the SPM, why not delete this sentence? Or there may be a simpler statement possible: "The presence of positive feedbacks in the carbon cycle means that further increases in CO2 emissions will lead to more than proportional increases in atmospheric concentrations." [Govt. of United States of America (Reviewer's comment ID #: 2023-862)]	
SPM-1362	A	15:5		What does "it" reference, the "feedback effect"? [Govt. of United States of America (Reviewer's comment ID #: 2023-863)]	Accepted that text needs to be improved
SPM-1363	A	15:6	15:6	suggest start a new sentence I.e. "...stabilisation level. There are still uncertainties..." [Joanna House (Reviewer's comment ID #: 109-80)]	Accepted that text needs to be improved
SPM-1364	A	15:7	15:7	where a link is shown to section [7.3], there should also be a link to the carbon cycle projections section in chapter 10: [10.4] [Chris Jones (Reviewer's comment ID #: 120-3)]	Accepted
SPM-1365	A	15:7	15:7	Refer here also to section 10.4. The numbers at 10-37-12 are slightly different, but probably that is an issue for the authors of Chapters 7 and 10 rather than the authors of the SPM. [Govt. of Netherlands (Reviewer's comment ID #: 2016-28)]	ACCEPT: add ref to 10.4
SPM-1366	A	15:7		Refer also to section 10.4, [Gerrit Burgers (Reviewer's comment ID #: 34-12)]	SEE COMMENT SPM-1364
SPM-1367	A	15:9	15:9	It is proposed to insert "also" after "Human activities affect". [Govt. of Austria (Reviewer's comment ID #: 2002-25)]	This bullet deleted for other reasons
SPM-1368	A	15:9	15:1 1	This bullet is unclear and not very meaningful. It can be either dropped or made more specific. [Olivier Boucher (Reviewer's comment ID #: 27-16)]	Accepted – Bullet dropped
SPM-1369	A	15:9	15:1 2	This sentence is so scientifically weak that you may want to remove it. [Pierre Friedlingstein (Reviewer's comment ID #: 77-44)]	Accepted – Bullet dropped
SPM-1370	A	15:9	15:1 1	this bullet is quite vague. C Could do with some key examples I think one point here that is not made is that all these constituents are highly reactive in the atmosphere, and they simultaneously affect climate and air quality, that climate changes could exacerbate air quality (e.g. with poorer air quality in for warmer temperatures I think), and air quality can affect climate directly (e.g. air pollution leading to increasing levels of trop O3 a greenhouse gas) and indirectly (eg. trop O3 levels affect the lifetime of CH4 in the atmosphere) [Joanna House (Reviewer's comment ID #: 109-81)]	. Bullet dropped
0-108	A	15:9	15:1 1	SPM-15; line 9-11; para can be misunderstood without quantification of the uncertainty in relation to certainty and the sign of the respective influencing	Bullet dropped

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				factor/gas. A few lines of further explanation seem to be needed here; [European Commission (Reviewer's comment ID #: 2008-80)]	
SPM-1371	A	15:9	15:9	It is proposed to insert "also" after "Human activities affect". [Klaus Radunsky (Reviewer's comment ID #: 204-25)]	See SPM-1367
SPM-1372	A	15:9	15:1 1	Would it be possible to include "land surface changes" in this list? [Andy Reisinger (Reviewer's comment ID #: 210-59)]	Rejected. Land surface changes small relative to GHG forcing by 22nd century (focus of this section).
SPM-1373	A	15:9		Important to note the other greenhouse gases, but this bullet is fairly weak. Strengthen the bullet and clearly connect to air quality. [Govt. of United States of America (Reviewer's comment ID #: 2023-864)]	Bullet dropped
SPM-1374	A	15:9	:11	Include "in addition to carbon dioxide" after "aerosols" [Govt. of United States of America (Reviewer's comment ID #: 2023-865)]	Unnecessary
SPM-1375	A	15:1 1	15:1 2	This last sentence of this bullet is either obvious, or obscure, depending on the background of the reader. The point is covered better in the following bullet. [Dennis Hartmann (Reviewer's comment ID #: 100-8)]	Reject – the next bullet is on an entirely different topic
SPM-1376	A	15:1 3	15:2 3	It is noted that weakening of MOC by up to 60% is quite significant and would require further consideration with regard to its consequences. This might also have an impact on the formation of hurricanes in the Carribic, as the MOC is also the mechanism to remove heat from this area. [Govt. of Austria (Reviewer's comment ID #: 2002-26)]	Reject. Speculative and insufficient evidence to be considered for the SPM.
