

## IPCC Working Group I Fourth Assessment Report

### *Expert Review Comments on First-Order Draft*

## Chapter 9

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

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

### Batch AB



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1	A	0:0	0:0	Great read, I particularly liked your use of appendices and tables [Piers Forster]	Noted - thanks
9-2	A	0:0		Nomenclature: Replace "bottom-up estimates" and "top-down estimates" [of radiative forcing] with "forward calculations" and "inverse calculations." The terms "forward" and "inverse" explicitly point to the nature and logical status of these two very different types of estimates. In contrast, "top-down" and "bottom-up" constitute jargon and convey little sense of the logical distinction. (See also next comment for further explanation.) [Theodore Anderson]	Accepted.
9-3	A	0:0		Excellent chapter. Table 9.1.1 is very useful. [Richard Anthes]	Noted - thanks
9-4	A	0:0		Why is the title "Understanding and Attributing..." , why not the classical couple "Detection and Attribution..."? "Understanding" is not very well defined. Climate change can be detected without being understood. [Fons Baede]	Noted. Chapter title and scope determined by AR4 scoping process.
9-5	A	0:0		Well written in general, but often too many details which makes it sometimes difficult to read. The main line and focus on detection and attribution of anthropogenic climate change is often lost due to all the details, the many numbers and the many individual studies addressed. For examples see below. [Fons Baede]	Noted – substantial parts of chapter rewritten.
9-6	A	0:0		The Chapter overlaps with other chapters, in particular Ch 3, 8 and 10. Examples see below. [Fons Baede]	Noted – overlaps reduced.
9-7	A	0:0		To my mind there is a weakness in the argumentation at several points in Ch9. The same issue arose in the TAR and does not seem to have been adequately addressed here. The assumption is made that the temporal pattern of aerosol forcing is known. It is asserted that the actual magnitude may be uncertain, but it is the temporal pattern that is key. However, nowhere in Ch2 is any information presented that shows that we can quantify the temporal changes in aerosol forcing. On the contrary, it is shown that the present day aerosol forcing lies between about -0.5 and -2 Wm <sup>-2</sup> while very little is said about the reliability of the temporal pattern, which presumably we know even less about than the present-day forcing, which can at least be observed. The only statement I could find in Ch2 was "Aerosol and ozone RF time-histories remain too uncertain to ascertain an accurate time-evolution of RF beyond the examples given in Figure 2.9.3." I see where this idea comes from, but it is flawed in my opinion. You are assuming that the uncertainty in forcing arises from examining the forcing in a range of models (which gives a seemingly large "error bar"), but that the same pattern is a robust feature of each	Impact of uncertainty of time evolution of aerosol forcing now discussed in 9.2



No.	Batch	Page:line		Comment	Notes
		From	To		
				model. Therefore you can simply scale the forcing up and down while maintaining its pattern. This approach is not justified based on the conclusions of ch2.G17 [Kenneth Carslaw]	
9-8	A	0:0		There are many processes that might change the temporal pattern of aerosol forcing which, chapter 2 touches on but doesn't really address because nobody has a model of sufficient complexity to do the calculation yet. The fact is, different models produce the same pattern of forcing because they all rely on the same emissions, approximately the same aerosol microphysics, and very crude aerosol-cloud interaction schemes. To make the assertion that the temporal pattern of aerosol forcing is known you need to provide the evidence and to justify why there are no processes that can change the simple pattern that emerges from all GCMs. No such evidence is presented anywhere in the report. [Kenneth Carslaw]	See 9-7.
9-9	A	0:0		Admittedly you do discuss uncertainties at some length in various places. But the key uncertainty that is not addressed is that in the temporal pattern of aerosol forcing. I would like to see an answer to the question: what if the temporal pattern of aerosol forcing were allowed to vary within its present-day uncertainty range over the last 200 years? I believe it would be possible to construct an aerosol temporal forcing pattern that produced a maximum forcing in 2004. In fact, this may be supported by Ch2 in their statement "The net result of these combined regional reductions and increases leads to uncertainty in whether the global SO <sub>2</sub> have risen or fallen since the 1980s". If emissions have risen and led to an increased direct and indirect RF since 1980 then the basis for your argument (that aerosol forcing has levelled off since 1980) would no longer hold. There are other results in the entire report that might be used to bolster your argument, but the other chapters don't make much of them, presumably because the uncertainties are too large. For example, the brightening of the surface since the 1980s suggests that aerosol forcing may have indeed levelled off or even reversed, but the link between this observation and aerosol changes remains tenuous at present. [Kenneth Carslaw]	See 9-7.
9-10	A	0:0		The section on sea level rise (9.5.1.3) is inadequate. [John Church]	As revised version of 5.5.7 now included in Ch 9.
9-11	A	0:0		I have to say that I was somewhat confused by the contents of this chapter given the prominence of the word "understanding" in the title. The chapter is dominated (with the exception of section 9.6) by discussion of formal detection and attribution studies. While these are of high importance (and I am an admirer of the way this group has formalised the approach to D&A) they are only part of the story in terms of the way we make sense of climate variability and change and use models and theory to build confidence in the predictions reviewed in chapters 10 and 11. There are a number of types of studies (e.g.	We have taken this point into consideration in revising the chapter. Some studies of this kind are taken into consideration, but space does not allow complete coverage.



No.	Batch	Page:line		Comment	Notes
		From	To		
				atmosphere models forced by prescribed SSTs, cloud resolving model experiments) which have been used to advance understanding. Perhaps these are reviewed in other chapters - if so then perhaps some pre-amble could be included to alert the reader to their whereabouts. Perhaps it would have been better to stick with the TAR heading "Detection of Climate Change and Attribution of Causes". Obviously the chapter is very long, but much of the discussion is rather in-depth (approximately 1 paragraph per cited paper) so could benefit from considerable editing down. [Matthew Collins]	
9-12	A	0:0		I suggest the inclusion of a work by Pasini et al. appeared on Ecological Modelling ( <a href="http://dx.doi.org/10.1016/j.ecolmodel.2005.08.012">http://dx.doi.org/10.1016/j.ecolmodel.2005.08.012</a> ) [Tiziano Colombo]	Noted. Paper assessed and decision taken not to discuss this work.
9-13	A	0:0		Very well documented and organized chapter. This is the first chapter to give importance to remaining uncertainties so that scientists and policy makers could understand better the situation and so get prepared by developing capacity building and appropriate control measures [Savitri GARIVAIT]	Noted - thanks
9-14	A	0:0		General The authors are far too "confident" of the value of models. They need to realise that no climate model has ever successfully predicted any future climate sequence and there seems to be a reluctance even to try it. Also a correlation, however plausible does not prove a cause and effect relationship, and does not make such a relationship "very likely" or "robust". It merely indicates a possible explanation. Every simulation involves adjustment of model parameters, so every single one is a different model. They cannot all be right, but they could all be wrong. [Vincent Gray]	Noted and rejected. We do not rely on simple correlation to prove cause and effect. Models used for climate simulation are, for example, able to reproduce the evolution of the climate of the 20 <sup>th</sup> century using only specified external forcings. Assessment of model skill in predictive mode is primarily a ch 8 issue. However, we note that coupled climate models are being used for seasonal to decadal forecasting, either experimentally, and in some cases operationally.
9-15	A	0:0		General This Chapter evades the difficulties faced by all models. They include an inability to explain why the greenhouse effect should suddenly begin in 1990, for surface readings, and in 2001 for the lower troposphere; why does the Arctic heat and the Antarctic cool, why does the land heat more than the sea, and how can the ocean be heated since 1975 by greenhouse gases in the atmosphere which only began to warm the surface in 1997? [Vincent Gray]	Noted and rejected. No evidence provided by the reviewer for most of these points. For example, where does the idea come from that greenhouse gases only began to warm "the surface in 1997"? We do discuss the reasons for differential land/ocean warming and



No.	Batch	Page:line		Comment	Notes
		From	To		
					lack of warming in parts of Antarctica.
9-16	A	0:0		General There should be some clarification as to what is meant by the term "anthropogenic". Does it mean emissions of greenhouse gases, only, or does it also include such human activities as building houses and factories, consuming fuel, draining wetlands, or removing forests? The distinction should be made more clearly, particularly in the publicly quoted statements of the IPCC. [Vincent Gray]	Noted. The glossary provides a definition. We qualify this word (e.g., by talking about anthropogenic ghg's) where appropriate.
9-17	A	0:0		General There is no scientific basis for what is "likely", "virtually certain". Etc. which are nothing more than subjective guesses. I have changed all of them to the correct qualitative terms. [Vincent Gray]	Rejected – these are expert assessments based to a large extent on quantitative studies. The terms are used throughout the chapter in the approved IPCC manner.
9-18	A	0:0		In spite of these few remarks, I found this chapter clear and informative. I think it meets the IPCC requirements. [Stéphane Hallegatte]	Noted - thanks
9-19	A	0:0		This chapter is extremely well written. I have very few comments to make. [Patrick Hamill]	Noted - thanks
9-20	A	0:0		I would like to CONGRATULATE the authors for their comprehensive survey on probabilistic assessment of climate sensitivity. This represents a key improvement as against TAR. Maybe, the pioneering role of Allen, Nature, 1999, Forest et al., Science, 2002, and Knutti et al., Nature, 2002, could be highlighted more. [Hermann Held]	Noted – thanks. All of these papers have been cited.
9-21	A	0:0		Overall a very well written chapter. [Gareth S. Jones]	Noted – thanks.
9-22	A	0:0		This Chapter uses the terms "Late Maunder Minimum" many times as the period of time that is associated with negative temperature anomalies during the late 17th and early 18th centuries deduced from various climate proxy indices. "Maunder Minimum" should only be used in reference to solar activity. It is vital that another expression be used to refer to the climate conditions of the late 17th-early 18th centuries. The Maunder minimum is a term first coined by J.A. Eddy, (Science 1976) to describe the period in which the Sun had a prolonged sunspot minimum, first noticed by E.W. Maunder in the 1890's. The Maunder Minimum is now generally used to describe the period of very low observed sunspot numbers between 1640 and 1720. Continued on next row... [Gareth S. Jones]	Noted. This term now used only to refer to a solar forcing period. The climatic period is now referred to either as the "little ice age" or by time period.
9-23	A	0:0		...continued from previous row The term "Late Maunder minimum" has been used in a few published climate studies	Noted – see 9-22.



No.	Batch	Page:line		Comment	Notes
		From	To		
				examining the last several hundred years, so it could be argued that there is a precedent for its use here in the context of a specific period of cool climate. However the use of an incorrect term in the past does not defend its use now, and it is vital that such a prestigious document as the IPCC FAR does not propagate the error. Its use is confusing and misleading, especially when used in a chapter that also refers to solar irradiance changes. It also suggests a-prior belief that the cool period (17-18th C) is due to lower solar activity. This is a debatable claim, but using the term may mislead a reader into believing that it is proven. Chapters 1,2 and 6 each use the term "Maunder Minimum" correctly. [Gareth S. Jones]	
9-24	A	0:0		Chapter 2 and chapter 10 define climate sensitivity parameter and climate sensitivity differently. The first is basically the temperature change per forcing whilst the latter is temperature response for a doubling of CO <sub>2</sub> . Within this chapter 9, what sensitivity is used should be defined more carefully and near the start to avoid any confusion. [Gareth S. Jones]	Definition given in 9.6
9-25	A	0:0		The draft chapter on attribution has given greater attention to attempts to attribute regional change to GHGs and other agents. Consideration of natural variability as a confounding factor in attribution studies remains a key consideration. In the current draft, consideration of natural variability even on a global scale appears contingent on a narrow set of literature (e.g. Figure 9.4.3 based on a to be submitted paper), perhaps narrower than need be the case. Furthermore, extending attribution to regional effects entails even greater detail in examination of assumptions with regard to, for example, assumed accuracy of model variability statistics on a regional scale. Suggest that if judgments are to be given on attribution, e.g. at a regional scale, all important assumptions be considered and a traceable account of how these assumptions have been incorporated into probability judgments be included. Otherwise it is unclear if probabilities conveyed are contingent probabilities, or if they are indeed best judgments. [Haroon Kheshgi]	Noted. Somewhat more attention is paid to the assessment of internal variability on global and regional scales. Consistency between simulated residual variability is evaluated in most detection studies, global and regional. Expert assessments on attribution, both global and regional, do attempt to account for uncertainties, with a conscious attempt to downweight conclusions when uncertainties can not be fully evaluated.
9-26	A	0:0		I get the impression that the search for a GHG signal of climate change in variables other than global and hemispheric surface temperature has tended to involve looking only for linear trends. Just as detailed analysis has indicated that the various human forcings did not create a linear increase in surface temperature, it would seem likely that the changes in other variables will not be linear through the 20th century. So, we should not be expecting to see linear trends in hurricanes or linear trends in circulations statistics (like the NAO), but should expect that there will be a more complex pattern (e.g., the NAO being influenced by the comparatively intense sulfate aerosol forcing in the North Atlantic region during the mid-20th century). To recognize this likelihood, quite a number	Some discussion has been added to 9.1 to point out that failure to detect may simply be due to limitations of the diagnostic used.



