

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – General

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0-1	0	0	0	0	0	<p>Comments by the Chinese government on Working Group I Contributions to the IPCC Fifth Assessment Report Climate Change 2013: The Physical Science Basis November 23, 2012</p> <p>The Chinese government appreciates and thanks the Bureau members, lead authors and Technical Support Unit of the Working Group I (WG I) Report - Climate Change 2013: The Physical Science Basis - a component of the Intergovernmental Panel on Climate Change (IPCC) Fifth Assessment Report (AR5) for their painstaking efforts made for the preparation. We believe that the report is a rather comprehensive assessment of the progress in the research into the physical science of climate change since the Fourth Assessment Report (AR4), the conclusions of which will serve as important scientific evidence for the international community to better understand the basic facts on, trends, impacts and causes of climate change.</p> <p>The Chinese government is very pleased to have this opportunity to comment on this report. In order to better reflect the objectivity, comprehensiveness and balance of an IPCC assessment report, we have made government comments on the revision of the report as follows with the hope that they will contribute to the subsequent modification process positively and constructively.</p> <p>1. Regarding Taiwan, China and Hong Kong, China, facts and data on China cited in the report There are errors concerning the expression of “Taiwan, Hong Kong and Beijing”, as noted in “Table 2.A.2: Overview of O3 trends reported in the literature, using data sets with at least 10 years of measures”, page 116, Chapter 2. In the first column of Table 2.A.2, “Taiwan, Surface” is to be changed to “Taiwan, China, Surface”; “Hong Kong, surface” to “Hong Kong, China, surface”; “Beijing boundary layer” to “Beijing, China, boundary layer”. In the second column of the table, at the same time, “YangMing mountain site in north of the country” is to be changed to “YangMing mountain site in north of the region”; “Composite of urban sites in the north of the country” to “Composite of urban sites in the north of the region”; “Composite of 4 sites in the south of the country” to “Composite of 4 sites in the south of the region”. In addition, the “Hong Kong” appearing in the legend of Figure 2.8, page 149, Chapter 2 is requested to be changed to “Hong Kong, China”. We also note that the maps given on page 151, Chapter 2 and page 202, Chapter 9 mark the Sino-Indian border with “McMahon Line” and no intermittent lines in the South China Sea. It is recommended to delete these maps or replace them with a border free one. We also note that there are quite a few occasions in Chapter 2 where cases and data on China are enumerated. In our view, some of them are not directly related with the assessed conclusions, nor are they representative. The deletion of such texts will not discount the validity of the conclusions or the integrity of the information. The specific proposed revision in this connection is found in the attached table of government comments.</p> <p>2. Regarding the slowing global warming since the late 1990s The data, figures and tables in Chapters 2, 3 and 10 of the report clearly show that the global average land surface temperature, sea surface temperature and regional sea level were rising at a slower rate in the period since 1998 than in the period from the 1950s to the end of the 20th century. In our view, although it does not mean that the global climate change mainly characterized with warming has ceased or reversed in trend, yet the above observed fact of wide concern since the AR4 should be elaborated on or explained in a more clear-cut and detailed manner in the AR5 WG I report.</p> <p>3. Regarding the expression of the attribution of climate change to human activities The assessment of the relationship between warming and human activities will be one of the most important conclusions in the IPCC AR5. As we note, the report states that it is extremely likely (e.g. a likelihood of above 95%) that human activities have caused more than half (more than 50%) of the observed increase in global average surface temperature since the 1950s. However, AR4 argued that “most of the observed increase in global average temperatures since the mid-20th century is very likely (e.g. a likelihood of over 90%) due to the observed increase in anthropogenic greenhouse gas concentrations”. Actually, in the above two statements extracted from AR5 and AR4 respectively, “extremely likely” and “very likely” are not describing exactly the same subject. In our view, in order to avoid misleading decision- or policy-makers, the report should explain the implications of this important conclusion and its difference with AR4 in terms of confidence levels in greater details. Otherwise, policy-makers may mistakenly believe that the AR5 conclusion on climate change attribution is simply an increase of confidence level to 95% (extremely likely) from 90% (very likely) in AR4. In addition, the present expression may mislead policymakers into thinking that it is the human activities conducted after 1950 that resulted in the most (more than 50%) observed average global surface temperature increase since the 1950s. We are of the view that the relevant conclusion needs clarifying that they are the</p>	<p>Thank you very much. Responses to the set of comments follow hereafter: 1) political consideration has been given to the regional names where possible, although consistency with underlying cited papers is also required in some instances.</p> <p>2) further emphasis has been given to climate over the last two decades, including the inclusion of a new box in Chapter 9, Box 9.2 "Climate Models and the Hiatus in Global-Mean Surface Warming of the Past 15 Years" which has also been elevated to the Technical Summary.</p> <p>3) Attribution statements in Chapter 10 have been refined and clarified. Technical Summary and SPM statements have been revised accordingly.</p> <p>4) reject -- the concept of cumulated emissions is well established in the peer-reviewed scientific literature and thus forms an integral component of the WGI contribution to the AR5. The discussion in the TS and the SPM is fully based on the careful and comprehensive assessment of the peer-reviewed literature provided in Chapter 12. Uncertainties associated with the concept are comprehensively dealt with throughout the WGI contribution, up to the TS in TFE8 (incl. Figure 1) and the SPM (see Figure SPM.9) in the Final Draft.</p> <p>5) non-english literature is being taken into account in the WGI contribution to AR5 to the extent possible and feasible for the author teams.</p> <p>6) the SPM has been substantially revised taking into account review comments made on the SPM.</p>

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						<p>human activities conducted since the industrial revolution (1750).</p> <p>4. Regarding the expression of the assessment of and conclusion on future emission budget under the 2°C target</p> <p>Chapter 12 gives an estimation of the global cumulative emission budget by 2100 under the 2°C target based on scenarios and models. The conclusion is quoted in the Executive Summary of Chapter 12 (lines 46-49, page 6) and in the Summary for Policymakers (lines 27-31, page 17). As we note, however, it is not an integral quotation. In Chapter 12, this issue is stated in the section of "limitation and conclusion" to indicate that there are still uncertainties with the estimation of global cumulative emissions, as found between lines 55-57 of page 66, Chapter 12: "It is important to note that the cumulative budget constraint does not consider non-CO2 forcings. Also, since those ranges are based on a set of scenarios available in the literature the interpretation in terms of likelihood is difficult." It is inappropriate for such an argument with much uncertainty to be cited as a key conclusion in the Technical Summary and the Summary for Policymakers. Therefore, it is proposed to take out relevant words from the Technical Summary and the Summary for Policymakers. If there is an insistence to have such elements reflected in the Technical Summary and the Summary for Policymakers, the representation must be integral, with an emphasis placed on the fact that it is an estimate with limitations and uncertainties, coupled with a quotation of lines 55-57, page 66, Chapter 12.</p> <p>In addition, we believe that in its reports, IPCC should still attach great importance to the reference of literature from developing countries and non-English-speaking countries. We also believe that taking into account the needs of non-professional readers, the Summary for Policymakers should be more friendly and accessible. Specific comments by the Chinese government on the second order draft of Working Group I AR5, which touch upon the Summary for Policymakers, the Executive Summary and various chapters of the report, are consolidated in the attached table. [Government of China]</p>	
0-2	0	0	0	0	0	<p>The preceding comments are a mere sampling of instances in which PDO, volcanic, and other factors affecting climate are dismissed as noise that "masks" real (anthropogenic) climate change. For example, nowhere is it mentioned how much the absence of volcanoes since Pinatubo has contributed to the meager warming of the past 16 years. While AGW is undoubtedly important, all of the conclusions and summary remarks I've read herein indicate that the compilation of the report begins with the assumption that AGW is the central factor. This assumption is so pervasive that it is impossible to change the report to address this issue. Perhaps there should be a disclaimer on the frontispiece that the report is intended to be a treatise focusing on anthropogenic causes of climate change, rather than an objective compendium on all causes and effects of climate change. [Richard Keen, United States of America]</p>	<p>reject; Both natural variability and internal variability are prominently discussed throughout the 14 Chapters and Appendices of the WGI AR5. Chapter 2, 3, and 4 deal with observed changes only without making any attempt to attribute these observed changes. Attribution of observed changes to a cause is the topic of Chapter 10.</p>
0-3	0	0	0	0	0	<p>GLOSSARY: Annex III (page 21). The glossary is a nicely crafted resource, to which I suggest just one addition. Chapter 2 often uses the phrase "column integrated water vapour" rather than the equivalent phrase "precipitable water" given in the annex and in many of the citations in Chapter 2. SUGGESTION: Please add "Column integrated water vapour" as an entry that simply states "See Precipitable water." Alternatively, insert "column integrated water vapour" as an alternative phrase for precipitable water. (In my FOD review of Chapter I suggested that either one phrase or the other be used consistently, and that is now done in the Chapter 2 narrative--but "precipitable water" is still in the title of various citations in the list of references.) [Forrest Mims, United States of America]</p>	<p>reject; the authors did not consider this term required elevation to the Glossary.</p>
0-4	0	0	0	0	0	<p>To restore some link between IPCC reports and observed reality, the report must address – but does not at present address – the now-pressing question why the key prediction of warming in earlier IPCC reports have proven to be significant exaggerations.</p> <p>Reason: The IPCC's credibility has already been damaged by its premature adoption and subsequent hasty abandonment of the now-discredited "hockey-stick" graph as its logo; by its rewriting its Second Assessment Report after submission of the scientists' final draft, to state the opposite of their finding that no discernible human influence on climate is detectable; by its declaration that all Himalayan ice would be gone in 25 years; and by its use of a dishonest statistical technique in 2007 falsely to suggest that the rate of global warming is accelerating. But the central damage to its credibility arises from the absence of anything like the warming it had predicted.</p> <p>Example: In 1990 the IPCC's central estimate was that warming would occur at 0.3 K/decade and that by now some 0.6 K warming would have occurred. Since then observations show warming has occurred at 0.14 K/decade and 0.3 K warming has occurred. There has been no global warming for 16 years. [Christopher</p>	<p>Noted. Climate projections from FAR to AR5 and comparison to observed changes in the physical climate system are comprehensively assessed in the WGI AR5. Recent changes in observed surface air temperature are assessed in Chapter 2. The Final Draft of Chapter 9, for example, includes in Box 9.2 "Climate Models and the Hiatus in Global-Mean Surface Warming of the Past 15 Years" an assessment of climate models compared to the observed evolution in global mean surface air temperature. This box is also elevated to the Technical Summary, as Box TS.3: "Climate Models and the Hiatus in Global-Mean Surface Warming of</p>

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						Monckton of Brechley, United Kingdom]	the Past 15 years". It is clear, however, that climate projections in previous IPCC reports were never intended to be near-term predictions and thus the comparison with observed temperature changes over years and up to 1-2 decades is not at all straightforward. Nevertheless, Figure 1.4 of Chapter 1 and Technical Summary TFE.3: "Comparing Projections from Previous IPCC Assessments" provide a summary of how climate projections presented in IPCC assessment reports from the FAR to the AR5 compare to observations as assessed in WGI AR5.
0-5	0	0	0	0	0	To restore lost credibility, all alterations by governments to the scientists' final draft must be visibly distinguished from it and referred back to all expert reviewers for comment before publication. Reason: Failure to make explicit the distinction between scientific and political content weakens the Assessment Reports by leaving readers wondering which findings are political. For this reason, I recommend the governments I advise to exercise caution before relying on the IPCC, which was founded as a political and not a scientific body. Example: During preparation of the Fourth Assessment Report (AR4, 2007), governments' political representatives decided by show of hands the "90% confidence" that more than half of the warming since 1960 was manmade. China had argued for no estimate; others had argued for 95%. Yet commentators unaware that this central decision was not scientific but political presented it as though it were a legitimate scientific finding. [Christopher Monckton of Brechley, United Kingdom]	reject; see panel approved policies and procedures which provide clear guidelines on the draft, review, and approval process.
0-6	0	0	0	0	0	To prevent recurrence of past scientific dishonesty, all alterations to the scientists' final draft after submission are to be visibly flagged and referred back to all expert reviewers for comment. Reason: The IPCC's Chairman, Dr. Pachauri, defended certain scientific errors in AR4 that exaggerated our influence on climate and had not been in the scientists' final draft. Example: The scientists' final draft showed a graph of global mean surface temperature anomalies since 1850, with one linear trend-line covering the entire period. Later, someone added three additional trend-lines, starting in 1900, 1950 and 1975 respectively, and added a false conclusion that since the trend-lines that began later rose more steeply manmade warming was accelerating. The same artifice would show a sine-wave, which has a zero trend, rising (or, if desired, falling) at an ever-faster rate, depending on the chosen start-points for the added trend-lines. Dr. Pachauri did not have this error corrected when asked. [Christopher Monckton of Brechley, United Kingdom]	reject; see panel approved policies and procedures which provide clear guidelines on the draft, review, and approval process.
0-7	0	0	0	0	0	To limit politicization of Assessment Reports, all material from non-peer-reviewed sources, such as environmental lobby groups, is to be excluded. Reason: 30% of all references listed in AR4 were not from reviewed papers in the learned journals but from the "gray literature": e.g. media handouts from environmental groups. While this practice continues, I cannot recommend the IPCC's reports as scientifically credible to the governments I advise. The Inter-Academy Council was asked to ban this practice but failed to do so. Example: For six months the IPCC's climate-science chairman, Dr. Pachauri, asserted that anyone who doubted the conclusion in AR4 that all the ice in the Himalayas would be gone within 25 years was "anti-science". Yet the conclusion had no scientific basis. It came from a polemic by a travel journalist. The lead author of the relevant chapter said he had known of the error but had decided not to correct it. [Christopher Monckton of Brechley, United Kingdom]	reject; see panel approved policies and procedures & guidance note on the usage of grey literature which provide clear guidelines on process.
0-8	0	0	0	0	0	To make explicit the magnitude and sign of any revisions to central climate-change projections compared with previous Assessment Reports, projections on all six original SRES emissions scenarios should be included. Reason: In the AR5 draft the goalposts have been moved by the use of scenarios incompatible with the original SRES scenarios. Yet governments need to have a clear idea of how fast the models' key projections are changing, and in which direction. For backward compatibility, projections similar to those in Fig. 10.26 of	noted. The IPCC assessment are required to be based on the most up-to-date science. The focus on RCPs reflects the scientific progress in the scenario-community that has been fed into the climate modelling community through CMIP5. A

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						the Fourth Assessment Report should be made under each of the six original scenarios. [Christopher Monckton of Brenchley, United Kingdom]	comprehensive discussion of RCPs is included in WGI AR5 Chapter 12, Section 12.3.1, and details about the comparison between RCPs vs. SRES (and CMIP5 vs. CMIP3) results is provided in Chapter 12 (section 12.4.9).
0-9	0	0	0	0	0	To respect the scientific method by enhancing the replicability of results shown in AR5, the data underlying all graphs in AR5, whether taken from cited learned papers or generated during the drafting, should be properly archived, with their data structures made explicit, and made available online to all. Reason: The credibility of the IPCC has been damaged by its failure to verify that material it has cited had been properly archived. Examples: The key projections on all six SRES emissions scenarios in AR4 were encapsulated in small-scale graphs at Fig. 10.26 (IPCC, 2007, p. 803). However, the data that underlay the graphs do not appear to have been archived. Also, the graph in TAR (IPCC, 2001) that purported to demonstrate the absence of the medieval warm period and the little ice age was withheld from researchers attempting to verify it for some considerable time after TAR was published. [Christopher Monckton of Brenchley, United Kingdom]	reject; see Panel-approved policies and procedures which provide clear guidelines on process and availability of material. Please also note that Multi-Model ensemble results based on CMIP are (and have been) publicly available. In addition, as part of the AR5, WGI will make the data underlying the figures in the WGI Annex I: Atlas of Global and Regional Climate Projections available through its website.
0-10	0	0	0	0	0	To clarify the process for determining climate sensitivity, the derivation and central estimate of the Planck or zero-feedback climate-sensitivity parameter should be made explicit. Reason: The Planck parameter is that quantity in Kelvin per Watt per square meter by which, where temperature feedbacks are non-existent or have not yet begun to act or sum to zero, a radiative forcing is multiplied to give the resultant temperature change. The magnitude of the contribution of feedbacks themselves to warming is separately dependent upon it. It is, therefore, a crucial quantity. Example: The only mention of the value of the Planck parameter in any previous Assessment Report is in a footnote on p. 631 of AR4, where its derivation is not made as clear as is desirable. It should also be expressed in Kelvin per Watt per square meter as an element in the climatic reference frame, rather than in Watts per square meter per Kelvin as though it were itself a feedback (Roe, 2009). [Christopher Monckton of Brenchley, United Kingdom]	reject. Chapter 9 provides a comprehensive overview of the Planck feedback as available from models (section 9.7.2, Tab 9.5) the TFS
0-11	0	0	0	0	0	To demonstrate the projected impact of temperature feedbacks over time, central estimates, with error-bars, of the evolution of the value of the climate-sensitivity parameter over the period from the instant when a forcing is applied to the time when equilibrium is attained should be evaluated, discussed, and presented as a graph. Reason: The impact of temperature feedbacks on the fundamental equation of climate sensitivity is expressed via increase over time in the value of the climate-sensitivity parameter ($\sim 0.3 \text{ K W m}^{-2}$ in the absence of feedbacks or where they sum to zero; $\sim 0.9 \text{ K W m}^{-2}$ at equilibrium after 1000-3000 years following a doubling of atmospheric CO_2 concentration). A graph of the evolution of the value of the climate-sensitivity parameter over time is necessary to make explicit the rate at which the IPCC considers global warming will increase. [Christopher Monckton of Brenchley, United Kingdom]	reject. The definition of climate sensitivity is clear and refers to equilibration. The time-relevant quantity is TCR which is also extensively considered in the report in Ch9, Ch10, and Ch12.
0-12	0	0	0	0	0	To clarify the method modelers use to determine climate sensitivity, AR5 should contain a table of temperature feedbacks, linearizing non-linear feedbacks where possible, providing a central estimate and error bars for each feedback, and making explicit the magnitudes of the respective contributions to forcing at equilibrium from direct forcings and from the feedbacks they trigger. Reason: Almost twice as much of the projected warming at CO_2 doubling comes from feedbacks as from CO_2 's direct forcing. Example: Though it is generally accepted that the direct warming from CO_2 is $<1.2 \text{ K}$, the multi-model mean central estimate that equilibrium warming at CO_2 doubling is 3.3 K (AR4, p. 798, box 10.2), implies an overall temperature feedback gain factor >2.8 , near-tripling the direct warming caused by atmospheric CO_2 enrichment. Yet it is only in the Fourth Assessment Report that the principal feedbacks the IPCC considers climate-relevant are quantified for the first time, and then only by reference to a single paper. For credibility, it is essential that feedback projections be put on an explicitly quantitative footing, with multiple sources for each feedback. [Christopher Monckton of Brenchley, United Kingdom]	reject. Feedbacks are comprehensively analysed for all models in Ch9 and summarized in Tab. 9.5
0-13	0	0	0	0	0	To increase credibility, the IPCC must tackle explicitly the fact that there has been no statistically-significant increase in global mean surface temperature for 16 years, and that this prolonged stasis in global warming	Noted. Recent changes in global mean surface temperature are comprehensively assessed in