SPM-1377	A	15:1 3	15:1 4	"In predicting weakening of the Atlantic MOC, it would be helpful to policymakers to briefly explain why this is important (i.e. what it does)."  [Govt. of Canada (Reviewer's comment ID #: 2004-108)]	Accepted text changed
SPM-1378	A	15:1 3	15:1 3	Refer to terms more widely used and known by policymakers, e.g. Thermohaline circulation [Govt. of Germany (Reviewer's comment ID #: 2011-241)]	MOC is observable – THC is conceptual – see Glossary. Literature now emphasises MOC.
SPM-1379	A	15:1 3	15:1 7	Models that do not include the meltwater from Greenland: needs to be qualified. [William Hare (Reviewer's comment ID #: 99-123)]	Rejected. Text ok.
SPM-1380	A	15:1 3	15:1 7	This bullet belongs earlier with imacts text, it is not a stabilisation model result. [Joanna House (Reviewer's comment ID #: 109-82)]	Accepted. Text moved
SPM-1381	A	15:1 3	15:1 6	This needs a bit of additionla explanation. Suggest inserting a sentence after the first "This will decrease the SN flow of warm water and decrease temeprature in NAO and Europe." [Joanna House (Reviewer's comment ID #: 109-83)]	Explanation of MOC signficance now added
SPM-1382	A	15:1 3	15:2 3	It is noted that weakening of MOC by up to 60% is quite significant and would require further consideration with regard to its consequences. This might also	See SPM-1376

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				have an impact on the formation of hurricanes in the Carribic, as the MOC is also the mechanism to remove heat from this area. [Klaus Radunsky (Reviewer's comment ID #: 204-26)]	
SPM-1383	A	15:13	15:17	The last sentence of this bullet point is a bit cryptic, especially given the amount of media coverage and public and policy interest that has been placed on this topic. The authors might wish to consider the following alternative, following from the second-to-last sentence: "..., but most models indicate that the MOC would slow down further beyond 2100 if warming is sustained. Knowledge is currently insufficient to determine whether a MOC slow-down and potential shut-down could be reversed within a few centuries after greenhouse gas concentrations have stabilised." [same reference sections] [Andy Reisinger (Reviewer's comment ID #: 210-60)]	Taken into account. Text modified
SPM-1384	A	15:13	15:17	The models referred to in this sentence lack representations of critical physics for the meridional overturning circulation (MOC), such as overflows where deep water forms and the details of convection, due to inherent computational limits. The models also fail to reproduce the observed abrupt changes seen in the paleoclimate record, which may be related to changes in the MOC. For these reasons this conclusion must be qualified. I suggest "No models suggest a shutdown of the MOC in the 21st century; however, the models lack critical representations of the physics of deepwater formation and fail to reproduce known past abrupt changes, implying that these conclusions remain tentative." [Jeffrey Severinghaus (Reviewer's comment ID #: 232-1)]	PARTLY ACCEPT: start bullet with "Current models", implying the current state of physical representation.
SPM-1385	A	15:13	:14	There's a need to explain what the implications are of the Atlantic meridional overturning circulation (MOC) by up to 60% by 2100. Consider linking to abrupt climate change. [Govt. of United States of America (Reviewer's comment ID #: 2023-866)]	Implications of MOC change have been added. Reject that there is a need to link to abrupt climate change here.
SPM-1386	A	15:14	15:15	This statement is not fully consistent with the one in chapter 11, page 32, lines 23-24 which mentions "a small possibility of cooling" [Govt. of France (Reviewer's comment ID #: 2010-118)]	Rejected. Chapter 11 text revised
SPM-1387	A	15:14		"Over the North Atlantic" should be "around the North Atlantic", see Chapter 10, p70, line 10 and p24, line 41. The Executive Summary of Chapter 10 is incorrect at page 5, line 46. [Gerrit Burgers (Reviewer's comment ID #: 34-13)]	REJECT: not only land surface temperatures but also SSTs.
SPM-1388	A	15:15		Remove the comma [Richard Soulen (Reviewer's comment ID #: 248-16)]	Accept
SPM-1389	A	15:16	15:17	No definition of abrupt given nor is it made clear that there may be an abrupt slowdown (rather than shutdown) and there is some observational evidence that	Rejected. Text reflects AR4models..

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				this might be occurring 5.3.2, Box 5.1. Figure 10.3.13 - some models show quite abrupt changes (slowdown) but the overplotting of the curves makes this difficult to see. The statements made are somewhat misleading. Re-word - No model suggests a complete shutdown of the MOC during the 21st century, but an abrupt slowdown cannot be ruled out. The likelihood of these and longer term changes cannot be evaluated with confidence. [Meric Srokosz (Reviewer's comment ID #: 250-2)]	
SPM-1390	A	15:19	15:23	"The point made in this paragraph about longterm SLR is good. However, can some more detail also be provided about SLR projections (even if just to say the results from the TAR still stand, if that's the case)? Results are only provided for one emission scenario."  [Govt. of Canada (Reviewer's comment ID #: 2004-109)]	Accepted. Bullet moved and modified.