No.	Batch	Page:line		Comment	Notes
		From	To		
				of findings in the chapter need to be more qualified about whether there has or has not been a human influence and what the explanations are of past variations (quite likely, they are not fully natural). As it was for the MSU issue, IPCC should be quite cautious here in coming to conclusions about what does or does not show a human influence, especially now that it is recognized that there has been a human influence on surface temperature through at least most of the 20th century. [Michael MacCracken]	
9-27	A	0:0		Opening Comment: In the Chapters that I am reviewing, I choose to not provide an anonymous review. This choice allows the various Chapter authors to contact me directly on matters of errors, concepts, or questions of disagreement. I have already performed thorough reviews of chapters 1-5. Due to the looming November 4th deadline for reviews, I am choosing to review Chapters 6-11 in a drastically shortened way. Rather than going through all of them as I did before, I am choosing to review only the Executive Summaries of chapters 6-11. There are some clear advantages for this strategy, independent of the obvious one of speeding up the very tedious reading and reviewing process. In the previous chapters I have reviewed, I have seen some significant disconnects between two obviously differing reporting strategies. First, it seems obvious to me that the fundamental purpose of these IPCC FAR reviews is to establish the case, or lack thereof, for many of the diverse aspects of the human-caused global warming problem. Second, it is noteworthy that this draft WG1 report is roughly twice as long as the WG1 IPCC TAR report. Third, it seems very obvious that the key IPCC assessment-relevant punchlines are hardly double those of IPCC TAR. It seems clear to me that the global-warming research-advancement doubling time scale is a lot closer to twenty years than it is to five years. The obvious conclusion for me is that we don't really need or desire to double the length of the WG1 chapter assessment every five years! For these nearly obvious reasons, and to help me and the other reviewers refocus on the fundamentally important conclusions that are centrally relevant to the IPCC's human-caused climate assessment's goals, I am thus choosing to reduce drastically my own submitted WG1 reviews. And, most importantly, this gives me a good shot at reviewing meaningfully all of remaining chapters 6-11 by the daunting November 4th reviewers' deadline. [Jerry Mahlman]	Noted.
9-28	A	0:0		I very much appreciated being able to review this Chapter 9. Its content is very close to the climate science that I deeply enjoy and respect. Indeed, the challenge of understanding and attributing the key clues as to how our body of climate- warming science is evolving and growing is clearly a testament to the many scientists who have contributed to this critically important chapter. It has been a privilege to read and evaluate many aspects of the Chapter. Unfortunately, time prohibits my reading the entire	Noted. We are on target for chapter length.

Confidential, Do Not Cite or Quote

Chapter 9: Batch AB (11/16/05)

Page 7 of 186



No.	Batch	Page:line		Comment	Notes
		From	To		
				Chapter, as does its unfortunately excessive length. This reviewer found its length, and the looming deadline, to diminish one's motivation to read it in the detail to which I would have preferred. My sincerest congratulations to the many participants who have contributed importantly to this very challenging, and emerging, diagnostically focussed discipline. [Jerry Mahlman]	
9-29	A	0:0		This is a nicely written chapter, but it should lay the foundation for WG2 chapter 19 on key vulnerabilities. The latter chapter has essentially done its own analysis of observed (and expected) changes of key global features. It would be cleaner and scientifically appropriate for that chapter to simply refer to this one. These two chapters should provide major outcomes for the AR4, and so they should be linked. [Michael Manton]	Noted. We have referred this comment to David Karoly, WG1-WG2 liaison.
9-30	A	0:0		The Chapter 9 Figures are excellent compared to those of Chapters 1 and 2; however, some figures (e.g., the axis labels of 9.5.3-9.5.5) could use improvement. [Lourdes Maurice]	Noted. Figures have been further improved.
9-31	A	0:0		The summary sections are generally a regurgitation of the information given in the section. The summaries should offer some definitive conclusions to policy makers. [Lourdes Maurice]	Noted – but summaries are there because we think a simplified overview of each section does provide information for decision-makers.
9-32	A	0:0		An overall problem with the chapter is confusing and inconsistent "quality" statements; while the authors have often used the IPCC terms "likely" "very Likely" etc they also readily drop in other "quality" terms such as "pervasive", "clearly indicate", "robust", "compelling evidence", "clearly identified", "substantially increased", "appear to be consistent" etc etc .. There is an obvious need to clean up the terminology and when/if likelihood statements are not made to have a very clear definition of the statement that is used. Despite this shortcoming the executive summary has done a reasonable job in actually summarising the chapter and does provide an assessment rather than a review. There is obvious overlap with Chapter 2 and Chapter 6 and I am confident rationalisation will occur. I also have a concern that the various "univariate" detection/attribution studies tend to be combined into a "multivariate" statement. Surely the fact that physical variables behave in physically consistent manner is important but no true multivariate study seems to have been made (or have I misunderstood something?). [Bryant McAvaney]	The terminology used in ch 9 has been vetted. Multivariable assessments, where we make them, are supported by physical arguments. Unfortunately, multivariable detection studies that simultaneously consider, for example surface temperature and sub-surface ocean temperature, are not yet generally available.
9-33	A	0:0		This entire chapter is extraordinarily verbose and poorly written. It repeats information from other sections, which is better presented there. [Stephen McIntyre]	Noted. The chapter has been revised. However, we note that this comment disagrees with general comments from most other reviewers.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-34	A	0:0		The key d&a issue is how you can distinguish solar forcing from greenhouse gas forcing. Explain this clearly both in the summary and in the introduction. [Stephen McIntyre]	Differences in the various forcings are explained in 9.2.
9-35	A	0:0		Overall this chapter is very weak, but it is weak in ways that would only be apparent to your lead authors if you included in that group some who were not so obviously committed to the validity of the single line of evidence being presented. The chapter juxtaposes many strong claims of detection of anthropogenic warming, using words like "pervasive", and "very likely" etc with quiet caveats that fully undermine such confidence. Even though the caveats are stated herein in the most minimal terms, they are sufficient to indicate to all but the most inattentive reader that the strong conclusions are overstated. When we unpack the actual arguments in the chapter it all turns out to rest on the weakest possible form of evidence: loose coherence between the behaviour of some models at a very aggregate level and data presented at a very aggregate level. Even at that, the coherence is adjudged after consideration of only a fraction of the potential explanatory mechanisms, ignoring all the signs of incoherence (without ever stating what degree of incoherence would suffice to falsify the working hypothesis), ignoring the fact that there are far more free parameters available for adjustment than there are data points to be fit, and presenting the methodology in enthusiastic, uncritical terms that only serve to raise mathematical doubts about what is actually being done. I will present these issues as best I can, and offer a few specific comments on individual pages. But since this chapter is the foundation for the overall position of the IPCC, and in turn for the many governments around the world setting policy based on IPCC reports, I would have expected a conscientious attempt to present a balanced, critical and cautious survey. That is not achieved in this chapter. If the lead authors believe the case herein to be validated by the volume of signal detection papers and the (relatively brief) time they have dwelt in the climate literature, I would respond by pointing out that the apparent volume all resides in one paradigmatic stream that has apparently received little scrutiny by people truly outside the IPCC community, and what little scrutiny it gets from those inside the community comes from people who share so many supportive assumptions as to confine actual debate to mere secondary issues which could not, in themselves, challenge the overall paradigm. [Ross McKittrick]	Noted – we will revise sections where reviewer provides specific comments that demonstrate lack of balance. We note that this comment disagrees with general comments of nearly all other reviewers. The formal detection studies on which this chapter mainly rests have been available in the refereed literature for over a decade (with increasing numbers of such studies over the last decade). There has been ample time for people outside the IPCC community to scrutinise and dispute these findings. No paper exists in the refereed literature, to our knowledge, that demonstrates that any of the formal detection studies are incorrect.
9-36	A	0:0		I was asked by the IPCC support unit to comment on the use of probabilistic language in this chapter. In general, I would say that the confidence levels are overstated. I take particular issue with the statement that it is "virtually certain" (9-3 line 7) that recent warming is not due to natural internal climate variability (NICV) alone. [Timothy Palmer]	Noted. Confidence levels have been re-assessed.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-37	A	0:0		The detection and attribution philosophy depends critically on being able to diagnose NICV from climate model integrations. This chapter refers to the previous chapter in support of the use of climate models. However, the analysis within this chapter, that climate models, whether forced or unforced, are unable to simulate the observed trends in the NAO, is reason enough not to be “virtually certain” about recent temperature trends not being due to NICV. [Timothy Palmer]	Rejected – the reviewer does not indicate how changes in the NAO could lead to all the climate changes that have been observed in the global temperature pattern in the ocean and atmosphere. We are unaware of any paper in the literature that demonstrates how an internal climate variability process could lead to the sort of climate changes we have seen. Nonetheless, we have added additional discussion on internal variability.
9-38	A	0:0		As mentioned above, there is a fundamental problem with all climate models used in AR4: they are based on deterministic bulk-formula parametrisations whose theoretical foundation is based on the notion of statistical equilibrium, which, at least in areas of strong diabatic forcing, is rarely attained. A consequence of such bulk-formula parametrisations is that considerable more energy is dissipated in climate models than in reality - energy that might otherwise be available to the large-scale atmospheric circulations. [Timothy Palmer]	Noted. Perhaps more relevant to chapter 8?
9-39	A	0:0		A specific example occurs when considering parametrisation of deep convection. Conventional parametrisations will diagnose convective instability and adjust the temperature and humidity profile inside the appropriate grid box. The available potential energy is implicitly converted to kinetic energy of (sub-grid) overturning circulations which is then assumed dissipated in the grid box when the adjustment to convective neutrality is reached. In practice, however, especially in cases of strong convective instability, the overturning kinetic energy is in part converted to rotational kinetic energy (Lilly, D.K., 1983: Stratified turbulence and the mesoscale variability of the atmosphere, JAS, 534, 2475-2492). This rotational kinetic energy is then capable of propagating upscale affecting mesoscale, synoptic scale, and larger scales. The MJO is thought to involve self-similar upscale interactions (cf diagnoses of George Kiladis, personal communication), and the inability of conventional parametrisations to represent such generation of sub- and near gridscale energy, rather dissipating it within the grid box, is a possible reason for the relatively poor performance of climate models simulating the MJO (with consequences for El Nino and global mean temperature). [Timothy Palmer]	See 9-38.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-40	A	0:0		Boundary layer momentum transfer is another parametrisation where there is almost certainly too much dissipation of energy in climate models. [Timothy Palmer]	See 9-38.
9-41	A	0:0		<p>Clearly one solution to this problem is (substantially) enhanced resolution so that deep convection is properly resolved - another is to backscatter some of the energy onto the grid that would otherwise be dissipated by the sub-grid parametrisations. Two recent studies that have assessed the effects of adding stochastic forcing at the near and sub-grid scales to represent such backscattering are:</p> <p>Palmer, T.N., Shutts, G.J., Hagedorn, R. Doblas-Reyes, F.J., Jung, T. Leutbecher, M., 2005: Representing model uncertainty in weather and climate prediction. Annual Review of Earth and Planetary Sciences, 33, 163-193.</p> <p>Jung, T., Palmer, T.N. and Shutts G.J., 2005: Influence of a stochastic parametrization on the frequency of occurrence of North Pacific weather regimes in the ECMWF model. GRL accepted for publication</p> <p>In the first paper, the impact on blocking frequency was assessed, in the second the impact on weather-regime frequency was assessed. For example, in the latter, it was found that without the stochastic backscatter parametrization, the model underestimated the occurrence of some of the sub-dominant weather regimes, and overestimated the occurrence of the dominant westerly regime (since the latter was strongly baroclinically unstable, this did not imply an underestimation of geopotential height variance). With stochastic parametrization, the frequency of occurrence of the sub-dominant regimes was much better simulated.</p> <p>Neither of these studies addressed decadal or longer-timescale NICV. However, it is entirely plausible that by making climate model parametrizations less dissipative, by backscattering some of the energy that reaches the sub-grid scales, estimates of NICV on decadal and longer timescales could be significantly enhanced - possibly by more than a factor of two for some quantities. For example, if the trend in the NAO is associated with forcing in the tropics, and the upscale energy cascades associated with convective to mesoscale and larger scales are being systematically mishandled in the tropics due to over dissipation, then with better representation of these scale interactions, either in models which resolve deep convection, or models with non-bulk-formula parametrizations, it may well be the case that the trend in the NAO can be modelled by NICV. Since convection is so pervasive, it is therefore entirely plausible that with a less dissipative representation of convective and other processes in the atmosphere, some low-frequency trends in global mean temperature will also be simulated by NICV. At the present we can't say that it will, but we equally can't say that it won't, hence the need for caution in the chapter.</p> <p>[Timothy Palmer]</p>	Noted – such a process could not, in our opinion, produce the wide range of very strong warming trends in global means and the wide-spread presence of these trends across regions and in ocean and over land. We have added a discussion of implications of wide spread warming of diverse parts of the climate system in 9.1.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-42	A	0:0		<p>[An attempt to quantify the inaccuracy of conventional parametrization is by coarse-graining cloud-resolved models. A paper Shutts, G.J. and T.N.Palmer, 2005: Statistical fluctuations in convective forcing computed from a cloud-resolving model and their relevance to the parametrisation problem. <i>J.Clim.</i> submitted has just been submitted to <i>J.Clim.</i> but I guess it is unlikely to be accepted by the time of the deadline for AR4.]</p> <p>This comment impacts on almost all aspects of this chapter and therefore I suggest more cautious wording of the confidence levels that one attaches to the detection attribution studies, and would recommend not to use the phrase “virtually certain”.</p> <p>[Timothy Palmer]</p>	Noted. These papers appear to be more relevant to Ch 8.
9-43	A	0:0		<p>While the latter part of the twentieth century is well covered, the first half half of the twentieth century (the warm forties) is only given a cursory treatment.</p> <p>The detailed spatio-temporal simulations of the latter part of the twentieth century are consistent with standard GAVS forcing attributions and hence throw no light on natural (chaotic) multi-decadal changes. Chapter 9 refers to the fact that static conditions at the last glacial maxima are consistent with GV and Milankovitch Solar. Left unexplained are the ‘natural/chaotic’ physical mechanisms that track the transition between the glacial-interglacial ‘equilibrium’ states.</p> <p>[David Ritson]</p>	Noted. Treatment of first half of 20 <sup>th</sup> century considered during revision.
9-44	A	0:0		<p>Peter Stott is one of your lead authors and was lead author in Stott et al (2000). Stott(2000) provided an excellent spatio-temporal comparison of observations and simulation results based on GAVS. This along with other results appeared to indicate unexplained warmth in the 1910-1940 centered in the Atlantic and North America, which has been the object of intensive speculation and might well provide a handle on multi-decadal natural fluctuations. Certainly this area deserves more coverage and discussion than is currently provided by Chapter 9.</p> <p>Parenthetically Stott (2000) provided spatio-temporal maps showing ‘Observed’, ‘Simulated’ and ‘Simulated-Observed’ data. The chapter 9 Figure 9.4.2 would be much more useful if a third column of ‘Simulated-Observed’ plots was provided, additional to the columns giving observed and simulated results.</p> <p>[David Ritson]</p>	Noted. See 9-43.
9-45	A	0:0		General comment: overall this chapter is in good shape. The authors have done an excellent job in bringing together the material. I have nevertheless critically reviewed the	Noted – thanks.