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						<p>notwithstanding record increases in CO2 concentrations does not fall within the intervals projected either by the models or by the IPCC in previous Assessment Reports.</p> <p>Reason: Researchers with the courage to question the official projections have long predicted that – though some warming from CO2 enrichment is to be expected – not very much warming will occur. The 16-year temperature stasis that has now occurred must be explicitly faced.</p> <p>Example: The world’s leading modelers wrote in 2008 that a stasis of 15 years or more would establish a discrepancy between what is modeled and what is predicted. To explain that discrepancy one might argue that the relatively weak warming signal from CO2 has been overlain by three recent natural influences: in late 2001 we entered a ~30-year cooling phase of the ~60-year cycle of the ocean oscillations; the current ~11-year solar cycle displays near-unprecedentedly weak solar activity, implying the possibility of a Dalton or even Maunder minimum in the coming decades; and there has recently been a double-dip La Niña.</p> <p>[Christopher Monckton of Brenchley, United Kingdom]</p>	Chapter 2 of the WGI AR5, and are further discussed in box 9.2 "Climate Models and the Hiatus in Global-Mean Surface Warming of the Past 15 Years". This box is being elevated to the Technical Summary as Box TS.3: "Climate Models and the Hiatus in Global-Mean Surface Warming of the Past 15 years". The SPM also covers the "hiatus period" in the observations and understanding sections.
0-14	0	0	0	0	0	<p>To reduce the near-certainty that governments will ignore the IPCC’s reports as irrelevant in current economic circumstances, a chapter should be added comparing the economic merits of mitigation and adaptation.</p> <p>Reason: When the IPCC was established, mitigation and adaptation were assigned to separate working groups in a manner calculated to prevent direct economic comparison between them. It is now clear that adaptation would be one or even two orders of magnitude more cost-effective than mitigation.</p> <p>Example: The Stern and Garnaut reports purported to set the costs of mitigation against the benefit in climate-related losses abated by focused adaptation. However, both reports were produced for governments aiming to justify substantial new sources of tax revenues. A more objective approach is now necessary. An economic chapter appropriately belongs to a physical-science assessment, since it is only when the IPCC’s physical projections are combined with the standard economic methodologies of inter-temporal investment appraisal that a mature conclusion on the cost-effectiveness of mitigation can be reached.</p> <p>[Christopher Monckton of Brenchley, United Kingdom]</p>	reject; economic merits of mitigation and adaptation are outside the scope of Working Group 1 of the IPCC. Please see assessment reports of Working Groups 2 and 3.
0-15	0	0	0			<p>The report contains multiple citations of studies depending on GISS ModelE, but inexplicably omits the critical analysis of GISS model E performance in Scafetta’s latest papers:</p> <p>N. Scafetta, “Empirical evidence for a celestial origin of the climate oscillations and its implications”. Journal of Atmospheric and Solar-Terrestrial Physics 72, 951–970 (2010), doi:10.1016/j.jastp.2010.04.015 http://www.fel.duke.edu/~scafetta/pdf/scafetta-JSTP2.pdf</p> <p>N. Scafetta, “Testing an astronomically based decadal-scale empirical harmonic climate model versus the IPCC (2007) general circulation climate models” Journal of Atmospheric and Solar-Terrestrial Physics. DOI: 10.1016/j.jastp.2011.12.005. http://www.fel.duke.edu/~scafetta/pdf/Scafetta_models_comparison_ATP.pdf</p> <p>In these papers it is argued that the global surface temperature presents clear evidences of a strong harmonic component associated to astronomical cycles. All climate models used by the IPCC have failed to reproduce these harmonics. Here’s an extended comment/summary of the above papers: http://www.forbes.com/sites/larrybell/2012/01/10/global-warming-no-natural-predictable-climate-change/ Here’s one in Swedish, but you can translate it with Google translator: http://translate.google.com/translate?sl=auto&tl=en&js=n&prev=_t&hl=en&ie=UTF-8&layout=2&eof=1&u=http%3A%2F%2Fwww.theclimatescam.se%2F2012%2F01%2F17%2Fmer-harmonisk-analys%2F [David Burton, United States of America]</p>	Noted. Model variability in general is dealt with in Chapter 9, and model response to forcing in Ch10, where several of Scafetta’s papers are discussed. IPCC WG1 principally assesses the peer-reviewed literature only.
0-16	0	0	0			<p>Considering the change in the definition of scenarios wrt AR4, it would be appropriate to introduce the rationale for the change to RCPs and the climate policy meaning of each RCP, with a reference to a more detailed IPCC document. This is only addressed in SPM lines 15 to 18. [Government of France]</p>	Noted. Details on the new set of scenarios, the RCPs, are not just addressed in the SPM. Details can, e.g., be found in WGI Chapter 1, Box 1.1, in Chapter 12, Sections 12.3.1 and 12.4.9, or in the Technical Summary, Box TS.6 "The New RCP Scenarios and CMIP5 Models"
0-17	0	0	0			<p>It would be useful to have a summary illustration on the evolution of uncertainties and confidence for the issues already addressed in AR4 and the elements that are addressed for the first time in AR5. [Government of France]</p>	Reject; It would be graphically challenging to provide an illustration across the many relevant quantities. However, a summary of key conclusions from the previous reports is provided in WGI AR5 Chapter 1,

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							Table 1.1. In addition, a comprehensive summary/comparison is provided for weather and climate extremes (Fig 1.9, Technical Summary, TFE 9, Table 1, and Summary for Policymakers Table SPM.1).
0-18	0	0	0			Mean sea level and extremes are addressed in Chapter 13 (which also addresses understanding and projections) and Chapter 3, which affects readability [Government of France]	True. But Chapters are based on a government approved outline. Chapter 13 was tasked to integrate material on Sea Level Change from all the preceding WGI AR5 chapters.
0-19	0	0	0			WGI Co-Chair / TSU review comments have been prepared by Thomas Stocker, Gian-Kasper Plattner, Simon Allen, Alexander Nauels, Yu Xia, and Melinda Tignor [Thomas Stocker/ WGI TSU, Switzerland]	noted
0-20	0	0	1			Consistency in assessment numbers: Because chapter assessments continue to be refined, please check carefully all values (and the uncertainty ranges) carefully between tables, figures, main text, and summary text within your chapter. If numbers are taken from other chapters, please also ensure the latest results are used. Specific examples will be highlighted in our chapter comments. [Thomas Stocker/ WGI TSU, Switzerland]	noted. Special consideration has been given to consistency in quantitative results in the WGI AR5.
0-21	0	0	2			Treatment of Uncertainty: please follow the IPCC guidance note carefully; use italics to highlight formal uncertainty assessments; use likelihood in conjunction with high/very high confidence only (except in exceptional cases); if likelihood is given for situations where confidence is less than 'high', we recommend to put confidence in brackets at the end of the sentence rather than combining both confidence and likelihood in text. Please note - usage of the formal terms from the uncertainty guidance note, (egg. "likely", "confidence" etc) should be restricted to the use within statements which report assessment findings. [Thomas Stocker/ WGI TSU, Switzerland]	noted. The Chapters carefully followed and applied the AR5 Guidance Note.
0-22	0	0	3			Format of Executive Summary: As agreed at the third lead author meeting, we would ask that all chapters follow a consistent style for the ES. 1) The first sentence (or two) of each paragraph should be bolded to highlight the key message, with the subsequent sentences providing the detailed quantitative assessment. 2) Statements should incorporate the IPCC Uncertainty Language 3) Each paragraph must include a traceability to the underlying sections/subsections where the key message was drawn from (to the second level section heading), indicated using square brackets at the end of each paragraph. 3) Paragraphs should be grouped together under subtitles. The use of bullets should be avoided. 4) Finally, because the ES should be short and concise, lengthy textbook or chapeau type introductory text should be avoided. [Thomas Stocker/ WGI TSU, Switzerland]	noted; All Chapters carefully followed and applied the guidance provided by the Co-Chairs/TSU
0-23	0	0	4			Cross-chapter references AR5: suggest to update cross-chapter references to not just refer to Chapter number but to refer to specific section if appropriate. [Thomas Stocker/ WGI TSU, Switzerland]	noted; advice considered by all Chapters.
0-24	0	0	5			References to AR4 and earlier IPCC assessments: be as specific as possible. Writing just AR4 without any reference is not useful to the reader. Please refer to specific chapter where possible. [Thomas Stocker/ WGI TSU, Switzerland]	noted; advice considered by all Chapters.
0-25	0	0	6			Use of acronyms: In order to improve overall readability of the report, we would like to suggest that you please avoid acronyms that are not needed and/or are not used in more than one section of your chapter. [Thomas Stocker/ WGI TSU, Switzerland]	noted; advice considered by all Chapters.
0-26	0	0	7			Personal pronouns: our strong preference is to minimize the usage of personal pronouns, e.g., we/us/our to the extent possible. Exceptions to this would be when the Chapter's assessments conclusions are presented as clear summary statements. [Thomas Stocker/ WGI TSU, Switzerland]	noted; advice considered by all Chapters.
0-27	0	0	8			Please make sure to provide updates of relevant data from your chapter that will be collected in Annex II - Climate System Scenario Tables, to the Annex II Chair. Also, please take the time to critically check all the entries in Annex II that are based on your Chapter assessment or that you are using in your chapter assessment. [Thomas Stocker/ WGI TSU, Switzerland]	noted; advice considered by all Chapters.
0-28	0	0				Some consistency needs to be applied across Ch 2, 9,10,11,12,14 to the index names used for the extremes	noted. Chapters have improved consistency with the

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						indices. For instance, annual maximum 5-day rainfall is referred to as R5dmax in Ch 12, RX5day in Ch 9, and R5d in Ch 14, and the warmest 10% of nights as TN90 in Ch10 and TN90p in Ch 2. This should be coordinated amongst all relevant chapters. [Lisa Alexander, Australia]	indices listed in Chapter 2, Box 2.4, Table 1.
0-29	0	0				The IPCC Special Report on Extremes is introduced many times in many chapters and is sometimes referred to as "IPCC SREX" or "SREX". Some consistency should be applied across the chapters and some space could be saved if the report was introduced once (e.g. in Chapter 1) and then only referred to as SREX across the other chapters. [Lisa Alexander, Australia]	noted
0-30	0	0				There is quite a lot of repetition between the chapters on how extremes are defined/described e.g. description of drought, temperature scaling of extreme precipitation, climate indices etc. I'm wondering if there is a way to describe some of the mechanisms/interactions of these phenomena in one place that could be referenced by other chapters, thus reducing the length and readability of each of the chapters? Either that or better coordination between the chapters is required to reduce repetition. This would likely affect Ch 1,2,7,9,10,11,12,14. [Lisa Alexander, Australia]	noted. Chapters have improved consistency with the indices listed in Chapter 2, Box 2.4, Table 1.
0-31	0	0				In the citations WMO 2010 and WMO 2011 are used for the same publication. [Pieter Aucamp, South Africa]	noted; however it is not clear which chapter this comment is referring to.
0-32	0	0				The correct citation is: WMO (World Meteorological Organization), Scientific Assessment of Ozone Depletion: 2010, Global Ozone Research and Monitoring Project-Report No. 52, 516 pp., Geneva, Switzerland, 2011. [Pieter Aucamp, South Africa]	noted
0-33	0	0				<p>Comments on the discussion of climate feedbacks in SOD</p> <p>Discussion of climate feedbacks in SOD is generally based on a zero-dimensional energy balance model (ZDM) of the climate system, in which all quantities are globally averaged. This model is made specific by means of a governing equation in three places in the Report, as listed below.</p> <p>1) Chapter 8, page 8-7, line 36. Here, the assumed relationship between a sustained forcing and the equilibrium response to it is embodied in the equation</p> $\Delta T = \lambda RF \quad (1)$ <p>The notation and terminology used in this instance are as follows: RF (Radiative Forcing) = Instantaneous change in net (down minus up) radiative flux (solar plus longwave; in Wm⁻²) due to an imposed change, evaluated at TOA or the tropopause. ΔT = equilibrium global mean surface temperature response to RF λ = equilibrium climate sensitivity parameter.</p> <p>2) Chapter 9, page 9-71, Eq. (9.1). Here, the time-dependent annual-mean output of an AOGCM subsequent to instantaneously increasing the atmospheric CO₂ content and then holding it fixed is modelled using the ZDM equation</p> $N = F - \alpha \Delta T \quad (2)$ <p>The notation and terminology used in this instance are: N = radiative imbalance at TOA (specified as positive downward in the source reference, Gregory et al, 2004) F = adjusted radiative forcing, again positive downward. α = climate feedback parameter ΔT = perturbation in the global mean surface temperature</p> <p>3) Glossary, page AIII-5. The Climate Feedback Parameter (λ) is here defined as</p>	<p>Taken into account On (A), Rejected on (B).</p> <p>Concerning (A), we have changed the sign convention of climate feedbacks in Ch09 such that individual negative feedbacks have negative sign, but total climate feedback has positive sign so that positive RF produces positive climate sensitivity. Table 9.5 makes explicit this sign change and explains why it is done. Equation 9.1 is no longer used.</p> <p>Concerning (B), the behaviour of the two-zone model in Bates (2012) deviates from that of the zero-dimensional model in a substantial way only if the tropics are "locally unstable", that is, outgoing TOA radiation decreases with surface temperature. This stands in stark contrast to observations as shown, for example, by Murphy, D. M.: Constraining climate sensitivity with linear fits to outgoing radiation. Geophys. Res. Lett., 37, L09704, doi: 09710.01029/02010GL042911.</p>

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						<p style="text-align: center;">$\Lambda = (\Delta Q - \Delta F) / \Delta T$ (3a)</p> <p>i.e., the assumed model is the ZDM, governed by</p> <p style="text-align: center;">$\Delta F = \Delta Q - \Lambda \Delta T$ (3b)</p> <p>The notation and terminology used in this instance are: Δ represents a time-dependent change with respect to an unperturbed climate Q = global mean radiative forcing (positive downward) F = heat flux into the ocean T = global mean surface air temperature.</p> <p>In relation to the above, I would like to offer some suggestions under two headings: (A) Notation and terminology, and (B) Discussion of the adequacy of the zero-dimensional model.</p> <p>(A) Notation and Terminology. I believe it would be useful to adopt uniform notation and terminology throughout the Report. At present there are a number of inconsistencies between Eqs. (1), (2) and (3), and between the Glossary and the text. i. ΔT is used to denote an equilibrium temperature perturbation in Eq. (1) and time-dependent temperature perturbations in Eqs. (2) and (3). A possible solution would be to use ΔT to denote an equilibrium perturbation and T' to denote a time-dependent perturbation. ii. The forcing is denoted RF in (1), F in (2) and ΔQ in (3). I suggest that a uniform notation be adopted for this quantity. iii. The time-dependent energy imbalance of the climate system is denoted N in Eq. (2) and ΔF in Eq. (3). Also, N is defined as the imbalance at TOA while ΔF is defined as the imbalance at the ocean surface. Do the authors wish to retain these separate physical definitions of imbalance in defining the climate feedback parameter? iv. The climate feedback parameter is denoted α in Eq. (2) and Λ in Eq. (3). Again (assuming the energy imbalance is taken uniformly to mean the TOA imbalance) a uniform notation should be adopted. v. All three of Eqs. (1), (2) and (3) use the sign convention that the forcing and the energy imbalance are positive downward. Given this, the climate feedback parameter (suppose we call it Λ) must be positive for the ZDM to be stable. In the Glossary and elsewhere, it is assumed that Λ can be partitioned into separate components; we can write</p> <p style="text-align: center;">$\Lambda = \Lambda_0 + \sum_{i>0} \Lambda_i$ (4)</p> <p>where Λ_0 denotes the Planck component and $\sum_{i>0} \Lambda_i$ denotes the sum of the water vapour, cloud, lapse rate, surface albedo, etc., components. The Planck component Λ_0 is the dominant component of Λ and (given the Glossary sign conventions) must be positive for stability; also, the sum of the other components must be negative (but of a magnitude smaller than Λ_0) to give an appreciable climate sensitivity. However, in Chapter 7, Fig 7.8, the sign convention is such that the Planck component is negative ($\sim -3.2 \text{ W m}^{-2} \text{ K}^{-1}$) and in Chapter 9, Fig. 9.44, the sign convention is such that $\sum_{i>0} \Lambda_i$ is positive ($\sim 2 \text{ W m}^{-2} \text{ K}^{-1}$). The sign conventions in these figures and in the surrounding text is therefore inconsistent with the Glossary sign conventions. This needs to be rectified. Also, the terminology of the legend of Fig. 9.44 (where the "sum of all feedbacks" is called ALL, and the "sum of feedbacks" is called ALL+Planck) is very confusing.</p> <p>(B) Discussion of the adequacy of the zero-dimensional model. Given the central place of the ZDM in the discussion of feedbacks in the Report, it is important that the validity and possible limitations of this model be discussed in some detail. In SOD, a discussion of this issue is given in Chapter 9 (page 9-71, lines 37-48). In that paragraph, three possible sources of uncertainty in the use of the ZDM are listed. I suggest that a fourth possible source of uncertainty be added to the list, namely the assumption that the climate feedback parameter (or radiative response coefficient) is globally uniform. There is</p>	