SPM-1391	A	15:19	15:19	replace first part of the sentence up to "commitments" by "sea level continues to rise on a much longer timescale after stabilisation of concentrations than temperature" to avoid the highly misleading term "commitment" (see comment number number 232 (on page 11 line 47). [Govt. of Germany (Reviewer's comment ID #: 2011-242)]	"Committment" will be replaced
SPM-1392	A	15:19	15:23	Please show (even better if also graphically) the full range of SLR projections for all SRES-scenarios. This would be very relevant information for policymakers. Comparison to TAR should be given as well. [Govt. of Germany (Reviewer's comment ID #: 2011-243)]	Summary sea level rise projections will be included in the SPM
SPM-1393	A	15:19	15:23	Needs to include full range of SRES estimates and 21st century estimates moved to the previous sections. [William Hare (Reviewer's comment ID #: 99-124)]	Summary sea level rise projections will be included in the SPM
SPM-1394	A	15:19	15:39	These bullets should go earlier with SRES imapcts, leaving this sections to deal witht stabilisation scenarios [Joanna House (Reviewer's comment ID #: 109-84)]	Agreed
SPM-1395	A	15:19	15:39	This summary of the sea-level rise projections is confusing in the extreme. It is not clear whether the contributions for the three dot points should be added. It is also evident that a full range of SRES scenarios has not been covered. How does the lay reader know what the "A1B Scenario" is? Nowhere in the SPM is the A1B Scenario described. These dot points (and other parts of the SPM, if they are as bad as this) are the laying of a political minefield, with projections open to whatever interpretation the quoter desires. We need to know the full range of projections for the full range of scenarios and model uncertainties. [John Hunter (Reviewer's comment ID #: 112-32)]	SLR projections for different scenarios will be included in the next draft. Text will be clarified. SRES markers will be covered in separate box.

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SPM-1396	A	15:1 9	15:2 3	<p>The logic of the first sentence cannot be sustained. Despite solar radiation penetrating and being absorbed within the ocean surface layer (and exceeding net longwave radiation loss at the surface) for millions of years, the warm ocean surface remains a thin lens. The temperature of the ocean surface is regulated by the net radiation absorbed, the conduction and evaporation to the atmospheric boundary layer, surface wind-driven upwelling, and entrainment of cold subsurface water across the thermocline as a consequence of the meridional overturning ocean circulation. A small increase in back radiation that is absorbed at the ocean surface is not likely to be mixed deeply into the ocean. Recently observed warming of the tropical ocean surface layer is very small and likely due to a reduction in the entrainment of cold subsurface water across the thermocline (see McPhaden and Zhang, 2002 for the tropical Pacific Ocean). The first sentence should be deleted.</p> <p>[William Kininmonth (Reviewer's comment ID #: 128-109)]</p>	Rejected. This argument does not address the ocean mixing and circulation mechanisms that contribute to deep ocean warming which is now both observed and simulated
SPM-1397	A	15:1 9	15:2 3	<p>This estimate of sea level rise is much too small--it is simply not credible given what occurred in the 20th century and the acceleration evident in the last decade. This range indicates that the rise over the 21st century could be about 25% less than observed for the 20th century--and there is no way that build-up on Antarctica could accomplish this in the face of the increases that are well-established to occur from mountain glaciers and the accelerating rate of change--now up to a rate of .3 m per century. Given how Greenland deteriorations is starting, the notion that .43 m is the upper limit seems patently absurd. At least, as compared to the TAR, IPCC is not saying this is "very likely" etc.--but this range is far too low for any scenario--and the large increases could also come from deterioration of Greenland, as paleo evidence has suggested is possible. I am just baffled by this estimate.</p> <p>[Michael MacCracken (Reviewer's comment ID #: 152-52)]</p>	Partly accepted. A range will be given for contributions to sea level raise from ice sheets in the next draft.
SPM-1398	A	15:1 9	15:2 3	<p>Move "sea level rise until 2100" to SPM-13, line 37. Give a more balanced outlook for the centuries thereafter, since this is a very important policy issue.</p> <p>[Govt. of Netherlands (Reviewer's comment ID #: 2016-29)]</p>	Agreed that SLR for the 21st century should be in the previous section
SPM-1399	A	15:1 9	15:1 9	<p>Is this bullet for total rise or the thermal expansion component?</p> <p>[Robert Nicholls (Reviewer's comment ID #: 191-4)]</p>	Text will be clarified
SPM-1400	A	15:1 9	15:3 9	<p>These three bullets should be reviewed and integrated -- the second and third bullets are reasonably clear and I think the problem lies with the ambiguity of the first bullet -- see previous comment</p> <p>[Robert Nicholls (Reviewer's comment ID #: 191-5)]</p>	Taken into account. Bullet text moved and reworded.