No.	Batch	Page:line		Comment	Notes
		From	To		
				manuscript, such as to strengthen its argumentation and clarity. [Christoph Schar]	
9-46	A	0:0		This chapter totally neglects the observational-statistical view of attributing forcing factors to climate change. Being an expert in statistical analysis but not in climate modelling I have not reviewed this chapter. [Christian-D. Schoenwiese]	Noted. Although the chapter rests mainly on formal detection/attribution studies, some less formal studies are discussed.
9-47	A	0:0		These are comments on Chapter 9 only. Comments on other sections will be provided later. Quotations from original in boldface type Throughout. Be consistent in use of first person plural: Page 3, line 25: "our confidence" That of the working group? Line 38: " We now have 6 more years of data". We the working group? We the scientists of the world? Page 6 line 20: "We have improved our understanding": That of the working group or of climate scientists generally? Page 10, line 42: Our knowledge: That of the working group or of climate scientist generally? Page 19, line 49: Our motivation for studying... That of the working group or of climate scientist generally? Page 22, line 22: Increases our confidence in our understanding. The working group's confidence in their own understanding? Why not: Increases understanding in present understanding? Page 24, line 39: We have substantially increased our understanding: We the working group? We the scientists of the world? Much better to say "Six more years of data are available"; "Understanding of the causes of uncertainty has improved." and the like. [Stephen E Schwartz]	Noted. Thanks. These points taken into consideration.
9-48	A	0:0		General remark: Recently another simulation covering the period 1500-2000 has been published (Stendel, M., I.A. Mogensen and J.H. Christensen, 2005a: Influence of various forcings on global climate in historical times using a coupled AOGCM. Clim. Dyn. 25, 10.1007/s00382-005-0041-4). In this study, the coupled model ECHAM4-OPYC has been used. ECHAM4 was run with a higher resolution than the ECHO-G runs cited (T42 instead of T30). Contrasting other coupled GCM studies, latitudinally-dependent volcanic forcing and temporally-variable vegetation have been used as forcing data. Data can be obtained from the first author of this paper (mas@dmu.dk). [Martin Stendel]	Noted. Thanks. Simulation now included.
9-49	A	0:0		Chapter 9: General comments. My overall comment on this chapter is that it is dominated by a statistical approach which, although necessary, perhaps does not contain enough physics and dynamics. There is a tremendous reliance on climate models without taking into account the known serious flaws in them, and this ought to be built into the assessment. It means that attribution of things done poorly, like all aspects of the hydrological cycle, are unlikely to	Noted. The revised chapter includes more discussion of physical consistency and physical arguments. There is now a bit more discussion of surface temperature and rainfall combined. We do rely heavily on D&A studies using



No.	Batch	Page:line		Comment	Notes
		From	To		
				be possible using this approach. There is too much blind acceptance of model results. There is a lot in the chapter on forcings and their links to climate yet almost nothing on feedbacks and how those affect uncertainties. Hence basic things like the strong negative correlation between surface temperature and rainfall over land, most notably in summer and the tropics, are ignored and temperature is discussed independently of precipitation. [Kevin Trenberth]	models simulated signals, but attempt to take all sources of uncertainty, including model uncertainty, into account in making expert assessments.
9-50	A	0:0		Hence the fact (noted in Chapter 3) that the reduced warming in the eastern United States is associated with wetter conditions (and more cloud and more latent vs sensible heat etc. etc) is missing. [An example of what I mean by absence of physics]. On p 35 there is a brief mention of this with respect to Australia. DTR and links to cloud is another case in point and again this is on p 35. Both of these are under the heading of "studies based on indices for temperature change" yet they should be fundamental considerations in 9.4.1 and 9.4.2. [Kevin Trenberth]	Noted – see response to 9-49.
9-51	A	0:0		Several sections have quite long summaries after each. I believe it is appropriate to have very short summaries but most of the material in the summaries should be in the executive summary and it is unnecessarily duplicative. Especially, Section 9.4.5 seems to be redundant. [Kevin Trenberth]	Noted – see response to 9-31.
9-52	A	0:0		Several parts of the Section 9.5 are much less thorough than earlier sections and either repeat or are even at odds with the observational chapters 3, 4 and 5. Rather than selectively reviewing the literature in Chapter 9 it would be much more efficient and consistent to use the material in Chapters 3-5. [Of course they were done in parallel so this is difficult]. From Chapter 5 it would be much better to use the assessment in 5.5 (in my view) than in 5.2 (which is just Levitus) as 5.5 includes other studies (Ishii and Willis) as well as Levitus (Section 9.5.1.2). The material on sea level is especially weak in 9.5.1.3 and contradicts the accelerated rise in sea level since 1992 that exists (whether it continues is another matter). Similarly 9.5.1.4 should refer more to Chapter 5 and Fig. 5.6.1 for a more complete assessment of salinity changes. The section on the THC is also weak (but it is in Chapter 5 too). In 9.5.2.1 it should note that models do ENSO poorly, so how can you tell whether there is a change or not? [Kevin Trenberth]	We have checked consistency with other chapters and, where appropriate, have used assessments from chapters 3, 4, 5.
9-53	A	0:0		The climate shift in 1976 was an observed change: not simulated in any model. Also the result for ENSO change depends a lot on metrics used to measure ENSO: SST vs SLP vs precipitation. Precipitation changes may increase even as SST changes are reduced, for instance. Chapter 3 has more complete discussions of NAM and SAM and notes the spurious trends in NCEP and ERA-40 reanalyses in SAM that seem to be not accounted	Noted. Section 9.5.2 now defers to Ch 3 where appropriate.



No.	Batch	Page:line		Comment	Notes
		From	To		
				for in 9.5.2.3.and in Fig. 9.5.2. [Kevin Trenberth]	
9-54	A	0:0		Section 9.5.2.6 is very disappointing. It needs to discuss problems with climate models (related to subgrid scale parameterization of convection which onsets prematurely and with insufficient intensity in all climate models so that it prejudices the environment against organized convection, such as hurricanes) and simulations of tropical storms, and I strongly disagree with the wimpy conclusions. The Pielke et al (2005) paper has been completely rewritten (since Katrina) and in any case is still wrong. The expectations (Trenberth 2005 Science) and findings (Emanuel, Webster et al) along with model expectations (in spite of their likely underestimates) (Knutson and Tuleya) surely make a strong case for more intense storms but little change in numbers. This problem carries over to Question 9.1. [Kevin Trenberth]	Reviewer's assessment on this aspect is noted. However, we believe that it is still premature to draw strong conclusions.
9-55	A	0:0		There are also major conceptual problems in dealing with precipitation. I suggest that Section 9.5.3 should be redone based on expectations of why precipitation should change? See Trenberth et al (2003). With increased GHGs there is a small acceleration of E and P and thus the hydrological cycle, but that can easily vanish with aerosols in the scenario. Good heavens look at the vertical scale of Fig 9.5.4! But robust changes in precipitation intensity and frequency should be sought and are strongly evident in the observational record (Chapter 3), in contrast to what is stated here, and also are found in models where they are properly analyzed. This is robust and it is quite misleading to focus on precipitation amount where there is no clear expectation. This problem carries over into Question 9.1. [Kevin Trenberth]	Noted – we have added some discussion of expectations, but we note that we already do what the reviewer asks (page 9-47, lines 27-28 and 42-45). We have improved discussion on similarities between observed and simulated intensity and frequency changes, but note that the available literature is limited.
9-56	A	0:0		In Section 9.6.1.1 there is a recitation of 9 studies: one per paragraph, and no synthesis. There is a major need for an assessment here. [Kevin Trenberth]	Synthesis added.
9-57	A	0:0		Throughout: Please use NAM rather than AO as it is neither Arctic nor an Oscillation. [Kevin Trenberth]	NAM used throughout.
9-1439	B	0:0		This chapter is very interesting because it stressed the explanatory warming factors currently and future of the ground leading to the climate change. However, the " climate change will have to be moderated " because it is not yet effective. It should be waited until all the modifications observed are confirmed over a long period to affirm it. Because there are still assumptions which is not confirmed yet. [Expédit Wilfrid VISSIN]	Don't understand this point. Sorry.
9-58	A	1:1	1:3	The scientific merit of the IPCC Assessment Report would be substantially improved by simply deleting this chapter. Understanding is a prerequisite before any credible	Scope of report and chapter determined by AR4 scoping process.



