Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
0-1	0	0	0	0	0	The chapter is well done. Especially, the major findings since AR4 have got much more evidence and support in the chapter. As for biogeochemical changes, the content includes only ocean carbon, oxygen and a little nutrient. Thus, readers wonder if the content could really cover or reflect biogeochemical change. Could section 3.8 include or add some other important biological factors? , e.g., chlorophyll-a is also an important biological factor. [Rongshuo Cai, China]	Thanks. The Chapter 3 specific comment has partly been taken into account and more material on nutrients and carbon has been added in the relevant Chapter 3 sections.
0-2	0	0	0	0	0	I am not a climate scientist, but a science writer. I wrote a book (in Dutch) about the current state of the global warming debate, which referred many times to TAR and AR4. For this book I interviewed a lot of climate scientists around the world and from all sides of the spectrum (from 'alarmist' to 'mainstream' to 'sceptic'). In the book I document a lot of 'sceptical' literature that was ignored or downplayed by IPCC. In the climategate emails we can read that in some cases this was done purposefully (eg Jones' "I can't see either of these papers in the next IPCC report. Kevin and I will keep them out somehow even if we have to redefine what the peer reviewed literature is.") The main problem was and is that legitimate 'sceptics' are not invited to participate as lead authors in the report. This is too bad, because I am convinced IPCC reports would be more balanced if there were one or two 'sceptics' involved in each chapter. For AR5 this is still not the case. Having said that, my overall impression is that AR5 is more balanced than AR4 and I congratulate the authors with this step in the right direction. Also it is impressive to see how many papers have been published in this field since AR4. The amount of work that has been put into this report is impressive as well and for many scientists AR5 will be an important reference book. At the same time it is clear that the report is written from a 'greenhouse perspective'. The basic assumption behind the report already is that anthropogenic greenhouse gases are the most important driver of the climate system at this moment. I regret this because the consequence is a view on the climate system that is too narrow. Everyone active in this field knows that a majority of the scientists 'believe' that CO2 and other greenhouse gases will determine the near future of the climate. But there is a non-negligible minority that has other views. As the report is so focused on TOA radiative forcing and therefore on the role of greenhouse gases, the reader could easily get the impression that this approach to the climate issue is the only one. This is definitely not the case. In my opinion this should be made explicit in the report. The right chapter to do this would be chapter 1. A good starting point is a paper by Pielke et al. 2009: The need to consider human forcings besides greenhouse gases. Eos, Vol. 90, No. 45, 10 November 2009, 413. This article presents three hypotheses of which only one can be true. Those three are: Hypothesis 1: Human influence on climate variability and change is of minimal importance, and natural causes dominate climate variations and changes on all time scales. In coming decades, the human influence will continue to be minimal. Hypothesis 2a: Although the natural causes of climate variations and changes are undoubtedly important, the human influences are significant and involve a diverse range of first-order climate forcings, including, but not limited to, the human input of carbon dioxide (CO2). Most, if not all, of these human influences on regional and global climate will continue to be of concern during the coming decades. Hypothesis 2b: Although the natural causes of climate variations and changes are undoubtedly important, the human influences are significant and are dominated by the emissions into the atmosphere of greenhouse gases, the most important of which is CO2. The adverse impact of these gases on regional and global climate constitutes the primary climate issue for the coming decades. Hypothesis 2b is the currently accepted paradigm. My impression is that around 10% of the scientists believe in hypothesis 1 (people believing in a strong role of the sun are here but also scientists like Spencer and Lindzen who believe in a very low climate sensitivity), 10% think hypothesis 2a is true (the Pielke hypothesis) and 80% adheres to the greenhouse hypothesis. Among lead authors of the WG1 AR5 report this ratio is definitely different and will be close to 100% adhering to hypothesis 2b which in popular terms is called the IPCC (consensus) view. In this stage it's impossible to start all over again, with a broader view on the climate issue. But at least the AR5 could make the reader and especially the policy makers aware that there are different hypotheses around. The place to do this is chapter 1. [Marcel Crok, The Netherlands]	Thank you for this comprehensive general comment. We agree that the amount of information available for the current assessment is impressive. Also, we are confident that the author team with its members covering a wide range of expertise in climate change science and a large amount of experience actually is capable of covering the broad view of climate science required to prepare a comprehensive, scientifically sound and robust assessment of the physical science basis of climate change based on (largely) the published peer-reviewed literature. In contrast, the reviewers claims about biases in the AR5 WGI author team and the AR5 report in general are largely provided without scientific evidence supporting them. A few specific responses to the reviewers comment: (1) The author selection process in WGI AR5, for example, which is based on nominations of experts from governments, IPCC observer organizations and the IPCC WGI Bureau, was primarily based on scientific excellence based on objective criteria such as area and amount of expertise to cover the agreed content of this report, or past scientific performance and impact. It is a matter of fact that out of the over 1000 nominated experts only 258 could be selected as CLAs/LAs/REs in WGI AR5. (2) The claim that the "basic assumption behind the report already is that anthropogenic greenhouse gases are the most important driver of the climate system" is unsupported. In fact, the AR5 starts of with 3 chapters on observed changes in Atmosphere and Surface, Ocean and Cryosphere, without yet addressing the question of what causes these observed changes. It's only in Chapter 10 where the question of attributing observed changes to causes is finally assessed. (3) Non-GHG factors affecting the climate system on local, regional and global scales are discussed comprehensively in several of the chapters throughout the report and we thus strongly disagree with the claim of the reviewer that "alternative hypotheses" that could explain certain changes are not treated appropriately in WGI AR5. Again, the reviewer does not provide any quantitative, scientific evidence supporting his claims.
0-3	0	0	0	0	0	Final comment: Although I have written many critical comments, my overall impression is that AR5 is more balanced on many topics than AR4. I praise the author teams for the enormous amount of work they have done, often in their spare time. One of the main problems in my opinion is that AR5 tries to be too complete. Many topics that are not so relevant from a policy perspective get the same amount of space as hotly debated topics. This probably can't be changed anymore in the second draft but is a major shortcoming. There should be more space for hotly debated topics like the quality of the temperature measurements, hockey stick, hot	Noted with thanks. The selection of topics that this report addresses is the result of a clearly instituted scoping process which involves the governments participating in the panel. All topics, that they have identified as policy-relevant, are therefore covered. In the revised SOD, Chapter 1 will specifically point to

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						spot in the tropics. I wish the authors all the best with processing all the comments and writing a second draft. Marcel Crok [Marcel Crok, The Netherlands]	the scientifically debated topics and point to the chapters where they are assessed.
0-4	0	0	0	0	0	General comments regarding presentation: (A) Many parts of the text is purely technical and loaded with acronyms, with little or no allowance for the less-than-specialist reader. In the electronic version of the AR5 hyperlinks could be made which allow the reader to quickly identify the meaning of acronyms. (B) In many instances brief explanations of the nature/origin of processes, where such an explanation is known, should be most helpful and this will not detract from the technical excellence of the text; (C) Many of the diagrams have unexplained acronyms in their legends and some do not have parameter plots on the Y axis. [Andrew Glikson, Australia]	Noted with thanks. Consistency of acronyms will be improved, and the glossary will be further developed for the SOD. A list of acronyms will be included in the published report.
0-5	0	0	0	0	0	General thematic comments: (1) In general the role of fires as a major feedback of warming is not emphasized in the AR5 draft. I suggest this aspect is re-examined and updated, for example with reference to recent papers such by Bowman et al. 2009 (Fire in the Earth system. Science, 24, 324, 481-484) and references included in this paper. (2) References to an internationally agreed upper limit of below 2 degrees celsius do not acknowledge that this level has nealry been reached, and is only masked by the emission of anthropogenic sulfur aerosols, estimated by the AR4 at -1.2 Watt/m2 (direct effects and cloud albedo effects). (3) The synergy of feedback processes consequent on global warming (cf. reduced CO2 sequestration by warming water, acidification and thereby lesser CaCO3 secretion, forest fires, methane release from permafrost and bogs) needs to be further emphasized. [Andrew Glikson, Australia]	(1) Noted. Fires are covered in the WGI FOD. For example, the role of fires on changing aerosol emissions is recognized and discussed in Section 7.3.6 (numbering refers to FOD and will change in SOD). In Chapter 11, the role of wildfires as a source of carbonaceous aerosols and ozone is specifically noted. Nevertheless, the references provided by the reviewer will be considered in the preparation of the SOD, e.g., by Chapter 2. Please note also that WGII will cover comprehensively the assessment of impacts from (climate change induced) fires. (2) This has now been taken into account in Chapter 12 SOD and will explicitly be mentioned in the stabilization section of the Chapter. (3) Noted.
0-6	0	0	0	0	0	Chapter 2 has uncertainty words in italics. The other two chpaters I looked at (5 and 10) didn't. I think that what Ch2 has done would help all other chapters. [Philip JONES, UK]	Uncertainty language will use italics throughout the WGI contribution to AR5 - see uncertainty guidance note (Mastrandrea et al., IPCC 2010)
0-7	0	0	0	0	0	Global Positioning System (GPS) radio occultation (RO) now represents a mature remote sensing technique (Anthes et al., 2008; Anthes, 2011) and provides highly stable atmospheric observations. It is based on GPS radio signals which are bent and retarded by the atmospheric refractivity field, related mainly to pressure and temperature, during their propagation to a GPS receiver on a Low Earth Orbit (LEO) satellite. An occultation event occurs whenever a GPS satellite sets (or rises from) behind the horizon and its signals are occulted by the Earth's limb. The fundamental measurement is the signal phase which is based on precise timing with atomic clocks. Potential clock errors of GPS or LEO satellites are removed by differencing methods using an additional GPS satellite as reference and by relating the measurement to even more stable oscillators on the ground. Thus, GPS RO is anchored to the international time standard and currently the only self-calibrated raw satellite measurement with SI traceability, in principle (Leroy et al., 2006; Baringer et al., 2010). [Gottfried Kirchengast, Austria]	Noted; Please note that this comment has also been considered and answered as part of the Chapter 2 FOD review comments.
0-8	0	0	0	0	0	We use an empirical downscaling method based on Self-Organizing Maps (SOMs) to produce high-resolution, downscaled precipitation projections over the state of Pennsylvania in the Mid-Atlantic region of the U.S. for the future period 2046-2065. In order to examine the sensitivity of precipitation change to the water vapor increase brought by global warming, we test two approaches to downscaling: one uses the specific humidity in the downscaling algorithm and the other does not. Application of the downscaling procedure to the model projections shows changes in the relative occupancy, but not the fundamental nature, of the simulated synoptic circulation states. Both downscaling approaches predict increases in annual and winter precipitation, consistent in sign with the raw output from General Circulation Models (GCMs) but considerably smaller in magnitude. For summer precipitation, larger discrepancies are seen between the raw and downscaled GCM projections, with a substantial dependence on which of the two downscaling approaches is used (downscaled precipitation changes employing specific humidity are smaller than those without it). Application of downscaling reduces the inter-GCM variation, suggesting that some of spread among models in the raw projected precipitation may result from differences in precipitation parameterization schemes rather than fundamentally different climate responses. Projected changes in the North Atlantic Oscillation (NAO) are found	Noted, but no action item discernible. No pointer to a specific Chapter or to a specific peer-reviewed paper.

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						to be significantly related to changes in winter precipitation in the downscaled results but not for the raw GCM results, suggesting that the downscaling more effectively captures the influence of climate dynamics on projected changes in winter precipitation. [Michael Mann, USA]	
0-9	0	0	0	0	0	The report exhibits a very strong bias towards the northern hemisphere. This can be largely though not entirely explained by the fact that much more research has been done on northern hemisphere issues and most readers of the AR5 will live in the Northern Hemisphere and so it is appropriate that the bias exists. However, the bias seems too strong, perhaps stemming from a greater familiarity with and interest in research on northern hemisphere topics and by northern hemisphere scientists by most IPCC authors. Again very understandable but i think there is scope to reduce. Here are two examples: (i) Chapter 2, Box 2.4, Figure 2: The current plot is unnecessarily NH/North Pacific-centric. The PDO index is used to create a near-global plot. The plot reveals a very strong signal in the North Pacific and equatorial Pacific as expected but it is washed out everywhere else. The PDO index is known to include a convolution of ENSO and Aleutian Low variability (Newman et al. JClim 2003). Aleutian Low variability will include a good deal of variability that has no commonality with variability over the South Pacific or the Indian Ocean. The plot gives the false impression that ENSO-like decadal variability is much less important away from the North Pacific. When considering the globe it is better to use an IPO index as has been done in many previous studies. This reveals substantial variability right across the globe (e.g. Folland et al. 1997 Geophys. Res. Lett.; Power et al. Climate Dynamics 1999; Power and Colman Climate Dynamics 2006). (ii) Chapter 2, hurricanes, page 82, lines 40-47. Time series of hurricanes over the North Atlantic are shown and discussed. This is great - interesting and relevant. Yet only half a sentence is given to the longest historical record of tropical cyclone activity in the southern hemisphere (Callaghan and Power 2011) in the same section. This record dates back to 1872, making it one of the longest historical records of tropical cyclone/hurricane activity in the world. A trend significant at the 90% level is also evident, making it highly relevant to the topic in this section. The robustness of this record was discussed by Callaghan and Power (2011) who concluded that confidence in the record is justified. This is based on their findings that:(i) the record exhibits a Poisson distribution (consistent with shorter satellite records of TC frequency);(ii) the variance in the first part of the record is the same as variance in the second part of the record;(ii) there is an ENSO imprint on the variability consistent with shorter satellite-based records; and(iv) the trend coincides with a trend in the SOI over the same period, and is therefore plausible. (v) in the unlikely event that tropical cyclones were in fact missed these would have occurred in the early part of the record and so their inclusion would therefore increase the magnitude of the downward trend.Given the reliability of the data, the statistical significance of the trend and the fact that the dataset provides the longest available record of tropical cyclone/hurricane activity in the Southern Hemisphere and one of the longest in the world, I think this warrants modification of the statement made in the Executive Summary of chapter 2 (and elsewhere in Chapter 2 and Chapter 14) to e.g. "With the exception of a downward trend in the number of severe tropical cyclones making landfall over north-eastern Australia since the late 19th century, there is low confidence in the fidelity of any reported regional trends in tropical cyclone activity on multidecadal timescales or longer".If this was a record from the northern hemisphere I would be amazed if the results were given such passing attention given the record length, robustness and relevance of the study to the primary topic i.e. identification of trends in extreme events. [Scott Power, Australia]	Noted. A more balanced geographical coverage of the assessment will be a focus of the SOD development in several of the Chapters. More detailed responses to the two specific Chapter 2 points are given hereafter. Point (i): Rejected. We consider IPO and PDO to be different indices of essentially the same thing: Pacific-Scale Decadal and Interdecadal Variability. We are not aware of a qualitative distinction between such phenomena as described by these two indices within the observational record; these are often used interchangeably (e.g., Deser et al. 2010 Annual Review of Marine Science). Despite being more influenced by the Aleutian Low (by construction), the PDO pattern does have an extension into the South Pacific. The preference for using the PDO index rather than the IPO in computing the regression pattern in Box 2.4 Figure 2 (Box 2.5 in SOD) is due to a relatively more robust procedure existing for the PDO (as opposed to the IPO) index definition (the 1st EOF as opposed to the 2nd or 3rd EOF, depending on the period). Furthermore, a trial use of the IPO index instead of the PDO has not in fact produced an easier-to-read pattern for the Southern Hemisphere. See also the response to comment #2-892; Point (ii) by the reviewer is noted. Although there was a mention of this record in the main text, more of a discussion including a figure was included in FAQ2.2. This FAQ has been re-written and the discussion and figure have been moved to the main text. We agree that there is likely to be somewhat higher confidence in the fidelity of the eastern Australian land-falling tropical cyclone record and this is now not only now reflected in the text but is also highlighted in FAQ2.2 Figure 2 including an update to the wording in the executive summary.
0-10	0	0	0	0	0	Power et al. (J Climate 2012, "Consensus on 21st century rainfall projections in climate models more widespread than previously thought", in press) concluded that: "there is a need to identify regions, variables and phenomena that are expected to be little affected by anthropogenic climate change, and to communicate this information to the wider community". Knowing that something is not going to change much, if at all, might be very valuable for some planners in the wider community. In the past the report did not distinguish between regions where projected changes are uncertain and regions where models agree that projected changes are small or zero. This means that we did not convey as much information in the AR4 as we could have (Power et al. J.Climate 2012). In the last report - because of this issue - we gave the impression that uncertainty is more widespread than it really is. We therefore did not communicate the projections as well as they could have been. The FOD currently highlights what will change but gives very little attention to topical things that will very likely not change. I strongly recommend that more attention is given in the projections chapters to what topical things are projected to exhibit small or zero change. This includes identification of such regions in projection maps but also extends to discussion of important phenomena for which there is a degree of confidence that it will not change. Recommend that more information on this is included in Executive Summaries. This will also	Agreed. Separating "no significant change" and models disagreeing is important. This was already done using a modified stippling in the FOD, e.g., in Chapter 12. For Figures in the SOD of both Chapters 11, 12, and the Atlas it is planned to indicate "robust change" and "no significant change" with different stippling/hatching where appropriate. The proposal to discuss things that do not change is noted but the comment is unspecific about aspects of climate this is meant to apply to. Nevertheless, e.g., Ch14 intends to provide detailed information regarding robustness of little or no change in regional projections, when this is assessed and the result of multiple independent lines of evidence.