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						<p>a considerable body of evidence suggesting that this assumption is not valid, and that the tropical and extratropical values of the radiative response coefficient may be quite different; a discussion of this issue is given in Bates (2012), Section 4.1. In that paper it is shown that if the tropical and extratropical values of the radiative response coefficient do differ appreciably, the perturbation dynamical heat transport between the tropics and extratropics may have a significant influence on the climate sensitivity. In such circumstances, the ZDM may be subject to significant limitations. I believe this issue is of sufficient importance to be referred to here.</p> <p>Reference. Bates, J. R., 2012. Climate stability and sensitivity in some simple conceptual models. <i>Climate Dynamics</i>, 38: 455-473. [J. Ray Bates, Ireland]</p>	
0-34	0	0				<p>There is a large variety in format for the Executive Summaries of the different chapters. Some start with a roadmap for their chapter, some not. Some have long paragraphs, some have short paragraphs. Some have statements in bold, other not. Not every chapter has provided cross-references. One executive summary uses "We ...". [Olivier Boucher, France]</p>	noted; for the final draft, Chapters have focused on a common format for their Executive Summaries.
0-35	0	0				<p>Except for a couple of chapters, there is no information in the executive summaries what the ranges refer to. Are they 1-sigma, 2-sigma, min-max ranges, 90% confidence level? The reader should be able to read the executive summary as stand alone text. [Olivier Boucher, France]</p>	noted; a consistent approach has been implemented for the final draft, and a footnote provided in the Executive Summary of all Chapters regarding the use of confidence intervals and likelihoods.
0-36	0	0				<p>An alternative to limiting comments to 1024 characters is to use a different spreadsheet program, rather than Excel. Kingsoft Office is a free alternative to Microsoft Office, and the spreadsheet component does not have Excel's 1024-character limitation. (LibreOffice or OpenOffice also might work, but I've not tried them.) [David Burton, United States of America]</p>	noted.
0-37	0	0				<p>I suggest to include the term "Foraminifera" in the Annex III: Glossary, in the same way that the terms "Diatoms" or "Pollen analysis" are already included. Foraminifera are mentioned often (27 times) in Chapter 5: Information from Paleoclimate Archives in relation to: recent CO2 reconstruction proxies (page 5-10, lines 11 and 50), SST reconstruction (page 5-31, line 9; page 5-33, lines 4 and 6), 18O records (page 5-40, line 19), References section (9 references), Table 5.A.1 (page 5-85), and Table 5.A.2 (page 5-86). [Alejandro Cearreta, Spain]</p>	reject; the authors did not consider this term required elevation to the Glossary of the WGI AR5.
0-38	0	0				<p>The WGI AR5 SOD is effective at reflecting the science of climate change, including generally the limitations of that science. However, arguably the key indicator of climate change at present – the melting of the Arctic – has been empirically demonstrated to be significantly faster than was predicted/projected in AR4, thus it is my duty to point out the importance and limits of the predictive approach still taken by WGI. This predictivism is ideological, not purely scientific, in two senses:</p> <p>1) Firstly the approach has been empirically demonstrated to be lacking – even falsified - by the experience of the limits of success in prediction in purely scientific terms - most importantly with Arctic melting – despite other successes.</p> <p>2) Secondly IPCC predictivism, at least unconsciously supports the ideology of industrialism. This is particularly so when combined with the neo-classical economic approach to policy throughout IPCC reports, which is supported by the implicit promise of many climate scientists to predict the 'costs' of climate change for CBA. However, IPCC predictivist science on its own supports industrialism by offering the promise of predicting/projecting the Earth System for policy purposes in a way that cannot be supported empirically or philosophically. The ideological nature of this can be seen in Charlesworth M & Okereke C (2010, Policy responses to rapid climate change: An epistemological critique of dominant approaches, <i>Global Environ. Change</i>, 20: 121-129, doi:10.1016/j.gloenvcha.2009.09.001) in particular, the discussion of Francis Bacon's utopia/dystopia New Atlantis. More importantly the limitations of the approach can be seen in the limited success of IPCC predictivism in reducing greenhouse emissions, since IPCCs first reports in 1990. Please see my comments on the first order draft for more specifics and suggestions of possible places to raise the above issue in the text of the WG1 assessment report text. [Mark Charlesworth, United Kingdom]</p>	noted

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0-39	0	0				The term "Clausius-Clapeyron" occurs in chapter 2 of the report 5 times, and also in chapters 3 (2), 7 (1), 10 (1), 11 (3), 12 (4) and 14 (1), in each case as support for the expectation that changes in specific humidity should be proportional to changes in temperature with a coefficient of ~7% K-1 (or that the coefficient should be smaller for precipitation). This suggests that a box or diagram about the expectation might be useful, if only to reduce repetition. The accompanying assumption of constant relative humidity could be assessed in the same place. [J. Graham Cogley, Canada]	The Clausius Clapeyron relationship is included as a Glossary entry (see Annex III).
0-40	0	0				I have exclusively focussed my review on chapter 5, and my comments are strictly formulated on sub-sections for which I estimate I have ALL the competences to evaluate the document. [Frédérique Eynaud, France]	noted
0-41	0	0				We strongly suggest adding the word "index" to the global average surface temperature so that the term to be employed becomes "global average surface temperature index" as the average does not represent a temperature in the strict thermodynamical sense. Nevertheless, the index has proven in all the IPCC Assessment reports and other publications to be a very useful metric for climate change. [Andrew Ferrone, Germany]	reject; "global average (or mean) surface (air) temperature" is a long established and easily understood term in the climate science and policy community.
0-42	0	0				Comments I have submitted are based on discussions and joint preparation with Michael Mastrandrea and Katharine Mach of the IPCC WGII TSU. [Christopher Field, United States of America]	noted
0-43	0	0				The style and format of the Executive Summaries are quite different between the chapters. I suggest a somewhat stronger coordination. [Jan Fuglestedt, Norway]	noted; for the final draft, Chapters have focused on a common format for their Executive Summaries.
0-44	0	0				Glossary: 1- The definition of ocean acidification on page AIII-20 is incorrect. The one to use is the one defined at the IPCC meeting in Okinawa (2011). "Ocean acidification refers to a reduction in the pH of the ocean over an extended period, typically decades or longer, which is caused primarily by uptake of carbon dioxide from the atmosphere, but can also be caused by other chemical additions or subtractions from the ocean. Anthropogenic ocean acidification refers to the component of pH reduction that is caused by human activity." The reference is: PCC, 2011: Workshop Report of the Intergovernmental Panel on Climate Change Workshop on Impacts of Ocean Acidification on Marine Biology and Ecosystems [Field, C.B., V. Barros, T.F. Stocker, D. Qin, K.J. Mach, G.-K. Plattner, M.D. Mastrandrea, M. Tignor and K.L. Ebi (eds.)]. IPCC Working Group II Technical Support Unit, Carnegie Institution, Stanford, California, United States of America, pp. 164. [Jean-Pierre Gattuso, France]	accept; Glossary entry is now based on the mentioned IPCC 2011 workshop report
0-45	0	0				Glossary: In the definition of pH, "+" in H+ should be a superscript. [Jean-Pierre Gattuso, France]	noted; copy editor.
0-46	0	0				Glossary: Plankton lives throughout the water column. I suggest: "Plankton Organisms, essentially microorganisms, living in aquatic systems. Phytoplankton lives in the upper layers and depend on photosynthesis for their energy supply. Zooplankton mostly feed on phytoplankton and other planktonic organisms such as Bacteria and Archaea." [Jean-Pierre Gattuso, France]	reject; proposal by the reviewer still refers to plankton living in the upper layers, so this is consistent with the existing WGI AR5 Glossary definition.
0-47	0	0				Glossary: The definition of the solubility pump does not mention its main outcome, which is physico-chemical process to transport dissolved inorganic carbon from the ocean's surface to its interior. [this is correctly addressed in the definition of the biological pump] [Jean-Pierre Gattuso, France]	accept; See revised Glossary definition

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0-48	0	0				Glossary: The definition of Gross Primary Production is either incomplete or wrong. The present definition applies to terrestrial primary producers, not aquatic ones. [Jean-Pierre Gattuso, France]	accept; See revised Glossary definition
0-49	0	0				This reviewer faces the embarrassing problem that the authors have ignored his previous comments and recommendations addressed in ROFOD, that the authors have not discussed nor even cited the relevant literature that the reviewer explicitly addressed, that the authors have masked relevant and important information previously available in Fig. 8.16c of FOD. Was it to avoid discussing it in spite of the previous explicit recommendation of the reviewer ? Again contrary to the explicit recommendation of this reviewer, the authors have even not summarized the mechanism of atmospheric greenhouse effect which is central in AR5, and the ingredients retained in the models, in particular regarding the dissipation of heat absorbed by greenhouse molecules (purely radiative or with a contribution of thermal conduction by collision processes ? How much of each ? Depending on altitude and/or pressure ?). [François Gervais, France]	There is no basis to suggest that the reviewers previous comments have been ignored. Review comments need to be considered, but not necessarily acted upon if authors disagree -- all responses to reviewer comments will be available upon completion of WGI AR5.
0-50	0	0				Again contrary to the explicit recommendation of this reviewer, information from infrared spectra, in particular emissivity spectra of the Earth and Output Longwave Radiation spectra, are still ignored in AR5. If one complies, therefore, with the common procedure of peer review process of scientific literature, the conclusion would be that the present « paper » is not suitable for publication in the present form. Major changes are required to reach and fulfill the criteria of a scientific work and the requirements of the scientific method. [François Gervais, France]	There is no basis to suggest that the reviewers previous comments have been ignored. Review comments need to be considered, but not necessarily acted upon if authors disagree -- all responses to reviewer comments will be available upon completion of WGI AR5.
0-51	0	0				The use of uncertainty information is inconsistent between chapters - for example, Chapter 2 uses +/- ranges, while Chapter 3 uses confidence intervals. [Government of Australia]	noted; a consistent approach has been implemented for the final draft to the extent possible, and a footnote is provided in the executive summary of all chapters regarding the use of confidence intervals and likelihoods.
0-52	0	0				The term 'irreversible', used in numerous places, should be clearly defined as its general use in the report (to describe an aspect of the climate system which would not revert to its previous state even if climate forcings reverted to their previous state) differs somewhat from the standard English meaning which describes something which cannot be reversed under any circumstances. The contrast is particularly striking at chapter 4 page 36, where a change is described at line 6 as 'irreversible' and then line 8 states that it would take several hundred years to reverse. [Government of Australia]	"Irreversibility" is defined in the glossary (see Annex III) and Chapter 4's useage is consistent with the definition: "Irreversibility: A perturbed state of a dynamical system is defined as irreversible on a given timescale, if the recovery timescale from this state due to natural processes is significantly longer than the time it takes for the system to reach this perturbed state. In the context of WGI, the time scale of interest is centennial to millennial. See also Tipping point."
0-53	0	0				There are numerous aspects which are covered in multiple chapters. More comprehensive cross-referencing would be of assistance to readers - for example, someone looking for information on observed sea level change would probably expect at first glance to find it in chapter 13, not chapter 3. It may also be worth stating explicitly where relevant (for example, description of regional changes at a higher level of detail) if more information is expected to be provided in the WG2 report. [Government of Australia]	noted; cross referencing between chapters, and between working group reports has been improved for the final draft.
0-54	0	0				The internal ordering of chapter 2 and 3 is inconsistent - chapter 3 leads with ocean temperatures, whereas in chapter 2 temperatures are in the middle of the chapter after chemistry and radiative forcing. Not sure if this can be changed but may be more logical for them to be consistent. [Government of Australia]	noted; Chapter 3 follows closely the Panel approved outline. Chapter 2 has revised its outline at the SOD stage to better reflect the process chain from observed changes in drivers, to changes in the radiation balance, to climate change. This proposal was also made as part of the FOD review comments to Chapter 2. Chapter 2 has followed this suggestion for rearrangement while covering all parts from the Panel approved outline.

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0-55	0	0				The baseline period for climate change projections is 1986-2005. As discussed in the report, because models are biased in their prediction of absolute values of observed variables, many methods for applying projections involve the application of 'change factors' to the climate of the 'baseline' period. However, there is no acknowledgement in the report of the difficulties in characterising an appropriate baseline 'climate' using a 20 year period for areas that have high interannual and decadal variability in rainfall and streamflow (such as SE Australia where 20 yr moving averages in streamflows, for example, can deviate more than +/- 20% from the long term average). Prudhomme et al (2010) J Hydrol 390, 198-209 (Section 4) discuss the fact that this is a non-trivial issue. This issue is further complicated by the fact that there is already a climate change "signal" in the baseline period. An explicit recognition of this issue in relevant sections of the report (SPM, TS, Chs 9 & 12 in particular) would be desirable. See also Ch 12 comment. [Government of Australia]	noted; This issue of internal variability is addressed in Chapter 12, in the uncertainty section, explicitly recognizing that no amount of averaging can get rid of it entirely, and discussing the relative importance of the source of variability depending on the regional scale, the time scale and the variable of interest. Please also refer to the treatment of this theme in Chapter 11.
0-56	0	0				Implicit in the time periods for which projections are provided (2016-2035 and 2081-2100) is the assumption that changes will occur gradually. Particularly for locations (such as southern Australia) where decreases in cool season precipitation (~April-Sept) will result from the southerly movement of the mid-latitude westerly wind belt and associated storm tracks, changes can (and indeed have been observed to - e.g. SWWA) occur in a more step-like fashion, as the synoptic systems which used to provide relatively reliable winter rainfall pass much more infrequently across the land mass. Jones (2012) also identifies step changes in temperature in the observational record for SEA. The possibility of step changes in climatic variables as well as trends should be discussed in the report. [The reference is: Jones, R. N. (2012), Detecting and attributing nonlinear anthropogenic regional warming in southeastern Australia, J. Geophys. Res., 117, D04105, doi:10.1029/2011JD016328.] [Government of Australia]	this is incorrect. There is no such assumption being made in the selection of 20-year averages for the standard projection period. Shorter term variability and step-like changes are not being excluded in the assessment as can be seen from, e.g., Chapters 11 and 12, and the annual mean timeseries in Annex I. However, assessed projected ranges always refer to a certain time period (e.g. 20 years) and this acts as a low-pass filter.
0-57	0	0				General comments [Government of Benin]	no comment provided
0-58	0	0				We find this draft Summary for Policymakers (SPM) of the WGI contribution to the Fifth Assessment Report (AR5), a very good paper, that presents new findings with quite enough illustrations. The document is very informative and provides the update figures and interesting results/findings with regard to the assessment of the scientific and technical aspects of climate change. Nevertheless, the drafting style needs to be improved (for e.g. some sentences do not convey the message in an appropriate way). In addition, tables and figures require some comments. It is also important to add a glossary. At last, I much appreciate all efforts made to elaborate this precious tool addressed to policymakers. Thanks so much to all authors involved in the preparation of this paper, [Government of Benin]	noted; comment applies to SPM and will be considered in the revisions.
0-59	0	0				We understand that a list of acronyms and regional abbreviations will be included in the report as an annex. In future, it would be helpful to include these during the SOD review. [Government of Canada]	noted
0-60	0	0				Draft as a whole seem to cover well the vast literature available. However, there are several sections and topics where clarity of the text could be enhanced. Especially, readability of the text should be improved. [Government of Finland]	noted
0-61	0	0				All figures need to be consistent in SPM, TS and in the underlying report. Remaining differences need to be explained. CMIP3 and CMIP5 need to be consistently presented across report, TS, SPM, including assessment of quality and uncertainties. [Government of Germany]	noted; Much effort has been made in the final draft to ensure figures are consistent between the SPM, TS, and underlying report. In fact, a number of additional Figures have been included in the TS to provide in the TS more direct links from the figures in SPM to the figures in the underlying Chapters. However, figures may not be identical, as by definition, the summary document may include less detail than contained in the underlying chapters.
0-62	0	0				General remark: We appreciate the fact, that certain cross cutting themes and cross cutting methodologies have been agreed on by the Panel. We are very grateful to the IPCC Vice chairs who have taken the responsibility for the implementation of the Panel's decisions on the treatment of cross cutting issues in AR5. Consistent treatment will greatly improve the value of AR5. [Government of Germany]	noted
0-63	0	0				How is "pre-industrial " defined? There should be a consistent definition and usage throughout the reports (across WGs). [Government of Germany]	"pre-industrial" is defined in the WGI Glossary (see Annex III).