SPM-	A	15:1	15:2	A statement about quantitative SLR for specific dates fits better in the preceding	Accepted. Bullet moved and text modified.

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1401		9	3	<p>section ("Further emissions of greenhouse gases..."), since this is where policymakers will look for concrete data that helps them in their planning. This current bullet point should concentrate more on the conceptual issue of long-term commitment to SLR and its principal components, i.e. thermal expansion and ice sheet melting. An example for such a sentence could be: "Even in experiments where greenhouse gas concentrations stabilise in 2100, sea-level rise due to thermal expansion in the 22nd and 23rd centuries is greater than in the 21st century, reaching an eventual level of 0.3-0.6m per degree of global warming [relative to pre-industrial or to present?]. The continued melting of ice sheets is expected to further contribute to long-term sea-level rise at a rate of ... [etc] [plus a reference to uncertainties]"</p> <p>[Andy Reisinger (Reviewer's comment ID #: 210-61)]</p>	
SPM-1402	A	15:19		<p>"owing to" may be a more difficult phrase for non-native English readers to understand than "because" or "since"</p> <p>[Daniel Murphy (Reviewer's comment ID #: 183-16)]</p>	Accepted
SPM-1403	A	15:19		<p>Not all sea-level rise has much longer times scales. The authors need to note the difference between melting and warming (look at the TAR Synthesis Report's treatment of inertia). Rewrite this bullet to make clear the different processes and time scales (i.e., short-term rise in sea level).</p> <p>[Govt. of United States of America (Reviewer's comment ID #: 2023-867)]</p>	Reject. Land ice melt and ocean warming have longer time scales than surface warming.
SPM-1404	A	15:20	15:21	<p>"This statement does not recognize the effect that slightly higher sea level rise has on storm surges that are very significant. "</p> <p>[Govt. of Canada (Reviewer's comment ID #: 2004-110)]</p>	WG1 is not covering effects of SLR – see WG2
SPM-1405	A	15:20	15:23	<p>Reference is for [10.6, 10.7] but the figures (numerical values) cannot be found in the chapter text. Reference should be verified.</p> <p>[Govt. of Japan (Reviewer's comment ID #: 2014-18)]</p>	Text has been revised and is consistent with chapter 10
SPM-1406	A	15:20	15:21	<p>The models referred to in this sentence include primarily the thermal expansion component of sea level rise, with little or no contribution from ice sheets. Yet other chapters detail strong evidence for a contribution from ice sheets. This fact MUST be included in this statement, otherwise it is highly misleading. Something like: "By 2100, sea level rise due to thermal expansion is projected to range from 0.14-0.43 m..." "Observational evidence strongly suggests that ice sheets contribute 1-2 times the thermal component to current sea level rise."</p> <p>[Jeffrey Severinghaus (Reviewer's comment ID #: 232-2)]</p>	ACCEPTED: sea level numbers will be revised entirely.
SPM-1407	A	15:21	15:21	<p>"from 0.14 to 0.43 m"</p> <p>[Olivier Boucher (Reviewer's comment ID #: 27-17)]</p>	Text has been revised and is consistent with chapter 10

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SPM-1408	A	15:2 1	15:2 1	"Useful to include in brackets what the A1B concentrations at 2100 are so there is a reference point of concentration with magnitude of impact."  [Govt. of Canada (Reviewer's comment ID #: 2004-111)]	Reject – GHG concentrations are model dependent so this would take more space than available or justified
SPM-1409	A	15:2 1	15:2 1	The word concentration is superfluous. [David Griggs (Reviewer's comment ID #: 90-7)]	Accepted
SPM-1410	A	15:2 1		See my comment to Chapter 10 to the range 0.14-0.43m [Gerrit Burgers (Reviewer's comment ID #: 34-18)]	Taken into account
SPM-1411	A	15:2 2	15:2 2	Should the "0.3-0.8 m" be added to the "0.14-0.43 m" rise for the 21st century? [John Hunter (Reviewer's comment ID #: 112-34)]	Text has been revised and is now clearer
SPM-1412	A	15:2 2	15:2 2	Does "the next two centuries" mean 2000-2200 or 2100-2300? [John Hunter (Reviewer's comment ID #: 112-35)]	Text clarified
SPM-1413	A	15:2 2	15:2 2	The phrase "next two centuries" is slightly ambiguous. You could say "next two centuries out to 2300" or "following two centuries". [A. Brett Mullan (Reviewer's comment ID #: 182-4)]	SEE COMENT SPM-1412
SPM-1414	A	15:2 2		Clarify by adding "in 2300" to "0.3-0.8m due to thermal expansion". [Gerrit Burgers (Reviewer's comment ID #: 34-17)]	Text clarified
SPM-1415	A	15:2 2		Since this section seems to be more about stabilisation, the bullets on SRES projections should go earlier (see specific line notes). I would like to see more here on quantifying stabilisation scenarios - this is what policy makers seem much more itnerested in than emissions projectcions - it answers the question of "what is dangerous claimte change" e.g. what will happen if we stabilise at 2 deg C? what cocentrations do this relate to and how would we have to cut emissions to acheieve it? what would be the sea level changes? how a particular stabilisation cocncentration level relates to near and long-term (2050/2100/equilibrium) temeprature and sea level changes. [Joanna House (Reviewer's comment ID #: 109-86)]	SRES projections will be dealt with more consistently in the previous section, but some stabilization runs start with SRES and that is used here.