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						benefit the scientific community because there is currently widespread misunderstanding that a lack of agreement on the sign of change is equivalent to a lack of agreement. It isn't. [Scott Power, Australia]	
0-11	0	0	0			File Upload Error / Your upload could not be processed. There was an error with your Review Form. / Please check line 162 of the "Comments" sheet. / For each comment you must at least specify a "Chapter" and a "From Page" -- Note that this CONTRADICTS the instructions on in the spreadsheet, which say to specify only the From Page for continuation lines. [David Burton, USA]	Noted and solved by WGI TSU IT support.
0-12	0	0	0			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: 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 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, in press. DOI: 10.1016/j.jastp.2011.12.005. http://www.fel.duke.edu/~scafetta/pdf/Scafetta_models_comparison_ATP.pdf 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&eotf=1&u=http%3A%2F%2Fwww.theclimatescam.se%2F2012%2F01%2F17%2Fmer-harmonisk-analys%2F [David Burton, USA]	Noted -- the papers referred to by the reviewer clearly state that there is no known physical mechanism to explain the proposed celestial modulation of climate, and so this is not included in the physically-based models whose results are assessed in this report, in particular in Chapter 9 "Evaluation of Climate Models". More specifically, the second paper by Scafetta referred to by the reviewer claims that not only the GISS but all climate models assessed in the IPCC reports significantly underestimate the magnitude of 20 and 60 years cycles apparently seen in the reconstructed global temperature. However, irrespective of whether the above mentioned periodicities are real or an artifact of the statistical analysis, this fact alone does not challenge validity of current climate models. Please note that the role of various known forcings is assessed comprehensively in Chapter 10 of the report.
0-13	0	0				I would like to congratulation all the authors for their excellent work. A tremendous amount of material has already been included and all the chapters I have looked at are on a good path. [Christof Appenzeller, Switzerland]	Thanks
0-14	0	0				It is better not to use acronyms in the executive summaries. Rather use the full text to prevent people reading only the summary to get confused. [Pieter Aucamp, South Africa]	Point noted and considered for SOD
0-15	0	0				Use the correct citation for the WMO 2010 Scientific Assessment of Ozone Depletion. It is NOT WMO 2011 or UNEP 2011 [Pieter Aucamp, South Africa]	Noted - specific comments of this nature should be made at the chapter level. We note that references will undergo a careful copy-editing during the process of finalization of the report.
0-16	0	0				A. Comments on the discussion of Lindzen and Choi (2009 and 2011) in AR5 FOD.Lindzen and Choi (2011; hereafter LC11) examined the rate of change of net (LW + SW) outgoing TOA radiation with surface temperature (dFlux↑/dSST) in the tropical band 20ºS - 20ºN on interannual timescales, comparing the values given by observations with those given by 11 AMIP GCMs. They found a substantial difference, with the observations indicating the tropical zone to be strongly stable (dFlux↑/dSST >B, where B is the blackbody radiative response) and the GCMs indicating it to be unstable (dFlux↑/dSST <0). They then used a simple two-zone (tropical/extratropical) energy balance model to estimate the effect of these different values of tropical dFlux↑/dSST on climate sensitivity and found large differences. I feel that the above results, both observational and theoretical, represent substantive issues that need to be addressed in AR5. In the current FOD, the LC11 paper is referred to in only one place (Chapter 7, page 18, lines 16-17) and its results are rejected there on the following grounds:(1) Another author (Dessler, 2010; hereafter D10) has found that among different GCMs there is no correlation between interannual and long-term cloud-radiative responses to surface temperature variations (lines 17-18);(2) D10 has also found that the GCMs do predict a cloud-radiative response on the interannual timescale that is consistent with the observations, thus increasing the credibility of the GCM predictions at longer timescales (lines 19-21).(3) Estimating climate sensitivity from observed interannual cloud-radiative responses using simple conceptual models [such as used by LC11] has no sound physical basis and has not been be shown to work consistently when applied to GCMs (lines 27-29). I believe	Rejected - The reviewer misrepresents the statements by Chapter 7 and by Dessler (2011). Nowhere is the basic 0-D energy balance model criticized; what is criticized is the interpretation of short-term cloud variations as being solely due to surface temperature change, whereas atmospheric circulation change plays a role. Furthermore, LC11 only report a discrepancy between models and atmosphere when considering atmosphere-only GCMs. They show that fully coupled models, the type actually used to make climate projections, do produce results that scatter around the observations. As pointed out by a number of earlier studies not acknowledged by LC11, including Sherwood et al. 2010 (Rev. Geophys) and others cited therein including Spencer and Braswell 2010, a model that does not include a prognostic ocean cannot be expected to reproduce real-world

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						the above arguments do not provide valid grounds for rejecting the LC11 results, for the following reasons:(a) The arguments (1), (2) and (3) are based on cloud-radiative response, whereas LC11's dFlux↑/dSST represents the total radiative response (due to the combined effects of blackbody, water vapour, cloud and all other processes that affect the outgoing LW+SW radiation) in the tropical band.(b) LC11's dFlux↑/dSST was calculated using time intervals over which there were significant SST variations (greater than 0.1ºC averaged over the tropical band). High correlations between dFlux↑ and dSST at lags of a few months were then found, permitting an estimate of dFlux↑/dSST in which one could have reasonable confidence. In contrast, D10 examined globally-averaged data, did not select intervals of significant temperature variation (thereby admitting significant noise), considered cloud-radiative rather than total responses, and did not obtain high correlations between the quantities being examined. (c) Taken together, arguments (1) and (2) can be paraphrased as follows: LC11's finding the the GCMs' interannual radiative responses differ from the observations is of no consequence, since the GCMs' interannual and long-term radiative responses are uncorrelated; on the other hand, D10's finding that the GCMs' interannual radiative responses agree with the observations is of consequence, increasing the credibility of the GCMs' long-term predictions. These inferences are inconsistent.(d) Argument (3) asserts that simple conceptual models, such as the commonly used zero-dimensional energy balance model (ZDM) or the two-zone energy balance model of LC11, have no sound physical basis. Unfortunately, if this is so, it makes little sense to use the zero-dimensional model as the conceptual basis for the discussion of climate feedbacks, as is done in all the IPCC reports and in the current AR5 FOD (see my comments on this topic in D below). In addition to their 2011 paper that is referred to in Chapter 7, Lindzen and Choi (2009; hereafter LC09) have earlier published related results that are referred in two separate chapters in AR5 FOD (Chapter 2, page 41, and Chapter 10, page 65). The LC09 results are again criticized on a number of grounds. However, LC11 have replied to these criticisms of their LC09 paper.Recommendation: I recommend that the results of LC09 and LC11 be treated as a substantive issue and that both papers be discussed in a coordinated manner. I feel that the most suitable chapter for discussing them is Chapter 9, perhaps somewhere in Section 9.1. I recommend that at a minimum it should be stated in AR5 that "The results of Lindzen and Choi (2011) represent a substantive issue that requires further investigation." ReferencesLindzen, R.S. and Choi, Y.-S., 2009. On the determination of climate feedbacks from ERBE data. Geophysical Research Letters, 36, L16705.Lindzen, R.S. and Choi, Y.-S., 2011. On the observational determination of climate sensitivity and its implications. Asia-Pacific Journal of Atmospheric Sciences, 47, 377-390. [J. Ray Bates, Ireland]	ocean-atmosphere statistical relationships even if the model is perfect. Indeed over the warmest oceans, the real-world statistical relationship between cloud properties and ocean temperature is dominated by the influence of the atmosphere on the ocean, not the reverse, as shown by Graham and Barnett 1993, also not acknowledged by LC11. Thus the results presented in LC11 and other studies do not support the allegations of LC11 that they have demonstrated model errors, because LC11 make simplistic assumptions that have already been falsified by previous work. There is not time to explain this in the IPCC chapter, but the text states accurately the gist of the situation.
0-17	0	0				B. Making use of the predictive information contained in the latitudinal variation of the troposphere-adjusted forcing (AF) as evaluated at the surfaceWhereas the global-mean tropopause-adjusted forcing (AF) provides a useful indication of the eventual change in global-mean surface temperature, it provides no information about how this change may be distributed with latitude. Recognizing that the global-mean AF is the same at all levels from TOA to the surface (though differently composed at the different levels), Alexeev (2003) examined how the AF is distributed with latitude at TOA and the surface in a GCM coupled to an ocean mixed layer on an aquaplanet. He found that at TOA the AF showed little latitudinal variation, but at the surface it exhibited a marked latitudinal structure, mainly due to the latitudinal variation of its latent heat component. He found that the latitudinal structure of the AF at the surface provided a useful predictor of the latitudinal structure of the eventual surface temperature change. The indications are that this is an area meriting further investigation.Recommendation: I recommend that the above comments be inserted somewhere in the report. Suitable points of insertion could be in Section 8.1.1.2 (replacing lines 31-32 on page 8-6) in or Section 9.7.4.1.Reference.Alexeev, V. A., 2003: Sensitivity to CO2 doubling of an atmospheric GCM coupled to an oceanic mixed layer: a linear analysis. Climate Dynamics, 20, 775-787. [J. Ray Bates, Ireland]	Rejected - Section 8.6.2 contains an extensive discussion of the most recent research on the relationship between the spatial pattern of radiative forcing and the eventual surface temperature response. As there is a marked difference in the response of land versus ocean to forcing, either homogeneous or inhomogeneous, and land is of course unevenly distributed, we do not place great weight on results from a study using an aquaplanet model and instead assess the results of more realistic GCMs in that section.
0-18	0	0				C. Comments on the different usages of the term "Climate Feedback" in IPCC AR5 FOD.A number of authors (e.g., Stephens, 2005; Bates, 2007; Roe, 2009) have stressed the importance of adopting clear and consistent definitions of Climate Feedback. In AR5 FOD, a Glossary definition of "Climate feedback" is not yet available. However, a partial reading of FOD shows that the term "climate feedback" is used in multiple senses that can possibly be in conflict. Some examples of these usages, which I have phrased according to my best understanding of the text, are as follows:Definition 1. [Chapter 1, p. 1-5, lines 48-57 and the accompanying Figure 1.1]."Any process that amplifies or reduces the size of the climate system's response to an external forcing agent is called a climate feedback."Definition 2. [Chapter 9, Section 9.7.4 and Chapter 7, p. 7-6, lines 28-41, plus Figure 7.1]"Any process that arises in response to an externally-forced perturbation in the global-	Taken into account. Glossary does include definition of climate feedback, which will be updated. All Chapters are revising their relevant text parts in order to improve consistency with the Glossary. More specifically, Chapter 1 has substantially revised the discussion on feedbacks, while keeping the discussion at the general level, however. This general description is not considered to be in conflict with some of the more detailed definitions provided in the

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						mean surface temperature ΔT and that in turn influences ΔT is called a climate feedback."More precise quantitative versions of Definition 2, based on a zero-dimensional conceptual model of the climate system, are also used:Definition 2(a). [Chapter 9, p. 9-64, lines 25-47]."Any process that influences the value of the climate feedback parameter α (defined below) is called a climate feedback. Feedbacks that increase α are defined to be negative, those that diminish α are defined to be positive. Feedbacks are measured in units of Wm-2K-1."The climate feedback parameter α is defined in terms of a zero-dimensional conceptual model of the climate system whose governing equation is N = F – αΔT (1)(N= net downward radiative energy flux at TOA, F = troposphere-adjusted forcing resulting from a doubling of CO2, ΔT = global-mean surface temperature perturbation). The numerical values of α and F are determined by identifying eq. (1) with the regression line obtained from the output of an AOGCM subject to a CO2 doubling.Definition 2(b) [Chapter 7, p. 7-14, lines 6-13; Chapter 5, P. 5-13 and Figure 5.4]"Any process that influences the value of the climate feedback factor f (defined below) is called a climate feedback. Feedbacks that increase f are defined to be positive, those that diminish f are defined to be negative. Feedbacks are dimensionless." The climate feedback factor f is again defined in terms of a zero-dimensional conceptual model of the climate system, whose solution for the equilibrium value of the 2 x CO2 global-mean surface temperature increment ΔT is written in the formΔT=ΔT0 /(1-f) (2)Here, ΔT0 is the zero-feedback 2 x CO2 equilibrium temperature increment, defined as ΔF/B, where ΔF is the stratosphere-adjusted 2 x CO2 radiative forcing and B is the blackbody radiative response coefficient [= (1/0.31) Wm-2K-1]. On imposing the CO2 doubling, ΔF is determined from initial calculations using the GCM's radiation code and ΔT is determined by integrating the GCM (usually coupled with a mixed layer ocean) to equilibrium. The feedback factor f is then determined by eq. (2).Comments: While Definitions 2(a) and 2(b) share the fact that they are both based on a zero-dimensional conceptual model of the climate system with the global-mean surface temperature perturbation as the basic variable, they differ in the following respects:• In Definition 2(a) the blackbody response is included in the feedbacks, whereas in Definition 2(b) the blackbody response defines the zero-feedback case;• In Definition 2(a) feedbacks have units of Wm-2K-1, whereas in Definition 2(b) they are dimensionless;• In Definition 2(a) the forcing F is the troposphere-adjusted forcing (=AF), which is the same at all levels from TOA to the surface (though differently composed at the different levels). In Definition 2(b) the forcing ΔF is the TOA stratosphere-adjusted forcing. In lines 11-13 the authors who use Definition 2(b) make it clear that they reject surface forcing as a predictor of overall response, thus rejecting Definition 2(a).Definition 3. [Chapter 5, p. 5-40, lines 43-46]"Any process that influences the rate at which the climate system returns asymptotically to its initial state after being subjected to a temporary external radiative forcing is called a climate feedback. A negative feedback exerts a stabilizing influence (increasing the asymptotic rate of decay of a perturbation), a positive feedback exerts a destabilizing one (decreasing the asymptotic rate of decay, or increasing the asymptotic rate of growth)."Comments: A feedback so defined corresponds to a stability-altering feedback in the terminology of Bates (2007). As shown in that paper, a process that exerts a negative feedback in the stability-altering sense does not necessarily exert a negative feedback in the sensitivity-altering sense of Definitions 2(a) and 2(b). Definition 4 [e.g., Chapter 2, p. 2-78, lines 26-27; Chapter 9, p. 9-60, lines 12-19]"If a climate variable A influences a climate variable B, and B in turn influences A, a climate feedback is said to exist between A and B. The feedback is positive if it amplifies both A and B, negative if it diminishes both A and B."Comments: This mutual-interaction feedback definition is similar to the definition of Climate Feedback given in the Glossary of AR4. Unlike Definitions 1, 2 and 3 above, it contains no reference to the response of the climate system to an external forcing. If a feedback so defined is initially positive and remains positive, it leads to instability.Recommendation: I recommend that the Second Order Draft of AR5 should include a Glossary with a definition (or definitions) of Climate Feedback that should be adhered to throughout the Report. I believe this would help the authors to avoid internal inconsistencies and would be very helpful to the readers.References.Bates, J.R., 2007. Some considerations of the concept of climate feedback. Quarterly Journal of the Royal Meteorological Society, 133, 545-560 (Erratum: p. 1071).Roe, G., 2009. Feedbacks, timescales, and seeing red. Annual Review of Earth and Planetary Sciences, 37, 93-115.Stephens, G.L., 2005. Cloud feedbacks in the climate system: a critical review. Journal of Climate, 18, 237-273. [J. Ray Bates, Ireland]	subsequent Chapters. Chapter 7 has removed the dimensionless definition 2b in response to of a large number of critical comments. Finally, with regard to Chapter 9, the reviewer gives an inaccurate description of what this Chapter actually does contain. Chapter 9 FOD, page 9-64 only deals with the climate sensitivity parameter and makes no implication (yet) about climate feedback. That said, the WG1 AR5 should be explicit about the "zero-feedback case" implying that the "Planck feedback" is present. The reviewer's distinction between stability-altering and sensitivity-altering feedbacks, and in particular the statement of his 2007 paper that a feedback might be negative in the sensitivity-altering sense but positive in the stability-altering sense, arises from a misconception of his numerical results (note that his review comment has the switch in feedback sign the other way around compared to his paper). In the case considered in Bates (2007), all feedbacks are negative, but he has transient perturbation growth arising from a specific choice of initial conditions. This happens in (mathematically) non-normal systems. See: (1) Trefethen, L. N., A. E. Trefethen, S. C. Reddy, and T. A. Driscoll, 1993: Hydrodynamic Stability without Eigenvalues. Science, 261, 578-584; (2) Marotzke, J., 1996: Analysis of thermohaline feedbacks. Decadal Climate Variability: Dynamics and Predictability, D. L. T. Anderson, and J. Willebrand, Eds., Springer-Verlag, Berlin, 333-378. Furthermore, transient growth of the global-mean temperature can even happen if through choice of parameters the system is symmetric and hence normal. In the reviewer's model, asymmetric choice of longwave radiation parameters implies that the global-mean temperature is not an eigenvector, and hence it can show transient growth under some initial conditions even if all eigenvalues are negative.
0-19	0	0				D. Comments on the adequacy of the zero-dimensional model as a conceptual model for discussing climate feedbacks.Given the widespread use of the zero-dimensional model as a conceptual model for discussing climate feedbacks, and the adoption of this model in AR5 FOD as the basis of the feedback Definitions 2(a)	Rejected - The concept of rapid adjustments has been introduced precisely to address this shortcoming of the zero-order forcing-response model. The types of