No.	Batch	Page:line		Comment	Notes
		From	To		
				attribution can take place. The chapter starts by putting the cart ahead of the horse - attributions are made left and right without ever laying a foundation to stand on. The objective of the Assessment Report should be to present a clear and convincing documentation of climate change, and avoid becoming a punching bag for climate change critics and skeptics. The place to start is with the observed record of greenhouse gas increases. These GHG increases have physical consequences, i.e., the GHGs produce radiative forcing that is driving the climate system to a new equilibrium. And, there is a global temperature record that verifies that that is indeed what is happening. If, for political reasons, this chapter needs to be retained, it should be rewritten as a synthesis of what has been learned in the earlier chapters, and moved to the end of the Report. If written well, "attribution" will become a self-evident conclusion that is based on the facts presented. [Andrew Lacis]	
9-59	A	1:1	1:1	In the discussion about regional trends, there was no reference to Douglass et al. (2004) who showed that current climate models do not capture the zonal trends in atmospheric temperatures measured with MSU and with radiosondes. This type of reference and discussion are needed for balance and a more complete assessment. Douglass et al., Geophys. Res. Lett., 31, L13208, doi:10.1029/2004GL020103 [Patrick Minnis]	Noted. This reference has been reconsidered.
9-60	A	1:38	1:38	Need to spell out LGM for more casual readers (and maybe give times for both the LGM and mid-Holocene here). [Michael MacCracken]	Noted.
9-61	A	2:3	2:16	An excellent summary of the current position: the only improvement I can suggest is that "the known" should be inserted after "when" in 1.8. [William Ingram]	ES redrafted.
9-62	A	2:3	5:33	A good summary overall & I see no need to compress - but if compression is wanted, I suggest 39 on p 3 to 19 on p 4, as they discuss areas where there is less new useful information, & possibly the last 3 paragraphs on p 5. [William Ingram]	ES redrafted.
9-63	A	2:36	2:37	Good - very clear, & accurate - summary [William Ingram]	Thanks.
9-64	A	2:50	2:51	contribution ... is likely to have been larger than the observed warming" is counter-intuitive & will seem nonsensical to some readers - I suggest "effect" instead of "contribution" [William Ingram]	ES redrafted.
9-65	A	3:0	5:	Executive summary. The problems here reflect those in the main report and as discussed in my general comments, and I do not offer details. It does seem long.	ES redrafted. Sea level rise now mentioned.



No.	Batch	Page:line		Comment	Notes
		From	To		
				However, it is surprising that there is nothing on glacier melt, ocean expansion and sea level rise, or salinity. [Kevin Trenberth]	
9-66	A	3:0	6:0	SCIENTIFIC COMMENTS ON THE CHAPTER 9 EXECUTIVE SUMMARY First, it is of importance for me to note that the Chapter's 95 pages is probably a factor of two too many for this kind of assessment report. Chapter 3, however, was 119 pages long and the authors will likely be forced to cut it in half, or more. Be prepared to make your case for 95 pages, or SHORTEN IT! You might, however, be able to negotiate on space with the authors in the second half of the excessively long Chapter 3, much of which has overlap with this Chapter 9. [Jerry Mahlman]	ES redrafted.
9-67	A	3:1	6:27	There is no scientific merit to be found in the Executive Summary. The presentation sounds like something put together by Greenpeace activists and their legal department. The points being made are made arbitrarily with legal sounding caveats without having established any foundation or basis in fact. The Executive Summary seems to be a political statement that is only designed to annoy greenhouse skeptics. Wasn't the IPCC Assessment Report intended to be a scientific document that would merit solid backing from the climate science community - instead of forcing many climate scientists into having to agree with greenhouse skeptic criticisms that this is indeed a report with a clear and obvious political agenda. Attribution can not happen until understanding has been clearly demonstrated. Once the facts of climate change have been established and understood, attribution will become self-evident to all. The Executive Summary as it stands is beyond redemption and should simply be deleted. [Andrew Lacis]	Rejected. ES summarizes Ch 9, which is based on the peer reviewed literature.
9-68	A	3:1		The abstract is overall OK. However, I believe you should state the objectives of the chapter, and explain the difference between "observation" and "detection". For readers that are not familiar with the topic, this is essential to mark the difference to chapter 3. It should be clear to the reader that "detection" is much more rigorous than "observation", even more rigorous than a statistically significant trend. [Christoph Schar]	We feel that the ES is not the place to explain what is meant by detection.
9-69	A	3:3	3:16	The phrase "greenhouse gas forcing...caused greater warming than observed" may be a little confusing for an executive summary. Perhaps "caused" should be replaced by "would have resulted in". This paragraph is careful to use the IPCC "language of uncertainty" up until the last two sentences. Can the language be applied to these sentences? [Matthew Collins]	"Caused" replaced. We decided not to use calibrated language as requested. This is a summary paragraph. Individual assessments discussed further down do you calibrated language.
9-70	A	3:3	3:16	I think the ES needs to start with a statement as to what the warming has been over the	Reject. This should be in the ES for



No.	Batch	Page:line		Comment	Notes
		From	To		
				time period in question, state what the level of confidence in it is, and refer to the appropriate chapters. [Robert Colman]	Chapter 3.
9-71	A	3:3	3:16	I think it needs too be spelt out somewhere in the ES what is behind the different levels of confidence in temperature changes being the result of 'internal variability' and 'internal variability + natural forcing'. I assume the difference is simply uncertainty from solar changes -- is this the case? [Robert Colman]	Noted. Uncertainties are summarized in the ES, but there is insufficient space to discuss assessment of different confidence levels. Confidence levels and forcing uncertainty are discussed in main chapter body.
9-72	A	3:4	3:5	The sentence starting 'Anthropogenic warming...' is a statement that warming caused by humans can be detected, but there are no levels of confidence with it -- so it comes across as a simple statement of fact -- i.e. certain, or close to it. This does not seem to me to match with the confidence statements made in the next 2 sentences. [Robert Colman]	ES redrafted.
9-73	A	3:4	3:5	Replace "can be detected" by "might be present" [Vincent Gray]	ES redrafted.
9-74	A	3:4	3:37	"Anthropogenic warming of the climate system is pervasive ..." ? The quantity that is being measured is temperature (of the surface, atmosphere and ocean). Temperature has no identifying label that would make it possible to identify any given temperature change as being "natural" or "anthropogenic" in its origin. The term "anthropogenic warming" is yet to be properly defined. In any case, it is hardly a scientifically credible description to be attached to observational data. [Andrew Lacis]	Rejected. "Anthropogenic warming" is both scientific and easily understood by decision makers and others.
9-75	A	3:4	3:7	Very well said. I am pleased that you, and IPCC, are still making good use of my informal "betting odds" approach to assessing levels of likelihood of climate-warming outcomes(J.D. Mahlman, SCIENCE, 1997, Vol. 278, pp 1416-17.) [Jerry Mahlman]	Noted.
9-76	A	3:4		Replace "pervasive" by "suspected" [Vincent Gray]	ES redrafted.
9-77	A	3:5	3:7	This is a difficult sentence to write! it strikes me as clumsy, however, because of the 'not' in the middle of each major clause. Can it be rewritten without the 'nots', (and possibly broken in half) as in "It is very likely that the warming during the past half-century has at least some anthropogenic component", and "It is exceptionally unlikely that the warming is due to ...", or similar? [Robert Colman]	Sentence redrafted.
9-78	A	3:5	3:7	It seems to me that the degree of confidence is understated in the use of "very likely" to indicate that human activities have played a role in recent climate change. As indicated on	Sentence redrafted.



No.	Batch	Page:line		Comment	Notes
		From	To		
				line 19, "anthropogenic forcing is responsible" and further expanded on lines 26-28--so without any qualification we know human activities have played a role. Perhaps it is the very obtuse wording of the sentence on lines 5-7 but this sentence sounds more consistent with the SAR than with all that we know now. There also seems to be a conflict between the "virtually certain" on line 7 and the "very unlikely" on line 43. What is needed in the summary is a straightforward positive statement and keep all the highly qualified text with, in essence the double negatives, to the main body of the text. The report needs to say that it is virtually certain that human activities played the major role in warming the climate over the past 50 years, and that anthropogenic warming influences now significantly exceed the variable influences of natural factors. [Michael MacCracken]	
9-79	A	3:6	3:10	It is counterintuitive that it is a) only "very likely" that natural internal plus forced variations are not the cause of recent warming when it is b) "virtually certain" that natural internal variations have not caused recent warming and c) natural forced variations have likely caused cooling. c) should cause a) to be even more certain than b)! I realise the problem is the incorporation of the wide error bars on c) into a), but a "policymaker" may be confused. [David Parker]	Redrafted. Consistency of assessments reconsidered.
9-80	A	3:6		Replace "very likely" with "possible" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-81	A	3:7	3:10	Delete from The warming . Line 7 to "warming" on line 10. This statement is nonsense. There have been several warming influences from natural events such as El Niño [Vincent Gray]	Rejected. The reviewer appears confused – the sentence he objects to has nothing to do with natural internal variability such as El Nino.
9-82	A	3:7	3:7	Stratospheric ozone is a greenhouse gas [William Ingram]	Noted. We think this point is a bit pedantic. O3 is a ghg, but because we wish to distinguish in the text between well mixed greenhouse gases, with increasing concentrations, and O3, which has a complex spatial distribution of change in abundance.
9-83	A	3:7	3:16	This is very honest and well stated. [Jerry Mahlman]	Noted. Thanks.
9-84	A	3:7		Delete from "virtually certain" to "alone" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-85	A	3:8	3:10	It is not at all clear what "at a time" is referring to--does this mean over the last half century, which is really a period rather than "a time". And if this is indeed referring to the past 50 years, then it essentially rules out natural factors creating the warming of the last 50 years, and so the statement in the preceding sentence can indeed be more positively stated (when one has sound indications that human influences are contributing to the warming and that natural influences are exerting a cooling influence, a really forthright positive statement needs to be made--and can be justified; there is no viable alternative explanation). Saying that it is only "very likely" that anthropogenic forcing is creating a warming influence seems seriously understated, even with the uncertainties about aerosols (and land cover change). This is all much too cautious, even using scientific standards of drawing conclusions. [Michael MacCracken]	These sentences have been re-drafted. The meaning of "a time" should be clear from the context. Assessments have been reconsidered.
9-86	A	3:10	3:11	"Combined .. ocean." sentence maybe clearer reversed (i.e. caveat before main result) ? [William Ingram]	ES redrafted.
9-87	A	3:10	3:12	How is the "likely" here consistent with the "very likely" in the preceding sentence? This should say that the full warming influence of the GHGs was very likely offset, to some extent, by a net cooling influence of anthropogenic aerosols. [Michael MacCracken]	Rephrased.
9-88	A	3:10		Replace "likely" by "possible" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-89	A	3:13		Replace "likely" by "possible" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-90	A	3:14	3:16	Just how does "anthropogenically influenced" atmospheric circulation differ from "non-anthropogenically influenced" atmospheric circulation? [Andrew Lacis]	We don't understand reviewer's point.
9-91	A	3:14		Replace "have likely" with "might have" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-92	A	3:15		Replace "becoming" with "thought to become" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-93	A	3:17	3:26	Again, an excellent summary of the current position [William Ingram]	Thanks.
9-94	A	3:18	3:23	The "conclusion that anthropogenic forcing is responsible for many of the changes in the	Redrafted.