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0-64	0	0				Much of the information both in the text and the graphics on the change in climate parameters is given wrt the reference level 1986-2005. The information needed by policy makers is however the change since pre-industrial conditions. This is one major flaw of this report. Under UNFCCC, countries have agreed to limit warming to below 2 degree C compared to the pre-industrial level. IPCC (across working groups) should respond to the clear policy need from UNFCCC and give information on the climate state for this reference level. This might not be possible for all variables, but we encourage the authors to provide information wrt to pre-industrial whenever possible. This statement applies to the entire report. Make sure the presentation of temperature changes is consistent with the presentation if the other Working groups so that references to impacts and mitigation scenarios can be made. [Government of Germany]	noted; Relevant text and graphics now include a listing of the offsets required to compare the changes reported in AR5 with pre-industrial, and other previously used reference periods. See for example, Table SPM 2, note (a).
0-65	0	0				Please be consistent with AR4. In particular it should be possible to compare trends. This is difficult, if reference periods are not consistent as e.g. for sea level rise (AR4: 2090-2099 relative to 1980-1999. AR5: 2081 to 2100, compared to 1986 to 2005). [Government of Germany]	noted; Relevant text and graphics now include a listing of the offsets required to compare the changes in temperature reported in AR5 with pre-industrial, and other previously used reference periods. However, we do not intend to repeat all the end of century projections for the 10-year AR4 averaging period 2090-2099 as for the AR5, we have decided to stick to 20-year average periods. Furthermore, complete comparability is limited by the transition from CMIP3 to CMIP5 and from SRES to RCP scenarios.
0-66	0	0				Reasons for the lower increase in the global mean temperature in the last decade must be given consistently in the TS, and throughout the report, information could be taken from Chapter 10. [Government of Germany]	accept; a comprehensive technical box has been added Chapter 9 (Box 9.2). This box draws from the assessment of various Chapters in WGI AR5. See also Technical Summary Box TS.3 and revised SPM.
0-67	0	0				The "Guidance Notes for Lead Authors of the AR4 on Addressing Uncertainties," says in para 4:"Be aware that the way in which a statement is framed will have an effect on how it is interpreted". This should be considered in this report. E.g. putting the level confidence at the beginning of the statement "There is Medium Confidence that the current recession of glacier length is unusual..." is much weaker than "The current recession of glacier length is unusual (Medium Confidence)." Or "Observations have recession of glacier length and it there is Medium Confidence that this recession is unusual..." [Government of Germany]	reject; the AR5 Guidance Note also states that key findings regarding a variable should be characterized using calibrated uncertainty language that conveys the most information to the reader. The current approach with the in-text uncertainty characterization does in many cases do this. And it has a long-standing implementation within the WGI reports.
0-68	0	0				The figures in the TS and even more in the SPM will be very important for outreach. They should be simple without diluting the scientific content. An informed layperson should understand the basic messages without reading the text. [Government of Germany]	noted
0-69	0	0				The treatment and communication of uncertainty is one of the most critical issues in IPCC. Therefore the agreed language in the "Guidance Notes for Lead Authors of the AR4 on Addressing Uncertainties" should be followed most closely. Please do not use expressions from AR4, this would dilute the findings of the report as they will become more difficult to understand for non-experts, i.e. most decision makers. [Government of Germany]	noted
0-70	0	0				The use of scientific jargon should be strictly avoided. All scientific terms should be supported by simple explanations and wherever possible short definitions in the text for non-experts. At least all expressions used in the SPM and TS should be explained in the Glossary. [Government of Germany]	noted; although it is not possible that the Glossary can cover all expressions used in the TS/SPM.
0-71	0	0				There are some inconsistencies between the SPM, the TS and the underlying report, both in the texts and in the figures. Please check and modify. [Government of Germany]	noted; more specific pointing to inconsistencies would have been appreciated.
0-72	0	0				Please check, if all figures in the SPM and in the TS are consistent with those in the underlying chapters. Currently there are especially inconsistencies with regard to figures showing time series of the temperature in models and in observations. The end point of the time series should be 2011, as far as possible, and this end point should be consistent throughout the report for a given variable. (See for example Fig TS.7, TS.12, Fig 10.1, Fig 9.8, Fig 11.12.) [Government of Germany]	Much effort has been made in the final draft to ensure figures are consistent between the SPM, TS, and underlying report. However, figures may not be identical, as by definition, the summary document may include less detail than contained in the underlying

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							chapters. It is not clear why the reviewer considers 2011 to be the necessary end point for time series. The end point will be as up-to-date as possible.
0-73	0	0				Please check, if the references for the figures in the SPM and the TS to the underlying report are correct. [Government of Germany]	noted
0-74	0	0				Glossary: include the term "polar vortex" in the Glossary [Government of Germany]	reject; this term has limited use in the report, and therefore the author team did not consider it warranted elevation to the Glossary.
0-75	0	0				Glossary: Thank you very much for the very helpful glossary. [Government of Germany]	noted
0-76	0	0				Glossary: Please add "lapse rate" in the Glossary [Government of Germany]	added to Glossary
0-77	0	0				Glossary: Please add a more detailed description of the scenario process, including information on the socio-economic scenarios, and the parallel approach for all three WGs. [Government of Germany]	reject; Glossary is not the place for a description of the scenario process. Details on the scenarios used in the WGI AR5 can, e.g., be found in WGI Chapter 1, Box 1.1, in Chapter 12, Sections 12.3.1 and 12.4.9, or in the Technical Summary, Box TS.6 "The New RCP Scenarios and CMIP5 Models"
0-78	0	0				Glossary: Please add the definition of chemical species like "CH4", "N2O" in the Glossary (as done for "CO2"). [Government of Germany]	added to Glossary
0-79	0	0				Glossary: Please include "lapse rate" in the Glossary [Government of Germany]	added to Glossary
0-80	0	0				Glossary: Please include "running mean" in Glossary. [Government of Germany]	reject; the author team did not consider this term warranted elevation to the Glossary. The specific details of how a running, smoothed, or filtered mean etc. are calculated should be available in table and figure captions.
0-81	0	0				Glossary: Please include abbreviation AOGCM for non-experts, at least in glossary. [Government of Germany]	added to Glossary
0-82	0	0				Glossary: Please include abbreviation OHC and it include it in the Glossary. [Government of Germany]	OHC' will be included in the list of abbreviations for the Final Draft
0-83	0	0				Glossary: Please include AMOC in the Glossary. [Government of Germany]	MOC is in the Glossary
0-84	0	0				Glossary: Please include Brewer Dobson circulation in the Glossary. [Government of Germany]	added to Glossary
0-85	0	0				Glossary: Please include QBO in the Glossary. [Government of Germany]	added to Glossary
0-86	0	0				Glossary: Please include SPCZ events, SACZ and MJO for non-experts, at least in the Glossary. [Government of Germany]	SPCZ and MJO have been added to Glossary
0-87	0	0				Glossary: Please include the C-isotopic ratio for non-experts, at least in the Glossary. [Government of Germany]	reject; the author team did not consider this term warranted elevation to the Glossary.
0-88	0	0				Glossary: Please include the term "forecast" in the Glossary. [Government of Germany]	reject; the author team did not consider this term warranted elevation to the Glossary.
0-89	0	0				Glossary: The content of the annexes as agreed by the Panel at IPCC-31 in the outline for AR5 has been changed by the authors. The following annexes have been agreed by the Panel: Annex I: Atlas of Global and Regional Climate Projections / Annex II: Glossary / Annex III: Acronyms and Regional Abbreviations / Annex IV: List of Authors / Annex V: List of Reviewers. The SOD has now an additional annex on "Climate System Scenario Tables", which is appreciated and very useful. However, we would like to ask for the provision of the agreed annex on "Acronyms and Regional Abbreviations" which is needed for efficient and effective use of the report. [Government of Germany]	The agreed Annex on acronyms will be available for the Final Report.

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Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
0-90	0	0				Glossary: Please include explanations about altimeters and tide gauges. [Government of Germany]	added to Glossary
0-91	0	0				Glossary: What is surface wave height? Please include, at least in the Glossary [Government of Germany]	reject; not a widely used term in the report. Note that 'Significant Wave Height' is, however, included in the Glossary.
0-92	0	0				Glossary: It would be very useful if all keywords that are included in the Glossary could be linked internally in the pdf-version of the document. At the moment, they are printed in red, if they are mentioned in other paras. [Government of Germany]	reject; there are currently no plans to include internal links.
0-93	0	0				There are also new aspects in the report compared to the earlier IPCC reports (3rd and 4th). For example, in earlier reports Radiative Forcing (RF) was used as a main parameter to quantify the energy balance of the earth-atmosphere system. In the present report Adjusted Forcing (AF) concept is introduced which allows all variables (except ocean temperature and sea ice cover) to adjust to perturbations. AF and RF values significantly differ for anthropogenic aerosols because aerosols affect for example the microphysics properties of clouds in much shorter time scales than compared to ocean temperature. [Government of India]	noted; comment does not seem to require a response.
0-94	0	0				Time series of anomalies such as temperature in this assessment report are depicted using some different baseline period, and this could cause confusion for policymakers. Therefore, please clearly indicate the baseline period in the caption of each figure. Furthermore, at least in SPM, please indicate the "before the Industrial Revolution" baseline when their baseline period is not "before the Industrial Revolution". Since the baseline period of the global warming negotiation in UNFCCC is set as "before the Industrial Revolution", a clear indicated baseline is very useful. [Government of Japan]	Relevant text and graphics now include a listing of the offsets required to compare the changes reported in AR5 with pre-industrial, and other previously used reference periods. See for example, Table SPM 2, note (a).
0-95	0	0				We congratulate the TSU and WG1 authors on the production of the Second Order Draft for AR5 and thank you for all the hard work. [Government of New Zealand]	noted
0-96	0	0				The report gives a good overview over observed and future change in different elements of climate. However since WGI AR5 also will serve as a basis for WGII, WGIII and the SYR where the risk assessment – probability x consequence – and risk management will be addressed, we propose that WGI report and SPM also include information of the probability related to relevant outcomes e.g. about the probability for relevant extreme events such as return values and return periods and about the probability to reach certain levels of warming. [Government of NORWAY]	noted; Return values and return periods for temperature and precipitation extremes are assessed in Chapters 2 and 12. See also the IPCC SREX which is considered a component of the overall WGI fifth assessment cycle.
0-97	0	0				In the Executive Summary the explanation of terms "high, medium, low confidence" should be give [Government of Poland]	accept; footnote has been added to the executive summaries introducing these terms. For more details see the IPCC guidance paper on uncertainty
0-98	0	0				FAQ are generally presented at the end of each section. It would be perhaps better to aggregate all FAQ at the end of each chapter to facilitate reading. [Government of Spain]	reject; FAQs will be repositioned where appropriate during the final layout of the chapters.
0-99	0	0				We welcome this report and thank the lead authors for their hard work. However the report perhaps overly long. Figures are often very complex, containing several small pictures and perhaps too colourful (with little consideration for the colour-blind or for B&W photocopying). In many cases the lines on figures are very 'thick' (large pt.), making precise interpretation impossible and often, figure captions are very long and detailed indeed. [Government of United Kingdom of Great Britain & Northern Ireland]	noted; improved attention to these stylistic details have be given for the final draft. Figures will undergo further editorial changes if needed.
0-100	0	0				The AR5 seems to be using a baseline climate for 1986-2005 against which changes are expressed. The choice of this 20-year period runs counter to 'standard' 30-year climate baselines. Also, this may lead to real confusion in the interpretation of the projections (e.g. see Fig. SPM5). The scientific and policy communities tend to refer to the 2C target relative to pre-industrial. On page SPM-13 the text uses both and the message is unclear whether we are facing a 2C or 4C world! In fact the numbers seem more conservative than in AR4 but I doubt they are in reality. 1986 to 2005 is taken as a baseline for global temperature rise and other parameters in some chapters. Text should be included that provides a clear approach for converting from 1986-2005, similar to that provided in the AR4. [Government of United Kingdom of Great Britain & Northern Ireland]	noted; relevant text and graphics now include a listing of the offsets required to compare the changes reported in AR5 with pre-industrial, and other previously used reference periods. See for example, Table SPM 2, note (a).
0-101	0	0				As was the case with AR4, it will be very important to define succinctly the meaning of 'virtually certain' etc in	noted; efforts have been made in the final draft to

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						terms of levels of confidence. Other words such as 'significant' (relative to what?), 'unusual' and 'unprecedented' are also introduced but not defined and not apparently used thereafter. [Government of United Kingdom of Great Britain & Northern Ireland]	avoid vague, poorly defined terms.
0-102	0	0				Figures and diagrams are generally based on anomalies rather than absolute changes. Very often, it would be sensible to include the absolute values as well. [Government of United Kingdom of Great Britain & Northern Ireland]	noted; decision to show anomalies vs. absolute values is based on the judgment of the chapter experts.
0-103	0	0				References to the RCPs and their characteristics need to be made consistent throughout the report. Ideally, there should be one section containing a detailed description of the RCPs and their characteristics to which all other chapters refer, rather than attempting to describe the RCPs each time. [Government of United Kingdom of Great Britain & Northern Ireland]	noted; Details on the scenarios used in the WGI AR5 can, e.g., be found in WGI Chapter 1, Box 1.1, in Chapter 12, Sections 12.3.1 and 12.4.9, or in the Technical Summary, Box TS.6 "The New RCP Scenarios and CMIP5 Models"
0-104	0	0				Too many unnecessary acronyms are used which often make the report (and especially the SPM) difficult to read, and means that the reader has to search for the explanation. One example is the use of "AF" in Chapter 6 instead of "airborne fraction". This isn't necessary and makes it harder for the reader, particularly when the same acronym is used for "adjusted forcing". Another is the use of IN instead of "ice nuclei". [Government of United Kingdom of Great Britain & Northern Ireland]	noted
0-105	0	0				It would be useful to include a reference in each chapter to an explanation of the terminology used for the un/certainty and probability categories used in the AR5 assessment process. [Government of United Kingdom of Great Britain & Northern Ireland]	footnote has been added to the executive summaries introducing these terms. More details are provided in Chapter 1 of the WGI AR5, Section 1.4, in the Technical Summary, Box TS.1, or in the IPCC Guidance Note on the consistent treatment of uncertainty.
0-106	0	0				There needs to be a more consistent way of characterising groups of greenhouse gas and other climate forcers throughout the whole report. Some chapters refer to long-lived GHGs (LLGHGs), some refer to well-mixed GHGs (WMGHGs), some refer to near-term climate forcers (NTCF). Depending on the chapter, methane is placed in any one or more of these groups. E.g. Chapter 6, p.7, line 4 refers to methane as a long-lived climate forcer. Chapter 8 refers to methane as both a WMGHG and a NTCF. We note that there are ongoing policy discussions on short-lived climate forcers (SLCFs), e.g. under UNEP, which also includes methane and on greenhouse gas metrics which refer to different groups of GHGs/climate forcers. The AR5 must provide concise and consistent information. [Government of United Kingdom of Great Britain & Northern Ireland]	noted; efforts have been made in the final draft to avoid such inconsistencies. Particular classification of CH4 in Ch6 has been corrected.
0-107	0	0				UK English or US English? Should be consistent throughout the report. [Government of United Kingdom of Great Britain & Northern Ireland]	noted; editorial.
0-108	0	0				SUGGESTION: perhaps Figure creators should be credited in captions or in the figure itself? They have all spent a lot of time making special figures which may never be published elsewhere and deserve to be recognised for this. [Ed Hawkins, United Kingdom]	Thanks for this suggestion. In the WGI AR5 all contributions to a particular Chapter are being acknowledged through their listing as CLA, LA, RE, or Contributing Author (CA) on the Chapter front page. However, partly as a result of the suggestion, the Chapter CLAs will now have the possibility to acknowledge individual contributions to the WGI AR5 ChXX in a newly created acknowledgment section to be included in the final report.
0-109	0	0				I understand the importance of radiative forcing in the discussion of the climate changes, but recent suggestions on the non-radiative forcing should not be ignored: e.g., Radiative Forcing of Climate Change: Expanding the Concept and Addressing Uncertainties Committee on Radiative Forcing Effects on Climate, Climate Research Committee, National Research Council. This is because the non-radiative processes largely affect local climates which are directly influence out local life. [Kiminori Itoh, Japan]	noted; Such forcings are being considered as part of the WGI AR5.
0-110	0	0				Before around 2007, a climatologist who claimed about the Medieval Warm Period was regarded as out of	noted; Intention of the comment is not clear.