SPM-1416	A	15:2 2		Insert "alone" after "thermal expansion" [Govt. of United Kingdom (Reviewer's comment ID #: 2022-155)]	Unnecessary
0-109	A	15:1 9	15:2 3	SPM-15; line 23; "those concentrations" refered to in line 23 have only indirectly been mentioned in this paragraph; clearer wording or numbers given here may be easier to read; [ European Commission (Reviewer's comment ID #: 2008-81)]	Text clarified
SPM-1417	A	15:2 3	15:2 3	at which concentrations? A1B shows changing concentration. [Joanna House (Reviewer's comment ID #: 109-85)]	Text clarified
SPM-	A	15:2	15:3	Concerning possible Greenland melting: a comment should be added on the	Accepted. Text added. Bullet split.

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1418		5	2	possibility that melting of the Greenland Ice sheet, should it occur, could be irreversible, even if global warming were to be eventually reversed. [Govt. of Australia (Reviewer's comment ID #: 2001-43)]	
SPM-1419	A	15:2 5	15:3 2	I am concerned that this summary might give the wrong impression that probability is rated as "unlikely" (in the sense defined in the TS) that Greenland will contribute more than 0.4m sea level rise per century by 2100, instead of "very unlikely". See also 10-67-44 where the estimate for the extra melting due to basal lubrication is relatively small. My recommendation: change the sentence "The melting rate would increase ... some recent observations" into "Dynamical processes may enhance the ice flow and lead to a faster increase of the melting rate, as suggested by some recent observations.", since the observations tell more about the present melting rate than the possible maximum melting rate and under what conditions those maximum melting rates can be achieved. [Gerrit Burgers (Reviewer's comment ID #: 34-16)]	Reject. Text ok as is.
SPM-1420	A	15:2 5	15:2 6	Is this "Models" or one model driven by different GCMs?. Other "models" show much faster loss of ice at lower temperature (Kilsholm et al 2004). In addition the model does not include the dynamical processes referred to later in this paragraph. Suggest rewording like: "A model driven by a range of GCMs and that does not include ice dynamical processes such as those now being observed...". Somehow this basic issue needs to be addressed here: there is a gap between models and observations that is quite unlike the situation for other domains of this issue and it needs to be confronted straight up and down in the SPM and elsewhere. [William Hare (Reviewer's comment ID #: 99-125)]	Accepted. Text modified.
SPM-1421	A	15:2 5	15:2 8	COMMENT: This phrase gives the misunderstanding that "3°C" are the threshold of Greenland Ice Sheet melting. Additional example and/or information are required. REASON: Chapter 10.7.4.3 description is "For a global warming of 3°C relative to present, models suggest Greenland would contribute 0.2-3.9mm/yr to sea level." Keep consistency with this Chapter. RECOMMEND: replace "initially contributing up to 0.4m sea level rise per century" with "initially contributing 0.02-0.39m sea level rise per century" [Govt. of Japan (Reviewer's comment ID #: 2014-19)]	Text will be clarified
SPM-1422	A	15:2 5	15:3 2	This paragraph is not consistent with paleoclimate reconstructions of Greenland temperatures during the Holocene Optimum, when temperatures were up to 5C warmer than they currently are for a period of several millennia. Currently	Rejected – the Holocene optimum is not being discussed here. If anything models used for projection have a cold bias over Greenland – see Fig 8.3.1

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				summer temperatures are colder than -10C over a wide area of Greenland and inhibit widespread Greenland melting. The reference should be more specific to Arctic sea ice and coastal land ice if it is to be retained. [William Kininmonth (Reviewer's comment ID #: 128-111)]	
SPM-1423	A	15:25	15:32	I think a new finding of possibility of sea level rise due to mass loss of the Greenland ice sheet is one of the topics of AR4. However, some people will misunderstand the sentence and believe that definitive description of 3 degree C in the sentence will be the threshold for GHG stabilization level. Therefore, I strongly suggest the original description in Chapter 10 is applied in SPM, without description related to A1B scenario, because many scientific uncertainties still remain in abrupt increase or decrease of temperature near the Greenland due to change of MOC, which will affect the SMB on the Greenland. "With global warming maintained above 3.1±1.6 degree C relative to pre-industrial level, the Greenland ice sheet would be eliminated, except for remnant glaciers on the mountains, raising sea-level by about 7m, initially at a rate of up to 0.4m per century for a global warming of 3 degree C relative to present level".  [Koki Maruyama (Reviewer's comment ID #: 169-4)]	REJECTED: MOC not relevant here in the context.