No.	Batch	Page:line		Comment	Notes
		From	To		
				climate system observed over the past century" is far too strong. The majority of studies (as highlighted in first paragraph of the executive summary) have looked only at large-scale temperature changes which have been shown, in any case, to be driven by a combination of anthropogenic and natural (both internal and external) factors. D&A of changes in other variables and at the regional scale is much less certain. Climate is much more than large-scale temperature. [Matthew Collins]	
9-95	A	3:18	3:23	Well said, although I would have assessed line 21 as being "very likely" given the lack of credible counter arguments. [Jerry Mahlman]	Redrafted.
9-96	A	3:19	3:19	The statement that the physical consistency of the many different lines of evidence now available "supports" a conclusion of anthropogenic warming is too weak in my opinion. Surely the physical consistency of so many different lines of evidence provides absolutely compelling evidence. [Robert Colman]	Redrafted.
9-97	A	3:19		Replace "is" by "might be" <sup>3</sup> [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change. Note conflict between this reviewer opinion and 9-96.
9-98	A	3:19	:20	The sentence "that anthropogenic forcing is responsible for many of the changes in the climate system" is a bit too diffuse. How about "is affecting many aspects of the climate system". [Christoph Schar]	Redrafted.
9-99	A	3:20	3:22	Again, this seems an understatement. While the data may only justify a "likely" for a particular year, for a period of a few decades, the confidence that late 20th century temperatures are greater than for any comparable earlier period seems to clearly justify saying "very likely" given the various statistical tests that have been done, etc. And this should say over "at least the past 1000 years". [Michael MacCracken]	Redrafted.
9-100	A	3:21		Replace "was likely" by "might have been" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-101	A	3:22	3:22	The last phrase of the sentence duplicates the text on lines 8-10. [Michael MacCracken]	Redrafted.
9-102	A	3:22		Replace "would likely" with "probably" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-103	A	3:25	3:32	These opinions are controversial. Delete [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-104	A	3:25	3:32	Very nicely stated, although I would have used "very likely" at the end of Line 32, given the evident lack of credible alternative explanations. [Jerry Mahlman]	Redrafted.
9-105	A	3:29	3:29	Change "used" to "relied on results from"--to make clear it is the results from the models that are used rather than implying, as the current wording does here, that the models are geared to this particular study. [Michael MacCracken]	Accepted.
9-106	A	3:30	3:30	Clarify that "surface" means land/ocean surface? [William Ingram]	Rejected. We think the meaning of surface is clear from the context.
9-107	A	3:31	3:32	Surely the chance that ALL results from these techniques are spurious is exceptionally small. The way it is written strikes me as too weak. [Robert Colman]	Redrafted.
9-108	A	3:34	3:36	This section is simply untrue. Delete it. The climate sometimes cools, for example between 1946-1978 [Vincent Gray]	Redrafted.
9-109	A	3:38	3:38	Replace first sentence with "Since the IPCC's Third Assessment Report, six more years of high quality instrumental data have become available." [Michael MacCracken]	No longer applies.
9-110	A	3:38		Insert "since the TAR" after "data" [Vincent Gray]	No longer applies.
9-111	A	3:40		Insert "surface" between "on" and "record" [Vincent Gray]	No longer applies.
9-112	A	3:42	3:42	Should there not be an uncertainty associated with 0.75 C? [Michael MacCracken]	No longer applies.
9-113	A	3:42		Replace "0.75K" by "between 0.57 and 0.62K" (see Table 3.3) The figure 0.75K refers to 1860-2004 ( see page 3_3, line 8) [Vincent Gray]	No longer applies.
9-114	A	3:43	3:43	The "very unlikely" should be strengthened to "exceptionally unlikely"--there is nothing like 1 chance in 10 to 20 that what we are seeing is due to natural causes alone--it is physically impossible given the CO2 increase. [Michael MacCracken]	No longer applies.
9-115	A	3:43		Replace "very unlikely" with "just possible" [Vincent Gray]	No longer applies.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-116	A	3:43		I could make a strong case here for "virtually certain", given that the quantities being evaluated are for intrinsically global quantities. [Jerry Mahlman]	No longer applies.
9-117	A	3:44	3:44	precipitation" -> "snowfall [William Ingram]	No longer applies.
9-118	A	3:46	3:51	This executive summary paragraph says that we understand what caused the recent warming and that based on the good agreement of models and observations we can therefore deduce how much cooling was contributed by aerosols. This reverse calculation is afforded a much greater level of confidence than the forward calculation (very low LOSU in Ch2 for aerosol). I do not see how this can be possible. To be consistent, the large uncertainties highlighted in ch2 need to be taken into account when doing the reverse calculation. If you took the uncertainties into account fully then surely the reverse calculation would then also have large uncertainties. Again, as emphasised in my previous comments, the reverse calculation appears to be 'reliable' only because you have not fully accounted for the uncertainty in the temporal pattern of aerosol forcing since 1750. No evidence is provided in the entire report that brings the forward and reverse calculations any nearer. It is *an* explanation but not necessarily *the* explanation. [Kenneth Carslaw]	Rejected. We feel that our "likely" assessment here accounts for uncertainties in forcing, model formulation, etc.
9-119	A	3:46	3:47	Attribution analyses are fine, but there is no substitute for physics. [Andrew Lacis]	Agreed. The chapter is based largely on physically-based attribution studies.
9-120	A	3:46	3:51	This is a very well written and convincing statement. [Jerry Mahlman]	Noted.
9-121	A	3:47	3:47	The use of the phrase "clearly indicate" reads to me as indicating certainty or virtual certainty. If the evidence here clearly indicates a response to greenhouse gas forcing, then this would seem to me to support stronger than a "very likely" conclusion, stated in the first paragraph of the executive summary. [Robert Colman]	Redrafted. Thanks.
9-122	A	3:47		Delete "clearly" [Vincent Gray]	Redrafted.
9-123	A	3:48	3:49	Change "account" to "account uncertainties associated with" and then end the sentence with "observations" instead of "observational" so it is clear that the uncertainties relate to modelling, forcing and observations and not just the last of these. [Michael MacCracken]	Accepted.
9-124	A	3:48		Replace "robust to" with "independent of" [Vincent Gray]	Accepted.
9-125	A	3:49	3:51	There is also the fact that due to the large heat capacity of the ocean there exists about	Noted.



No.	Batch	Page:line		Comment	Notes
		From	To		
				0.85 W/m <sup>2</sup> of unrealized warming. The climate system simply hasn't had time to respond to the accumulated GHG radiative forcing. [Andrew Lacis]	
9-126	A	3:49	3:51	This sentence seems to duplicate lines 10-12. [Michael MacCracken]	First paragraph is an overall summary of the ES, so some repetition is inevitable.
9-127	A	3:50		Replace "is likely to " with "could possibly" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-128	A	3:53		Replace "has been" by "might have been" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-129	A	3:55	3:55	"annual total" reads oddly & I doubt is what is meant - "global-mean" ? [William Ingram]	Clarified.
9-130	A	3:57	4:1	I doubt the model-simulated changes are underestimated" (i.e. people think them to be smaller than they actually are), though they may be smaller than the observed changes [William Ingram]	Thanks. Redrafted.
9-131	A	4:0		The executive summary is confusing because of different types of nomenclature for expressing likelihood or confidence. [Bryant McAvaney]	Redrafted.
9-132	A	4:2	4:3	Delete from "while the influence" to "forcings". This is simply not true [Vincent Gray]	Redrafted.
9-133	A	4:2	4:3	It is not at all clear what the phrase "while the influence of greenhouse gas increases can be separated from other forcings" means. Perhaps it would help if the "while" were replaced by "and" but as written, the meaning is obscure, at best. [Michael MacCracken]	Redrafted.
9-134	A	4:4		Replace "is likely" with "may be" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-135	A	4:7	4:41	The hedging statements of "very likely been affected or influenced by anthropogenic forcing" do not really add any credence or credibility. They may well be true, but there is no factual evidence presented to document and support these contentions - it may exist in other chapters, but an effective summary would cite the relevant evidence. [Andrew Lacis]	Reviewer seems to be saying that any indication of any uncertainty should be removed, ie that we should claim certainty. Rejected.
9-136	A	4:7	4:7	Given that the sentence is saying only that anthropogenic forcing "has contributed to" the warming of the upper ocean, how can this be only "very likely." There is no doubt that	Confidence levels reassessed.