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						date. But, after only five years, the MWP has been established (or re-established). I wonder if such a kind of scientific field can convince the public and governments. [Kiminori Itoh, Japan]	
0-111	0	0				General: I have examined chapters 1, 2, 7 and 8 carefully and provided comments and suggestions for modifications. (also SPM and TS). Although in many cases questions that I raise regarding chapters 1 and 2 are dealt with in later chapters, I maintain these questions, believing that they merit mention in the early chapters. I have looked at and appreciated some of the other chapters but except for the executive summaries and introductions, I have not had the time or expertise to provide many comments. [Robert Kandel, France]	noted
0-112	0	0				<p>My comment deals with the basic physical approach to the climate change and the sensitivity of the climate for radiative forcing.</p> <p>The weakest feature of WG1 SOD is that there is no experimental evidence about the sensitivity of the climate. The estimation is based on the theoretical circulation models (about 60 different), which are conflicting with each other particularly in the cloud feedback. There is no real test possibility. The climate science should look for more realistic methods to estimate the sensitivity.</p> <p>During a few years we have searched for another approach to the climate change problem. In our first paper¹ we derived the climate sensitivity and the response time using very simple methods and merely the observed or measured values of the climate. The sensitivity was about 0.06 K/W/m² corresponding to 0.24 °C with the doubling of CO₂ concentration. The response time was about 1.3 months. Note that we have only one adjustable parameter G. In the first paper we derived a new expression for the temperature change as follows</p> $\Delta T = R\Delta Q - R\Delta G \cdot (p - p_e), \quad (1)$ <p>where ΔQ is the change of the forcing, $\Delta G = \text{constant} \cdot \Delta \theta / \theta$, θ is the relative humidity, and p, p_e are the saturation water vapor pressure at the present condition and at the temperature 255 K, respectively. Eq. (1) can be rewritten in the following form:</p> $\Delta T = R\Delta Q + \Delta R \cdot 247 \text{ W/m}^2. \quad (2)$ <p>[Jyrki Kauppinen, Finland]</p>	<p>reject; in the WGI AR5, many lines of evidence are being considered in the assessment of Equilibrium Climate Sensitivity, including observational evidence. See Chapter 12, Box 12.2 and Technical Summary, TFE.8 for a summary of the assessment.</p> <p>The reviewer's reasoning contains fundamental flaws, such as the incorrect claim that climate models prescribe the relative humidity; internally inconsistent conversion between adjustment timescale and climate sensitivity; incorrect assumption that the forced response to the seasonal cycle in insolation permits an evaluation of climate feedbacks.</p>
0-113	0	0				<p>In the submitted manuscript 2 we demonstrated the usefulness of this approach. It turns out that ΔR is zero, if the relative humidity is constant, which is also the basic assumption in most of the circulation models. If the relative humidity changes, then ΔG and ΔR are proportional to that change in the relative humidity and we have to use two terms to calculate ΔT. The key process in the climate is the change of the average water mass flow through the atmosphere. The change of the mass flow changes further the relative humidity, cloudiness, and precipitation. We can calculate that one per cent increase in the low cloud cover decreases the temperature by 0.11 °C.</p> <p>Using the measured anomalies for the mean global temperature and the low cloud cover between years 1983 and 2008 we can calculate very well the temperature anomaly using Eq. (1) or (2).</p> <p>About 90% of the changes are due to the changes of cloudiness and less than 10% due to the CO₂. In addition, similar results can be calculated using the change of relative humidity. In this case those changes have to be multiplied by -0.2 °C/% approximately. The relative humidity explains the temperature anomaly from 1960 up to this date. (not included in the manuscript).</p> <p>[Jyrki Kauppinen, Finland]</p>	see response to comment 0-112
0-114	0	0				<p>Very rough estimation can be derived from the fact that at the present condition of the climate $T - T_0 \approx RQ$, where Q is the total forcing (326 W/m²), $T_0 = 255$ K. From the above simple equation we have</p> $\Delta T \approx R\Delta Q + \Delta R \cdot Q$ <p>which is Eq. (2) with $Q = 247$ W/m². The forcing Q in this equation is not 326 W/m², because the non-linear curve of R as a function of T (see Fig. 1 in paper 1).</p>	see response to comment 0-112

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						The sensitivity in WG1 SOD is estimated by the circulation models, which include a wrong assumption that the relative humidity is constant. This assumption is the same as $\Delta R = 0$ or the second term is zero. This is not true in the real climate. So it is very clear when explaining the temperature increase of 0.8 °C during the last century only by the first term $R\Delta Q$ that R is much bigger ($\approx 1 \text{ Km}^2/\text{W}$) than ours (0.06 Km^2/W). [Jyrki Kauppinen, Finland]	
0-115	0	0				The manuscript "Major feedback factors and effect of the cloud cover in the climate", J.Kauppinen, J.Heinonen, and P.Malmi derives the climate sensitivity around the present atmospheric condition. In this paper we were able to calculate R and G directly from the global energy budget 3. The sensitivity is the same as in paper 1 within 2%. This study gave the major portions in the negative feedback coefficients, which are roughly clouds 63%, latent cooling 28% and water vapor 9 %. (The manuscript is not included in this comment). It is important to note that we have used at least three completely different methods all based on the different experimental observations. All the derived sensitivity values are in very good agreement. So the WG1 SOD has more than one order of magnitude bigger sensitivity of the climate than our results. There are many experimental situations, which proof that the sensitivity in WG1 SOD is strongly overestimated. In this comment I mention only two. The first one is the so called faint young sun paradox (see paper 1), which is valid if the climate sensitivity is too big. The second case is the response time and the step response of the climate. The response time is RC, where C is an effective global heat capacity per m2. Our R gives the global time constant of about 1.3 months, which is in a good agreement with the observed phase lag between the solstice and the temperature maximum in the summer (1.15 months). The WG1 SOD value gives the lag of 2.7 months, which is certainly wrong. The warmest time of the summer is not in the middle of September !! The big R deforms also the step response of the climate, which has an unrealistic slope corresponding to a very long response time. [Jyrki Kauppinen, Finland]	see response to comment 0-112
0-116	0	0				We think that the WG1 report is misleading, because the contribution of greenhouse gases like CO2 is strongly overestimated due to a wrong basic assumption and the report cannot explain the other climate changes. For the ordinary people (not scientist!) the strong influence of CO2 on the climate is difficult to believe, because the global mean temperature has been slightly decreasing during the last 15 years in spite of the significant increasing of CO2. WG1 SOD gives a very poor explanation but our manuscript explains it very clearly in Fig. 3 up to 2008. On the other hand, we understand that it is not realistic to stop the whole process. However, we hope that the WG1 SOD is transparent and takes into account all the scientific work. So we insist that our model and results should be included in some form in the report. In addition it is also very important to emphasize that the circulation models do not solve the climate problem. It is most important is to realize that the constant relative humidity is very bad assumption in the climate. [Jyrki Kauppinen, Finland]	see response to comment 0-112
0-117	0	0				1. Jyrki Kauppinen, Jorma T. Heinonen, and Pekka J. Malmi, "Major Portions in Climate Change: Physical Approach", International Review of Physics Vol 5. No. 5 260-270, (2011) [Jyrki Kauppinen, Finland]	see response to comment 0-112
0-118	0	0				2. Jyrki Kauppinen, Jorma T. Heinonen, and Pekka J. Malmi, " Influence of clouds on the global mean temperature of the climate", submitted [Jyrki Kauppinen, Finland]	see response to comment 0-112
0-119	0	0				3. K.E.Trenberth, J.T.Fasullo, and J. Kiehl, " Earth's global energy budget", Bulletin of the American Meteorological Society, 90:311-324, (2009) [Jyrki Kauppinen, Finland]	see response to comment 0-112
0-120	0	0				Refs. 1 and 2 have been emailed to wg1-it@ipcc.unibe.ch [Jyrki Kauppinen, Finland]	see response to comment 0-112
0-121	0	0				OBS! My comments should include refs. 1 and 2 ! However, it is very difficult to include these papers in this workbook. [Jyrki Kauppinen, Finland]	see response to comment 0-112
0-122	0	0				The IPCC WG1 report will be the first IPCC publication that refers to the RCP scenarios which are highlighted in the draft and used across the summaries and many of the chapters. Being their first appearance it is	noted; Please see Chapter 1, Box 1.1, and further detail provided in Chapter 12, Sections 12.3.1 and

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						important that the RCPs be described accurately; however, their description in the draft is in many places incorrect and can be misleading. Furthermore, descriptions are given without referring to the peer reviewed literature from the developers of the RCPs. It is important that the RCPs be described in an accurate and balanced way, referencing the peer reviewed literature for their description; chapter 1 page 23 does a good job of this however the summaries and other chapters do not. Suggest that descriptions of the RCPs across the entire SOD be reviewed and compared to their description in the literature (e.g. from van Vuuren et al. 2011, Climatic Change 109:5-31) [HAROON KHESHGI, United States of America]	12.4.9
0-123	0	0				The IPCC WG1 SOD contains many instances of excellent assessments of uncertainty and their traceable accounts. However, I found instances where traceable accounts cannot be found, and where a uniform standard of assessment is not applied. Given the differing and sometimes complex arguments presented to justify a confidence and likelihood assessment in each instance, it is difficult yet still important to check across the entire draft to ensure that traceable accounts and uniform standards are applied. I expect that simple the statistics of CMIP5 models is not sufficient to justify and confidence/likelihood assessment, and there are many good examples where these statistics contribute to the assessed confidence/likelihood alongside other evidence and rationale. However, in some instances this is also not the case. Suggest that every designation of confidence/likelihood (particularly in the SPM) should be checked and traced. [HAROON KHESHGI, United States of America]	noted; this is important and was a major focus for the final draft.
0-124	0	0				When discussing accelerations over time (glaciers, sea level) there is lack of clarity about what is actually being used for the acceleration term. Various folk are estimating $a*t^2$ or $0.5*a*t^2$ (reproducing the acceleration in Velicogna, 2009 requires the latter) and reporting a or 2a in either case. There may be differences when discussing quadratic vs acceleration. In many cases the original paper does not discuss what is done (e.g., Church et al., Velicogna, 2009; Rignot et al., 2011). IPCC needs to be clear what it means when discussing acceleration. A realclimate blog post argues strongly for one position on what should be reported but I do not think it is necessarily uniform http://www.realclimate.org/index.php/archives/2012/11/dont-estimate-acceleration-by-fitting-a-quadratic/ . Regardless of convention, IPCC should be clear what it means by acceleration and quadratic and if there is a difference [Matt King, Australia]	acceleration is usually estimated by taking the difference of trends during two subsequent time intervals, divided by the distance of the time intervals.
0-125	0	0				This is a very interesting and useful scientific report on climate changes. I have no further comment on the second draft only some misprinted words as denoted. [Jiemjai Kreasuwun, Thailand]	noted
0-126	0	0				The Atlas and all other comparisons are using time slices including 20 years. For temperature this might be ok, for rainfall it is NOT. Please follow the guidelines given by the WMO (30years). Have a look at common good practice guide lines like the following: Kreienkamp et al. 2012: Good practice for the usage of climate model simulation results - A discussion Paper. Environmental Systems Research, 1:9 doi:10.1186/2193-2697-1-9 [Frank Kreienkamp, Germany]	noted; But it was decided to stick to 20-years are the WGI AR5 agreed averaging period.
0-127	0	0				Almost all the chapters have cited the reference of 'submitted' papers. The honorable authors need to check the validity of such papers in this report. [Umesh Kulshrestha, India]	noted; All such papers have been carefully checked to ensure they met the accepted publication deadline of March 15, 2013.
0-128	0	0				It is very good that FAQs are incorporated in each chapter of the draft report. But in my opinion, it would be more appropriate if FAQs are given together as an Annexure for the benefit of reader community. [Umesh Kulshrestha, India]	thanks; FAQs are published in the Chapters where they reside. Buut in addition, please note that the final FAQ's will be extracted and published in a separate brochure to the WGI AR5.
0-129	0	0				The draft contains some problematic use of words like "most likely" "prediction" "accurate estimates" and "central estimates" which will only serve to further confuse the decision-makers who invariably cease upon certainty statements for there planning purposes. This behavioural characteristic of decision-makers means that the significance of a full range of plausible futures gets lost in translation and the potential impacts on decisions that have long timeframes like stormwater systems, flood levees, urban settlements get locked into average and best estimate assumptions about the future rather than the range informing more adaptive and possibly transformational changes that could reduce impacts on future populations and the systems that enable them to function. I have commented where these words appear but a complete search through the	noted; specific instances will be carefully checked, see corresponding responses.

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						WG1 report should be made to avoid this behavioural effect of teh information in the report. [Judy Lawrence, New Zealand]	
0-130	0	0				I have no further comments to make based on the fact that my previous comments (on the first draft) were handled adequately. [Seoung Soo Lee, United States of America]	noted
0-131	0	0				It would have been better to include the CO2-, CH4- and N2O-relevant sections from Chapter 2 (i.e.2.2.) into Chapter 6 to avoid duplication of Figures and text. [Ingeborg Levin, Germany]	noted; Panel approved outline had coverage of changes in atmospheric composition to be in Chapter 2 "Observations: Atmosphere and Surface"
0-132	0	0				Throughout the AR5 (e.g., Chapter SPM, pg. 12, line 50; Chapter TS, pg. 4, line 24; Chapter 1, pg. 2, ln 45), the authors place a hyphen between an adverb and adjective (e.g., "globally-averaged," "independently-derived," "Globally-averaged," respectively). Such a hyphen is not normally required. [Julian Levy, U.S.A.]	noted; editorial.
0-133	0	0				Treatment of uncertainty in the second order draft is not consistent with the instructions to authors given in the 2010 "Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties". In particular, the statement that "Author teams are therefore encouraged to provide information on the tails of distributions of key variables, reporting quantitative estimates when possible and supplying qualitative assessments and evaluations when appropriate." has not been covered for projections of sea level rise. More focus needs to be placed on the evidence that is able to provide some limits to the ranges for key aspects of climate change such as equilibrium climate sensitivity and sea level rise over the next 100 years. [Martin Manning, New Zealand]	reject; treatment of uncertainty in WGI AR5 is consistent with the 2010 Guidance Note. More focus has been placed on the tails of the distribution in the Final Draft. Likelihoods of tails are given where an assessment is possible (e.g. for ECS), otherwise it is explicitly stated that such an assessment cannot be made (e.g. upper limit of SLR).
0-134	0	0				The "thermodynamic" precipitation change ("wet get wetter; dry get drier") is mentioned several times in the report. I recommend caution in presenting this appealingly simple narrative. There are caveats that are appropriately made in Chapter 7 (offsetting effects from weakening tropical circulations; less relevant over land regions; affected by circulation shifts). The "dry get drier" is most relevant over subtropical oceans, which is a sparsely populated region. For more populous arid land regions, this argument cannot be straightforwardly applied (precipitation cannot exceed evaporation because soil moisture is limited). When considering the zonal component of precipitation changes in low latitudes, the consensus projection is for the dry regions (e.g., equatorial east Pacific) to have more of an increase in precipitation to the wet regions (e.g., maritime continent; see Fig. 12.10 and the analysis of Seager et al. 2010); this is attributable to weakening Walker circulations. "Dry get drier" is a simple message to communicate, but cannot generally be applied in isolation (e.g., one gets the wrong impression for how the east-west component of the tropical Pacific precipitation will change). Focusing too narrowly on the thermodynamic precipitation argument may affect public perceptions of what should be expected under climate change, as well as those scientists working in related areas (climate impacts, past climates, etc). [Timothy Merlis, United States of America]	noted; see assessment in Chapters 7, 11 and 12. For example, Chapter 12, Section 12.4.5.2 "Patterns of Projected Average Precipitation Changes" for example explicitly states that "Over continents, this simple wet-get-wetter and dry-get-drier type of response fails for some important regions such as the Amazon"
0-135	0	0				No further comments this draft. [Benjamin R Miller, United States of America]	noted
0-136	0	0				Some check needs to be made that the words used to denote significance and confidence levels are used consistently for those purposes , and not in there every day use, especially in the summaries. For the key results, the logic leading to the significance level should be given (eg significance of statistical tests, allowance for modelling, statictical and observational uncertainty etc.), and the arguments leading to the confidence levels assigned [John Mitchell, United Kingdom]	noted; authors are required to follow the IPCC uncertainty guidance document when characterising uncertainty in an assessment finding in terms of confidence or likelihood.
0-137	0	0				I would like in the final report to avoid to the maximum the use of acronyms to improve readability. Despite familiarity with the subject, it took me some time to read the report fluently (example: use black carbon instead of BC , global warming potential instead of GWP, etc..). I would appreciate also that each time scenarios are considered, an easy way should be found to have them on hand (why not an hyperlink). [Christian Muller, Belgium]	noted; list of acronyms will be provided in the Final Report.
0-138	0	0				I did not find a place in the report to make recommendation for future research, in my specialty, which is natural forcings, I would see two points: 1: the trigger effect of the variations of solar UV. It is an important drive on the upper atmosphere energetics through the chemistry. The space measurement series does not yet cover a full solar cycle and there is disagreement on the effects of solar variations UV and extreme UV variations on climate and weather. A second point which I would like to point is the lack of study of non linear	reject; recommendations for future research are not within the mandate of the IPCC.

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						effects which could lead to extreme events with energetically weak triggers. [Christian Muller, Belgium]	
0-139	0	0				Concerning FAQs: It would be very helpful, if in all FAQs the same period for describing 'the last decades' (or in fact the period with predominantly anthropogenic forcing) is used, if possible. Since in earlier reports, the main message (predominant anthropogenic influence on temperature change) always referred to 1950, I would propose to use this reference if ever possible. [Urs Neu, Switzerland]	noted; consistency has been achieved to the extent possible in the Final Draft.
0-140	0	0				In many chapters, after dates (years) sometimes the expressen 'CE' is added. I haven't ever heard of that and this is probably true for many readers. 'AD' instead might be more known, but in general the use of 'CE' is very inconsistent over and within chapters (some use it, some do not; the 'threshold' when to use it is different: it is e.g. never used for 1979 but sometimes for 1930, sometimes only before 1900 or 1800). I propose to skip this labeling, since there are almost always specific years like 1436 or 1865, and nobody will think of something else than of 'AD'. 'CE' does not help but only puts up questions. [Urs Neu, Switzerland]	CE (common era) is used in Chapter 5. The abbreviation will be added to the acronym list for the final draft.
0-141	0	0				Comment 1: Observed acceleration in the rate of climate change should be summarized in the highlighted-SPM statements . [Thomas Dunning Newbury, United States of America]	reject; the SPM does show a complete time series of surface warming from 1850 to present in Figure SPM.1 (and changes in other climate system quantities for shorter periods in Figure SPM.2). Specific SPM statements related to temperature consider the warming over the past 30 years within a long term perspective, and also address the observed change in surface temperature over the past 15 years.
0-142	0	0				The first highlighted statement in the Summary for Policymakers (SPM) is about increased confidence in the primary conclusion of the previous assessment—the Fourth Assessment Report (AR4) from 2007. The AR4 concluded that “warming of the climate system is unequivocal” but attributed only “medium confidence” to a supporting detail. Now, five years later, the AR5 describes increased confidence in unequivocal climate change because of new observations, longer data sets, and more paleoclimatic information. The AR5 Working Group I report (WGI) contains extensive evidence that the rate of climate change is accelerating, but the SPM contains no summary statements about the evidence. The discrepancy might be objectionable to policymakers especially from low-lying and island nations. [Thomas Dunning Newbury, United States of America]	noted; the SPM is based on the comprehensive underlying chapter assessment, and thus, statements referring to 'accelerating' climate change would need to be supported by the chapter assessment. Such a statement is given in the SPM regarding an accelerated rate of global mean sea level during the last two centuries, based on the evidence assessed in chapters 3, 5, and 13.
0-143	0	0				The Summary for Policymakers illustrates acceleration of several climate-change anomalies is Figure SPM.1, including the Stratospheric Temperature Anomaly, the Northern Hemisphere Snow Cover Extent Anomaly, the Land Surface Air Temperature Anomaly, and Ocean Heat Content Anomaly (0-77m). Another SPM figure illustrates accelerating changes in the observed summer extent of Arctic sea ice (1950-2010, black/grey curves in a white-padded panel of Figure SPM.4). [Thomas Dunning Newbury, United States of America]	noted; the intention of this comment is not clear -- no action proposed. Note that detecting an 'acceleration' in any trend requires a comprehensive statistical analyses, and cannot be determined from the simple visual interpretation of a time series figure as given in Figures SPM.1 and SPM.2. See chapters 2, 3, and 4 for the underlying assessment of the quantities plotted in these SPM figures.
0-144	0	0				The Technical Summary describes other accelerating trends. For example, “(o)bservations indicate that the Greenland Ice Sheet is very likely to be experiencing a net loss of mass due to increased surface melting and run off, and increased ice outflow”; and the “. . . rate of loss is likely to have increased over the last two decades” (lines 23 to 25 on page TS-12). [Thomas Dunning Newbury, United States of America]	noted; the intention of this comment is not clear -- no action proposed.
0-145	0	0				A second example is that “(t)he Antarctic Ice Sheet is also likely to be in a state of net mass loss and its contribution to sea level is also likely to be increasing through time”; and “(a)cceleration in ice outflow has been observed since the 1990s, especially in the Amundsen Sea sector of West Antarctica” (lines 35 to 37 on page TS-12). [Thomas Dunning Newbury, United States of America]	noted; the intention of this comment is not clear -- no action proposed.
0-146	0	0				Chapter 4 on cryosphere observations reports that “(t)here is robust evidence in high agreement that the Greenland Ice Sheet has lost mass since the early 1990s, and that the rate of loss has increased (page 4-4, lines 17-18). Furthermore, record melt of the Arctic summer sea-ice cover was observed during summer 2012. The record melt is not reported in the IPCC WGI SOD, but it was reported by other UN organizations	noted; the record low in summer minimum ice extent is discussed and assessed in the Final draft of Chapter 4 (section 4.2.2.1)