SPM-1424	A	15:25	15:32	This bullet point appears less clear than the statement in the executive summary of chapter 10. Suggested re-wording: "The Greenland ice sheet is expected to be eliminated over several hundreds to thousands of years, except for remnant glaciers in the mountains, if global warming were to be maintained above 3.1±1.6°C relative to pre-industrial. This would raise sea-level by about 7m, initially at a rate of up to 0.4m per century. The rate of melting would be increased if ice-flow was accelerated by lubrication due to surface meltwater. If the ice sheet were removed, there is medium likelihood that it could not be regenerated even if the climate were subsequently returned to pre-industrial." It might be better not to make specific reference to SRES scenarios because we are talking about multi-century time scales. BUT it would be helpful to have a lay-person's text box and figure earlier that allows readers to generally link the temperature of "3.1±1.6°C relative to pre-industrial" to appropriate CO2-equivalent concentrations, based on average climate sensitivity (since this is what matters of such long time scales). [Andy Reisinger (Reviewer's comment ID #: 210-62)]	Text has been revised and is consistent with chapter 10
SPM-1425	A	15:25	:28	The reader may also think of large floating ice sheets: "Changes in the Antarctic and Greenland grounded ice sheets MAY signific....." Also, has the number changed from the TAR? It used to be that a warming of 3°C AT GREENLAND would melt it, attainable by a 1.5°C global mean warming (not 3°C as noted	Taken into account. Bullet changed.

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				here). If correct as written, note the change since the TAR because this is significant. [Govt. of United States of America (Reviewer’s comment ID #: 2023-868)]	
SPM-1426	A	15:25	:32	This bullet needs be reworked in its entirety. For example, “This level of warming” is an awkward sentence construct. Does “this” refer back to up to 0.4 per century? There is a jump at the end to paleoclimate data suggesting a sea-level rise contribution of several meters. [Govt. of United States of America (Reviewer’s comment ID #: 2023-869)]	Text has been completely revised
SPM-1427	A	15:27	15:27	Is this 0.4 m SL rise *in addition* to the 0.14 to 0.43 m for the A1B scenario referred to in the bullet above? If so, I'd state it explicitly. [Melinda Marquis (Reviewer’s comment ID #: 162-103)]	Text has been completely revised
SPM-1428	A	15:28	15:29	This sentence is not very clear. Do the recent observations suggest that there is an increase in melting rate or that there are processes that could lead to an increase in melting rate? [Olivier Boucher (Reviewer’s comment ID #: 27-18)]	Text revised
SPM-1429	A	15:28	15:28	"Explain what a dynamical process is, and give at least one example." [Govt. of Canada (Reviewer’s comment ID #: 2004-112)]	Rejected – for this level of detail reader must refer to cited section of the report
SPM-1430	A	15:28	15:29	This sentence is around the wrong way as it makes the observations logically inferior to the model: Fast ice dynamical change are occurring and these exceed model estimate and this implies that the models are underestimating future contributions to SLR. [William Hare (Reviewer’s comment ID #: 99-126)]	Accepted. Reworded.
SPM-1431	A	15:29	15:32	A reference to Chapter 6.4 for this statement should be added at the end of this bullet. [Govt. of Netherlands (Reviewer’s comment ID #: 2016-30)]	Agreed
SPM-1432	A	15:29		Replace “occur” with “be reached” [Govt. of United States of America (Reviewer’s comment ID #: 2023-870)]	Text revised.