No.	Batch	Page:line		Comment	Notes
		From	To		
				there has been a contribution--that is clear from basic physics. It is perhaps only "very likely" that anthropogenic influences explain most of the warming of the upper ocean. [Michael MacCracken]	
9-137	A	4:7	4:11	Very well said. [Jerry Mahlman]	Noted.
9-138	A	4:7		Replace "very likely" with "possible" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-139	A	4:8	4:11	Delete from "combined" to "ocean". It would be very surprising if the effects did not consist of "combined anthropogenic and natural external influences" even if it is uncertain [Vincent Gray]	Redrafted.
9-140	A	4:10	4:10	What does 'considerable uncertainty' mean? There is not nearly enough uncertainty to upset the conclusions that are being drawn--though there is enough uncertainty that the climate sensitivity cannot be pinned down further than has been the prevailing range. This sentence needs to put the phrase "considerable uncertainty" into context--or better yet state this all a bit differently. [Michael MacCracken]	Redrafted.
9-141	A	4:13	4:13	I think I understand what you are trying to say, but a change in globally-averaged temperature implies changes in regional temperatures as a logical consequence, whether or not any detection threshold is reached in a particular local analysis. So the likelihood of some regional change cannot be lower than in the global case, and indeed in principle could be higher. [James Annan]	Redrafted.
9-142	A	4:13	4:23	Surely we know that regional temperatures MUST have been affected by anthropogenic forcing, if we know that they have at the global scale, because the global is just the average of lots of regional change. So the conclusion (heading to the section) that "Regional surface temperatures have LIKELY been affected by anthropogenic forcing" is too weak, considering our level of confidence on the global scale changes. What we might be less certain about, however is the DETECTION at the regional scale. [Robert Colman]	Redrafted.
9-143	A	4:13	4:23	The level of confidence in the heading of the paragraph being only "likely" comes across as inconsistent with the level of confidence expressed in the paragraph, including statements such as "provide compelling evidence" and "clearly identified". [Robert Colman]	Redrafted.
9-144	A	4:13		Replace "have likely been" with "are thought to be" [Vincent Gray]	Redrafted.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-145	A	4:14		Replace "shown" by "elucidated" [Vincent Gray]	Redrafted.
9-146	A	4:15	4:17	Nomenclature is not used properly here--we do not "detect" significant anthropogenic effects--we "detect" change and "attribute" the change, at least to some degree, to human influences. So, there needs to be some rewording here, indicating, for example, that models are successfully simulating the pattern of observed changes over individual continents and it is very likely that these changes can be attributed to anthropogenic influences on the climate. [Michael MacCracken]	Redrafted.
9-147	A	4:17		Delete "compelling" [Vincent Gray]	Redrafted.
9-148	A	4:19		Replace "clearly" by "considered to be" [Vincent Gray]	Redrafted.
9-149	A	4:20	4:23	Delete from "As with the" on line 2 to "variability" on line 23. It depends what you mean by "evidence" For what? All the results are subjective, not definite. [Vincent Gray]	No longer applies.
9-150	A	4:21		I would choose "very unlikely" here. A number of the interior continental warming/drying areas are now quite evident. [Jerry Mahlman]	No longer applies
9-151	A	4:25		Replace "have likely" with "have thought to have" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-152	A	4:26	4:29	New evidence in Chap 3 shows not change in diurnal temp range since about 1980, so this attribution needs to be either deleted or reworked [Thomas Karl]	No longer applies.
9-153	A	4:29	4:30	"well" is too strong - "better" better (& then "In contrast" could be dropped) [William Ingram]	Redrafted.
9-154	A	4:29		Replace "detected" with "postulated" <sup>4</sup> [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-155	A	4:30		"the same study was unable to ...". Has the study (I assume you mean Stott et al 2004) really attempted and been unable to identify anthropogenic influence in indices of extremely warm days? If you mean the Stott et al study, you might rather use "has not attempted". But there are many studies that have observed pronounced trends in heat-wave indices. [Christoph Schar]	Redrafted.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-156	A	4:31		.. it is likely... (or very likely?). [Jerry Mahlman]	Rejected. We don't think we can assess confidence in this case.
9-157	A	4:32	4:33	The reference to the European heat wave ("such as...the unprecedented 2003 European heat wave") is gratuitous. First, "unprecedented" since when? Since ever? Was it worse than the heat wave of 1214? Of course you might respond by saying that we don't know if there was a European heat wave in 1214, but that's exactly the point: no one knows if 2003 was "unprecedented." In the past the IPCC has been quick to dismiss anomalous individual events and data series by saying the focus must be on "climate"--as in long term averages. Yet here you have an anomalous event that cannot be evaluated for its rarity without knowing something about the tails of distributions when there is too little data to reliably characterize those tails. Also, if you are going to discuss anomalous "warm" events, why not also discuss anomalous "cold" events? Have there been no unusual cold snaps anywhere in the world? Or would it be "off-message" to bring those up? Finally, the European heat wave led to a staggering number of deaths in France, but these can be attributed in large measure to the failure of public authorities to protect vulnerable citizens, and may specifically have been exacerbated by the fact that the heat wave coincided with so many civil servant vacations. Other places in the world experience the same summertime heat without tens of thousands of people dying. References to the European heat wave might allow the IPCC to use a body count as a grotesque way to draw attention to its report, but don't pretend that it is scientific writing. [Ross McKittrick]	Redrafted.
9-158	A	4:32		Replace "substantially" by "possibly" [Vincent Gray]	Redrafted.
9-159	A	4:35	4:41	Well said. [Jerry Mahlman]	Thanks.
9-160	A	4:35		Insert "thought to be" after "is" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-161	A	4:36	4:37	Does this mean natural forcing is an (equally plausible) attribution? [Richard Wood]	See 9-193.
9-162	A	4:36		Replace "unlikely to be " with possibly" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-163	A	4:36		Replace "and" by "but they" [Vincent Gray]	Accepted.
9-164	A	4:39	4:39	Chapter 4, Section 4.5.3 now reports recession in Norway and New Zealand. [David Parker]	Redrafted.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-165	A	4:39	4:40	...in northern Europe, where increases in precipitation appear to be increasing glacier volumes... This is inconsistent with Ch. 4, (p.4-22, lines 47-53): glaciers advanced in westernmost Scandinavia in the 1990's, but this has been reversed to a retreat since 2000 and glaciers further inland were retreating already in the 1990's. [Jouni Räisänen]	Redrafted.
9-166	A	4:40	4:41	Are the changes consistent with those expected under historical anthropogenic forcing (i.e. and attribution-like statement) or future, much stronger, anthropogenic forcing? It reads very much like an attribution statement which I suspect it isn't. [Matthew Collins]	Redrafted.
9-167	A	4:40		Replace "expected" by "postulated" [Vincent Gray]	Redrafted.
9-168	A	4:42	4:43	This seems much too optimistic - the inconsistencies in tropospheric temperature are better appreciated but certainly not "resolved" (meaning we know what actually happened): rather it is accepted that we don't know (& I doubt we ever will). Otherwise an excellent paragraph. [William Ingram]	Redrafted.
9-169	A	4:43	4:47	What does the word "Significant" mean at the beginning of this paragraph. It would be better to adopt the "language of uncertainty". Are the trends unlikely to be due to natural internal variability? There appears to be no mention of the NAO in this statement. Is it considered separate from the NAM or the same mode of variability? [Matthew Collins]	Redrafted. There is insufficient space in the ES to discuss differences between the NAM and NAO.
9-170	A	4:43	4:47	Given the uncertainty of the sustaining nature of the "annual modes", I would stick with "likely" here. [Jerry Mahlman]	Noted.
9-171	A	4:44		Replace "are likely to" by "may" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-172	A	4:45	4:45	"too small" suggests that the models are wrong, which is not necessarily the case. Suggest "...smaller than observed over recent decades, suggesting a role for internal variability and/or model error". [Richard Wood]	Redrafted
9-173	A	4:46		Insert "can" between "changes" and "simulate" [Vincent Gray]	Redrafted. The models do simulate the trend, so don't need "can".
9-174	A	4:47	4:47	While warming I guess does lead to an increase in the global average of sea level pressure as the atmosphere is holding more water vapor, I presume what is being referred to here are detectable changes in the global patterns of sea level pressure that can be attributed to	The NAM and SAM are patterns.



No.	Batch	Page:line		Comment	Notes
		From	To		
				human influences (not again to be careful on detection of change and attribution to human influences). [Michael MacCracken]	
9-175	A	4:47		Delete from "leading" to pressure" It does not follow that it is true [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-176	A	4:49	4:51	This sentence is hard to understand, and should be rewritten. [Robert Colman]	Rewritten.
9-177	A	4:49	4:49	"low-frequency" unnecessarily obscure for a general readership compared with "long-term" - but just what either means is unclear: I guess "longer timescales than the Schwabe ("11-year") solar cycle" is meant: if so, say so. [William Ingram]	Redrafted.
9-178	A	4:49	4:49	It would have been more convincing to first demonstrate that the "natural" hydrological cycle and its natural variability is actually understood before venturing opinions about whether the impact of external influences is emerging. [Andrew Lacis]	This is discussed in Chapter 3.
9-179	A	4:49	5:4	this strikes me as being quite speculative. [Jerry Mahlman]	Redrafted.
9-180	A	4:49		Replace "emerging" with "equivocal" [Vincent Gray]	Rejected. We feel that the evidence is emerging.
9-181	A	4:50	:51	The meaning of "consistent with 205h century ... simulations" is not clear to me. [Christoph Schar]	Redrafted.
9-182	A	4:52	4:53	This sounds like a convoluted attempt to avoid saying "...but model simulations do not generally capture the full amplitude of the apparent response." Why not just say that? [Richard Wood]	Redrafted.
9-183	A	4:53	4:56	Chapter 3 shows that some studies find changes in very heavy precip that clearly are beyond what would be expected from natural variability (at least in a statistical sense) so I think this statement needs revision to indicate "these changes are clearly distinguishable from natural variability in selected regions" [Thomas Karl]	Redrafted. We feel that we need to be cautious in our assessment of heavy precipitation change.
9-184	A	4:54	4:54	carbonaceous aerosols don't seem a good example given that Jones & al (2005: no longer "in press" as listed) find the pattern of climate response they produce to be indistinguishable from anthropogenic sulphate aerosol, so we may consider existing "anthropogenic sulphate aerosol" results to be "anthropogenic sulphate and carbonaceous aerosol" results [William Ingram]	No longer applies.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-185	A	4:57	4:57	somewhat more uncertain than estimated" obscure: I suggest "less certain than they formally appear [William Ingram]	Redrafted.
9-186	A	5:2	5:4	The link between monsoons and Sahel rainfall and climate forcing seems tenuous. Could we have a "likely"-type statement here? [Matthew Collins]	Confidence assessment added.
9-187	A	5:2	5:4	This sounds very vague. Suggest simply say that a clear attribution has not been made. [Richard Wood]	See 9-186
9-188	A	5:6	5:13	While the text is OK, the italicised statement that "Climate models are able to reproduce climate conditions in the recent geological past" is too strong. Models have only been tested in a handful of time-periods, principally the mid-holocene and the LGM. In the case of the former, models are unable to fully reproduce the extension of the monsoon into the Sahel or the reduced ENSO activity consistent with the palaeo-evidence. There are similar discrepancies for LGM simulations. It might be better to write "Climate models have been used to simulate the recent geological past". [Matthew Collins]	Redrafted. This paragraph has a new heading.
9-189	A	5:6	5:13	This needs to be turned over to the "Paleoclimate" Chapter 6, in my opinion(and you can cut your page count!) [Jerry Mahlman]	Disagree, although this paragraph has been redrafted.
9-190	A	5:6	5:13	It is not correct to say that climate models have been able to reproduce the LGM. In the simulations the continental ice sheets and their properties are specified, whereas a true climate model would be able to simulate these features of the LGM from first principles, and would not have to take them as given. [Peter Stone]	Rephrased. We say that boundary conditions are specified.
9-191	A	5:6		Add at end "by adjustment of model parameters" [Vincent Gray]	Rejected.
9-192	A	5:8	5:8	Last Glacial Maximum (LGM) [Bernard Seguin]	Accepted.
9-193	A	5:9	5:17	Good summary of these facts - but completely omits the reason they matter & why anything should be said in the Executive Summary, i.e. that d&a uses the models' internal variability to estimate how (un)likely things would be by chance, so if this were modelled badly enough all the d&a results would be wrong. The 1st sentence of this para needs to explain this as briefly as possible. [William Ingram]	Revised – we now try to state why this is important.
9-194	A	5:11	5:11	I suggest "confirmed" for "increased confidence" [William Ingram]	Rejected – confirmed expresses to high a level of confidence.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-195	A	5:15	5:24	Again the italicised statement is too strong. Perhaps "New estimates of climate sensitivity constrained by observations have been made". The phrase which begins "Best estimates..." and quotes the usual 1.5-4.5 degC range seems like a hang-over from the TAR and bears no relation to, for example, the studies highlighted in figure 9.6.1 in which for two estimates the median is greater than 4.5 degC. The statement that the TCR is very unlikely (i.e. <5%) to be less than 4.5 degC is too strong considering that it is only based on one method. [Matthew Collins]	Rejected. Better suggests that an improvement has been made, which is our assessment of the current state of play. TCR assessment has be reconsidered.
9-196	A	5:15	:24	Climate sensitivity is dealt with in this chapter and in chapters 8 and 10. I suggest that all information on Climate Sensitivity be brought together in one chapter, f.e. in Ch 10 [Fons Baede]	Rejected. The focus here is on observational constraints. Ch 9 does contribute to the sensitivity box in Ch 10.
9-197	A	5:16	5:17	If it isn't mentioned elsewhere, it might be worth mentioning that radiative forcing is 1/4 the solar output (took me a little while to work out what you meant, anyway). [James Annan]	We don't understand the comment.
9-198	A	5:20	5:20	"affect the confidence in the size of" obscure [William Ingram]	Redrafted.
9-199	A	5:20	5:21	There is no basis offered as to why climate sensitivity to doubled CO2 might be as low as 1 degree C. (Although, an inaccurate radiation model could easily accomplish the trick.) Climate feedbacks are known to be both positive and negative. However, the overall sum of the feedbacks is decidedly positive (see Hansen et al., 1984). That is why the equilibrium temperature response for doubled CO2 is about 2.7 degrees C, instead of 1.2 to 1.3 degrees C, where the latter is the equilibrium global mean temperature response if there are no feedback effects operating. [Andrew Lacis]	Redrafted.
9-200	A	5:21	5:22	I think the summary needs to include a statement that the best estimate of climate sensitivity is 3 K. Instead you quote the extreme values of the distribution, 1.5 K and 4.5 K, but the distribution is not uniform and peaks at 3 K. Saying our best estimate is 3 K plus or minus 1.5 K gives a better representation of the situation. [Alan Robock]	Accepted.
9-201	A	5:22	5:24	This use of "transient sensitivity" is different from the TAR (70 years at 1% = doubled CO2). Does this represent a uniform policy across the AR4? It would be sensible to be consistent at least within the whole document. [James Annan]	Noted.
9-202	A	5:22	5:24	The upper limit of climate sensitivity remains elusive, although a number of scientists are comfortable with asserting that it is Very Unlikely to be well above the 4.5C range. [Jerry Mahlman]	Noted.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-203	A	5:26		Add at end "from model simulations" [Vincent Gray]	Reject. Comes from data and models and theory.
9-204	A	5:27	5:28	Replace "is largely consistent" on line 28 to "forcing" on line 29 with "can be simulated with climate models by suitable adjustment of their parameters" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change. Reviewer appears to be suggesting that C20C runs used very different parameter values to other model runs, just to fit the data, but provides no evidence of this assertion.
9-205	A	5:27	5:35	Well said. [Jerry Mahlman]	Noted.
9-206	A	5:27	5:35	This is a very tricky paragraph. It's important not to give the impression that there were preconceptions to start with, i.e. the detector/attributor should start from the observations with an open mind. I was unsure about "Our confidence in our assessment of the role of humans in the recent climate evolution has increased considerably since the TAR because...". Is that some general assessment of the role of humans (if so what is the assessment? The headline statement of the TAR was the 'new and stronger evidence' re the warming of the last 50 years). Or is it the assessment of changes in particular variables? I feel a more defensible statement (and one which would have more impact) would simply be: "Since the TAR a signal of anthropogenic change has been detected in more variables [then list a few]. Confidence in understanding of changes has increased because of ongoing improvement in models and because some apparent inconsistencies..." [Richard Wood]	Redrafted.
9-207	A	5:27	9:35	The overall summary and the text are very scholarly and thorough. I am pointing out few inconsistencies. The claimed consistency with top of atmosphere radiation can not be justified. In fact the Wielicki et al study shows that the anomalies seen in ERBE are much larger than that simulated by models. Similarly, the consistency in precip trends really depend on how one analyzes the data. For example, as Hulme et al show the land average precip decreased since the 1950s (also see Chapter on observed climate change), the period when the global average Temp warmed. This signal is clearly inconsistent with most model predictions. [Veerabhadran Ramanathan]	Redrafted.
9-208	A	5:28		I would stick with "mainly radiative forcing" for simplicity here. Non-radiative forcings are not quantitatively important, to first order. [Jerry Mahlman]	Rejected. We think the notion of external forcing is clear.
9-209	A	5:31	5:32	Better wording would be "Confidence in these findings regarding the role of ..." as saying	Redrafted.