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						(UNESCO Media Services on 24.08.2012) and, before the WGI 15 March 2013 cut-off for accepted papers, will result probably in numerous technical publications such as the following one: Showstack, R. 2012. Arctic sea ice minimum extent. Eos Trans. AGU, 93(40), 388, doi: 10.1029/2012EO400005. [Thomas Dunning Newbury, United States of America]	
0-147	0	0				Also, the cryosphere chapter could provide cross references to the WGII descriptions of natural climate-change effects that are accelerating. An example is in the WGII chapter on Polar Regions (section 28.2.2.1 Arctic). The section describes satellite-derived estimates of summer, open-water, primary production from 1989 to 2009 over the whole Arctic Ocean (Arrigo, K.R., and G.L. van Dijken. 2011. Secular trends in Arctic Ocean net primary production. Journal of Geophysical Research 116. C)9011. doi:10.1029/2011JC007151). Annual changes in the primary production (their Figure 13C) illustrate acceleration that is similar to the illustrations of Arctic summer sea-ice extent in the WGI chapter on cryosphere observations. [Thomas Dunning Newbury, United States of America]	reject; the reviewer gives no clear reasoning as to why this emphasis on 'accelerating' climate change is required and why cross-reference would be warranted between the working group reports.
0-148	0	0				The absence of AR5 cross references and a summary statement about acceleration might create problems also for the next assessment report (AR6) in another seven years. Acceleration of climate change is likely to be more obvious by 2020, and the reaction of policymakers might be "why such a surprise" and "why such a sudden change in evidence?" If the AR5 includes a summary statement that acceleration of climate change is likely, any AR6 conclusion about high confidence in acceleration might be more acceptable. [Thomas Dunning Newbury, United States of America]	reject; the WGI AR5 is, and can only be, based on the current scientific basis as assessed in the underlying chapters.
0-149	0	0				The following two sentences are a possible highlighted-summary statement for the SPM: Acceleration in the rate of climate change is shown by several global indicators, including the Stratospheric Temperature Anomaly, the Northern Hemisphere Snow Cover Extent Anomaly, the Land Surface Air Temperature Anomaly, the Ocean Heat Content Anomaly (0-77m), and the extent of Arctic summer sea ice. However, the level of confidence in these indicators varies widely. (SPM.1, SPM.4) [Thomas Dunning Newbury, United States of America]	reject; the SPM is based on the comprehensive underlying chapter assessment, and thus, statements referring to 'accelerating' climate change would need to be supported by the chapter assessment.
0-150	0	0				Another alternative would be an addition to the third highlighted statement about extreme events (lines 46-48, page SPM-3). The two concepts (an extreme event, and an accelerating trend) are similar. The reference in the third highlighted statement to "many" extreme events might be an extreme statement in itself because the SPM describes an event as "extreme" only once: ". . . extreme sea levels have increased since 1970, and this is mainly caused by rising mean sea level" (lines 51-52, SPM-5). So, the following row contains changes could be made to the third highlighted statement (the proposed additions are in CAPITAL LETTERS, and the proposed deletions are in brackets): [Thomas Dunning Newbury, United States of America]	reject; the SPM is based on the comprehensive underlying chapter assessment, and thus, statements referring to 'accelerating' climate change would need to be supported by the chapter assessment.
0-151	0	0				"Changes in many extreme weather and climate events have been observed. ACCELERATION IN THE RATE OF CLIMATE CHANGE IS SHOWN BY SEVERAL GLOBAL INDICATORS, INCLUDING THE STRATOSPHERIC TEMPERATURE ANOMALY, THE NORTHEN HEMISPHERE SNOW COVER EXTENT ANOMALY, THE LAND SURFACE AIR TEMPERATURE ANOMALY, THE OCEAN HEAT CONTENT ANOMALY (0-700M), AND THE EXTENT OF ARCTIC SUMMER SEA ICE. (but) HOWEVER, the level of confidence in these changes varies widely depending on type of extreme and regions considered (Overall the most robust global changes are seen in measures of temperature {FAQ 2.2, 2.6} (see ALSO Table SPM.1, FIGURES SPM.1 AND SPM.4). End of comment 1 [Thomas Dunning Newbury, United States of America]	reject; the SPM is based on the comprehensive underlying chapter assessment, and thus, statements referring to 'accelerating' climate change would need to be supported by the chapter assessment.
0-152	0	0				Comment 2: Please ask an author to review the figure legends about Arctic summer sea-ice extent. [Thomas Dunning Newbury, United States of America]	noted; comment does not point to a specific problem in the legend.
0-153	0	0				Estimates of Arctic sea-ice extent are graphed in several WGI figures. Some of the graphs are for summer sea-ice extent, and some are for annual average sea-ice extent. The summer minimum during 2007 was distinctly low but the annual average was not, so the graphed values for 2007 help with distinction of graphs	noted; intention of comment not clear.

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						for summer ice extent and annual average ice extent. I think that all of the graphs of Arctic ice extent should be reviewed by an author like Dr. Josefino Comiso, Coordinating Lead Author for Chapter 4, Cryosphere Observations. The following row has references to five graphs with distinctly low values for 2007, but with different labels: [Thomas Dunning Newbury, United States of America]	
0-154	0	0				SPM-1, labeled “Arctic Summer (JAS) Sea-Ice Extent” SPM-4, labeled “Arctic Sea Ice” 4.3 (a) labeled “Annual Average” and (b) labeled “Seasonal ice extent in the Arctic for the periods JAS and OND” 4.7 (a) labeled “Ice Extent, anomaly, Arctic ice extent and concentration from satellite passive microwave observations” FAQ 4.2, Figure 1, labeled “Extent (10 to the sixth/km squared)” End of comment 2 [Thomas Dunning Newbury, United States of America]	noted; labels are corrected in final draft.
0-155	0	0				Comment 3: Please ask an editor to remove two unnecessary phrases from several chapters, and to review the spelling of two specific place names. [Thomas Dunning Newbury, United States of America]	noted, all chapters are subject to copy-editing.
0-156	0	0				SPECIFIC PLACE NAMES The specific place names are the Antarctic Ice Sheet and the Greenland Ice Sheet; they are not always spelled with capitals. The following are examples, including some in the important SPM, of the place names with capitals: Antarctic Ice Sheet SPM-5, line 17; TS-7, line 56; and 4-4, line 25 Greenland Ice Sheet SPM-5, line 13; 1-13, line 35; and 4-4, line 17 The following are examples, including some in the SPM, of the place names without capitals: Antarctic ice sheet SPM-11, line 11; TS-8, line 33; and 4-6, line 9 Greenland ice sheet SPM-11, line 23; TS-10, line 33; and 4-4, line 49 [Thomas Dunning Newbury, United States of America]	noted, all chapters are subject to copy-editing.
0-157	0	0				UNNECESSARY PHRASES Many summary sentences start with the unnecessary phrases “it is” or “there is.” The phrases introduce substantial information in the Chapter 3 Executive Summary, the Technical Summary, and especially the SPM. However, the words “it” and “there” and “is” contribute no information about climate change. An example of their unnecessary use occurs on page SPM-4, lines 52 to 55: It is very likely that the subtropical gyres in the North Pacific and South Pacific have expanded and strengthened since 1993. There is no evidence for decadal trends in the transports of the Atlantic Meridional Overturning Circulation (AMOC), and the Antarctic Circumpolar Current. {3.6, Figure 3.10, Figure 3.11} The important climate-change information would be clearer without the “it is” and “there is” phrases in the	noted, all chapters are subject to copy-editing.

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						sentences, as follows: Subtropical gyres in the North Pacific and South Pacific have very likely expanded and strengthened since 1993. However, decadal trends are not evident in the transports of the Atlantic Meridional Overturning Circulation (AMOC), and the Antarctic Circumpolar Current. {3.6, Figure 3.10, Figure 3.11} End of comment 3 [Thomas Dunning Newbury, United States of America]	
0-158	0	0				I consider that this SOD of the IPCC AR5 (WGI) has been improved significantly with respect to the FOD. However it is very difficult to detect were the corrections I made were included, since the new texts, figures and tables introduce a systematic change in the number of lines and even of pages. In what follows, I make suggestions about the possible improvement of the SOD. I included in color an in black, several suggestions I made in the FOD (color text), but were not considered and some comments. The normal text corresponds to the new comments and suggestions. [Rubén D Piacentini, Argentina]	noted
0-159	0	0				A general indication must be given if the uncertainty (ϵ) associated with a measured value (X), in the form $X \pm \epsilon$, is considered within one or two standard deviations (or in another form). For example: "The uncertainty of a quantity is given with two standard deviations, except were otherwise stated". Also, when an Anomaly is presented, like in air temperature, the reference quantity must be indicated in the figure (right vertical axis, for example) or in the figure captions. Care must be taken if this value was considered in the calculation of the anomaly with more significant numbers than the value to be subtracted, in order to avoid the appearance of a new source of error (uncertainty). [Rubén D Piacentini, Argentina]	noted; a consistent approach has been implemented for the Final Draft to the extent possible, and a footnote is provided in the executive summary of all Chapters regarding the uncertainty interval used. All figures using anomalies should report the reference period in the figure caption.
0-160	0	0				Uncertainties need to be detailed in all figures with data (as error bars) or at least must be indicated in the corresponding figure caption. For example, in Figure TS.12 "Observations", in TFE.9 Figure 1, etc. [Rubén D Piacentini, Argentina]	noted; where uncertainty values are available and assessed in the underlying chapter, an indication of uncertainty has been included in the relevant figures as a shaded band, error bars etc.
0-161	0	0				NOTE: I would like to point out that I will introduce comments on Annex II in what follows and as part of the General comments (0), since there is no possibility to include the corresponding symbol (All) in the first column. This column is only devoted to General (0), Chapter numbers (1, 2, ..., 14) or Atlas. [Rubén D Piacentini, Argentina]	noted
0-162	0	0				Annex II, page 9, line 11: About "Table All.2.7: Anthropogenic C6F14 emissions (Gg yr ⁻¹)". The numbers for each decade in the different columns, corresponding to RCP2.6, 4.5, 6.0 and 8.5, are the same (except only one, the value 0.429 at RCP6.0 in 2010 in place of 0.430 in the rest of the line). Is this correct (if this is so, please explain with a Note at the end of the table) or is it a typographical error? [Rubén D Piacentini, Argentina]	This is correct as published by the RCP authors. It does look odd, however, and since the extra decimal space is not really needed, we have rounded to two decimal places, eliminating this apparent discrepancy.
0-163	0	0				Annex II, page 15, line 7: About "Table All.3.2: [PLACEHOLDER FOR SECOND ORDER DRAFT] Natural CH4 emissions (Tg yr ⁻¹)". Do not forget to drop this figure, if no AR5 budget estimates for changing natural emissions. [Rubén D Piacentini, Argentina]	noted
0-164	0	0				Annex II, page 17, line 11: About "Table All.4.7: C6F14 abundance (ppt)". The same comment as for All-9, line 11. The numbers for each decade in the different columns, corresponding to RCP2.6, 4.5, 6.0 and 8.5, are the same. Is this correct (if this is so, please explain with a Note at the end of the table) or is it a typographical error? [Rubén D Piacentini, Argentina]	This is correct to the accuracy shown here. The small differences in emissions do not show up in abundance..
0-165	0	0				It is very much appreciated to compare findings with those from AR4 and to explain any differences, e.g. with respect to assigned uncertainty. [Klaus Radunsky, Austria]	noted
0-166	0	0				It is noted that information in executive summaries is sometimes incomplete because it lacks the time horizon and/or the emission scenarios for which the statement has been made. [Klaus Radunsky, Austria]	noted; These details are indeed crucial and will be included for the final draft to the extent possible.
0-167	0	0				It should be considered to summarize findings also in executive summaries by tables, especially if the executive summary is very systematic and comprehensive. The text could be shortened with a focus on the most relevant issues. [Klaus Radunsky, Austria]	noted; Effort has been made for the final draft to ensure all chapters use a common format for their executive summaries, with an emphasis on elevating

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							the most relevant statements from the underlying chapter assessment.
0-168	0	0				For the time it is unclear which scenarios are highlighted in the executive summaries. It would be helpful if the same scenarios are used in all three reports of the working groups as well as in the Synthesis report later on. In this context scientists are reminded that scenarios that are consistent with a 1.5 and 2 degrees increase in global temperature and BAU would be very policy relevant. [Klaus Radunsky, Austria]	noted; To the extent possible, the executive summaries include results based on a full range of the RCP scenarios, and where statements are scenario specific, these details are spelled out.
0-169	0	0				<p>FROM COMMENT ON FOD; still applicable</p> <p>I commend the authors on a fine survey of present understanding of the climate system, of refinements in that understanding. However what comes through in the document, and I find myself doing the same thing in my comments below, is a focus on details instead of the big picture. The big picture is that the increase in CO2 and other long lived GHGs has resulted in a perturbation of the radiation balance, about 3 W m-2, that is substantial in the context of the GH effect 150 W m-2, the difference between radiation emitted at the Sfc, about 390 W m-2, and that exiting the TOA, 240 W m-2. Because of the persistence of these GHGs in the atmosphere Earth is committed to a forcing of this magnitude for circa 1 century. Absent major changes in future emissions the planet is committed to doubling and redoubling of equivalent CO2, with forcing going up to roughly 7 W m-2. This would have major effect on climate. Here are the reasons why. Here are some of the expected consequences in physical climate. These could be expected to have these sorts of consequences on ecosystems, agriculture, human society. It is this sort of big picture that I find missing in the document.</p> <p>This report will be even bigger and thicker than the previous one. It is as if there is some sense on the part of the writers that the more that is included, the better to make the case to the world (and perhaps to the skeptics, legitimate ones and deniers) that the climate system is well understood in all its minutiae, in support of actions that might be taken to mitigate. My own feeling in this respect is that that approach will not work. Better to present big picture items and their uncertainties, and the consequences of those uncertainties. Less is more in making the case for present understanding. Here is the range of expected temperature response to this or that course of action. Here is the justification for that. I expect that this could be done at about one third the length of the present draft. And the case would be stronger. Perhaps relegate much of what is in the present report to Appendices.</p> <p>I am marking this comment as pertinent to all chapters and would hope at minimum that this comment is conveyed to all chapter lead authors. However even better would be that the approach and objectives of the report as a whole be re-examined in a meeting of the report lead authors and chapter lead authors to at least discuss this matter and decide on a uniform goal. [Stephen E Schwartz, United States of America]</p>	noted; some chapters have significantly reduced in length for the final draft, and there has been further use of supplementary information and appendices.
0-170	0	0				Throughout: Graphs of quantities against latitude should be plotted against sin(lat) (area weighted) so as not to give distorted impression. [Stephen E Schwartz, United States of America]	reject; latitude is more accessible a quantity than sin(lat).
0-171	0	0				Anomaly is important for looking at trends. But absolute values are important as well, for many purposes. I would recommend where possible, in addition to stating the time period that served as the basis of a reported anomaly time series, also to give the value of the climatological mean that was subtracted so that it is possible to reconstruct absolute values from anomaly time series. [Stephen E Schwartz, United States of America]	noted; details on the base period used for the calculation of anomalies are reported in all relevant figure captions.
0-172	0	0				Throughout the report I would suggest for all line graphs, bar graphs, and the like provide data in supplemental files, thereby permitting reader/user to replot the data, make calculations on the data, calculate trends over sub periods, etc. [Stephen E Schwartz, United States of America]	noted; Chapters have been encouraged to make use of supplementary material to provide background information on the figures. This is particularly important for the figures elevated to the high level summary documents. The provision of underlying data is at the discretion of authors, and their institutions and might depend on further rules defined by the owners of the data.
0-173	0	0				Throughout, pay attention to use of first person plural. Suggest be limited to the meaning "we, the authors of the chapter/report" and not "we, the scientific community" or "we, the general public" or the like. I cite some	noted; personal pronouns should generally be avoided.