SPM-1433	A	15:30	15:32	It is questioned whether it is a good idea to compare the level of warming with a level some 125,000 years ago. This is because the reader might not be so familiar with the world and its ecosystems at that time. At least the Technical Summary should include some short description of what we know and what may be relevant in this context in a box. Otherwise this comparison adds little value. [Govt. of Austria (Reviewer’s comment ID #: 2002-27)]	Rejected – the bullet is independent of any consideration of ecosystems. The physics of large scale long term interactions between ice sheets and global climate can be expected to remain consistent in the absence of major geological changes
SPM-1434	A	15:30	15:32	It is questioned whether it is a good idea to compare the level of warming with a level some 125,000 years ago. This is because the reader might not be so familiar with the world and its ecosystems at that time. At least the Technical Summary	SEE COMMENT SPM-1433

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				should include some short description of what we know and what may be relevant in this context in a box. Otherwise this comparison adds little value. [Klaus Radunsky (Reviewer's comment ID #: 204-27)]	
SPM-1435	A	15:3 1		Change to read "...a comparable response to warming during the last ..." [Govt. of United States of America (Reviewer's comment ID #: 2023-871)]	Text revised
SPM-1436	A	15:3 2	15:3 2	Change "Arctic melting" to "melting of the Greenland Ice Sheet" in that the ice sheet is where most of the sea level rise came from [Michael MacCracken (Reviewer's comment ID #: 152-53)]	Accepted. Text changed.
SPM-1437	A	15:3 2	15:3 2	Add reference to Ch 6.4 at the end of this line. Note the overlap with SPM-9, 26-28.  [Govt. of Netherlands (Reviewer's comment ID #: 2016-31)]	Agreed
SPM-1438	A	15:3 2		A reference to section 6.4 should be added for the paleoclimate statement at the end of the bullet. [Gerrit Burgers (Reviewer's comment ID #: 34-15)]	SEE COMMENT SPM-1437
SPM-1439	A	15:3 2		Replace "widespread Arctic" with "polar". Change "several" to "4-6". [Govt. of United States of America (Reviewer's comment ID #: 2023-872)]	Rejected – the paleo evidence is focused on the Arctic
SPM-1440	A	15:3 4	15:3 9	Again this sentence is around the wrong way as it makes the observations logically inferior to the model: As with the GIS fast ice dynamical changes are occurring and these exceed model estimates and this implies that the models are underestimating future contributions to SLR. I would suggest simply explaining the known dynamics of the ice sheet: Increased accumulation due to warming, likely (timeframe) loss of or weakening of ice shelves due to surface warming and/or basal melting, potential for rapid ice stream movement (already observed) and then a range of estimates for the contribution using an if this assumption then that SLR contribution logic. Another model than the one cited in Chapter 10 (Warner, Budd et al 1998) indicates a substantial sensitivity of the ice sheet to basal melting induce loss of ice shelves. In addition refernce could be made to the predicted vulnerability of ice shelves (eg Williams et al 2002) [William Hare (Reviewer's comment ID #: 99-127)]	Rejected – this section is about long term projections not observations. However, new text will address range of SLR estimates consistent with current observations.
SPM-1441	A	15:3 4	15:3 5	There is "widespread surface melting" on the margins of Antarctica (Liu et al, 2006) but at present this does not translate into large amounts of ablation but it is relevant to the future behaviour of the ice sheet (ice stream motion due to basal lubrication, ice shelf viability etc. This therefore needs to be qualified. Liu, H., L. Wang, and K. C. Jezek (2006). "Spatiotemporal variations of snowmelt in Antarctica derived from satellite scanning multichannel microwave radiometer and Special Sensor Microwave Imager data (1978–2004)." J. Geophys. Res.	Paragraph revised – but note refers to projections not current conditions

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				111(F1): 1-20.  [William Hare (Reviewer's comment ID #: 99-128)]	
SPM-1442	A	15:3 4	15:3 9	Antarctic is isolated from the warmer oceans by the Antarctic Circumpolar Current with upwelling cold water on its southern boundary adjacent to Antarctica. Ocean warming will not impact on the Antarctic ice shelves to accelerate ice melt. The third sentence "However, in response to weakening of ice shelves by ocean warming or surface melting at the margins, ice flow could accelerate" should be deleted. The last sentence should read: "Such effects are uncertain".  [William Kininmonth (Reviewer's comment ID #: 128-112)]	Rejected – ocean warming is pervasive and can affect upwelling waters
SPM-1443	A	15:3 4	15:3 8	I was really disappointed that the discussion of Antarctic conditions in this assessment only rarely (at least in the SPM and TS) differentiated between East and West Antarctic conditions--they are quite different and the potential for their loss is quite different. There really needs to be a point about each of them separately--not lumping the two together.  [Michael MacCracken (Reviewer's comment ID #: 152-54)]	Rejected. Length constraints of the SPM don't allow for elaboration here
SPM-1444	A	15:3 4	15:3 9	The way that uncertainties are described in this bullet point is not very clear. It might be better to be more descriptive and replace the last two sentences with something like: "However, recent observations have indicated a possibility of increased ice flows in response to further ice shelf weakening by ocean warming or surface melting at the margins. Such effects could offset or outweigh increased snowfall particularly in the West Antarctic Ice Sheet but there are currently no sufficiently robust models to reliably simulate such processes."  [Andy Reisinger (Reviewer's comment ID #: 210-63)]	Rejected, would be misleading in implying that it's just that the models aren't adequate somehow. The issue goes beyond modelling (at least global modelling) to limits on understanding of the physical processes.