No.	Batch	Page:line		Comment	Notes
		From	To		
				"Our confidence" seems to personalize this to the authors rather than generalizing this to the whole scientific community. These findings are achieving something objective, not personal. [Michael MacCracken]	
9-210	A	5:32	5:33	Delete from "because on line 32 to "system" in line 33. How can a "signal" "emerge?" [Vincent Gray]	Redrafted.
9-211	A	5:33		.... the climate system, mostly because ... [Jerry Mahlman]	Redrafted.
9-212	A	5:34	5:34	Be careful with the phrase largely resolved --- change to "because some apparent inconsistencies in the observational record (notably for tropospheric satellite derived temperatures) have been resolved" [Thomas Karl]	Redrafted.
9-213	A	5:38	5:42	I disagree. Go take a look at the quantitative conclusions of chapter 3 to get a more realistic perspective. Please also note that NASA's measurement systems have been woefully and unapologetically inept in obtaining the information that we need to evaluate the magnitudes of various external radiative forcings. [Jerry Mahlman]	We don't understand the comment. We state that estimates of radiative forcing change are uncertain, and the reviewer seems to agree.
9-214	A	5:38		delete "change" [Fons Baede]	Accepted.
9-215	A	5:44	5:55	I think that this Chapter is making a mistake by getting into the "radiative forcing business here. This is hard work, and the Radiative Forcing Chapter 2 has already covered this in far more detail than can be covered well here. I suggest that the Co-ordinating Lead Authors of this Chapter get into contact with V. Ramaswamy of Chapter 2 on this. This should give you an opportunity to combine forces productively. [Jerry Mahlman]	Redrafted.
9-216	A	5:51	5:52	Instead of saying "There are greater uncertainties with respect to other variables" this should be saying "There is less confidence with respect to other variables" as the uncertainties are not really comparable directly since the measures are different. [Michael MacCracken]	Accepted.
9-217	A	5:54	5:55	First, why say "errors" rather than "uncertainties" or "limitations" with respect to forcing and model physics (and should this not also say model resolution--or is that included?)--the wording just seems pejorative and makes it sound like we know we are making mistakes. In addition, saying "is not presently understood" seems like a quite strong statement, sort of implying that we really have to have the "extent" pinned down pretty precisely before we can say anything at all. In that the preceding sentence indicates that there is a disagreement, I would suggest that the last sentence say something like "The relative importance of uncertainties and limitations in observations, forcing, and model	Revised along lines suggested.



No.	Batch	Page:line		Comment	Notes
		From	To		
				physics and structure in contributing to the quantitative disagreements between model results and observed changes is under intensive study." [Michael MacCracken]	
9-218	A	6:0		Overall, this is an excellent Executive Summary, validated by its careful consideration of all the available data. I am very pleased to see the level of diagnostic honesty that is employed in this detection/attribution chapter. I note here that the "regional trends" topic has been addressed independently in Chapter 3, with rather inconclusive results. It might be useful to get into contact with them. [Jerry Mahlman]	Noted.
9-219	A	6:1	6:19	Well said. [Jerry Mahlman]	Noted.
9-220	A	6:4	6:4	There will always be uncertainties, so saying "uncertainties remain" is not really helpful--one has to say how important they are. It would really be preferable here to be structuring the sentence to address the situation with respect to the IPCC lexicon of levels of confidence. So, I would change "uncertainties remain" to something like "achieving a very high level of confidence will always be limited" or something similar. [Michael MacCracken]	Revised along lines suggested.
9-221	A	6:11	6:13	Replace this paragraph by "There is no evidence, from temperature measurements in the free atmosphere, that they are influenced by anthropogenic factors." [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-222	A	6:11	6:13	I would change the phrase to read "though uncertainties remain in both the radiosonde and satellite records, and these uncertainties make it difficult to precisely define the anthropogenic contribution to change of the free atmospheric temperature." I go to plus for uncertainties as the two data records have different uncertainties (both in terms of source and size), and rephrase the rest because the problem is that we cannot get too precise an estimate. [Michael MacCracken]	Rephrased.
9-223	A	6:11	6:13	This is a very important point. [Jerry Mahlman]	Noted.
9-224	A	6:11		Use agreed "likelihood" terminology. Rather than "increased confidence" use "likely". [Fons Baede]	Redrafted.
9-225	A	6:11	:20	Avoid "we", because it is not clear who "we" are. [Fons Baede]	Avoided.
9-226	A	6:12	6:12	"separate" seems meaningless & confusing to me: omit or replace with a clear word [William Ingram]	"separate" deleted.
9-227	A	6:15	6:18	A very well balanced statement.	Noted.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Jerry Mahlman]	
9-228	A	6:15		... changes in extremes, and an improved ability to assess ... It is important to note that these kinds of diagnostics are intrinsically data limited due to the difficulty in differentiating "extremes" out of current data sets that are experiencing problems with data fidelity. When we are chasing "extremes", it only gets more difficult. [Jerry Mahlman]	Noted.
9-229	A	6:18	6:18	Change "studies of" to "and attribution of changes in"--as the key issues are both detection and attribution--and we are looking at changes and not just what the values are. [Michael MacCracken]	Accepted.
9-230	A	6:20	6:25	I don't think you can end here without a more focussed discussion on cloud-radiative feedback uncertainty since you introduced it earlier here. We still have to deal with this contribution to our uncertainty more than we would like. [Jerry Mahlman]	Beyond the scope of Ch 9.
9-231	A	6:47	6:47	Saying detection means identification is meaningless unless the meaning of identification had been given ! [William Ingram]	Redrafted.
9-232	A	6:49	6:50	"from" should be "in", but even so, "expected" is ambiguous - the important point is that our expectation is (or should be) independent of the observations, i.e. based only on our knowledge of the forcings & the likely response of the system [William Ingram]	Redrafted.
9-233	A	6:56	87:7	Throughout the text I see "model" where "GCM" or "GCM or EMIC" or "climate model" seems to be meant: very seldom is this said explicitly, though other models are also relevant and are also referred to. Thus the reader cannot tell what is meant unless he already knows - a very poor state of affairs. I suggest saying "GCM" or "GCM or EMIC" or "climate model" wherever needed to make the meaning clear, everywhere the word "model" currently appears. [William Ingram]	Noted and redrafted.
9-234	A	7:1		The introduction is long but certainly is keyed to needs of policy makers. A decision will need to be made on the relative balance between the introduction and rest of chapter - probably by condensing still further some of the detail in following sections. [Bryant McAvaney]	Noted.
9-235	A	7:1		General comment about introduction: I find that the introduction should undergo a major revision. It should be easier to follow for a reader that is not in this field. It should make a clearer distinction between observation, understanding, detection and attribution. In particular, it should define the meaning / purpose of "understanding climate change". In its present form, it is primarily written from the detection / attribution standpoint. Also, the introduction should briefly discuss methodological aspects (one paragraph or so). You	Redrafted. Thank you for this thoughtful comment.