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						and 2(b) above, it is important that the extent to which this model is a valid conceptual model of the climate system be discussed. Instances are known, especially in the presence of aerosols, where TOA radiative forcing does not give a good indication of the sign of the resulting equilibrium global and annual mean surface temperature response (e.g., Cook and Highwood, 2004); thus there are circumstances in which the zero-dimensional model can seriously break down. In a theoretical investigation comparing the zero-dimensional model with two-zone conceptual models that include dynamical heat transport, Bates (2012) has found that a necessary condition for the validity of the zero-dimensional model is that the TOA radiative response to surface temperature perturbations in the tropics and extratropics be close in value. If this condition is not met, as indicated by some studies, and negative aerosol forcing as well as positive GHG forcing is included, the results of Bates (2012) show that the global-mean TOA forcing may not be of the same sign as the equilibrium global-mean surface temperature response. In these circumstances, it may be necessary in future to extend the zero-dimensional model to a two-zone (or higher order) model that includes dynamical interaction to obtain a valid low-order conceptual model for discussing climate sensitivity and feedbacks. Recommendation: I recommend that a discussion of the validity and limitations of the zero-dimensional model as a conceptual model for the discussion of climate feedbacks be included at some point in the report (a suitable place could be Section 9.7.4.3.3, “Sources of uncertainty in modeled climate sensitivity”).Bates, J. R., 2012. Climate stability and sensitivity in some simple conceptual models. Climate Dynamics, 38, 455-473.Cook, J. and Highwood, E.J., 2004. Climate response to tropospheric absorbing aerosols in an intermediate general-circulation model. Quarterly Journal of the Royal Meteorological Society, 130, 175-191. [J. Ray Bates, Ireland]	internal adjustment that can occur go far beyond the particular example offered by the reviewer, and not all can be discussed explicitly given space limitations in this report. Quantifications of the rapid adjustments presented in the report will, however, include the responses noted by the reviewer to the extent that the forcings and atmospheric dynamics are correctly represented in the GCMs used. Moreover, the behavior 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.
0-20	0	0				A glossary of all the abbreviations used in the document is highly recommended [CATHERINE BELTRAN, France]	Noted - list of acronyms will be included in the published report
0-21	0	0				While awareness of uncertainty is essential for any presentation of scientific data, it must not be overstressed in a way that any observations and projections will be cast into doubt by decision makers. The diction should take into account the field's responsibility, for the consequences of mistakenly denying the incidence of Climate Change having consequences much more severe than the opposite misapprehension. [Christian Reiner Boehm, Germany]	Noted. The AR5 uncertainty guidance note (Mastrandrea et al., IPCC 2010) addresses both under- and over-estimating uncertainties.
0-22	0	0				Given the emphasis on prediction in this FOD and previous IPCC Assessment Reports I feel that it is my responsibility as a citizen to point out that this predictive assumption is based on Baconian ethical (materialist utilitarian) and epistemological assumptions. The issues with Baconian epistemological assumptions have been discussed in some depth in Charlesworth and Okereke (2010). The FOD, in particular Chapters 9 and 12, reinforces the analysis of limits of climate epistemology in Charlesworth and Okereke (2010). The issues with the ethical assumption are mentioned briefly in that article and are explored in more detail in my manuscript which is nearing completion. It will be worth summarising. Charlesworth and Okereke (2010) demonstrates that prediction cannot be robustly and unquestionably used to produce costs for economic cost benefit analysis. CBA nearly completely dominates climate policy; this strongly suggests that the wrong policy tools are being used (and often imposed). Palmer and Finlay (2003) referenced in Charlesworth and Okereke provides sufficient evidence to question whether most people in the world would start with the ethics of economics; instead most would start with the virtue epistemology of prudence, precaution and wisdom plus the virtue ethics of justice not inequality, moderation not consumerism, courage not timidity or fool hardiness and perhaps care, respect and hope. There are many other sources of evidence that point in the same direction. To oversimplify for clarity, economics based on inadequate prediction is irrational and undemocratic. I am not sure of the place of these thoughts in WG1 documents but the Stern Review stated its ethical assumptions, even if it did not consider other ethical positions. To hopefully clarify the point, the approach of this first order draft and previous AR documents is based on questionable epistemological and ethical assumptions. As the authors have done such an excellent job of clarifying the significance of tipping points, inertia and feedbacks in Chapter One of the FOD it may be relevant to mention the broad assumptions, noting that these are contested. I will quite understand their strategic and ethical judgement if they decide not to – if not then it should go in the summary for policy makers or at least the synthesis report. [Mark Charlesworth, United Kingdom of Great Britain & Northern Ireland]	Noted - no action. Ethical, economical and policy aspects are beyond the mandate of WG1.
0-23	0	0				Overall remark. The document reports lots of data, some new, but fails to a large degree to provide policy	First Order Drafts of both Technical Summary and

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						makers with information that they need. It reports details of the trees but fails to describe the forest. Forcing by 2100 will rise to ca. 5% of the natural GHE if CO2 is not controlled. [Robert Charlson, USA]	Summary for Policymakers will be available at the stage of the SOD review of the Chapters.
0-24	0	0				In general we feel that the AR5 is in good shape, and the overall impression is that lots of new material is embedded in the FOD. It reflect well the evolution in the amount of scientific material that has been published since AR4, and we expect that even more material and studies will be included as the process evolves. The process of choosing what to include in the executive summaries seems not to be the main focus so far. This very important task should be given more attention when the upcoming SOD is prepared, as it usually serves as basis for the summary for policymakers. [Øyvind Christophersen, Norway]	Thanks - Executive summaries will continue to be developed for the SOD.
0-25	0	0				Treatment of uncertainty is an important issue, and you may consider to be more in line with the structure in the outline document by collecting the discription regarding uncertainties in Chapter 1 and in the latter chapters refer to chapter 1. [Øyvind Christophersen, Norway]	Noted - Assessment of Uncertainty was so far and will be an important component of the SOD and beyond.
0-26	0	0				One issue that should be given priority in the further work with the report is regional changes and projections. It is good to have Annex 1, but it is felt that the resukts could have been described in text, and you may consider to include some of this information in Chapter 11 and 12. Futhermore, it is important that authors of the WGI report cooperates with authors of WGII in order to provide the necessary material for them. [Øyvind Christophersen, Norway]	Agree - all material presented in Annex 1: "Atlas of Global and Regional Climate Projections" will need to be firmly rooted in Chapters 11, 12, and 14 in the WGI AR5 SOD. In particular the regional assessments provided in Chapter 14 will be better linked to Annex I in the SOD. And we certainly appreciate the importance of Annex 1 to WGII, which is the primary reason for adding this new element to the WGI contribution to AR5 in the first place.
0-27	0	0				I am an engineer, my work is to make instruments for scientists working in the meteorological field so I'm not exactly a lay reader but I'm not a scientist either. I'm honored that the WGI has selected me for reviewing the FOD of WGI; it shows that indeed, the IPCC tries to involve a wider community and should be congratulated on that point. Being an engineer, I don't write publications myself (except on instrumentation), I hope my contribution will be different than scientists' contribution who are more in the heat of the debate; hopefully my comments will make your contribution more accessible to a wider community following the wishes of IPCC (line 20-21, p. 1-3). [Francois DANIS, France]	Noted with thanks.
0-28	0	0				Chapter 2 (Observations: Atmosphere and Surface), Section 2.4.3 (Aerosols) [Panuganti China Sattilingam Devara, India]	Comment unclear. No action.
0-29	0	0				We 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]	The average is a well-known quantity that represents the central value of temperatures globally. The useful metric mentioned was the average temperature.
0-30	0	0				We suggest to use the term "Climate engineering" rather than "Geoengenieering" for large scale manipulation of the climate system as proposed by Feichter and Leisner (2009). Climate engineering seems more approriate as geoengenieering, which may also include other manipulations not necessary targeted at the climate system. REFERENCE: Feichter, Johann/Leisner, Thomas (2009): Climate engineering: A critical review of approaches to modify the global energy balance. European Physical Journal-Special Topics 2009, 176, 81-92. [Andrew Ferrone, Germany]	Noted. This is a valid comment even though "geoengineering" is often used to mean "climate engineering". We will consider flagging this terminology issue in the SOD of Chapter 1. Yet, the general overarching term used in the literature is 'geoengineering', and it's thus included in the IPCC approved outline of the WGI AR5. To the extent possible, chapters refer to the specific technique applied, including (solar) radiation management or carbon dioxide removal techniques.
0-31	0	0				Chapters 2 and 14 both contain sometimes similar atmospheric circulation information. Significant cross referal is needed. Should any part of this area of chapter 2, however small go in Chapter 14? And vice-versa. [Christopher Folland, United Kingdom of Great Britain & Northern Ireland]	Noted. Cross-chapter consistency and reduction of unnecessary overlap and duplication will be a focus of the SOD development.
0-32	0	0				1 - The comments numbered 1 to 20 are the sequential paragraphs of the general introductory comment of	The relationship between observed temperature

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						this reviewer's report. The elementary approach to answer the question of the suspected relationship between earth global temperature and anthropogenic emissions of CO2 would suggest to plot the latter, global temperature, versus the former, CO2 concentration, and check whether a correlation is found. To plot each of the quantities, temperature, CO2 concentration in air, CO2 emissions, versus time, even if useful, is insufficient. There are many others quantities that increase with time, viz. life expectancy, world population, debt of France, debt of US, the weight of a number of persons... It does not mean that there exists a straightforward correlation and a causal relationship from each to any other. [François GERVAIS, France]	variability and increasing CO2 concentrations and other human-induced and natural drivers (external forcings) of climate (including tropospheric aerosols, solar output variability, explosive volcanic eruptions etc) is considered in detail in Chapter 10 where the extent to which each of these factors have contributed to observed temperature variability is assessed. This includes fingerprint studies which evaluate the extent to which patterns in space and time of response to external forcing (fingerprints) from climate model simulations explain observed climate change. Therefore the assessment does not simply look at correlations between global mean temperature and CO2 concentrations as suggested by the reviewer.
0-33	0	0				2 - If one plots HadCruT temperatures vs CO2 concentrations measured in the atmosphere at Mauna Loa, one does find some possible correlation from 1975 to 1998, but not afterwards. After 1998, the slope of the linear regression becomes almost flat instead of remaining ascending. Why ? Very likely because natural climate variability has to be taken into account also. It might even mask greenhouse effect, as recognized in the AR5 draft (Ch 2 Page 4 lines 44-48, Ch 2 Page 5 lines 7-21, for example). [François GERVAIS, France]	Chapter 10 assesses the extent to which external forcings of climate and internal variability contribute to observed variability of global temperatures. The assessment does take account of natural internal climate variability and natural external forcings of climate and the assessment is not based on a simple correlation between CO2 concentrations and global mean temperatures since there is other information, including the extent to which internal variability can affect global temperatures to take into account.
0-34	0	0				3 - N. Scafetta (J. Atmospheric & Solar-Terrestrial Physics 71 (2009) 1916) has performed a Fourier analysis of HadCruT global temperature data. Scafetta found a number of resonances that he compared with the ones corresponding to the motion of the sun with respect to the barycenter of the solar system. He points towards an almost perfect coincidence of the frequencies of the various resonance peaks. The probability that these coincidences be fortuitous is near zero. This study appears most interesting. Why is it not cited, quoted and discussed in the AR5 draft ? [François GERVAIS, France]	Noted. This paper has been considered and the citation will be included in the revised draft of Chapter 10 assessment.
0-35	0	0				4 - The main resonance reported by Scafetta corresponds to a ~ 60 years-period oscillation. This is explicitly discussed by A. Mazzarella, N. Scafetta, Theor. Appl. Climatol, DOI 10.1007/s00704-011-0499-4, and by C. Loehle and N. Scafetta, Open Atmospheric Science Journal 5 (2011) 74. This 60 years-period sinusoid is not a surprise. Several previous papers noted a similar oscillation with a similar period in various proxies e.g. length of the day, AMO, PDO, ENSO, JISAO indices, and even in fishing productivity, see Klyashtorin, L.B. and A.A. Lyubushin, Cyclic Climatic Change and Fish Productivity, Ed. G.D. Sharp, VNIRO, Moscow (2007). See also Swanson K.L., Tsonis A.A., Geophys. Res. Lett. 36 (2009) LO6711, Schlesinger, M.E., N. Ramankutty, Nature 367 (1994) 723, Loehle, C., Ecological modelling 171 (2004) 433, Zhen Shan, L., Sun Xian, Meteorol Atmos. Phys. 95 (2007) 115. This short list is not exhaustive. [François GERVAIS, France]	The 60 year oscillation hypothesis is not ignored but is assessed in Chapter 10 FOD (section 10.3.1.1.3) including citation of Loehle and Scafetta (note that the name was misspelled in FOD citation which has been corrected in the SOD). Further citations have been added to support the Chapter 10 assessment.
0-36	0	0				5 - Akasofu (Natural Science 2 (2010) 1211), founder of Arctic Research Center, parenthetically author of more than 500 research papers which were cited more than 13,000 times according to ISI web of knowledge, suggested that the global temperatures for at least two centuries seem to fit a global increase that might have been initiated at the end of the little ice age added to a sinusoid the period of which is consistent with the one reported by Scafetta, by authors cited hereon, and by others. Again, why is Akasofu's work not cited, quoted and discussed ? Is the oscillatory component with the period of 60 years not well visible as the grey part of Box 2.2 Fig.1 Bottom, on page 2-127 of the AR5 draft ? The 60 years oscillation is also seen in Fig. 2.36, on page 2-165. [François GERVAIS, France]	Noted. The 60 year oscillation hypothesis is not ignored but is assessed in Chapter 10 FOD (section 10.3.1.1.3) including citation of Loehle and Scafetta (note that the name was misspelled in FOD citation which has been corrected in the SOD). Further citations have been added to support the Chapter 10 assessment.
0-37	0	0				6 - Why is the 60 years-period oscillation ignored when it is so straightforwardly visible ? It is also easily seen that the slope of the ascending part of the sinusoid which fits the decadal variability noted by the authors cited above, and observed in the figures of the AR5 draft noted above, was maximum in the eighties/nineties. If the climate models have been fitted or calibrated (as is suggested by the ingredient "parameter adjustment" in Fig.	Noted. The 60 year oscillation hypothesis is not ignored but is assessed in Chapter 10 FOD (section 10.3.1.1.3) including citation of Loehle and Scafetta (note that the name was misspelled in FOD citation