SPM-1445	A	15:3 4	15:3 9	This statement does not square with recent GRACE measurements, that clearly show that Antarctica is losing mass. The papers that found an increase in surface elevation in Antarctica, which was interpreted as a gain in mass due to greater snowfall, are probably biased by an inadequate treatment of the snow layer on top of the polar glacier. This snow layer (firn) compacts with time to form ice, and the compaction rate is highly temperature-dependent. Small variations in temperature can therefore create large variations in surface elevation that are unrelated to mass, because the snow contains roughly half air and half ice. In any case, the GRACE measurements are the gold standard here, because they measure mass, not elevation. Mass is the relevant quantity for sea level. I suggest to reword this as: "Antarctica is expected to behave differently from Greenland due to the absence of widespread surface melting; contributions to sea	Statement now revised and is consistent with assessment in chapters 4 and 10

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				level rise will mostly be due to ice flow. Ice flow models are currently unable to simulate future contributions to sea level rise with confidence. However, observational data show that Antarctica is currently losing mass, and probably will continue to do so in the future." [Jeffrey Severinghaus (Reviewer's comment ID #: 232-3)]	
SPM-1446	A	15:3 4	:39	Change "It is expected to gain ice through increased snowfall in the 21st century, acting to reduce global sea-level rise by about 0.1 m per century" to "Models suggest that it will gain ice through increased snowfall in the 21st century, acting to reduce sea level; [10.7] but observations suggest that it has lost ice and added to sea level during the last decade [4.6]." Chapters 4 and 10 reflect different points of view; the SPM ought to reflect both points of view. [Govt. of United States of America (Reviewer's comment ID #: 2023-873)]	Taken into account. Text modified.
SPM-1447	A	15:3 5	15:3 8	Reference is for [10.7] however this data was not found in the chapter text. There is reference to it in the Chapter 10 Executive Summary, however. Reference should be verified. [Govt. of Japan (Reviewer's comment ID #: 2014-20)]	add ref to 10.6
SPM-1448	A	15:3 6	15:3 6	Is the "0.1 m per century" reduction in global sea level rise already included in the sea level rise figure of .14 - .43 m at line 21 of this page?  [Govt. of Canada (Reviewer's comment ID #: 2004-113)]	Text clarified
SPM-1449	A	15:3 6	15:3 8	In reference to conditions offsetting the building of snow on the Antarctic Ice Sheet, "ice flow could accelerate" should be changed to read, "ice sheet flow could accelerate." [Govt. of Japan (Reviewer's comment ID #: 2014-21)]	Distinction seems insignificant
SPM-1451	A	15:3 8		Replace "could" with "might" [Govt. of United States of America (Reviewer's comment ID #: 2023-874)]	Distinction seems minor – "could" used elsewhere in para – no change.
SPM-1452	A	84:2 3	84:2 4	It should add figTS-30 into this place to understand the changes of the temperature for the future 100 years and 200 as well as 300 years. [Zong-Ci Zhao (Reviewer's comment ID #: 302-4)]	Intended location unclear from comment
SPM-1453	B	9:17	9:17	There is no "coherent behavior". [Stephen McIntyre (Reviewer's comment ID #: 4-1)]	Rejected – see chapter 6
SPM-1454	B	11:2 4	11:2 4	You say "increase confidence" - but the confidence levels of Mann et al are flawed and none of the other studies claim similar confidence intervals. The tone, in some ways, is more reserved than TAR. [Stephen McIntyre (Reviewer's comment ID #: 4-2)]	Rejected. There is additional data from new methods and analyses and that is the basis for increased confidence.
SPM-1455	B	11:2 4	11:2 4	I disagree that you can assign "likely" the warmest - the proxy reconstructions are very intereconnected and depend too much proxies known to be flawed,	Rejected, conclusion supported by post TAR published literature. See also definition of "likely" in

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				while at the same time not reconciling contrary evidence of higher medieval treelines. This should onl [Stephen McIntyre (Reviewer’s comment ID #: 4-117)]	IPCC usage.