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						<p>examples from Chapter 1 that illustrate the shifting meaning associated with first person plural and the associated ambiguity. Not exhaustive.</p> <p>Page 1-2, line 10: "our planet"; maybe not ambiguous.</p> <p>Page 1-2, line 25: "improve our knowledge"; certainly not the same people as the implied reference of "our" in "our planet". Maybe better simply "improve knowledge".</p> <p>Page 1-5, line 6 "Here we describe briefly some of the key concepts affecting the Earth's climate" Appropriate.</p> <p>Page 1-7, line 19: "Even in the absence of external forcing we observe periodic and chaotic variation" Ambiguous who is doing the observing; certainly a shift of reference of the pronoun; it is not the chapter authors who are doing the observing. Better passive: "Even in the absence of external forcing periodic and chaotic variation is observed" or "is exhibited" which is even better if it is a geophysical phenomenon that is independent of the observer.</p> <p>Page 1-8, line 12: "Numerical models include what we know about the laws of physics and chemistry"; again ambiguous; who knows this; chapter authors? the modelers? the science community?; simply "Numerical models include what is known about the laws of physics and chemistry"</p> <p>Page 1-14, line 50: "To maintain a degree of terminological clarity we distinguish between". Appropriate. We the chapter authors.</p> <p>Page 1-23, line 33: "There is still considerable uncertainty around emissions, because we cannot accurately predict future social choices" Who is doing the predicting? Not the chapter authors; not the modelers; society at large? Simply" because future social choices cannot be accurately predicted."</p> <p>Page1-24, line 11: "Climate science has made many important advances since the last assessment report, due to improvements in measurements and data analysis in the cryosphere, atmosphere, land and ocean systems. We also have better understanding and modelling of clouds, sea ice, aerosols, small-scale ocean mixing, the carbon cycle and other processes. We can now better constrain projections, thanks to refinements in the way models are evaluated based on observations." Who has the better understanding? I think it is the climate scientists, not the chapter authors. Who is doing the constraining? Is it modelers?</p> <p>Page 1-24, line 41: "When we project climate, we need plausible estimates of human emissions of greenhouse gases and aerosol precursors"; again it is not the chapter authors who are doing the projecting; it is modeling groups somewhere or perhaps a different working group altogether. Simply "For climate projections plausible estimates are required."</p> <p>Page 1-24 line 14: "how far into the future we are looking". Who is doing the looking?</p> <p>I rest my case. [Stephen E Schwartz, United States of America]</p>	
0-174	0	0				<p>"Medium confidence". This phrase is at best awkward, although perhaps it is too late in the day to change it. Confidence is not like a shirt size or done-ness of a steak. Suggest better "Moderate confidence," throughout. [Stephen E Schwartz, United States of America]</p>	noted; this terminology is based on the IPCC AR5 uncertainty guidance note (Mastandrea et al., 2010).
0-175	0	0				<p>Throughout the report I would recommend care be taken to distinguish temperature, temperature change, temperature anomaly. It is easy to slip into jargon, but these are different quantities.</p> <p>Example, Chapter 10, page 10-9, Line 43: Coloured dots in panel (a) in Box 10.1, Figure 1 show observed annual global mean temperatures from 1861–2010.</p> <p>Caption states: Observed global annual mean temperatures relative to 1880– 1920; it is clear from the numbers that these are temperature changes, as implied by "relative"; but certainly not "temperatures."</p>	noted; accuracy and specification of details has been a focus of the revisions for the Final Draft.

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						<p>Text at line 54 states that in panel b "temperatures are plotted" and caption states likewise, but axis label says "observed warming" and it is not clear relative to what, but again from the magnitudes these are temperature differences of some sort.</p> <p>Caption Figure 9.8: "Observed and simulated annual mean global average anomaly time series of surface air temperature". The quantity plotted is anomaly.</p> <p>I rest my case. [Stephen E Schwartz, United States of America]</p>	
0-176	0	0				<p>What is the accuracy in forcing that is sufficient? The Cloud and Aerosol chapter (7) states: (page 7-3, line 47)</p> <p>"It remains unclear what level of sophistication in global aerosol and climate models is required to estimate aerosol-radiation and aerosol-cloud interactions to a sufficient accuracy."</p> <p>But it is not stated there, nor so far as I can tell, anywhere in the Report what accuracy is sufficient, and for what purpose: Representing forcing in 20th century runs? Representing forcing in future climate runs. Determining the climate sensitivity empirically? Inferring the committed warming by LLGHGs? Advising policymakers how much ghg forcing is allowable for a given increase in global mean surface temperature?</p> <p>The concept of sufficient accuracy is terribly important and needs to be addressed and highlighted. Perhaps a table of sufficient accuracy for various purposes, and a discussion of the implications of not knowing this forcing to a given accuracy.</p> <p>I made an initial attempt to discuss some of these issues previously:</p> <p>Uncertainty requirements in radiative forcing of climate change. Schwartz S. E., J. Air Waste Management Assoc. 54, 1351-1359 (2004). [Stephen E Schwartz, United States of America]</p>	<p>Noted; The statement in the Executive Summary of Chapter 7 has been revised in order to avoid referring to an "accuracy" level.</p> <p>We agree with the reviewer that this is an important and interesting concept. However, what is considered sufficient would ultimately not only depend on the quantity of interest, on the questions asked, but also on further considerations that go beyond the physical science basis of climate change, the topic of this WGI assessment. This is thus not synthesized in a table in the WGI AR5 as suggested by the reviewer.</p>
0-177	0	0				<p>I read through chapters 4,9,10 with sections relevant to Arctic climate. I think some better coordination between them might be useful (see notes below) [Axel Schweiger, United States of America]</p>	<p>noted; no specific action suggested.</p>
0-178	0	0				<p>In general, I am missing particularly in Chapters 4, 9, or 11, a discussion about the influence of Arctic sea ice loss on the occurrence or changes in extreme temperatures. In the recent literature you find quite a few papers that discuss this issue, for instance: (1) Petoukhov and Semenov 2010, A link between reduced Barents-Kara sea ice and cold winter extremes over northern continents, JGR 115, D21111. (2) Outten and Essau 2011, A link between Arctic sea ice and recent cooling trends over Eurasia, Climatic Change Letter, doi:10.1007/s10584-011-0334-z. (3) Francis et al. 2012, Evidence linking Arctic amplification to extreme weather in mid-latitudes, GRL 39, L06801. (4) Yang and Christensen 2012, Arctic sea ice reduction and European cold winters in CMIP5 climate change experiments, GRL 39, L20707. These references could be also considered in the discussion of polar amplification in chapter 12 (12.4.3.1 page 31). [Jana Sillmann, Canada]</p>	<p>Taken into account. The issue is dealt with in Ch14, Section 14.8.6.</p>
0-179	0	0				<p>It is very pleasing to see the statement on page 3-30, lines 10-12, that the current observing system is capable of resolving the long-term rate of sea-level rise, assuming continued measurements. Likewise the excellent concluding paragraph to Chapter 3 on page 3-42, lines 39-47. This raises a general question regarding other chapters that deal with observations. In a number of instances it is stated that the observational record is not adequate for assessing this or that. This is of course the primary thing that has to be stated in an IPCC assessment. But the general reader, and people with responsibilities for observing systems in particular, may wish or need to know whether current observing systems are adequate in the sense that continuation of the current observing systems would enable key assessments to be made in the future when a sufficiently long record has been amassed, or whether the current observing system, if continued over time, would remain fundamentally inadequate for assessing trends. It is perhaps asking too much at this stage for this to be done comprehensively, but perhaps authors could be asked to keep this point in mind when revising their text in the</p>	<p>noted; We recognize the usefulness of what the reviewer is suggesting, however, what may be considered 'adequate' now, may prove to be 'inadequate' as our scientific understanding of a given climate component change and measurement uncertainty etc evolve. In addition, it is not within the mandate of the IPCC to evaluate or endorse specific measurement programs, or future research needs.</p>

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						light of the detailed comments they receive as a result of this round of the review process. [Adrian Simmons, United Kingdom]	
0-180	0	0				"Model error" is sometimes used to refer to the difference between model values and observations (as in traditional RMS error) and sometimes to refer to structural model error (due to shortcomings in the mathematical structure of the model, as discussed in LA Smith, (2002) What Might We Learn from Climate Forecasts? Proc. National Acad. Sci. USA 4 (99): 2487-2492, and the related discussion of this topic in the AR4). These two types of error should be kept distinct throughout the report. Where is the discussion of structural model error and its likely impacts on all the modelling results presented? Is it possible to more clearly distinguish hard scientific results independent of model fidelity (while supported by today's models) from interpretation of model output? [Leonard Smith, United Kingdom]	Taken into account. Such a conceptual discussion was included in Ch09 SOD, but had to be omitted for space reasons. Though not at this fundamental abstract level, Ch09 does contain an extensive discussion about where models are based on equations we know and where not (e.g., Box 9.1).
0-181	0	0				Consistency of language. There are a number of words which are not in the glossary and/or whose meaning (or meanings) should be clarified. "Robust", "coherent", "short-lived", "external forcing" even "trend" (the current definition, read broadly, applies to almost every meteorological observation), and "trend uncertainties" (a term difficult to reconcile with the current glossary definition). Similarly it is repeatedly unclear what is intended by "improved" or "improved simulation" (as in chapter 9), quantitative agreement within the observational uncertainty? closer to quantitative agreement? qualitative agreement due to more realistic simulation of phenomena? reduction of some drastically unrealistic aspect of the model? While this is sometimes clear to some scientists by the context, it is not clear to all scientists the wider target audience: clarity on what exactly is intended by "improved" in each usage would be of value. [Leonard Smith, United Kingdom]	noted; accuracy, specification of details, and consistent use of terminology has been a focus of the revisions for the Final Draft.
0-182	0	0				There is a tendency throughout the report to note "improvement" without stating whether or not the model simulation are realistic or merely improved yet unrealistic. Clear identification of the boundaries (in space, time) of realism for a given phenomena, and how this decreases with lead time of the projection would be of great value. Credibility is rarely criticized, even in Chapter 9. The word "realistic" where it is used, is unclear. Similarly phrases like "long-term" need to be quantified, at least in the glossary, and if so used consistently throughout the report. [Leonard Smith, United Kingdom]	noted; specification of details, consistent and non-diffuse use of terminology has been a focus of the revisions for the Final Draft. The word "realistic" is no longer used in the summary assessments of Ch09 ES.
0-183	0	0				Clear discussion, with examples, of impacts which are likely, very likely, or virtually certain to be misinformative when extracted from CMIP5 models would be of great value. This will be a function of lead-time, of course, and the variables of interest. The continuing very high confidence in realistic simulation on continental and larger scales should be accompanied, throughout the report, with as clear a demarcation as possible as to where the simulations (by variable) are likely (very likely or virtually certain) to break down. This will, of course, be a function of lead-time and relevant spatial scale. Arguably it will also be a function of model response (greater under conditions far from those observed, as in high concentrations of CO2 or high temperatures globally). The aim here is to clarify where model output is likely to mislead, as discussed by Smith, LA and Stern, N (2011) Uncertainty in science and its role in climate policy Phil. Trans. R. Soc. A (2011), 369, 1-24. and LS Smith (2012) Predictability and Insight, Int J Forecasting (in review). [Leonard Smith, United Kingdom]	Noted. Desirable as it may be, the reviewer's request cannot at present be fulfilled in this general way. However, Ch09 contains a (brief) summary assessment of the difficulty of translating skill in simulating past climate into confidence in projections. Note that model reliability is not a simple function of lead time; short-term predictions are harder because they must aim at also predicting climate variability with the correct phase, whereas long-term projections average over variability and are thus easier in principle.
0-184	0	0				My key comment: I think the issue of positive carbon cycle feedbacks from melting permafrost is a major issue of policy relevance, given its potential size and given developments in quantification since AR4. It is dealt with well in FAQ6.2, but then badly in the TS and SPM. Crucially, the TS (TS-21 lines 17-18) appears inconsistent in that it suggests lower confidence than is justified. The TS text should be adjusted to reflect FAQ6.2 which states "modelling studies and expert judgments indicate with medium agreement that a potential combined release of up to 200 PgC as carbon dioxide equivalent [or 100PgC as CO2, 5PgC as CH4] could occur until the year 2100." I then cannot find the provenance of the numbers cited in the Summary for Policymakers (SPM-17 line 17 - how does 33-400PgC relate to the numbers in FAQ6.2 or the rest of Ch6?). These numbers should be verified. [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	values have been checked, revised, and are now consistent throughout Chapters, TS and SPM.
0-185	0	0				Well done to all the authors and editorial teams! The AR5 will be a great achievement and a vital resource in helping the world respond to climate change. Your contributions are much appreciated. [Stephen Smith, United Kingdom of Great Britain & Northern Ireland]	Thank you.

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0-186	0	0				I'm afraid I left myself far too little time to study the draft in the detail I would have liked, but I hope you will find the few following general comments constructive and useful. I have only looked at the SPM, TS and chapters 2 (observations) and 10 (attribution). My comments mainly relate to the difficulty encountered in navigating throughout the report to find the origins of statements made, and the justification behind them. Had it been easier to do this, I feel sure I would have been able to make a more thorough examination, and a better contribution to this important exercise. [Stephen G G Smith, United Kingdom]	noted
0-187	0	0				With the evidence presented the way it is, I have not been able to drill down to the sources of the many "probability" statements ("likely", "virtually certain" etc). I feel sure a more effective system of referencing must be possible. [Stephen G G Smith, United Kingdom]	noted; ensuring clear traceability from the summary statements through to the underlying chapter assessment has been a key focus of the revisions for the final draft.
0-188	0	0				From my reading so far, there does not seem to have been any serious attention paid to terrestrial temperature influences other than those relating directly, or indirectly, to the radiation budget. [Stephen G G Smith, United Kingdom]	not correct. In chapters dealing with observations, model evaluation, D&A, and projections, natural variability, and modes, e.g. ENSO, PDO, etc are considered when assessing temperatures.
0-189	0	0				I would seriously question the appropriateness of any FAQ section in an objective technical submission. This smacks of distracting the reader into addressing "straw man" issues. I suspect the denier camp will jump all over this! [Stephen G G Smith, United Kingdom]	reject; FAQs complement the comprehensive technical chapter assessment - they do not attempt to replace or distract from the comprehensive technical chapter assessment.
0-190	0	0				In the "attribution" chapter, I expected to find a traceable (and easy to follow) sequence of logical reasoning to lead the reader from the "observations" to the conclusion that human emissions of CO2 were causing the observed effects. I could not find such a logic train. [Stephen G G Smith, United Kingdom]	Taken into account. The Chapter 10 Executive Summary has been completely revised with traceable paragraphs back to chapter subsections. The Executive Summary now clearly explains that the observed warming is inconsistent with internal variability alone and with the expected response to natural forcings, and can only explained by anthropogenic forcing. Note that attribution to CO2 in particular has not been attempted or achieved in the literature, but attribution to greenhouse gases collectively and to all anthropogenic forcings are discussed in detail in Chapter 10.
0-191	0	0				I think all chapters would benefit from a drastic reduction in "jargon" and esoteric language. In my capacity as a technical team leader, I have reviewed and approved/rejected a great many papers on numerous subjects. The best ones are those couched in language the average, English speaking public are able to follow, regardless of whether or not they are skilled in the relevant technical knowledge. People (even government representatives) are not generally impressed by "elitism". [Stephen G G Smith, United Kingdom]	noted; But the use of technical language is difficult to avoid when the priority of the authors is to complete an accurate and comprehensive assessment of the underlying science. It should not be seen as any sign of "Elitism". The inclusion of a Glossary, and a acronym list as part of the WGI AR5 should help guide the reader through such technical language.
0-192	0	0				In all previous reports the contributing authors to each chapter have had their nationality / country of residence identified. Is this decision to preclude this information deliberate? Are there potential ramifications for future engagement of certain individuals and / or countries in the process moving forwards if 'national' contributions are no longer recognized in this way? Perhaps it was a decision of IPCC member countries and I missed it. But if it wasn't then careful thought about potential ramifications for national government buy-in and author participation would seem warranted. [Peter Thorne, United States of America]	noted; these details for the Contributing Authors will be included in the Final Report.
0-193	0	0				Across the three chapters I reviewed I found many cases of chapter over-reach whereby issues that were under the direct purview of other chapters were being discussed. This feels both dangerous and unnecessary. Dangerous in that it will be worded differently even if the intended meaning is the same and language pedants will take aim at that. Worse, in some cases the actual text will almost certainly be divergent enough in its conclusions to cause substantive issues. Unnecessary in that we really should cover each issue once to the extent possible and doing so multiple times in multiple chapters simply leads to bloat. I would request CLAs to	noted; Reducing overlap and ensuring cross referencing between chapters and working groups has been a focus of the revisions for the final draft.