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						9.1 and "model tuning" in the text) to this ascending part of a sinusoid, their projection to the future would be obviously overestimated. [François GERVAIS, France]	which has been corrected in the SOD). Further citations have been added to support the Chapter 10 assessment. Furthermore, Chapter 9 has revised the text on model tuning in order to clarify the meaning of "parameter adjustment"; as a consequence, Figure 9.1 has been deleted for the SOD.
0-38	0	0				7 - In the AR5 draft, several paragraphs heavily insist on the warming just observed in the eighties/nineties period, whereas the latest period 2005-2010, when explicitly documented in the AR5 draft, indicates a net change of regime with warming indicators decelerating (see details noted in the other parts of this reviewer's report). The last decade just corresponds with the top of the 60 years sinusoid. Earth temperature and other indicators consistently did not display further significant increase during the last decade. The observations of the next fifteen years will be of considerable interest from this point of view. Ignoring published papers dealing with decadal climate oscillations, in particular the ~ 60 years one which shows the largest amplitude, will aliment suspicion of "cherry picking". [François GERVAIS, France]	Noted. Chapter 2 will be discussing the very recent short trend in more detail in the SOD. However, we note again here that the 60 year oscillation hypothesis was not ignored in the FOD, but in fact assessed in Chapter 10 FOD (section 10.3.1.1.3) including citation of Loehle and Scafetta (note name was misspelled in FOD citation which has been corrected in the SOD). Further citations have been added as part of the revisions of the SOD to support the Chapter 10 assessment. In addition, we also note that the temperature variability over recent years and the assessment of its causes was not ignored either but in fact was assessed in Chapter 10 FOD, including a section on the evolution of global temperature over the last decade (10.3.1.1)
0-39	0	0				8 - Another global weakness of the AR5 draft is that the basic mechanism of the atmospheric greenhouse effect — central in the report — is not documented. If a report aims to be convincing, it has at least to recall the basic physics. The report has to explain how an adiabatic system that receives the same quantity of heat by radiation from the sun, and if it shows a roughly constant albedo, could warm on the long term. In the absence of cited paper demonstrating the physics of the atmospheric greenhouse effect, is it not the task of WGI ? Atmosphere with spectral windows which are transparent to the infrared electromagnetic radiation emitted by the earth, and made of convective fluid instead of a solid glass window, is not expected to behave like a conventional greenhouse, or a car parked in the sun. [François GERVAIS, France]	Rejected -- AR5 is building upon earlier IPCC assessment reports, where for example, Chapter 1 in the SAR, TAR and AR4 do introduce the greenhouse effect. Please note, however, that IPCC reports are not meant to replace science text books on, e.g., fundamental physics of the climate system. Nevertheless, Chapter 1 of AR5 FOD does discuss, e.g., the greenhouse effect, but certainly not in the detail desired by the reviewer.
0-40	0	0				9 - Chapter 9, Page 7 lines 25-26 recognize that there is no set of equations that describes the global climate. At least the equations related to the greenhouse effect have to be stated explicitly. This is essential because if the AR4 and previous reports would have been convincing enough, why would there be a need to publish the AR5 ? In the climate models, earth seems to be treated like a blackbody. A blackbody implies emissivity = absorptivity (Kirchhoff law), and reflectivity is zero. But earth albedo = 0.3. Reflectivity is a component of albedo. Albedo is partly related to clouds, themselves related to water vapor, itself the gas giving rise to the main greenhouse effect in the atmosphere. The physics, therefore, has to be clarified in this mess, before applying Stefan-Boltzmann and Planck's laws. [François GERVAIS, France]	Rejected -- AR5 is building upon earlier IPCC assessment reports, where for example, Chapter 1 in the SAR, TAR and AR4 do introduce the greenhouse effect. Please note, however, that IPCC reports are not meant to replace science text books on, e.g., fundamental physics of the climate system.
0-41	0	0				10 - The comments below are restricted to CO2 but may apply to other greenhouse gases too. Radiative transfer is considered in climate models. In vacuum, a molecule cannot transfer energy by thermal conduction or convection like in air. It can transfer energy by radiative process only. According to Kirchhoff law, a CO2 molecule in vacuum emits radiation at frequencies where it absorbs, viz. mainly the stretching mode at 4.3 micrometers, and the bending mode at 15 micrometers of wavelength. A situation which is entirely different is encountered in condensed matter. If one heats a solid with a monochromatic radiation, for example with a CO2 laser at 10.6 micrometers, the solid emits according to the blackbody profile function multiplied by its proper spectral emissivity depending on its own reflectivity and its own absorptivity. The solid does not emit at the sole wavelength of 10.6 micrometers corresponding to its heating. [François GERVAIS, France]	The reviewers suggestion/request is not clear. No action.
0-42	0	0				11 - In condensed matter at room temperature, the transfer of heat by emission is found negligible compared to the thermal conduction by acoustic phonons. Atmosphere is not void but it is not condensed matter either. It lies in between. Where exactly ? Atmosphere contains molecules. The molecules dissipate absorbed heat possibly by reemission but also mainly by thermal conduction (related to collisions with neighbor molecules,	The reviewers suggestion/request is not clear. No action.

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						mainly N2 and O2) and also by convection. Most of the molecules of air like O2 or N2 do not emit anything in the infrared because they are transparent in this wavelength range, at least to a first order of approximation. They are heated by collisions with greenhouse gas molecules via thermal conduction and convection processes. [François GERVAIS, France]	
0-43	0	0				12 - In what proportions CO2 molecules also reemit radiation instead of dissipating their absorbed heat via collision processes, presumably depends on pressure and, therefore, altitude. What are the proportions of radiative, thermal conductive and convective ingredients retained in the models, depending on altitude ? This is a central question which is not or insufficiently addressed in the report, at least compared to the many details developed in other chapters. [François GERVAIS, France]	Taken into account - the relevant text in Chapter 8 has been revised, including further description that detailed longwave radiation schemes (which include CO2) compare well to observed radiative fluxes. Chapter 8 mentions that radiation schemes used in GCMs have a larger uncertainty, but still reproduce the observed longwave fluxes within some few percent. Reference to the recent paper by Oreoloulos et al., JGR (2012) is included. This good agreement indicates that treatment of important radiative processes including pressure dependence for CO2 has been taken sufficiently into account.
0-44	0	0				13 - A CO2 laser beam, for example, travels in air without being absorbed. As it is not an absorber at 10.6 micrometers, air is not an emitter at the CO2 laser infrared wavelength. Air, therefore, does NOT behave like a blackbody. Air heated at ~ 600°C consistently does not emit red radiation contrary to what is expected for a blackbody at this temperature and contrary to what is observed by the human eye for condensed matter like coal or wood or ceramics heated at this temperature. To apply Planck and Stefan-Boltzmann equations to air, therefore, would be incorrect. [François GERVAIS, France]	Rejected - atmospheric radiative transfer is well documented in text books and application of theory compares well with observations radiative fluxes.
0-45	0	0				14 - Do the models, or some models, treat atmosphere like vacuum by neglecting thermal conduction of air and convection too ? Or is air treated as fluid matter and then what is the fraction of heat transferred by thermal conduction and what is the other fraction transferred by convection retained in the models compared to the fraction transferred by thermal emission, depending on altitude and pressure ? This question is central because if thermal transfers are considered as purely radiative, neglecting thermal conduction and convection, the role of greenhouse effect and associated radiative forcing could be strongly overestimated. Although essential, the question is not addressed in the AR5 draft. [François GERVAIS, France]	Rejected - see also response to comment 0-44; Global climate models do represent diffusion of heat and convection, in addition to radiative transfer. This is described in various published papers and text books. The task for WGI AR5 is to provide a comprehensive, scientifically sound and robust up-to-date assessment of the physical science basis of climate change, not to proved a historical review.
0-46	0	0				15 - To further address the question and at least partly try answering it, I suggest including in the report and discussing the infrared emission spectrum of the earth measured by a satellite in clear sky conditions. What shows for example Fig. 6.6 of the review book of G. Petty, A first course in atmospheric radiation, sundog Publishing Co. (2006), is something for the earth radiance measured through the atmosphere, which roughly resembles the black body (Planck's law) profile centered at the temperature of ~ 255 K, as expected, but with a deep "hole" in the emission spectrum at the blackbody maximum. [François GERVAIS, France]	Noted.
0-47	0	0				16 - The hole addressed in comment 15, the depth of which is ~ 3/4 of the Planck's radiance curve value, is centered at 15 micrometers, viz. precisely at the frequency of the CO2 vibration mode which is the most relevant for anthropogenic greenhouse effect. This result suggests that atmospheric CO2 does absorb emission of the earth at 15 micrometers as everybody knows, but also that, on an average, only ~ 1/4 of the heat absorbed by the molecules is reemitted and ~ 3/4 is dissipated by collision processes. This is not a model calculation or a projection, it is an experimental result. [François GERVAIS, France]	The reviewers suggestion/request is not clear. No action.
0-48	0	0				17 - The hole addressed in comments 15 and 16 in the infrared radiance spectrum at 15 micrometers of wavelength is a key point forgotten in the AR5 draft and that I recommend discussing. If CO2 molecules which lie well above this opaque coating of ~ 10 meters-thick above ground or sea level which cuts all radiation emitted by the earth at this wavelength, indeed do no longer "see" the radiation emitted by the earth, or see it strongly attenuated, they cannot heat, or they heat less, and they do not contribute, or they contribute less, therefore, to the greenhouse effect. This "attenuated" greenhouse effect might concern the major part of CO2 molecules in air, since, in spite of the fact that CO2 is more dense than air, most of them presumably lie above the altitude of 10 meters. The picture presented here is simplified of course. But it captures an essential	Noted.

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						ingredient which seems to be missing in the radiative transfer models of climate. [François GERVAIS, France]	
0-49	0	0				18 - Ignoring the "attenuation" of the CO2 greenhouse effect detailed above might be the main reason why the "hot spots" predicted by GCM models are not observed experimentally under the tropics. This might be also the main reason why the warming of the Antarctic pole predicted by the GCM models is not observed experimentally. In other words, the greenhouse effect of CO2 seems to be not far from saturation. This is consistent with the fact that CO2 (and methane) concentration follows temperature changes, instead of preceding them, as is observed in ice core data. This picture, "not far from saturation" or "attenuated CO2 greenhouse effect", is not considered in the GCM models but it derives straightforwardly from the inspection of the infrared spectrum emitted by the earth through the atmosphere. At least, it has to be considered in the report and discussed. [François GERVAIS, France]	Rejected - the connection to upper-tropospheric temperature trends is unsubstantiated. Models and observations agree that warming is small to nonexistent around Antarctica.
0-50	0	0				19 - The discussion of ingredients and inputs included in the climate models are of central importance since, as stated in Chapter 9, climate models are the primary tools for making projections of future climate. Physical ingredients, approximations and inputs are neither presented nor discussed in AR5 draft, or at least insufficiently. It is not sufficient to show that a model "works" to some extent, after some "model tuning" (Chapter 9 Page 7 Line 18) and having made ad hoc adjustments, by comparison with data themselves with large error bars. The procedure does not provide a proof. The discussion of the basic physical ingredients outlined in this introduction, and how they are selected in the models, seems of central importance, and would deserve in the AR5 report an entire chapter, or an Annex, or at least a long paragraph, if the aim and goal of the report is to become convincing. [François GERVAIS, France]	Taken into account -- text on tuning of models in Chapter 9 has been revised somewhat. However, we note that an IPCC Assessment is NOT a review of the science underlying climate models. The role of Chapter 9 is specifically to evaluate climate models.
0-51	0	0				20 - The aim and goal of the AR5 report are to try to confirm scientifically that anthropogenic emissions of CO2 would be the main cause of global warming, or not. I regret to admit that in its present form, the AR5 draft the conclusions of which are based on models which are not substantiated from the point of view of arguments recalled hereon, is not convincing to this physicist expert reviewer. The reasons of my strong reservations will be further detailed in what follows. [François GERVAIS, France]	Reviewers strong reservations are noted.
0-52	0	0				Somewhere in the report there needs to be a section which examines the consistency between observed and projected trends. It is useful to point out where we are already seeing the signs of some of the changes that are being projected and equally interesting to look at and try and understand any inconsistencies between observations and projections. Perhaps this is best placed in Chapter 12 as the observations will already have been discussed earlier in the report. [David Griggs, Australia]	Agreed, but consistency between observed and projected changes (incl. trends) is in fact already covered in Chapters 9,10,11,13 of the WGI AR5 FOD. In particular, Chapter 10 deals with comparison between observed and modeled trends and also has a subsection drawing out the implications for climate system properties and projections. Chapter 11 FOD, in Figure 11.11 (b), shows the projected range of global mean surface air temperature change derived using an approach in which the fit between observations and model simulations of the past is used to scale projections of the future (after Allen et al, 2000; Stott and Kettleborough, 2002). Finally, Chapter 13 FOD does specifically address this topic for sea level in its section 13.4.7.
0-53	0	0				Overall this is an excellent report and the authors as always have done an outstanding job. One point that needs to be continually borne in mind is that this report is an assessment by the Lead Authors, not a review of the literature. In many places the authors have done this well but in others they have fallen into the temptation to simply review the literature and summarise it rather than make an essessment based on this information. [David Griggs, Australia]	Agree - transition from literature review to an assessment will be further developed by all Chapters when preparing the SOD.
0-54	0	0				The report as a whole has very little on stratosphere-troposphere coupling. It may be that IPCC scientists view this as of marginal interest but, given that WCRP has a very active project dedicated to Stratospheric Processes and their Role in Climate, it might be expected to have some sort of profile. There are short relevant sections in 9.5.3.2 and 14.2.9.3 on annular modes but there have been so many advances recently in more general understanding of strat-trop coupling that I would have thought a short section devoted to it, within section 10.3 which "assesses causes of change in the atmopshere...", would be useful. [Joanna Haigh,	Noted. As correctly pointed out by the reviewer, the topic of stratosphere-troposphere coupling and it's link to climate change is covered to some extent in Chapters 9 and 14. Chapter 9 and 14 are both considering expanding the existing assessment in the SOD. But as far as projections are concerned, there

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						UK]	currently is little material to assess and in the robustness of existing results still has to be assessed in the light of CMIP5 results emerging. is considering. In addition to Chapters 9 and 14, Chapter 10 FOD does consider stratospheric trends and processes relevant to attribution of changes, but the chapter does need to stick to the brief of assessing the causes of changes and therefore only discusses stratospheric processes where they are relevant. Finally, we do appreciate the substantial progress that has been made in this specific area of research, but the IPCC WGI AR5 is not, and cannot be a comprehensive scientific review.
0-55	0	0				Confusing split of geoengineering between Chapters 6 & 7. The inclusion of the FAQ7.3 Fig.1 overview of CDR techniques in Ch.7 seems particularly anomalous, given review in section 6.5.1. [Joanna Haigh, UK]	The rationale of this split is that a specific method of geoengineering is assessed in the chapter relevant to the dominant physical process of that method. Chapter 6 on "Carbon and other Biogeochemical Cycles" is dealing with the physical science basis of CO2 removal techniques, while Chapter 7 on "Clouds and Aerosols" is dealing with the physical science basis of (solar) radiation management techniques. The FAQ on geoengineering then combines the information from both Chapters in Chapter 7.
0-56	0	0				The Ministry of Land and Rsources, P.R.China started to launch a series of research projects about geological records of paleoclimate changes in 2010. The geological records inlcude stalagmite in karst caves, salt lake deposits in northerwest China, peat deposit in Qinghai-Tibet plateau, loess in northern China, etc. The first phase of the projects will be finished by the end of 2012. We are willing to provide the reserch results to the working group for reference of Chapter 5. [aibing Hao, China]	Noted, thanks
0-57	0	0				Many chapters, in particular 2 and 10 refer to changes in streamflow and runoff inlcuding floods. However, the text does not significanntly reflect the hydrological research on the topic during the last 5 years. If hydrology is to be covered by WGI there is an urgent need to summarise findings both related to observed changes in streamflow including floods and the research output related to expected changes in streamflow and floods. There has also been numerous studies published in the scientific litterature on hydrological drought. It should therefore be stressed in the introduction if only meteorological drought is to be discussed in the report. [Hege Hisdal, Norway]	Noted. Chapter 2 will consider adding more references, especially the references provided by the reviewer in another comment from the reviewer (#0-58); Chapter 10, however, assesses the causes of observed changes and therefore only assesses literature that relates to the attribution of changes in streamflow, floods and drought to external forcings on climate.
0-58	0	0				In general inforamtion about regioal variability in river runoff is very limited compared to what is said about trends in temperature and precipitation although recent publications cover the topic (A) for Europe: Stahl, K., Hisdal, H., Hannaford, J., Tallaksen, L. M., van Lanen, H. A. J., Sauquet, E., Demuth, S., Fendekova, M., and Jódar, J. (2010) Streamflow trends in Europe: evidence from a dataset of near-natural catchments, Hydrol. Earth Syst. Sci., 14, 2367-2382, doi:10.5194/hessd-14-2367-2010. for Northern-Europe (the Nordic countries): Wilson, D., Hisdal, H., Lawrence, D. (2010) Has streamflow changed in the Nordic countries? – Recent trends and comparisons to hydrological projections. Journal of Hydrology, 394, 334-346. In the latter paper the observed changes are compared to hydrological projections for the future (B) for the the USA e.g.: Krakauer, N. Y. and Fung, I.: Mapping and attribution of change in streamflow in the coterminous United States, Hydrol. Earth Syst. Sci., 12, 1111–1120, 2008.) [Hege Hisdal, Norway]	Noted. Several Chapters will consider the possibility to expand coverage of river runoff using additional references on observed and projected changes, including those references provided by the reviewer. For projections, however, it will need to be seen to what extent available climate change projections and corresponding references explicitly address river runoff and to what extent those will allow the WGI author teams to provide robust assessment conclusions.
0-59	0	0				The whole AR5 report is basically a "Quasi Global" assessment report. This is clearly shown in the maps used in the report. Ca 96% of the reported maps, are not "global", as such (as Mercator Projections, they lack aobut 8% of the global surface, e.g., from 60 - 90 degrees N and S. The most interestiing regions, for climate, i.e., the polar regions, between 60º – 90º N and S, are not illustrated at all. This is because the Mercator projection is used more or less throughout the whole report. There are only 4 out of 100 maps that show the circumpolar	Rejected -- We don't agree with the reviewers interpretation of the map projections and the claim that the assessment presented basically is "Quasi Global" because the Polar Regions are not illustrated at all. We refer the reviewer to Annex 1 of the WGI

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						projections in addition to the normal Mercator projections (valid from 60º – 90º N and S). Furthermore, the Quasi Global nature of the AR5 draft report is strengthened by the fact that the text mainly refers to the zone between 60º – 90º N and S. Therefore, it should clearly be stated in the introduction, that this is actually a Quasi Global assessment, as the polar regions (60º – 90º N and S) defy measurements and representation, due to lacking data and also lacking technology for display. The Editors should seriosly consider updating the whole report to make it more up-to date and make it at least "seem" like a Global assessment. This could be done in two ways: either by plotting all maps on the Molweide true area projection, and/or to use Google Earth to illustrate all data, displaying them in the true hemispherical fashion, using three to four images per map. This could really boost the report. [Martin Hovland, Norway]	contribution which specifically includes polar projections. Then the reviewer correctly points out that data coverage is not equally distributed over the globe, but we think this fact is appropriately acknowledged and discussed in the Chapters. Finally, we don't understand the comment about the "lacking technology for display" and would again refer the reviewer to Annex 1.
0-60	0	0				I congratulate all Lead Authors, Contributing Authors and Editors for their efforts on this valuable report. I have explained my specific comments on my report. Please find the attched repor. [SELAHATTIN INCECIK, TURKEY]	Noted with thanks.
0-61	0	0				The word “global” is commonly used with 2 effectively opposite meanings: “global-mean” or “globally distributed”. IPCC should be careful to avoid this ambiguity. It is not. For example, the Executive Summary of Chapter 2 first uses the word explicitly in the first sense “Globally averaged” (2-3 l 5). 2 line on (2-3 l 7) it uses “global” alone, but the context makes it clear it means “covering the variation over the globe”. The reader must therefore assume the next use of the word, another 2 lines on (2-3 l 10) means the same, but I'm fairly sure it wouldn't be true & the opposite meaning is meant. Then 2-3 l 17 uses the explicit “global average” again, but 2-3 l 21 uses a phrase that sounds like the opposite, but the precise numbers later indicate the mean must again be meant. The 2-3 l 26 is completely unambiguous - has the troposphere warmed on average over the globe, or everywhere over the globe? That is less than half of one page, but the problem is just as bad elsewhere. (I can't say everywhere, as I have read far less of the report than I had hoped to, but I have seen nowhere where the word is used clearly.) [William Ingram, UK]	Agreed. This ambiguity in the usage of "global" ("global mean" vs. "globally distributed") should be avoided. This will be considered in the SOD development where applicable.
0-62	0	0				I would like to congratulate all the authors for the overall quality of the FOD. [Gareth S Jones, UK]	Noted with thanks
0-63	0	0				I have concentrated my review on Chapter 6 [Per Erik Karlsson, Sweden]	Noted
0-64	0	0				I've noted two examples of phenomena starting in 1950 that are attributed to anthropogenic causes, and two more cases are noted below. In all four cases, time series records of the same, or similar, data is available from prior to 1950, and would give a different impression if included. 1950 is the end of a warm period in North America (due partly to a PDO shift in 1947) and the start of a cold period than ended 30 years later (when the PDO shifted to warm). Thus, the selection of 1950 as a starting time biases subsequent observations towards a "warming" signal that would not appear if the longer time series were used. Much of this "warming" signal since 1950 is due to decadal oceanic cycles (PDO), but since the models do not replicate the PDO and therefore can find no natural cause for the warming, the report presumes that the change must be anthropogenic. [Richard Keen, USA]	Chapter 10 on "Detection and Attribution: from Global to Regional" considers a range of timescales, the starting point for many of which are determined by the start of reliable observational datasets with sufficient coverage to make meaningful assessments. Instrumental near surface temperature records start in the mid 19th century but in addition to considering changes over the century timescale Chapter 10 also assesses changes since 1951 since this is a period when there is better data coverage than earlier and uncertainties in forcings are better understood.
0-65	0	0				All science is based on theory. Any report on the state of a science, such as is being done here with climate science, should provide a theoretical context for the science being discussed. This report is severely lacking in any examination and discussion of the theories and assumptions upon which the scientific findings are based.A search on the keyword “theory” reveals only a handful of results. In Chapter 1, pages 2, 3, and 4 all make reference to “theory” being a central part in the analysis of the climate system. However, there is no reference to an explicit description of the theory or set of theories that scientists follow to understand the climate system. Other parts of the report mention “theory” only with regards to a particular aspect of the climate system, such as black body radiation theory (Chapter 1), extreme value theory (Chapters 2 & 12), Milankovitch theory (Chapter 5), nucleation theory (Chapter 7), and the theory of marine ice sheet instability (Chapter 13), and so on. For the most part scientists studying the global climate system have no explicit theory of the global system. In this approach the climate system is founded in a plethora of physical and chemical laws that are used to formulate the equations in the climate models. The general theory of how the climate system operates, then, is implicit in the climate models. In other words, there is no real distinction between the climate models and the theory of the climate system. Thus, it would be fair to state this. It is also relevant to	IPCC AR5 is building upon earlier IPCC assessment reports. IPCC reports are not meant to replace science text books on, e.g., fundamental physics of the climate system.

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						mention that conventional climate models assume the earth does not behave as a living system. [Lee Klinger, USA]	
0-66	0	0				Expanding on the lack of theoretical treatment in this report, there is a general theory of the earth system that has been described and researched in the scientific literature that has received no mention here. Gaia theory is a well-known concept in the climate science community. It is the theory that the earth is a self-organizing, self-regulating complex system. Gaia theory, and related models describing complex systems, have received no mention in this report. This appears to me to be a glaring omission in a document portrayed as a "comprehensive" assessment of global climate change. Gaia theory is a valid global system theory that has profound implications for climate change. Both Gaia theory and complex systems theory deserve some treatment and mention here. [Lee Klinger, USA]	IPCC reports are not meant to replace science text books, and they are an assessment of the available scientific evidence. If there is robust scientific evidence for the self-regulation of the climate system, this will be part of the assessment.
0-67	0	0				This assessment report correctly notes that expert judgement is needed, and that experts tend to be overconfident (Chapter 1, page 14, line 4), but it does not take this into account when formulating confidence in models and data . This is particularly relevant for attribution and projections, two issues that are of paramount importance to policy makers. The usefulness of the report would be strengthened if its confidence and uncertainty assessments were based on broader, well documented expert panels, including sceptical scientists. [Gerbrand KOMEN, Netherlands]	Rejected - Assessment is fundamentally based on evidence and agreement as reported in the scientific literature. The assessment of uncertainty can include an element of expert judgment of the author team, selected for their expertise in the relevant topics.
0-68	0	0				The IPCC AR5 Report is very interesting and useful. Updated information on climate change and its impacts is necessary for people all over the world to understand and realize the changing environment so that they are able to search for the best way to mitigate and cope up with the climate change and associated adverse impacts. [Jiemjai kreasuwun, Thailand]	Noted, thanks.
0-69	0	0				The text says woefully little about the existence and potential role of external forcing in climate change. While external forcings may be beyond the responsibility of the IPCC in assessing climate change, their existence should be discussed, at least, on a qualitative level. Equally important is to discuss the potential uncertainty that might be introduced in predictions of future climate by external forcings. Would one volcanic eruption of the size we have experienced in the past render the current predictions totally unreliable? Would a minor increase in the solar constant? Would other external forcings? Such concerns were voiced in AR4. They should, at least, be mentioned in AR5. [Julian Levy, United States of America]	Rejected - External climate forcings such as solar and volcanic forcings, are comprehensively assessed and explicitly addressed in multiple chapters of the WGI AR5 FOD, e.g., Chapters 2, 5, 8, 10, and 11.
0-70	0	0				There is no mention of the possibility that anything more than a small perturbation to the climate could alter climatic processes as we currently understand them. It should be mentioned that we assume that climatic processes, as we understand them today, will continue in the future--even if there are changes in emissions and climatic variables. As a corollary, the AR5 should mention that if those processes are altered, the conclusions of AR5 could be invalidated. [Julian Levy, United States of America]	Rejected -- the claim that the conclusions of AR5 could be invalidated if climate processes "as we understand them today" will be altered is unsupported. The "small perturbation" claim is in contradiction with quantitative evidence assessed from the paleorecords in Chapter 5.
0-71	0	0				The AR5 FOD, as presented, is incomplete. The biggest issues to arise out of AR4 resulted because each chapter was summarized; the report, as a whole, was summarized; and, then, that summary was further summarized and simplified for policymakers and the press. The Executive Summary for policymakers was so simplified that, in some ways, it led readers to certainties that were not justified if one were to read the full report. Without inclusion of those summaries, it is impossible to give a complete assessment of the AR5. I hope that the SOD will include the text, figures, annexes, AND summaries. [Julian Levy, United States of America]	The WGI AR5 First Order Drafts of the Technical summary and Summary for Policymakers will be available at the stage of the SOD expert/government review.
0-72	0	0				I guess this is the job of a copy editor later, but there are many errors in English syntax throughout the text, and a few spelling issues. [Brent Lofgren, USA]	Agreed - copy editing.
0-73	0	0				Currently, long parts of the chapters I have read (2,9) are listing references without putting them into context. I would encourage the authors to add more cross references between chapters, to group references in a sensible way, to compare references, show connections, agreement or disagreement, and to comment on the results. [Douglas Maraun, Germany]	Agreed - transition from literature review to an assessment will be further developed by all Chapters when preparing the SOD, including improved cross chapter consistency and referencing.
0-74	0	0				At the outset, the authors deserve to be congratulated to have done a fairly good synthesis of the published results into coherent chapter. This chapter, as it evolves through the remaining rounds of revision would, no	Noted with thanks.

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						doubt, become a reference document for scientists and policymakers. It is possible that when such large-scale synthesis is made, some of the recent papers would have skipped the attention/ or might not have been referred. This is a serious lacuna for this otherwise good work. [K KRISHNA MOORTHY, INDIA]	
0-75	0	0				It is explicit that this report focuses the post-AR4 scenario. The main drawback of AR4 was inadequate regional representation of aerosol and cloud characteristics, especially from some of the global hotspots such as Asia and Africa. Subsequent to AR4, there has been a tremendous thrust on regional characterization of aerosols and clouds over India, China, E-Asia and Africa through network observations, field campaigns, aircraft, balloons, and satellites. A large number of high quality papers have emerged on aerosols, their optical, chemical and microphysical properties, extensive measurements of BC over India and China, even some limited trends, vertical profiling using balloons and aircrafts, multi-platform field experiments and so on. There have been very new findings on BC layers in the free troposphere changing the stability and raising questions as 'do BC make their homes?' There have been papers on self-lofting of BC to stratosphere. Measurements of BC over snow-covered Himalayas, new particle formation from pre-cursors, large absorption of Asian dust, distinct source impacts and long-range transport are documented. [K KRISHNA MOORTHY, INDIA]	The SOD will present more information on cloud climatology, mostly from satellite instruments, which have a global coverage, in particular in the dedicated Chapter on "Clouds and Aerosols". The chapter 7 author team appreciates that there is a wealth of aerosol measurements now available over Asia and Africa. Figure 7.9 (as numbered in the FOD) is an attempt to synthesize the information available.
0-76	0	0				Unfortunately, the report does not cover those, but still focuses mostly on the more accurately characterized regions of Europe and America, where the information were more accurate even at AR4 timeframe. This deficiency in the literature coverage, assessment strategy and executive summary stands out conspicuously. I strongly feel that if this is accounted for, the AR5 report would become a much more authentic document. It is hoped that the next round of revisions would take care of this [K KRISHNA MOORTHY, INDIA]	As mentioned in the reply to the general FOD review comment #76, Figure 7.9 is an attempt to synthesize aerosol information available from observations including those from Africa and Asia and refers to the available literature.
0-77	0	0				For the review process, might I suggest in future an accommodation for those with physical challenges in interfacing with Excel. This program interfaces poorly with voice recognition software such as Dragon NaturallySpeaking, creating substantial challenges for those of us who depend entirely on voice recognition for typing. [J. David Neelin, United States]	Comment has been passed on to our IT specialists for consideration.
0-78	0	0				A list (look up table) of all acronyms used in each chapter would be very helpful. [Christian Ohneiser, France]	Agreed - A list of acronyms will be included in the published report.
0-79	0	0				Careful proof read and spell check is needed. [Christian Ohneiser, France]	Noted - copy editing.
0-80	0	0				Box 3.1 from Chaper 3 and Box 13.1 from Chapter 13 seem to be very similar. The authors should consider whether both boxes are necessary and avoid repetition if possible. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Noted -- However, the boxes mentioned provide distinct information and serve different purposes and thus both will remain in the SOD. While Box 3.1 focus lies on where on earth the additional energy is stored, Box 13.1 focus is on the energy balance. Energy storage is a critically important observation, thus it's coverage in Chapter 3, but it is only is about 20% of what is covered in Box 13.1, which directly draws upon the results from Box 3.1 and elsewhere to address issues related to Sea Level Changes.
0-81	0	0				I consider that the IPCC AR5-WGI is in general an excellent report that summarize in detail the results obtained by the climate change scientific community the last years. Also, it introduces new results and conclusions. In relation to the items of the different Chapters that I analyzed in detail, in general, they were very well written. In what follows, I make suggestions about the possible improvement of AR5-WGI. [Rubén D Piacentini, Argentina]	Noted, thanks.
0-82	0	0				A general indication must be given if the uncertainty (ε) associated with a measured value (X), in the form X ± ε, is considered within one or two standard deviations (or in another form). For example: “The uncertainty of a physical/chemical 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 (rigth 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 substracted, in order to avoid the appearence of a new source of error (uncertainty). [Rubén D Piacentini, Argentina]	Agreed - homogenization and cross-chapter consistency will be improved at the SOD stage.

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0-83	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. [Rubén D Piacentini, Argentina]	Agreed - this will be a focus of the SOD development.
0-84	0	0				Please, try to make the different images with the same (high) quality. For example, now it is possible to make an excellent 3D representation of mountains, oceans, lands, etc. In some of them the quality is very good but in others this is not the case. Also, the words in the inner part of some figures are ussually too small. For example, in Fig 1.1: "aerosol", "absorbed by Earth´s surface", etc. [Rubén D Piacentini, Argentina]	Noted, thanks. Figure development is ongoing for the SOD.
0-85	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 (AII) in the first column. This column is only devoted to General (0),Chapter numbers (1, 2,….., 14) or Atlas. [Rubén D Piacentini, Argentina]	Noted. All Annex II related comments have been shared with contact person of the Annex II Editorial Team
0-86	0	0				Annex II, page 9, line 11: About “Table AII.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 per the formally listed emissions in the RCP distribution files, but we will round off to two places to avoid such a minor problem with the RCPs.
0-87	0	0				Annex II, page 15, line 7: About “Table AII.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]	Yes, this table will be rooted in Chapter 6 or else it will be dropped.
0-88	0	0				Annex II, page 17, line 11: About “Table AII.4.7: C6F14 abundance (ppt)”. The same comment as for AII-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]	No, this is not a typo. Dropping unnecessary precision will be considered for the SOD.
0-89	0	0				The huge seize of the report is noted (1633 pages). This seize reflects the amount of the published material, but also the number of chapters. It will be a significant challenge to avoid any inconsistences and mistakes in such large report. Quality has to be the top priority in order to support consolidating the reputation of the IPCC. [Klaus Radunsky, Austria]	Agreed -- WGI's top priority is quality, which must be based on scientific rigor, robustness, comprehensiveness, and transparency in the assessment.
0-90	0	0				Given the quite technical nature of some of the chapters it is suggested to develop a comprehensive glossary for the next version in order to improve the understanding of the report. [Klaus Radunsky, Austria]	Glossary will be further developed for the SOD.
0-91	0	0				The first paragraph of the Execituve Sumnmary includes the following very policy relevant message (modified): The scientific knowledge derived from observations, theoretical evidence and modelling studies has continued to increase and to further strengthen (the conclusion that) human activities are the primary driver of climate change (observed in the past 40 years). There will be a need to explicitly link that statement to the main findings of the individual chapters and to provide an explanation on such link, especially if such link is not so obvious for policy makers but only for specialists. Such statement needs further qualification because there are also findings that could not confirm some of the conclusions from the AR4. The SPM might need to include an overview of findings that strengthen this statement and that weaken former statements and why overall the authors came to the above conclusion. [Klaus Radunsky, Austria]	Taken into account - This comment refers to the Chapter 1 Executive Summary. In fact, Chapter 1 does make that statement based on the discussion in the chapter and in other WGI chapters.
0-92	0	0				The inclusion of FAQs is very much apprecaited and it is noted that those questions are usually policy relevant. However, it is noted that the explanations provided are not explicitly linked to the material assessed in the underlying chapters. It is recommended to further improve the treatment of FQAs and to explain/justify the conclusion/statement of the authors to the extent feasible. This is in particular relevant in cases where different conclusions have been published in the literature (e.g. the relevance of natural climate variability versus forcing by changes in GHG concentrations in the atmosphere). [Klaus Radunsky, Austria]	Noted - the development of the FAQs is ongoing for the SOD.
0-93	0	0				Congraulation and thanks for a such a great and excellent report. It has been improved very much respect to privioues version. [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Noted, thanks.
0-94	0	0				I have problem with different periods for calculating the anomalies. Of course there are some difficulties for the availability of data for different climatic parameter and also different resources for measuring them. But you	Noted. To the extent that is scientifically sound, homogenization of the reference periods within and