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						identify passages of text that are cross-talk with other chapters given their remit and at a minimum ensure consistency and preferably remove text from chapters where the discussion is less under their direct charge and 'donate' to the more appropriate chapter. [Peter Thorne, United States of America]	
0-194	0	0				I felt many of the FAQs to be unduly dense and technical in nature and uneven in tone still. [Peter Thorne, United States of America]	All FAQs have undergone further refinement and editing for the Final Draft.
0-195	0	0				Congratulations on this Second Order Draft with the FOD Technical Summary. The use of the Thematic Focus Elements gives a very good structure to the TS and makes it easy to search for some key elements. Also the boxes are in general clear and give a good overview. Some improvements: I believe that the uncertainty language is used inconsistently. This language is already difficult to understand and using it differently in different chapter makes it incomprehensible. For example, According to the Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties: Assign a likelihood for the event or outcomes, for which confidence should be "high" or "very high" (see Paragraphs 8-10). In this case, the level of confidence need not be explicitly stated. So When a likelihood statement of 'likely' or even higher, for example 'very likely', is given, it is not necessary to still give a confidence statement when this is high or very high confidence confidence. Also some of the figures / graphs are unclear because the explanation is insufficient but most of the time because there is too much information in one graph (or presented in a incomprehensible way). [Line van Kesteren, the Netherlands]	Thanks, noted. On the uncertainty comment: the AR5 guidance note clearly states that the characterization of uncertainty using calibrated uncertainty language should be done in a way that conveys the most information to the reader. The wording used in the Guidance Note regarding complementing likelihood statements with a confidence statement if confidence is high or very high is "need not be explicitly stated" and thus does not prohibit it's addition. On the "incomprehensible figures" comment: noted, in this Final Draft many figures have been substantially edited and are now hopefully more comprehensible.
0-196	0	0				There seem to be very little discussion of the drivers for climate change prior to the industrial era, such as a discussion on the roman climate optimum (warm temperatures possibly on par with current temperatures) the medieval warm period (temperatures on par or slightly lower than current) and the little ice age (temperatures much lower than current), all prior to the industrial era. It is likely that human influence was minimal during these climate extremes and a better understanding of the natural drivers that led to these extremes would help in determining the human effects on climate change. [Andrejs Vanags, United States of America]	reject; the pre-instrumental period is being covered comprehensively through the assessment provided in WGI AR5 Chapter 5: Paleoclimate Archives. See, e.g., Section 5.2 "Pre-Industrial Perspective on Radiative Forcing Factors".
0-197	0	0				This comment refers to multiple chapters, e.g. Ch 8, 9 and 10. The net RF has been increased substantially in comparison with AR4 (net anthropogenic forcing in AR5 2.4 W/m2 ; in AR4 1.6 W/m2). Only a small portion of this increase can be explained by an increase in CO2 concentration, the bulk of the difference is due to the aerosol forcing being estimated as less negative in AR5 of AR4. The climate sensitivity (ECS) in CMIP5 is similar in range to that in CMIP3. Ocean heat uptake is in the order 0.5 W/m2 (Chapter 3), i.e. not much different from the estimate in AR4. Thus I would expect the modeled temperature increase to be larger in CMIP5 than in CMIP3, but judging from figure 10.1, it is (very) slightly lower. This requires an explanation. Alternatively put, the observed warming is a function of climate sensitivity, net radiative (or adjusted) forcing and energy imbalance (as diagnosed by e.g. ocean heat uptake). It seems that the net forcing in AR5 is much higher than in AR4, without concomitant changes in (a combination of) the other parameters. I would expect this change in RF to be accompanied by a) more warming postdicted by the CMIP5 models b) a lower climate sensitivity in CMIP5 c) a stronger energy imbalance, i.e. stronger ocean heat uptake, d) a combination thereof or e) I'm missing something (e.g. I'm not sure if the larger forcing figure is the average net forcing of the CMIP5 ensemble?) - bit this clearly needs explaining to avoid people taking this apparent inconsistency and drawing their own favourite conclusions from it. [Bart Verheggen, Netherlands]	noted; the energy budget and its components has carefully been revisited for the Final Draft based on intensive cross-chapter discussions. This has resulted in a dedicated box 13.1 and TFE.4 in the TS.
0-198	0	0				To follow up on the previous comment, a simple energy balance calculation with the three factors mentioned in the above comments would be very helpful for non-specialized readers to gain an appreciation for how the climate system works and in doing so, would make climate modeling less of a black box to them. See for an example Ramanathan and Feng (Atmos Environ 2009, Air pollution, greenhouse gases and climate change: Global and regional perspectives, page 40): "Global average surface temperatures have increased by about 0.75 degrees Celsius since the beginning of the industrial revolution, of which ~0.6 °C is attributable to human activities. The total radiative forcing by greenhouse gases is around 3 W/m2, with which we have 'committed' the planet to warm up by 2.4 °C (1.6-3.6 °C), according to a climate sensitivity of 3 °C (2-4.5 °C) for a doubling of CO2. The observed amount of warming thus far has been less than this, because part of the excess energy is stored in the oceans (amounting to ~0.5 °C), and the remainder (~1.3 °C) has been masked by the cooling effect of anthropogenic aerosols." In line with my comment above, it seems that the AR5 numbers however	noted; the energy budget and its components has carefully been revisited for the Final Draft based on intensive cross-chapter discussions. This has resulted in a dedicated box 13.1 and TFE.4 in the TS.

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						are not internally consistent. [Bart Verheggen, Netherlands]	
0-199	0	0				The author names in the references should include all authors rather than using et al. [Soydoia Vinitnantharat, Thailand]	Editorial.
0-200	0	0				My main feeling when reading the report was that I was immersed in a blizzard of words. If this report is to be useful, over the next year you will need to hire some really good editors. [David Webb, United Kingdom]	noted; But the use of technical language is difficult to avoid when the priority of the authors is to complete an accurate and comprehensive assessment of the underlying science. The inclusion of a Glossary, and an acronym list as part of the WGI AR5 should help guide the reader through such technical language.
0-201	0	0				The report makes much use of various levels of probability and confidence i.e. "very likely", "medium confidence", etc.. Because of the importance of these words and phrases, they need to be defined within the document - not in a further publication. [David Webb, United Kingdom]	noted; See section 1.4 of Chapter 1 and Technical Summary, Box TS.1, or the revised AR5 Guidance Note on the Consistent Treatment of Uncertainty. In addition, a footnote has been added to all WGI AR5 chapter Executive Summaries introducing these terms at their first useage.
0-202	0	0				I have a problem with the continuing wide spread in climate sensitivities of the models used in this report. My concerns are stated more specifically in my comment on Section TS.5.3 (Technical Summary, page 38). However this spread in sensitivities is so central to the IPCC project that I believe it needs to be flagged here. [David Webb, United Kingdom]	noted; intention of the comment at this level is unclear. No action proposed.
0-203	0	0				One problem with a report of this size is the increased probability of internal inconsistencies. Many of my comments below concern one set of statements that imply that the observed increase in surface salinity implies an increase in the hydrological cycle and other statements that imply the hydrological cycle has hardly changed and the connection is not valid. To prevent problems later on, you will need a good devil's advocate to go through all the text before you publish the final version of the report. [David Webb, United Kingdom]	noted; Reducing overlap and ensuring cross referencing and consistency between chapters and working groups has been a focus of the revisions for the final draft.
0-204	0	1	69	299	69	There is updated of list of references to 2012. Yes, but as what I write before for the first draft version, there are two major comments for WG1AR5 FOD [YEHIA HAFEZ, Saudi Arabia]	noted; comment unclear and no action proposed.
0-205	0	1	69	299	69	1- The anomalies studies and figures must be taken for the mean period (1981-2010) instead of (1961-1990). This mean became in the history record !!!!! [YEHIA HAFEZ, Saudi Arabia]	noted; Based on the underlying assessed literature, the anomalies reported may vary between quantities. For projections in WGI AR5, the standard reference period 1986-2005 has been chosen.
0-206	0	1	69	299	69	2- All The figures in AR5 for the future periods must be ended by 2050 or 2100 only not more. To eliminate and avoid the uncertainty of the models. [YEHIA HAFEZ, Saudi Arabia]	reject; it is within the mandate and scope of the WGI AR5 report to provide projections beyond 2100.
0-207	0	1	69	299	69	Please, return to my comments of the first draft [YEHIA HAFEZ, Saudi Arabia]	noted; all comments to the First Order Draft have been considered by the authors and responses will be made available after completion of the WGI AR5 report.
0-208	0	1	69	299	69	Anyway, This is some comments of chapter 1 [YEHIA HAFEZ, Saudi Arabia]	noted; no action proposed
0-209	0	1				Nicely written and useful SPM [Cathy Clerbaux, France]	Thanks.
0-210	0	2				Glossary: Please include explanation of "aragonit". [Government of Germany]	reject; the authors did not consider this term required elevation to the Glossary.
0-211	0	4				Glossary: Carbon Dioxide Removal: see our comments on "Geoengineering in chapter 6. Suggestion for new Text: "Carbon Dioxide Removal (CDR) Carbon Dioxide Removal refers to several methods that have been proposed to influence the removal of atmospheric carbon dioxide and increase its storage in soil, biomass,	see the comprehensive definitions provided for CDR in the Glossary

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						ocean and geological reservoirs. See also Geoengineering and Solar radiation management (SRM)." [Government of Germany]	
0-212	0	4				Glossary: Please include explanation of "blocking". [Government of Germany]	has been added to the Glossary.
0-213	0	5	1	5	39	Comment 4: Table 4.1 distinguishes between ice-on-land and ice-in-the-ocean; this distinction is important for the assessment, and should be repeated in the Summary for Policymakers. [Thomas Dunning Newbury, United States of America]	reject; the Reviewer does not provide reasoning why this distinction would be important in the SPM. The SPM does provide specific statements for the various components - glaciers, ice sheets, sea ice etc.
0-214	0	5	1	5	39	The cryospheric information in the Summary for Policymakers will be combined with the summaries from the other WGI sections, and with the summaries of natural and socioeconomic changes from the WG II and III reports. In the cryospheric section, the relationships should be noted between (1) cryospheric "driving" forces and (2) resulting changes that are summarized in other parts of WGI and in WGII and III. The cryospheric driving forces are important, partly because of well-known satellite-images of sea-ice changes, and because of threatening sea-level rise. [Thomas Dunning Newbury, United States of America]	reject; the detection and attribution chapter of Working Group II is the appropriate location for the relationships to be assessed between physical changes in the cryosphere, and related impacts.
0-215	0	5	1	5	39	An important distinction in cryospheric driving forces is listed in the first table in the cryospheric chapter (Table 4.1). It makes a basic distinction between "ice on land" and "ice in the ocean." The distinction is appropriate for this assessment because the first one affects primarily the sea level and coastal cities; and the second one affects primarily the habitats of polar bear and other ice-dependent animals. Unfortunately, the distinction between ice-on-land and ice-in-the-ocean is made in neither the Technical Summary nor the Summary for Policymakers. The Technical Summary makes distinctions among changes in sea ice, glaciers, ice sheets, snow cover, and permafrost (sections TS 2.5.4, 5, and 6). Furthermore, the important Summary for Policymakers distinguishes only high-confidence changes in glaciers, the Greenland Ice Sheet, the Antarctic Ice Sheet, Arctic sea ice, Antarctic sea ice, and snow (page SPM-5). [Thomas Dunning Newbury, United States of America]	reject; The summary documents of the WGI contribution give significant attention to both observed and projected changes in sea ice, and to changes in sea level. This provides a comprehensive basis for WGII to assess related impacts. It is unclear why a further distinction between 'ice on land' and 'ice in the ocean' at the level of the TS or SPM would be necessary or helpful.
0-216	0	5	1	5	39	The organization of the SPM sections on Cryosphere Observations could be similar to the organization in Table 4.1. If it was arranged thus, the first sub-section would summarize changes in ice-on-land (SPM-5, line 7). The first paragraphs would summarize changes in the Greenland Ice Sheet, the West Antarctic Ice Sheet, and the East Antarctic Ice Sheet (lines 13-15 and 17-21). The next paragraphs would be those about changes in glaciers and snow (lines 7-11 and 13-15). The paragraphs could cross reference, for example, the summary statements about the ice sheets and glaciers in the AR5 chapter about freshwater resources (WG II, Chapter 3, page 3, lines 7-12). [Thomas Dunning Newbury, United States of America]	reject; the SPM is structured around the policy-relevant key findings of the WGI assessment.
0-217	0	5	1	5	39	Changes in individual ice sheets are partly a regional issue but, once the ice is melted, the change becomes a driving force of global issues. For that reason, a sub-section about changes of "ice-on-land" should conclude with statements about the combined affect on sea level—statements that can be checked for inadvertent contradictions with those about sea-level observations (see page SPM-5), and about Coastland systems and low-lying areas, about Polar Regions, and about Small islands (WGII, Chapters 8, 28 and 29). The cryospheric-summary statement could cross-reference, for example, the assumption in the chapter on Coastal systems about "... a sea-level rise of 0.5 to 2.0 m ..." by 2100 (WGII Chapter 5, page 3, lines 4-5 and 20-25). A primary subject of WGII Chapter 19 about Emergent Risks and Key Vulnerabilities is sea-level rise. The chapter about small islands summarizes that "... we have low confidence that sea-level rise will cause widespread destruction of islands in the next few decades ..." (WGII, Chapter 29, page 2, line 49). [Thomas Dunning Newbury, United States of America]	reject; The comprehensive assessment of the underlying science in WGI provides the basis for the assessment of impacts given in WGII. Chapters 4 and 13 of WGI both provide quantification of the individual contributions of changes in ice sheets to sea level rise. The SPM focusses on the key policy-relevant assessment of total change in mean global sea level.
0-218	0	5	1	5	39	The second sub-section would be about ice-in-the-ocean. The first and second paragraphs would be those about changes in Arctic and Antarctic sea ice and floating ice shelves (lines 23-27 and 29-30). The section would end with summary statements about reductions in different types of sea-ice habitats. The cryospheric evidence should be cross referenced for specific statements about sea ice (WGII Chapter 28 about Polar Regions, page 2, lines 43-45, and page 3, lines 36-50). [Thomas Dunning Newbury, United States of America]	reject; the SPM is structured around the policy-relevant key findings of the WGI assessment.
0-219	0	5	1	5	39	A separate, third sub-section would summarize changes in permafrost and methane emissions. Further, the	reject; the SPM is structured around the policy-

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						cryospheric evidence for a specific statement should be included: "(t)here is some evidence, for example, in the reduction of (summer) sea-ice extent in the Arctic and in the west Antarctic Peninsula, that the changes are non-linear, and may be accelerating." [Thomas Dunning Newbury, United States of America]	relevant key findings of the WGI assessment.
0-220	0	5	1	5	39	The following row contains an example of a high-lighted statement (lines 3-6), modified to reflect the basic distinction between ice-on-land and ice-in-the-ocean (proposed additions are in CAPITAL LETTERS, proposed deletions are in brackets): [Thomas Dunning Newbury, United States of America]	noted; no specific action proposed
0-221	0	5	1	5	39	More comprehensive and improved observations strengthen the evidence that ICE-ON-LAND (E.G., the ice sheets (are losing mass), glaciers, AND SNOW COVER) (are) IS shrinking globally AND RAISING SEA LEVEL, (and) that ICE-IN-THE-OCEAN (E.G., THE ARCTIC SUMMER sea ice cover) is SHRINKING AND reducing HABITAT FOR SUMMER-ICE-DEPENDENT ANIMALS (in the Arctic, and snow cover is decreasing) and permafrost is thawing AND RELEASING METHANE in the Northern Hemisphere. Ice is being lost from many of the components of the cryosphere, although there are significant regional differences in the rates of loss. {4.2-4.6, Table 4.1} [Thomas Dunning Newbury, United States of America]	reject; Reviewer does not provide a compelling reasoning why this distinction would be important or helpful. The policymakers are well aware that ice sheets are on land, and sea ice is in the ocean. The combination of physical changes with impacts (habitat loss) is a topic for the WGII assessment.
0-222	0	5	1	5	39	These changes of the cryospheric information in the SPM could be made also in the Technical Summary. End of comments [Thomas Dunning Newbury, United States of America]	reject; Reviewer does not provide a compelling reasoning why this distinction would be important or helpful in both TS and SPM.
0-223	0	6	6	6	7	I refer to the Second Annex, TableAll.1.3 where you have not had the courage to include decadal temperature figures for 2010. [Vincent Gray, New Zealand]	noted; reviewer comment is unclear - the table does provide an average for the period 2000 - 2009 (the last available complete decade).
0-224	0	6				Glossary: Climate sensitivity: Please give units of different quantities. [Government of Germany]	noted; units are included
0-225	0	7				Glossary: Transient climate response: The sentence start after "that is, at year 70..." is unclear. [Government of Germany]	noted; Glossary definition has been revised
0-226	0	12				Glossary: Geoengineering: See our comments on Geoengineering in chapter 6. Suggestion for new Text: "Geoengineering A set of proposed methods and technologies that aim to alleviate the overall impacts of climate change by large-scale intentional interventions to the climate system. Typically these methods seek to either a) reduce the amount of solar energy absorbed by the climate system (solar radiation management) or b) increase net carbon dioxide removal from the atmosphere (carbon dioxide removal). In this report geoengineering does not include carbon capture and storage at the point of combustion, but does include free-air capture of CO2. Research on geoengineering is still in its early stages – both concerning its potential efficacy or efficiency and associated risks and side effects." [Government of Germany]	noted; Glossary definition has been revised.
0-227	0	18				Metric: The term Metric is also used for "emission metric", please include this aspect in the definition, using text from Chapter 8, page 50, line 30-37 [Government of Germany]	reject; the term "metric" is defined in general terms only in the WGI AR5 Glossary. More specific details might be added in the Chapters.
0-228	0	25	1	25	1	In Annex iii , Glossary, there is no definition of "Sea Level" [Vincent Gray, New Zealand]	Don't understand the reviewers comment - Both 'relative sea level' and 'mean sea level' are included in the glossary.
0-229	0	26				SAM: Please add definition of the SAM index (difference between SLP ...), the current definition does not really include SAM. [Government of Germany]	noted; Definition has been added to the Glossary.
0-230	0	26				Glossary: Solar radiation management: see comments on "Geoengineering in chapter 6. Suggestion for new Text: "Solar radiation management (SRM) SRM refers to the intentional modification of the Earth's shortwave radiative budget with the aim of reducing climate change according to a given metric (e.g., surface temperature, precipitation, regional impacts). Artificial injection of stratospheric aerosols and boundary layer cloud brightening are two examples of SRM methods. The more general term radiation management would also include methods that seek to modify some fast-responding elements of the infrared radiation budget, for example by altering cirrus cloud cover. See also Carbon dioxide removal (CDR) and Geoengineering." [Government of Germany]	noted; Glossary definition has been revised.