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						see in Section 2 anomalies that was calculated to 1961-90 and found another period in the other chapter. I suppose it should be mentioned in general and for the whole of the country. [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	across WGI Chapters is a focus of the SOD development.
0-95	0	0				There are many text in the whole of the report that was discussed the results of the AR4 and developments since the IPCC Fourth Assessment Report (AR4). I suggest instead of many text, in each chapter one or two tables based on the subjectsmay be written. It may be a kind reference for comparing the result of AR4 and what has received after AR4. [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	The presentation of the AR4 - AR5 comparisons is for the individual Chapters to consider.
0-96	0	0				In general, the whole of country need to revised for using similar words, gargon, ….. [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Glossary and list of acronyms will be further developed for the SOD. A list of acronyms will be included in the published report.
0-97	0	0				The quality of some figures are not well. Some of them are very complex and need to be modified. In my views some of them are same as commercial figures and are not scintific. And some of them are very simple and they have repeated many times in prievioues assessment reports. [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Noted -- Figure development is ongoing for the SOD.
0-98	0	0				I recoomed to lead authors to try all the refreces that relates to their region and the other region. It seems in some cases the lead author try to bring the refrences that belong to their region. [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Comment is unspecific and unclear. No action.
0-99	0	0				The leader should try to avoid repeated sentences. In some section, It seems you read the AR4. [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	The AR4 forms the basis for the updated assessment provided in the AR5, and thus, some repetition is often necessary and sometimes even unavoidable.
0-100	0	0				The caption of some figures are so long that are same as an article, in contrast of a number of them that have a very short describtion. [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Figure and Figure caption development is ongoing for the SOD.
0-101	0	0				Some of textes need to have refrences for following by user. [Fatemeh Rahimzadeh, Iran, Islamic Republic of]	Agreed - the assessment needs to be, and will be, supported with appropriate references from the peer-reviewed literature.
0-102	0	0				Comment on AR5's near complete omission of the massive evidence for a solar-magnetic climate driverMy training is in economics where we are very familiar with what statisticians call "the omitted variable problem" (or when it is intentional, "omitted variable fraud"). Whenever an explanatory variable is omitted from a statistical analysis, its explanatory power gets misattributed to any correlated variables that are included. This problem is manifest at the very highest level of AR5, and is built into each step of its analysis.For the 1750-2010 period examined, two variables correlate strongly the observed warming (and hence with each other). Solar magnetic activity and atmospheric CO2 were both trending upwards over the period, and both stepped up to much higher levels over the second half of the 20th century. This pair of correlations with temperature change give rise to the two main competing theories of 20th century warming. Was it driven by rapidly increasing human release of CO2, or by the 80 year "grand maximum" of solar activity that began in the early 1920's. ("Grand minima and maxima of solar activity: new observational constraints," Usoskin et al. 2007.)The empirical evidence in favor of the solar explanation is overwhelming. Dozens of peer-reviewed studies have found a very high degree of correlation (.5 to .8) between solar-magnetic activity and global temperature going back many thousands of years (Bond 2001, Neff 2001, Shaviv 2003, Usoskin 2005, and many others listed below). In other words, solar activity "explains," in the statistical sense, 50 to 80% of past temperature change.Such a high degree of correlation over such long time periods implies causality, which can only go one way. Global temperature cannot be driving solar activity, so there must be some mechanism by which solar activity is driving or modulating global temperature change. The high degree of correlation also suggests that solar activity is the PRIMARY driver of global temperature on every time scale studied (which is pretty much every time scale but the Milankovitch cycle).In contrast, CO2 and temperature records reveal no discernable warming effect of CO2. There is a correlation between CO2 and temperature, but with CO2 changes following temperature changes by an average of about 800 years (Caillon 2003), indicating that it is temperature change that is driving CO2 change (as it should, since warming oceans are able to hold less CO2). This does not rule out the possibility that CO2 also drives temperature, and in theory a doubling of CO2 should cause about a 1	Rejected -- Cosmic rays are dealt with in Chapter 7. Some aspects addressed in this comprehensive review are covered by material from Chapter 10 on "Detection and Attribution: From Global to Regional". However, given the offensive tone and formulations in the last 4 paragraphs of this review comment, this particular comment is not considered further.

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						degree increase in temperature before any feedback effects are accounted, but feedbacks could be negative, so there no reason, just from what we know about the greenhouse mechanism, that CO2 has to be a significant player. The one thing we can say is that whatever the warming effect of CO2, it is not detectable in the raw CO2 vs. temperature data.This is in glaring contrast to solar activity, which lights up like a neon sign in the raw data. Literally dozens of studies finding .5 to .8 degrees of correlation with temperature. So how is it that the IPCC's current generation of general circulation models start with the ASSUMPTION that CO2 has done 40 times as much to warm the planet as solar activity since 1750? This is the ratio of AR5's radiative forcing estimates for variation in CO2 and variation in total solar effects listed in table 8.9 on page 8-45. RF for CO2 is entered as 2.79 W/m^2 while RF for total solar effects is entered as .07 W/m^2. The 50% driver of global temperature according to mountains of temperature correlation data is ASSUMED to have 1/40th the warming effect of something whose warming effect is not even discernable in the temperature record. And this is on the INPUT side of the GCM's. The models aren't using gigaflops of computing power to FIND that CO2 has that much larger a warming effect. The warming ratio is fixed at the outset. Garbage in, garbage out. [Sorry for using ALL CAPS for emphasis but Excel is not letting me use italics.]The "how" is very simple. The 40 times greater warming effect of CO2 is achieved by blatant omitted variable fraud. As I will fully document, all of the evidence for a strong solar magnetic driver of climate is simply left out of AR5. Of the many careful empirical studies that show a high correlation between solar activity and climate, not a single one is even mentioned ANYWHERE in the First Order Draft. On page 7-50, line 52, there is a single reference to a single paper (Kirkby 2007) where the text suggests some correlation between solar activity and climate, but it fails to mention even that the correlation to temperature is positive, never mind its dramatic magnitude, or the numerous repeated findings of this result. And that's it. One oblique reference in the entire report. A person reading AR5 from cover to cover would come away with not even a hint that for more than ten years a veritable flood of studies have been finding solar activity to explain something on the order of half of all past temperature variation. It is COMPLETELY omitted.As a result, AR5 misattributes virtually all of the explanatory power of solar-magnetic activity to the correlated CO2 variable. This misattribution can be found both in AR5's analytical discussions and in its statistical estimations and projections, and the error could not be more consequential. If it is solar-magnetic activity that drives climate then the sun's recent descent into a state of profound quiescence portends imminent global cooling, possibly rapid and severe, and unlike warming, cooling is actually dangerous, and really can feed back on itself in runaway fashion.Nothing could be more perverse in such a circumstance than to unplug the modern world in a misbegotten jihad against CO2. The IPCC's omitted variable fraud must stop. AR5's misattribution of 20th century warming to CO2 must stop. The EVIDENCE overwhelmingly supports the solar-magnetic warming theory. The only support for the CO2 theory is the fact that models built on it can achieve a reasonable fit to the last couple centuries of temperature history, but that is only because CO2 is roughly correlated with solar activity over this period, while these models themselves are invalidated by their demonstrable omitted variable fraud. If warming is attributed to solar-magnetic effects at all in accordance with the evidence then the warming that is left to attribute to CO2 becomes utterly benign. With natural temperature variation almost certainly both substantially larger than CO2 effects, and headed in the cooling direction, the expected external value of CO2 is unambiguously positive. If anything, we should subsidizing and promoting increases in atmospheric CO2, exactly the opposite of the Executive Summary's opening claim that developments since AR4 "further strengthen the basis for human activities being the primary driver in the concerns about climate change." (Page 1-2, lines 4-5.)As someone who recognizes the scientific errors in this disastrous report, I can at least make sure that the issue is put properly before the authors of AR5. Thus I am documenting as concisely as possible the solar-magnetic omission and the errors it leads to. The discussion is substantial but I have kept it well under the character limit for a single comment. This comment is being submitted as a top-level comment on AR5 as a whole, and it is being submitted unaltered as a comment on three different sub-chapter headings where the omitted solar-magnetic evidence ought to be taken into account (on FAQ 5.2 starting on page 5-43, on section 7.4.7 starting on page 7-50, and on table 8.6 starting on page 8-45).A sample of the omitted evidenceListed below are a few of the most prominent and compelling studies that have found a high correlation between solar activity and climate, together with a semi-random collection of similar findings, totaling two dozen citations all together. It would be easy to list two dozen more, but the purpose here is just to show a sample of the omitted evidence, to document up-front the existence and validity of it. Included are brief descriptions of the findings for about ten of the studies. None of the observed correlations are reported anywhere in AR5. The first four are the ones I mentioned above:Bond et al. 2001, "Persistent Solar Influence on North Atlantic Climate During the	

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						Holocene," Science. Excerpt from Bond: "Over the last 12,000 years virtually every centennial time scale increase in drift ice documented in our North Atlantic records was tied to a distinct interval of variable and, overall, reduced solar output."Neff et al. 2001, "Strong coherence between solar variability and the monsoon in Oman between 9 and 6 kyr ago," Nature. Finding from Neff: Correlation coefficients of .55 and .60.Usoskin et. al. 2005, "Solar Activity Over the Last 1150 years: does it Correlate with Climate?" Proc. 13th Cool Stars Workshop.Excerpt from Usoskin: "The long term trends in solar data and in northern hemisphere temperatures have a correlation coefficient of about 0.7 — .8 at a 94% — 98% confidence level."Shaviv and Veizer, 2003, "Celestial driver of Phanerozoic climate?" GSA Today.Excerpt from Shaviv: "We find that at least 66% of the variance in the paleotemperature trend could be attributed to CRF [Cosmic Ray Flux] variations likely due to solar system passages through the spiral arms of the galaxy." [Not strictly due to solar activity, but implicating the GCR, or CRF, that solar activity modulates.]Plenty of anti-CO2 alarmists know about this stuff. Mike Lockwood and Claus Fröhlich, for instance, in their 2007 paper: "Recent oppositely directed trends in solar climate forcings and the global mean surface air temperature" (Proc. R. Soc. A), began by documenting how "[a] number of studies have indicated that solar variations had an effect on preindustrial climate throughout the Holocene." In support, they cited 17 papers: the Bond and Neff articles from above, plus Davis & Shafer 1992; Jirikowic et al. 1993; Davis 1994; vanGeel et al. 1998; Yu&Ito 1999; Hu et al. 2003; Sarnthein et al. 2003; Christla et al. 2004; Prasad et al. 2004; Wei & Wang 2004; Maasch et al. 2005; Mayewski et al. 2005; Wang et al. 2005a; Bard & Frank 2006; and Polissar et al. 2006.The correlations in a lot of these papers are not directly to temperature. They are to temperature proxies, some of which have a complex relationship with temperature, like Neff 2001, which found a correlation between solar activity and rainfall. Even so, the correlations tend to be strong, as if the whole gyre is somehow moving in broad synchrony with solar activity.Some studies do examine correlations between solar activity proxies and direct temperature proxies, like the ratio of Oxygen18 to Oxygen16 in geologic samples. One such study was highlighted in Kirkby 2007. Mangini et. al. 2005, "Reconstruction of temperature in the Central Alps during the past 2000 yr from a δ18O stalagmite record," found:Excerpt from Mangini: "... a high correlation between δ18O in SPA 12 and D14C (r =0.61). The maxima of δ18O coincide with solar minima (Dalton, Maunder, Sporer, Wolf, as well as with minima at around AD 700, 500 and 300). This correlation indicates that the variability of δ18O is driven by solar changes, in agreement with previous results on Holocene stalagmites from Oman, and from Central Germany."And that's just old stuff. Want some new stuff? Here are four random recent papers.Ogurtsov et al, 2010, "Variations in tree ring stable isotope records from northern Finland and their possible connection to solar activity," JASTP.Excerpt from Ogurtsov: "Statistical analysis of the carbon and oxygen stable isotope records reveals variations in the periods around 100, 11 and 3 years. A century scale connection between the 13C/12C record and solar activity is most evident."Di Rita, 2011, "A possible solar pacemaker for Holocene fluctuations of a salt-marsh in southern Italy," Quaternary International.Excerpt from Di Rita: "The chronological correspondence between the ages of saltmarsh vegetation reductions and the minimum concentration values of 10Be in the GISP2 ice core supports the hypothesis that important fluctuations in the extent of the salt-marsh in the coastal Tavoliere plain are related to variations of solar activity."Raspopov et al, 2011, "Variations in climate parameters at time intervals from hundreds to tens of millions of years in the past and its relation to solar activity," JASTP. Excerpt from Raspopov: "Our analysis of 200-year climatic oscillations in modern times and also data of other researchers referred to above suggest that these climatic oscillations can be attributed to solar forcing. The results obtained in our study for climatic variations millions of years ago indicate, in our opinion, that the 200- year solar cycle exerted a strong influence on climate parameters at those time intervals as well."Tan et al, 2011, "Climate patterns in north central China during the last 1800 yr and their possible driving force," Clim. Past.Excerpt from Tan: "Solar activity may be the dominant force that drove the same-phase variations of the temperature and precipitation in north central China."Saltmarshes, precipitation, "oscillations." It's all so science-fair. How about something just plain scary? Solheim et al. 2011, "Temperature prognosis based on long sunspot cycle 23," (not sure if this has been published yet, but you can find it here: http://www.au.agwscam.com/pdf/SolheimSolarTemperature.pdf).Excerpt from Solheim: "We find that for the Norwegian local stations investigated that 30-90% of the temperature increase in this period may be attributed to the Sun. For the average of 60 European stations we find ≈ 60% and globally (HadCRUT3) ≈ 50%. The same relations predict a temperature decrease of ≈ 0.9°C globally and 1.1−1.7°C for the Norwegian stations investigated from solar cycle 23 to 24."First Chapter 5 error: omitting all solar variables besides TSIChapter 5, the paleo observations chapter, is the right place for the evidence for a solar-magnetic climate driver to be	

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						introduced because most of this evidence is obtained from the deposition of cosmogenic isotopes in various paleologic strata: ice cores, geologic cores and tree rings. When solar activity is strong, less galactic cosmic radiation (GCR) is able to penetrate the solar wind and reach earth, so variation in cosmogenic isotopes found in time-dated strata serves as a proxy for solar activity. But when chapter 5 does get around to looking at cosmogenic records, it only looks at how they can be used to reconstruct total solar irradiance (TSI). It never even hints at the flood of studies that show a high degree of correlation between solar activity and various paleo proxies for climate and temperature!This occurs under the subheading "FAQ 5.2: Is the Sun a Major Driver of Climate Changes?" which is placed as an addendum to Chapter 5, starting on page 5-43. This FAQ mentions the long-period chang in TSI that come with orbital variation (Milankovitch cycles), a factor which hasn't changed enough since 1750 to account for any significant amount of the warming since that date. Neither can TSI be responsible for significant recent warming because, as solar activity jumps dramatically up and down over the roughly 11 year solar cycle, TSI is known to remain remarkably stable, varying only .1 to .2% (as noted on page 5-43, line 53).Thus, concludes FAQ 5.2, solar variation cannot be responsible for any significant amount of the warming since 1750. But it is only able to reach this conclusion by completely omitting any consideration those solar variables other than TSI that could be affecting global temperature. Unlike TSI, solar wind speed and pressure vary considerably over the solar cycle and between solar cycles. So do the Ap index and the F10.7cm radio flux progression. The GCR that the solar wind modulates, the neutron counts measured at Climax and Oulu and other locations, can vary by a full order of magnitude over the solar cycle. In contrast, TSI varies so little that it is called "the solar constant." If there is a mechanism by which solar variation is driving global temperature, it is most likely to work through those solar variables that actually vary significantly with solar activity. Yet the discussion in FAQ 5.2 pretends that these other solar variables do not even exist.So that's the first error in FAQ 5.2: pretending to have addressed the range of possible solar effects while studiously neglecting to mention that there are a bunch of solar variables that, unlike TSI, vary tremendously over the solar cycle and might affect our climate in ways that we do not yet understand. We in-effect live inside of the sun's "atmosphere," the extended corona created by the sun's magnetic field and the solar wind. AR5 simply assumes that this solar environment has no effect on global climate, and they do it by rank omission of the relevant variables. The omitted variable problems that result are not an accident. They are omitted variable fraud.Second Chapter 5 error: the highly irrational assumption that temperature would be driven by the trend in solar activity rather than the levelPerhaps in an effort to justify ignoring all solar variables other than TSI, FAQ 5.2 ends with what it presents as a general reason to dismiss the possibility that solar variation made any significant contribution to late 20th century warming by ANY mechanism. Page 5-44, lines 25-28: "[The sun can't be] a major driver of the climate changes over the past 40 years because instrumental TSI and SSI records contain no significant trend; whereas records of global mean temperature and GHG concentrations contain significant trends of increasing values. This lack of agreement in trends demonstrates that the Sun did not play a role during this period."TSI peaks at the high point of the solar cycle, just as the other solar variables do, so no matter what solar variable you look at, it can't have been the cause of recent warming, because these variables showed no upward trend over this period, right? Wrong. That's like saying you can't heat a pot of water by turning the flame to maximum and leaving it there, that you have to turn up the flame slooooooowly if you want the water to heat. It is incredible to see something so completely unscientific in AR5, passing as highly vetted science.And the "flame" DID stay on maximum. Again, there was an 80 year "grand maximum" of solar activity starting in the early 1920's (Usoskin 2007). AR5 is in-effect assuming that the oceans had already equilibrated to whatever temperature forcing effect this high level of solar activity might have. Otherwise the continued temperature forcing from the continued high level of solar activity would have caused continued warming.Claims of rapid ocean equilibration have been made (Schwartz 2007), but they don't stand up to scrutiny. In order to get his result, Schwartz used an energy balance model with the oceans represented by a single heat sink. That is, he assumed that the whole ocean changed temperature at once! Once you move to a 2 heat sink model where it takes time for heat to transfer from one ocean layer to another (Kirk-Davidoff 2009), it becomes clear that the rapid temperature adjustment of the ocean surface tells us next to nothing about how long it takes for the ocean to equilibrate to a long term forcing. The paleo-temperature record is typified by multi-century warming and cooling phases, suggesting that equilibration can easily take centuries, making it ludicrous to assume that the warming effect of a grand maximum that began in the 1920's must have been spent by 1970 or 1980 or by ANY particular date. So no, there is no way to save the utterly incompetent argument in FAQ 5.2 that a solar driver of temperature can only cause warming when it is on the increase. If solar wind pressure or GCR does in some way drive global	

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						temperature, there is every reason to believe that it would have continued to warm the planet for as long as solar activity remained at grand maximum levels. There is NO EXCUSE for the IPCC to be omitting these variables, which are much more likely than TSI to be responsible for the high observed degree of correlation between solar activity and climate. For chapter 5 to be tenable, all of the now massive evidence that there is SOME mechanism by which solar activity is driving MOST temperature change must be laid out in full.Technical note: misattribution is assigned manually in AR5, but the concept is the same as for purely statistical omitted variable fraudIf TSI and the other solar variables all move roughly together, won't omitting the solar variables other than TSI cause any explanatory power they might have to be attributed to TSI rather than CO2, since they are more closely correlated with TSI?In a purely statistical estimation scheme yes, but the IPCC uses a combination of parameterized elements and estimated elements, and one of the elements that is parameterized is radiative forcings of CO2 and TSI, meaning that their relative warming effects are parameterized as well, with CO2 being assigned 40 times the warming effect of TSI over the 1750 to 2010 period. This parameterization means that the explanatory power of the omitted solar magnetic variables gets attributed forty parts to CO2 for every one part to TSI. This structure forces the misattribution onto CO2. You can think of it a manual assignment of the misattribution.The general concept of the omitted variable remains the same. There is only so much attribution for warming to go around (100%). If attribution is given to the solar-magnetic variables in accordance with the evidence from the historic and paleo records—at least 50%—then there less than 50% that can possibly be attributable to other causes.Which again beings the scientific competence of IPCC into question. If CO2 has 40 times the warming effect of the 50% driver of global temperature (total solar effects), that makes it what? The 2000% driver of global temperature?Chapter 7 inverts the scientific method, using theory to dismiss evidenceWhere chapter 5 simply pretends that no solar variable other than TSI exists, Chapter 7 doesn't have that option. It is tasked to address directly the possibility that variables like the solar wind and GCR could be affecting climate. But Chapter 7 still comes up with a way to avoid mentioning any of the massive evidence that there must be SOME mechanism by which solar activity is driving climate. Just as it starts to touch on the subject, it jumps instead to examining the tenability of PARTICULAR THEORIES about the mechanism by which solar activity might drive climate. This happens right at the beginning of section 7.4.7.1. "Correlations Between Cosmic Rays and Properties of Aerosols and Clouds." This is on page 7-50, lines 50-53:"Many empirical relationships or correlations have been reported between GCR or cosmogenic isotope archives and some aspects of the climate system, such as SSTs in the Pacific Ocean (Meehl et al., 2009), some reconstruction of past climate (Kirkby, 2007) or tree rings (Dengel et al., 2009). We focus here on observed relationships between GCR and aerosol- and cloud-properties."The first sentence of 7.4.7.1 is as close as AR5 comes to making any mention of overwhelming evidence that there is SOME mechanism by which solar activity drives global temperature. The Kirkby citation suggest some correlation between solar activity and climate, but what the correlation might be is completely obscured, and that's it. The second sentence effects the transition into looking at the evidence for particular theories of the mechanism involved. A short discussion later, the evidence for these particular mechanisms is asserted (quite tendentiously) to be "too weak" for the mechanisms to be "climatically-significant" (page 7-52, lines 33-35). This proclaimed weakness in turn becomes the rationale for omitting the mechanisms from the IPCC's general circulation models, and hence from the projections that are made with those models.What do the AR5 draft authors do with the overwhelming evidence that there is SOME mechanism at work that makes solar magnetic the primary driver of global temperature? So they don't like the particular theories offered. They have to still acknowledge that SOME such mechanism must be at work, don't they? Ahh, but readers don't know about that evidence, because it was skipped over with that single oblique reference to Kirkby 2007, and AR5 continues as if the evidence doesn't exist. They never use it. They never mention it. They never think about it. It is GONE. They declare their dissatisfaction with the available theories for how such a mechanism would work, and use this as an excuse to completely ignore the massive evidence that there is some such mechanism at work. This is an exact inversion of the scientific method, which says that evidence always trumps theory. The IPCC is throwing away the evidence for a solar-magnetic driver of climate because it isn't satisfied with the theories that have been proposed to account for it. This is the DEFINITION of anti-science: putting theory (or ideology, or ANYTHING) over evidence. Evidence has to be the trump card, or its not science. The IPCC is engaged in actual, definitional, anti-science, exactly inverting the scientific method.It is as if a pre-Newtonian "scientist" were to predict that a rock released into the air will waft away on the breeze, because we understand the force that the breeze imparts on the rock, but we have no good theory of the mechanism by which heavy objects are pulled to the ground. We should therefore ignore the overwhelming	

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						evidence that there is SOME mechanism that pulls heavy objects to the ground, and until such time as we can identify the mechanism, proceed as if no such mechanism existed. This is what the IPCC is actually doing with the solar-climate evidence. Y'all aren't scientists. You are pure, definitional, ANTI-SCIENTISTS.More anti-science: Chapter 7 repeats the second Chapter 5 error You know, that bit about thinking that a climate driver can only cause continued warming if its own level continues to increase? Chapter 7 says it again: just leaving a proposed climate driver on maximum can't possibly cause warming. From page 7-52, lines 35-37: Moreover it should be noted that one study infers no trend in cosmic ray intensity over the last 50 years (McCracken and Beer 2007).And that's the end of the section, AR5's punctuation mark on why solar activity and GCR should be dismissed as an explanation for late 20th century warming. This is anti-scientific in its own way. Scientists are supposed to be smart. They aren't supposed to think that you have to slowly turn up the flame under a pot of water in order to heat it. You could collect every imbecile in the world together and not a one of them would ever come up with the idea that they have to turn the heat up slowly. It's beyond stupid. It's like, insanely stupid. And multiple chapter-writing teams are proclaiming the same nonsense. Fruitcakes.Okay, I guess that means I'm ready to wrap up. Y'all have taken all these tens of billions of dollars of research money and used it perpetrate a fraud. As I have documented above, you have perpetrated the grandest and most blatant example of omitted variable fraud in history, but so far only the skeptic half the world knows it. You still have a shot, before global cooling is an established fact, to make a rapid turn around and save some shred of your reputations. But if AR5 comes out insisting that CO2 is a dominant warming influence just as global cooling is becoming an established fact, then you all are finished on the spot. You'll still have your filthy lucre, but the tap is going to turn off, and your reputations will be destroyed forever.Can you imagine a worse juxtoposition? And this is what the evidence says is going to happen, ALL of that evidence that you have been so studiously omitting. I'm eager for your embarrassment, but I would much rather see you save yourselves, so that the needed policy reversals can some that much sooner. The anti-CO2 policies that your fraudulent "science" has supported are right now destroying the world economy. You idiots are KILLING our future. Please wake up and try to save your own reputations before your lunatic anti-science ruins us all.End comment [Alec Rawls, United States]	
0-103	0	0				The report overall is very good. Readers will particularly benefit from the detailed descripption and review of different exisiting sources of data and results. Having said that, I feel the report is not ready to make any clear assertions about the state of warming in climatic conditions. Particularly I find it a but speculative when the authors mention phrases like "medium confidence" or "low confidence" or "high confidence". Such phrases in particular make the findings of the report weak. Also in the section on temperatures and rainfall, the authors mention only increase or decrease -- no specific numbers have been mentioned. The report can benefit from some more specific confident reporting. [Shouraseni Roy, USA]	IPCC AR5 uncertainty guidance note (Mastrandrea et al., IPCC 2010) discusses the basis for assigning evidence/agreement, levels of confidence and/or likelihood. Quantitative assessment will be further developed for SOD.
0-104	0	0				Natural (internal) climate variability is much more of a consideration in AR5 than in previous assessments. In this respect, large-scale ocean-atmosphere circulation phenomena, teleconnection and the related indices are reviewed in several chapters (2, 5, 9, 10, 14). However the treatment is inconsistent within and especially between chapters. Table 1 in Box 2.4 is an excellent summary of the various climate circulation phenomena and associated indices. Reference should be made to this Table wherever the forcing of short-term climate variability is discussed throughout the report of WGI. [David Sauchyn, Canada]	Noted. Cross-referencing and cross-chapter consistency will be a focus of the SOD.
0-105	0	0				Chapter 3 of the IPCC SREX report (2012; Seneviratne, Nicholls, et al. 2012), is not consistently referred to in the FOD version of the different chapters. Chapters 2 and 10 referred to it thoroughly, while in particular Chapters 12 and 14 are either not referring to it or overlooked significant parts of that chapter, although directly relevant to their assessment. While this is likely due to timing issues between the reports, it is important that this be satisfactorily addressed in the preparation of the SOD. [Sonia Seneviratne, Switzerland]	The use of SREX, and reference to, as a basis for the WGI assessment of extremes will be improved for the SOD. In particular, Chapter 14 will refer thoroughly to SREX in its regional subsections in the SOD.
0-106	0	0				Chapter 3 of the IPCC SREX report (2012) provided revised definitions of regions for assessments, based on inputs of regional experts. These are particularly relevant in South America and Europe. In South America, the traditional "Giorgi" regions ignore the impact of the Andes for climate on the continent, an issue that is addressed in the revised definitions from the SREX. In Europe, the distinct features of continental European climate are considered, which is also justified by the number of pubications specifically distinguishing the Mediterranean, central European and northern European climate regions. For these various reasons, I would strongly recommend that the AR5 use the improved specification of geographical regions introduced in the	Agreed - the WGI AR5 SOD will use the regions as defined in Chapter 3 of SREX.

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						SREX. This would also help considering the regional assessments from the SREX (SPM Fig. 4a & 4b; Chapter 3 Figs. 3.5 and 3.7; Tables 3.2 and 3.3) in the AR5. [Sonia Seneviratne, Switzerland]	
0-107	0	0				The style of references often seems to list publications in alphabetical rather than chronological order when cited together in a parenthesis. This does not seem consistent with the usual scientific practice (and also not with the AR4 formatting as far as I could tell). [Sonia Seneviratne, Switzerland]	Will be taken care at the copyedit stage of the report.
0-108	0	0				There needs to be some consistency in definitions of Earth System Models (ESMs) throughout the report. Chapter 9 page 3 bullet 2 refers to ESMs as “models which include climatically important biogeochemical processes” whereas chapter 12 page 3 line 23 says ESMs are models “which have an interactive carbon cycle”. It would be good to have the same definition throughout the report or a new nomenclature to distinguish the two if they are two different types of model - I realise some so-called Earth System models like HadGEM2-ES can be forced by concentrations of CO2 and do not have an interactive carbon cycle, so I suspect there is a distinction to be made. If it is not made, then people will become very confused. [David Sexton, UK]	Agreed - the Glossary will be available during the SOD expert/government review. It will include a definition of Earth System Models (ESMs).
0-109	0	0				the choice of time period for the present climate as reference period 1986-2005 in comparison with the future time periods (2046-2065 and 2081-2100) makes it difficult to include a consistent comparison between CMIP3 and CMIP5 model simulations and some reanalysis like ERA40 that end at 2001 cannot be used efficiently for model validation purposes [Jana Sillmann, Canada]	Noted. We certainly appreciate that the chosen reference period of 1986-2005 makes it more difficult to directly compare CMIP3 with many of the CMIP5 model results presented. However, the choice of the "optimal" reference period was the subject of a great deal of discussion and interaction amongst authors from all relevant chapters of the WGI AR5. The pros and cons of several choices were carefully weighed before the decision for 1986-2005 was taken. One of the key considerations leading to this decisions was for example that using a reference period too far back (e.g., 1961-1990 or 1981-2000) would have created substantial problems with short term projections and associated uncertainty estimates, since much of the projected change from the reference period would have already happened. Another reason for choosing the reference period 1986-2005 is that it corresponds to the last 20 years of the CMIP5 historical experiments, with the RCP scenarios starting in 2006. In this context, we would also note that direct comparisons between CMIP5 and CMIP3 projections are still being made in the AR5, for example in Chapter 11, where maps showing e.g. the emergence of differences in regional mean surface air temperatures across CMIP5 and CMIP3 (A1B) scenarios are presented.
0-110	0	0				I would suggest choosing the reference period 1981-2000 to be able to have a consistent comparison between available reanalyses and CMIP3 and CMIP5 models at least in regard to the considered time periods. Dealing with different future emission scenarios (RCPs versus SRES scenarios makes it already difficult to compare CMIP3 and CMIP5 simulations. so authors do not have to argue about different scenarios as well as about a 5 year-difference in reference period and resulting (if any) discrepancies between the different time periods. [Jana Sillmann, Canada]	Noted. We certainly appreciate that the chosen reference period of 1986-2005 makes it more difficult to directly compare CMIP3 with many of the CMIP5 model results presented. However, the choice of the "optimal" reference period was the subject of a great deal of discussion and negotiation amongst representatives from all relevant chapters of the WGI AR5. The pros and cons of several choices were carefully weighed before the decision for 1986-2005 was taken. One of the key considerations leading to this decisions was for example that using a reference period too far back (e.g., 1961-1990 or 1981-2000)

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							would have created substantial problems with short term projections and associated uncertainty estimates, since much of the projected change from the reference period would have already happened. Another reason for choosing the reference period 1986-2005 is that it corresponds to the last 20 years of the CMIP5 historical experiments, with the RCP scenarios starting in 2006. In this context, we would also note that direct comparisons between CMIP5 and CMIP3 projections are still being made in the AR5, for example in Chapter 11, where maps showing e.g. the emergence of differences in regional mean surface air temperatures across CMIP5 and CMIP3 (A1B) scenarios are presented.
0-111	0	0				having the same reference period also would make it easier to illustrate future changes in CMIP3 and CMIP5 simulations relative to the present climate in the same plot [Jana Sillmann, Canada]	Noted. We certainly appreciate that the chosen reference period of 1986-2005 makes it more difficult to directly compare CMIP3 with many of the CMIP5 model results presented. However, the choice of the "optimal" reference period was the subject of a great deal of discussion and interaction amongst the authors of all relevant chapters of the WGI AR5. The pros and cons of several choices were carefully weighed before the decision for 1986-2005 was taken. One of the key considerations leading to this decisions was for example that using a reference period too far back (e.g., 1961-1990) would have created substantial problems with short term projections and associated uncertainty estimates, since much of the projected change from the reference period would have already happened. Another reason for choosing the reference period 1986-2005 is that it corresponds to the last 20 years of the CMIP5 historical experiments, with the RCP scenarios starting in 2006. In this context, we would also note that direct comparisons between CMIP5 and CMIP3 projections are still being made in the AR5, for example in Chapter 11, where maps showing e.g. the emergence of differences in regional mean surface air temperatures across CMIP5 and CMIP3 (A1B) scenarios are presented.
0-112	0	0				Initial general comment: This (Draft AR5) is already a most comprehensive and excellent report. My suggestions/comments are aimed at various items that can be improved substantially for its eventual audience/readership. You may note that the review contains numerous global comments and specific suggestions for revisions that focus on both the science of climate change as well as how that is to be presented/communicated to the outside world that comprises the general public, all other scientists, and policy and decision makers. Given the recent history of the IPCC, as also the international political (or policy level, if you prefer) discussion currently going on, I would imagine that this is of paramount importance--communicating the science accurately and in a manner that can be understood by the multi-sectoral and multi-disciplinary international community. Given the above, permit me to suggest that the below specific review comments be communicated, as you wish, to the lead authors (Co-Chairs) of this report and perhaps as appropriate to an IPCC Plenary and leave it up to them to decide how to introduce the revisions suggested that could impact all chapters of the report. I believe that it would greatly benefit the target readership as well as, of course, the IPCC. The below are personal opinions not representing that of any country, agency or	Noted with thanks.

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						program, a caveat that I wish to underscore.] [Sushel Unninayar, USA]	
0-113	0	0				A consistent, complete and simple enough glossary adds to the transparency, credibility and readability of the report. Given the importance of definitions, and given that definitions were in the past sometimes changed hastily during the Final Plenary, I strongly suggest that the Glossary should be part of the material reviewed, with highlights of the changes made to definitions used previously in IPCC reports. Please add the Glossary (with these highlights) to the list of reviewed material no later than the combined Expert/Government review round. This will help to avoid many unneccessary discussions during the Plenary. [Jean-Pascal van Ypersele, Belgium]	The WGI AR5 Glossary will be available for the SOD as Annex 3. Annexes are not typically reviewed, however. Reviewers are invited to comment on specific usage of terms in the context of relevant chapters.
0-114	0	0				Given the importance for the IPCC reports of being policy-relevant (while not prescriptive), a number of choices related to the communication of key data, results, and conclusions need to be made in a strategic manner in the early phase of the report writing, so that governments and other decision-makers can make the best use of the report. Three aspects are particularly important in that perspective (avoiding moving scales of comparison; avoiding highlighting a particular scenario; using physical units that are close to those used in climate policy, while being scientifically correct). They are described below. [Jean-Pascal van Ypersele, Belgium]	Noted.
0-115	0	0				1) Avoiding moving scales of comparison: The evolution from and comparison with the key results and conclusions of previous IPCC reports need to be facilitated. This implies, among other things, that changes in the definitions of key concepts since the previous assessments need to be made with extreme caution and justification, and highlighted both in the report and in the Glossary (which should be part of the reviewed material). It implies also that the reference period used to define a change in climate must be as stable as possible. For example, temperature changes in the key table of SRES-based results (known by most policy-makers) are expressed as 2090-2099 relative to 1980-1999. The corresponding period used in AR5 seems in many cases (e.g. in the Atlas) to be 2080-2099 (or 2081-2100!) relative to 1986-2005. This "moving scale" hinders comparison between assessments, and decreases the policy-relevance of the IPCC reports. Please make an effort to fix these problems. [Jean-Pascal van Ypersele, Belgium]	Noted. We certainly appreciate that the chosen reference period of 1986-2005 makes it more difficult to directly compare CMIP3 with many of the CMIP5 model results presented as well as comparing AR5 to AR4. However, the choice of the "optimal" reference period was the subject of a great deal of discussion and interaction amongst the authors of all relevant chapters of the WGI AR5. The pros and cons of several choices were carefully weighed before the decision for 1986-2005 was taken. One of the key considerations leading to this decisions was for example that using a reference period too far back (e.g., 1961-1990 or 1981-2000) would have created substantial problems with short term projections and associated uncertainty estimates, since much of the projected change from the reference period would have already happened. Another reason for choosing the reference period 1986-2005 is that it corresponds to the last 20 years of the CMIP5 historical experiments, with the RCP scenarios starting in 2006. In this context, we would also note that direct comparisons between CMIP5 and CMIP3 projections are still being made in the AR5, for example in Chapter 11, where maps showing e.g. the emergence of differences in regional mean surface air temperatures across CMIP5 and CMIP3 (A1B) scenarios are presented.
0-116	0	0				2) Avoiding highlighting a particular scenario (part 1): It should be remembered that, in order to maximize policy-relevance of IPCC reports, the IPCC Plenary requested the RCP set to be "compatible with the full range of stabilization, mitigation and baseline emissions scenarios available in the current literature". Low scenarios help to understand the minimum amount of climate change the planet will experience, and high scenarios help to understand the worst cases, which is essential in risk-management perspective. It is therefore important that the results given span the full range of assessed scenarios whenever possible, and avoid highlighting one of the scenarios only. [Jean-Pascal van Ypersele, Belgium]	We don't understand the reviewer comment. In the WGI FOD, including in Annex 1: Atlas of Global and Regional Climate Projections". all four RCPs have been extensively covered in addition to some of the IPCC SRES scenarios and many academic scenarios spanning a wide range of possible futures
0-117	0	0				2) Avoiding highlighting a particular scenario (part 2): This is particularly true for the Atlas, which only shows maps for the RCP4.5 scenario (and the times series to show how the area-average response varies among	We don't understand the reviewer comment about highlighting of a particular scenario in the context of

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						the other scenarios is not readily usable to obtain the other maps). The scenario process led to the deliberate choice to define four scenarios, to span the full range and avoid the subjective selection of one scenario "in the middle". Chosing to highlight RCP4.5 (e.g. in the Atlas) is implicitly policy-prescriptive. At least RCP2.6 and RCP 8.5 should be reflected in the same way as RCP4.5. Please consider how to fix this. [Jean-Pascal van Ypersele, Belgium]	the WGI FOD. In the WGI FOD, including in Annex 1: Atlas of Global and Regional Climate Projections" and it's 3 online supplementary materials, all four RCPs have been extensively covered (in addition to some of the IPCC SRES scenarios and many academic scenarios spanning a wide range of possible futures).
0-118	0	0				3) Using physical units that are close to those used in climate policy, while being scientifically correct: While it was usual to discuss carbon dioxide emissions in terms of "PgC" in the IPCC First Assessment report in 1990, we are now in a world where policy-makers are used to think in terms of (metric) "tonnes of (equivalent) carbon dioxide", which is the official accounting unit used under the UNFCCC and its Kyoto Protocol. What is exchanged on carbon markets are tonnes of CO2eq, not PgC or TgC. The "gap" discussed in the context of climate negotiations on the 2020 or 2050 horizon is also expressed in GtCO2eq. The IPCC WGI would make a significant contribution in reducing the confusion between carbon units and CO2 units by sticking to the latter and using multiple of metric "tonnes" whenever possible. NB: The issue is not only between C, CO2 and CO2eq but other GHG also suffer from this kind of confusion. [Jean-Pascal van Ypersele, Belgium]	Rejected -- For carbon, Petagrams of Carbon (PgC) is the unit dominantly used in the assessed and peer-reviewed scientific literature, and will thus be used in the SOD of WGI AR5.
0-119	0	0				In general, more attention should be given to the need for an objective assessment of critical arguments frequently read about the physical aspects of climate change, to the extent they are published in scientific journals. Some of those arguments contribute to the doubt some have about the IPCC conclusions. The IPCC would provide a very useful service to the community by assessing these arguments in an objective way. If the IPCC does not do that in its reports, decision-makers might be confused when they read about these arguments in other sources of information, which might be less objective that the IPCC is. [Jean-Pascal van Ypersele, Belgium]	The WGI AR5 does provide a comprehensive and objective assessment based on the available scientific literature, as in fact have previous IPCC WGI assessment reports! Nevertheless, in the SOD of Chapter 1, topics that are scientifically debated in the peer-reviewed literature will be specifically highlighted, with explicit pointers to the Chapters where these topics are assessed.
0-120	0	0				why not using AR1 AR2, AR3 and AR5 in the text and figures in order to be consistent with AR4 instead of FAR, SAR etc. This will increase the readibility of the figures, without one has to find the abbreviations in the text [Elie Verleyen, Belgium]	These abbreviations are widely used and are based on the common IPCC terminology. Like all acronyms in the WGI AR5 Chapters, these acronyms are introduced when first used in the text.
0-121	0	0				A List of Abbreviations should be compiled. [Manfred Wendisch, Germany]	Agreed - this will be done for the SOD and a list of acronyms will also be included in the published report.
0-122	0	0				Multiple lines of evidence which should essentially form the basis of the key assessment statements need to be based on and supported by the cited references,i.e, avoid presenting assessment findings which are only supported by single citations. In addition, the multiple lines of evidence that lead to a key finding should be thoroughly discussed in the chapters. [Thomas Stocker/ WGI TSU, Switzerland]	Noted and agreed. Will be addressed for SOD.
0-123	0	0				Consider to use a common template for all schematic figures across chapters based on AR4 concept (e.g., AR4 Figure 7.3). [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Will be considered for SOD.
0-124	0	0				A consistent usage and application of the AR5 uncertainty guidance is required for all key statements resulting from the assessment. [Thomas Stocker/ WGI TSU, Switzerland]	Noted and agreed. Will be addressed for SOD.
0-125	0	0				Acronyms used should be used consistently across the chapters -- TSU will help and provide an initial list to work from [Thomas Stocker/ WGI TSU, Switzerland]	Noted and agreed. Will be addressed for SOD.
0-126	0	0				We propose that when confidence information is illustrated graphically (figures/tables), consistent levels of colour coding are used, e.g., see Fig 8.23. We propose that such a general scheme would best be introduced in Chapter 1 as part of the discussion of treatment of uncertainty in AR5 [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Will be considered for SOD.
0-127	0	0				Avoid using the term 'IPCC models' and, if applicable, refer to the CMIPs instead (this obviously applies particularly to Chapters 9 to 14. [Thomas Stocker/ WGI TSU, Switzerland]	Noted and agreed. Will be addressed for SOD.
0-128	0	0				All chapters dealing with extreme events should use the WGI-related assessment results from the recent IPCC Special Report on Extreme Events (i.e., Chapter 3) as the starting point for their assessment. SREX in	Noted and agreed. Will be addressed for SOD.

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						combination with the WGI AR4, should form the basis for the updates provided here in many chapters with regard to climate extremes. [Thomas Stocker/ WGI TSU, Switzerland]	
0-129	0	0				All chapters are encouraged to include a paragraph at the end of each main section, that synthesises the key findings of the assessment, using the uncertainty language. This will be extremely useful in the development of the Chapter ES, the WGI TS and SPM of WGI. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. Will be considered for SOD.
0-130	0	0				Given the page constraints for all chapters, please consider where overlaps and redundancies can be reduced. Careful cross-chapter coordination and discussions are encouraged. [Thomas Stocker/ WGI TSU, Switzerland]	Noted.
0-131	0	0				Please consider to establish consistency with SREX regions. The SREX Chapter 3 author team carefully discussed and revised the AR4 regions to develop a more physically based separation of regions, eg, the South American regions. [Thomas Stocker/ WGI TSU, Switzerland]	Noted and agreed. Will be addressed for SOD.
0-132	0	0				FAQ general: Consistent structure is needed- Italicized chapeau (opening paragraph) which provides the answer in brief / body of response / inclusion of a compelling graphic that supports the response [Thomas Stocker/ WGI TSU, Switzerland]	Noted and agreed. Will be addressed for SOD.
0-133	0	0				FAQ general: The Chapeau is particularly important, and is intended to provide a concise overview and response to the FAQ. [Thomas Stocker/ WGI TSU, Switzerland]	Noted.
0-134	0	0				FAQ general: FAQs need to be stand alone - this implies no citations to external sources or to other sections of the Chapter. Figure legends and captions need to contain all necessary information regarding data sources. [Thomas Stocker/ WGI TSU, Switzerland]	Noted.
0-135	0	0				FAQ general: The reserved, formal uncertainty language (see IPCC uncertainty guidance) must be avoided in an FAQ, because we don't expect the target audience with the AR5 uncertainty guidance document. Please formulate using, easily understandable alternative terminology in these instances. [Thomas Stocker/ WGI TSU, Switzerland]	Noted and agreed. Will be addressed for SOD.
0-136	0	0				Figures general: A consistent use of stippling/hatching to show model agreement/disagreement is required both within and between chapters to the extent possible. Sometimes stippling/hatching is used to indicate where there is agreement, sometimes where there is no agreement. The significance levels used are inconsistent between figures and between chapters (1 sigma, 2 sigma, etc.). [Thomas Stocker/ WGI TSU, Switzerland]	Agreed. Consistent approach will be considered for the SOD by the relevant Chapters.
0-137	0	0				Figures general: Please ensure units are provided for all colour scales and are indicated on the axis. [Thomas Stocker/ WGI TSU, Switzerland]	Noted and agreed. Will be addressed for SOD.
0-138	0	0				Citing sections from BAMS State of the Climate 2010 (Arndt et al. 2011) and other years: As noted above I think it would be better for the reader and fairer for the section authors if the specific section of BAMS State of the Climate was cited rather than the whole document. I have commented on cases within the sections I have reviewed but there are likely more cases than this. [Kate Willett, UK]	Agreed. Specific references for report sections (including BAMs and IPCC reports) are required and will be included for the SOD.
0-139	0	0				In my reading of (some of) the chapters, I encountered emissions and reservoirs being referred to sometimes in terms of tonnes of carbon and sometimes tonnes of CO2. The more this can be harmonised across the report (including with the other WGs) the easier it will be for the reader, especially non-specialists. [Richard Wood, UK]	Noted. For carbon, Petagrams of Carbon (PgC) is the unit dominantly used in the assessed scientific literature, and will thus be used in the SOD of WGI AR5.
0-140	0	1	1	1		medinibhandari@hotmail.com [Medani Bhandari, Nepal]	Comment unclear. No action.
0-141	0	1	1	200	70	There are two major comments for WG1AR5 FOD [PROF. YEHIA HAFEZ, Egypt]	Noted.
0-142	0	1	1	200	70	1- The anomalies studies and figures must be taken for the mean period (1981-2010) instead of (1961-1990). [PROF. YEHIA HAFEZ, Egypt]	Rejected -- The AR5 depends very heavily on CMIP5 projections. This experiment used data up to and including 2005 to initialize projections. Years after this are part of the projection period. 2005 is therefore the

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							latest year that can be included that separates the projection period from the non-projection period. When assessing models it is important that the assessment period covers only non-projection years, for which a larger amount of observational estimates of external forcing are available and were used to force the models. It was also decided that one core period be used for assessment and as a reference period for the projections. Hence inclusion of years after 2005 was not considered as favorably as the inclusion of years in the core period chosen.
0-143	0	1	1	200	70	2- All The figures in AR5 for the future periods must be ended by 2050 only instead of 2100 or more. To elliminate and avoid the uncertainty. [PROF. YEHIA HAFEZ, Egypt]	Rejected -- The assessment of uncertainty, e.g., of future climate projections will be provided for all projections, i.e. before and after 2050. In particular, uncertainty beyond 2050, is a key component of the WGI AR5 as obvious from the IPCC Panel approved outline with Chapter 12 dedicated to "Long-term Climate Change: Projections, Commitments and Irreversibility". More specific guidance on the treatment of uncertainty in the IPCC AR5 is available from Mastrandrea et al., IPCC 2010.
0-144	0	1	1	200	70	these two comments will made this report more recent and liable to the real. Since the last 10 years had a global warming and notable climatic changes.In addition to that IPCC using science and technology modelling of 2011. we have not the science and technology of 2015 as example. Every think (science, our knowelge of climate change and climatic models) will change through the next only 10 years. So do not talk about more than 2050 for saifty. e.g (No anyone since only one year had expected that there is a speed more than the speed of light). Now we have particles have a speed more than the light itself. So please all the chapters (1-14) must rewritten in this sence. [PROF. YEHIA HAFEZ, Egypt]	Rejected - The assessment of uncertainty, e.g., of future climate projections beyond 2050, is a key component of the WGI AR5 as obvious from the IPCC Panel approved outline with Chapter 12 dedicated to "Long-term Climate Change: Projections, Commitments and Irreversibility". More specific guidance on the treatment of uncertainty in the IPCC AR5 is available from Mastrandrea et al., IPCC 2010.
0-145	0	1	1	200	70	I WILL WRITE MY COMMENTS FOR CHAPTER ONE ONLY UNTILL YOUR TEAM MAKE THE ABOVE MODIFICATIONS THROUGH ALL CHAPTERS (1-14). [PROF. YEHIA HAFEZ, Egypt]	Noted.
0-146	0	1				Comment to full document: Executive summaries have different styles [Larry Thomason, United States of America]	Noted - consistency in the styles of the Executive Summaries throughout the WGI AR5 contribution will be a focus of the SOD development.
0-147	0	1				Comment to full document: To me, there is inconsistent use of the AR5 uncertainty protocol from chapter to chapter [Larry Thomason, United States of America]	Noted - consistency in the implementation of the AR5 uncertainty guidance note (Mastrandrea et al. IPCC 2010) will be a focus of the SOD development.
0-148	0	2	5			"human activities being the primary driver in the concerns about climate change" could be "human activities being the primary driver in the concerns about current (or ongoing) climate change" [José Daniel Pabón-Caicedo, Colombia]	Taken into account - this sentence in Chapter 1 has been revised.