No.	Batch	Page:line		Comment	Notes
		From	To		
				should explain that statistical concepts are used that enable us to attach (in principle) probabilities to detection and attribution results. [Christoph Schar]	
9-236	A	7:3		“Climate Change is defined legally by the Framework Convention on Climate Change 1992 as “a change of climate which is attributed directly or indirectly to human activity that alters the composition of the global atmosphere and which is in addition to natural climate variability observed over comparable time periods” Do you agree with this definition which has been agreed by many Governments? If you do not, not only should you say so, but you should change the words “Climate Change” in the title of your report to avoid ambiguity [Vincent Gray]	Rejected. IPCC has never used the same definition as the FCCC and this has been discussed during previous assessments.
9-237	A	7:5	7:14	This whole paragraph is highly dubious. Chaos theory is used as an excuse for all the variabilities that you cannot currently explain, implying that they can never be understood. You are certain to be proved wrong [Vincent Gray]	Rejected. There is no such use of chaos in this paragraph – nor is there any implication that variations we currently do not understand can never be understood. All this paragraph says is that some variability is chaotic.
9-238	A	7:5	7:14	Although I have not read all the chapters, one issue that seems to need a good cross-check across chapters is in the defining of internal and external forcing (and variability). In the paleo chapter (Ch. 6), there are instances when the glacial-interglacial variations in vegetation, atmospheric composition, and ice amount are considered external, whereas for at least some of the models used to study the current climate, these are considered internal feedbacks and not forcings. I do not think there is consistency across the IPCC chapters, and this needs to be checked, and some clear and consistent explanations given; this is attempted on lines 31-40, but there remain inconsistencies with chapter 6, for example. Given where we are headed with modeling (and having the definitions change as models become more and more complex will become confusing), I would favor limiting external to (for example) solar, volcanic, tectonics/isostatic adjustment, continental drift, asteroid/comet impacts, and human influences--and keep everything else internal (so vegetation feedback, the carbon cycle, atmospheric chemistry, etc.) even though at times we do specify the response of some of these latter feedbacks in some models. But having the paleo chapter talk about, for example, vegetation changes as a forcing rather than a feedback, seems confusing to me. [Michael MacCracken]	We generally limit external forcing as discussed by the reviewer.
9-239	A	7:13		The chapter refers to internal variability but never mentions that the largest natural internal variability is from ENSO. It would be helpful to do so, and also note it is poorly done in models.	Some mention of ENSO added, but evaluation is a Ch 8 issue.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Kevin Trenberth]	
9-240	A	7:16	7:22	This paragraph is meaningless Delete it [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change..
9-241	A	7:17	7:17	I would start the sentence with the word "However," [Michael MacCracken]	Accepted.
9-242	A	7:24	7:26	This definition is different from that of the FCCC 1992 and you should say so. [Vincent Gray]	Rejected. The distinction between the IPCC and FCCC definitions is made in the glossary.
9-243	A	7:24		Replace "refers to" with "is" and do the same in the glossary [Vincent Gray]	Rejected. We prefer to defer to the glossary.
9-244	A	7:27		Replace "refers" with "deals" [Vincent Gray]	See 9-245.
9-245	A	7:27	:29	Definition of climate change: Is this the same definition as used in other chapters of the FAR? It certainly should! I am a bit concerned about the term "MAINLY to change due to external forcing". I believe many people define climate change with respect to natural interannual variability, or with respect to anthropogenic interference. The issue is delicate and requires some care. [Christoph Schar]	Rephrased. We use the glossary definition, and wish to point out here that the primary concern of this chapter is with forced climate change.
9-246	A	7:28		Replace "to change" with "with changes" [Vincent Gray]	See 9-245.
9-247	A	7:31	7:40	The text distinguishes between internal dynamics and externally-forced changes as if they were independent. There is no reason to believe that this the case. In particular, it is known that internal process can be responsible both for internal variability and for amplification or damping of externally-driven changes. The text should state something like "Neglecting the (existing) interactions between natural variability and externally-driven changes,..." [Stéphane Hallegatte]	Redrafted.
9-248	A	7:32	7:34	This sentence is confusing. Is it supposed to mean that Solar and Volcanic changes can contribute to "natural internal variability"? I do not think this is correct. The correct interpretation is that Solar and Volcanic changes can contribute to "natural climate variability" but not to "natural internal climate variability". e.g. "Some external influences, such as changes in solar radiation and volcanism, occur naturally and can be considered part of the natural variability of the climate system but are not considered part of the natural internal variability of the climate system". Natural and anthropogenic driven external influences should both not be considered part of internal climate variability.	Redrafted.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Gareth S. Jones]	
9-249	A	7:34		Insert after "external changes"; "defined as "anthropogenic" [Vincent Gray]	Rejected. The sentence appears to be clear as is. The glossary defines anthropogenic.
9-250	A	7:35		Insert after "revolution", "urbanization" [Vincent Gray]	Rejected. The sentence already refers to land use change.
9-251	A	7:38		But the models contain flaws, even though they are getting better. [Kevin Trenberth]	We don't say the models are perfect, or that the expectations are correct.
9-252	A	7:39	7:40	Delete from "The most" on line 39 to "studies" on line 40. The sentence is meaningless and also ungrammatical. [Vincent Gray]	Rejected. We have improved the grammar, but stand by the statement.
9-253	A	7:39	7:39	awkward sentence: do you need "of" after "assessment"? [Thomas Knutson]	Accepted.
9-254	A	7:42	9:41	Statistical analyses are useful, but there is no substitute for physics and observations. See for example, climate trend simulations by Hansen et al. (1997, 2002, 2005) where the effects of observed and inferred changes in radiative forcing constituents are modeled and compared to measured temperature changes over past decades. Changes in GHGs, which contribute most of the radiative forcing, are well documented, and their radiative effects have been accurately computed. The transfer of heat energy within the atmosphere and ocean are also being modeled with good accuracy - as evident from the good agreement between the modeled and observed trend of heat storage in the oceans (Hansen et al., 2005). There is no reason to ignore this information - this is what helps us to understand what is, and what has been happening in the climate system. [Andrew Lacis]	Section redrafted. See also 9-235.
9-255	A	7:44	7:55	I am not sure that the new definition of detection is explained particularly well. If we take the TAR definition, i.e. "an observed change is significantly different (in a statistical sense) than can be explained by natural internal variability" then is the new definition "an observed change is significantly different (in a statistical sense) than can be explained by natural internal variability and is consistent with our understanding of the climate system"? (It would be worth explicitly quoting the TAR definition to make it clear what the change is.) If this is the case then I am not sure the definition is really more precise. I can write down a mathematical expression for the TAR definition, the new definition seems to have some expert/value judgement associated with it. [Matthew Collins]	We now to the glossary definition.
9-256	A	7:44	7:54	This paragraph is nonsense. But I suppose you are devoted to it [Vincent Gray]	Noted.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-257	A	7:44	7:54	I wish to point to the authors one of the earliest studies on detection of CO <sub>2</sub> signals was published by Madden and Ramanathan (Science, 109, p. 763, 1980) in 1980 which correctly predicted that the warming will rise above the background noise and be detected by year 2000. This paper also discusses the various metrics that should be used to detect and attribute the warming due to greenhouse gases.. [Veerabhadran Ramanathan]	Noted.
9-258	A	7:44	9:41	This entire section introduces concepts of climate change detection and attribution that is far too narrow and inconsistent with the way in which the terms have been used in the past. Using the conceptual framework provided implies that we could never detect climate change without knowing the cause of the change. It presumes we know all the forcings that could have an impact on climate. This can only be tested through attribution. It negates the possibility of identifying a climate change without knowing possible forcings. This is backwards, most often we identify a climate change and then try to understand possible forcings. For, example Medieval Warming period from Paleo -- first identified as a change in climate -- then we try to understand the forcings, not simultaneously. I think the definition of detection is too narrow and negates a lot of observational and exploratory work in identifying climate changes. [Thomas Karl]	See 9-255.
9-259	A	7:45	7:45	"...were defined in the TAR (IPCC 2001)..." This is good. In this chapter almost all acronyms are defined when first introduced. This aspect of good technical writing usage was (unfortunately) not followed in the other chapter I reviewed. [Patrick Hamill]	Noted.
9-260	A	7:46	:49	What is an "expected" response? "Expected" on what grounds? "Expected" seems to imply that the detected change is understood, whereas "understanding" is part of the "attribution" process. Suppose we find an "unexpected" but statistically significant response in the observations? Why is this definition of "detection" more precise than the TAR definition? [Fons Baede]	Redrafted. See 9-255.
9-261	A	7:47	8:35	Detection (p7, 147) is defined as "the identification of one or more expected responses to changes in external forcing". Attribution (p8, 128) as detection plus ... (b) demonstration that the detected change is consistent with the estimated responses to the given combination of anthropogenic and natural forcing". I doubt that this distinction becomes clear to an outsider. A more careful discussion is needed. Also, I have my reservations about the change in the definition of "detection" since the TAR. Please explain why this change was needed. [Christoph Schar]	Redrafted. See 9-255.
9-262	A	7:49		"This evaluation..." should read probably "This identification...".	Redrafted. See 9-255.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Fons Baede]	
9-263	A	7:53	8:1	Obscure [William Ingram]	Redrafted.
9-264	A	7:57	8:2	Should reference Chapter 8 to justify climate models as appropriate tools. [Robert Colman]	Accepted.
9-265	A	7:57	8:2	Should note that models also based on observational aspects of climate [Robert Colman]	Discussion broadened.
9-266	A	7:57	8:2	Delete from "Climate models" in line 57 to "system" in Line 2. The sentence is meaningless and ungrammatical [Vincent Gray]	Redrafted.
9-267	A	8:0		Table 9.1.1a (I interpret the "Instructions" as meaning I make these comments as if the Tables & Figures appear where the main text says "INSERT", not using the repeated page numbers where they actually occur.) 3rd column, 4th cell (GHG wmg>obs) - this should be Very Likely [William Ingram]	Table revised.
9-268	A	8:0		Table 9.1.1a 3rd column, 5th cell (GHG wmg detn continentally) - this should be Likely bordering on Very Likely [William Ingram]	Table revised.
9-269	A	8:0		Table 9.1.1a 3rd column, 9th cell (trop height d&a) - this should also be Likely bordering on Very Likely [William Ingram]	Table revised.
9-270	A	8:0		Table 9.1.1a 3rd column, 2nd cell (wmg not wholly natural) - this should be Very Likely bordering on Virtually Certain [William Ingram]	Table revised.
9-271	A	8:0		Table 9.1.1b 3rd column, 3rd cell (cyclones) should be Unlikely [William Ingram]	Table revised.
9-272	A	8:1		This applies only if the models are good and can simulate current climate. [Kevin Trenberth]	Rejected. We don't claim the models are perfect. Paragraph revised.
9-273	A	8:4		Delete "firm" [Vincent Gray]	Accepted.
9-274	A	8:8	8:10	It is always possible..." There is a slight inconsistency here. The sentence could either use the phrase "...at < 5% level..." or in the next line drop the words "less than." I prefer the second choice, so the sentence would read, "...a result that is found to be significant, at say the 5% level, ...would have occurred in any case with 1 chance in 20 in an ... [Patrick Hamill]	Accepted.
9-275	A	8:8		Delete "absolutely"	Rejected.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Vincent Gray]	
9-276	A	8:15		Replace "anomalous" with "additional" [Vincent Gray]	"Anomalous" deleted.
9-277	A	8:26	8:35	This paragraph provides a much-needed caveat, but then its lessons seem to be ignored in everything that follows. Moreover it is not enough to say that, in effect, "attribution" is not possible so we will simply redefine the term to mean something that is possible, but which doesn't actually constitute "attribution". The paragraph should say: "Detection does not itself imply attribution of the detected change to an assumed cause. As noted in the SAR (IPCC, 1996) and the TAR, unequivocal attribution would require controlled experimentation with our climate system. That, of course, is not possible, and thus from a practical perspective attribution of anthropogenic climate change is not possible. The best that we can do is what might be better termed "pseudo-attribution," which is understood to mean: (a) detection as defined above, (b) demonstration that the detected change is consistent with model-generated estimates of the response of the climate system to a particular combination of natural and anthropogenic forcing, and (c) demonstration that the detected change is not consistent with model-generated estimates of alternative, physically-plausible explanations of recent climate change that exclude anthropogenic forcing. In what follows, the term "attribution" should be understood to mean conditions (a)-(c), with the caveat that these would not be considered sufficient to establish attribution of cause in an experimental science. Moreover, even within the canons of non-experimental science, conditions (a)-(c) are weakened by their reliance on two strong assumptions. First, the models that generate the signals being used to evaluate consistency against null hypotheses must be assumed to provide accurate representations of how the actual climate system responds. To the extent this is not a valid assumption, conclusions about attribution are correspondingly weakened. Second, the conclusions assume that all relevant causal forces that might cause the climate system to change, and all sources of internal variability that might induce change without external forcing, are represented in the statistical model. To the extent that major potential forcing agents, such as indirect solar mechanisms, land-use change, water vapour feedbacks, cloud feedbacks, and other such physical phenomena are either left out of climate models or are inadequately represented, the statistical model will potentially suffer from omitted variable bias and the results may thereby be incorrect." [Ross McKittrick]	Rejected. The reviewers concerns are discussed.
9-278	A	8:26		Delete "immediately" [Vincent Gray]	Accepted.
9-279	A	8:30	8:35	I like the fact that the authors have clearly defined exactly what is meant. [Patrick Hamill]	Noted.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-280	A	8:36		Add a separate line "Attribution" in this report does not therefore mean that a proven relationship has been established" [Vincent Gray]	Rejected. As the reviewer knows, absolute "proof" is generally impossible to obtain from observational evidence. This should not prevent us from drawing conclusions when adequate evidence is available.
9-281	A	8:37	8:50	This paragraph raises a key point about estimates of internal variability. By dismissing observation-based estimates of residual variability on the grounds that the time series are "short relative to the timescales of interest" you are implying that the variance changes so much over time that even 150 years of data does not suffice to estimate it. If this is really true, then why is the same data sufficient for detection, as well as everything else in the report? Also, you are effectively saying the data are nonstationary. But the entire rest of your statistical model, including the Appendix I material, relies on the assumption of stationarity. [Ross McKittrick]	Rejected. The text discusses the ability to estimate interdecadal scale variability from records of 15 or fewer decades. Such estimates are subject to considerable sampling variability.
9-282	A	8:39		Replace "anomalous" with "additional" [Vincent Gray]	"Anomalous" deleted.
9-283	A	8:42	8:42	other estimates of variability [Stephen McIntyre]	We don't understand the comment.
9-284	A	8:42		But the models are flawed and natural variability, like ENSO, is not that well done. [Kevin Trenberth]	The following sentence points out that validation of model simulated variability is important.
9-285	A	8:44		Replace "validation" with "evaluation" [Vincent Gray]	Accepted.
9-286	A	9:6	9:6	I believe you mean to say: "A further part of an attribution analysis is ruling out the possibility that the observed change is consistent with alternative explanations..." or "...demonstrating that the observed change is not consistent with..." [Thomas Knutson]	Revised as suggested.
9-287	A	9:15	9:27	Unclear. There is a well-known distinction between several types of uncertainty: (1) the value uncertainty (see IPCC guidance on uncertainty) is the uncertainty on model parameters (it is not too difficult to cope with that) and (2) the uncertainty in the representation of physical processes in models, i.e. the structural uncertainty. But I think that things can be made cleared using the following categories: Structural uncertainty is due to our lack of knowledge on several layers: 1. the physical layer : one part of uncertainty arises from our poor understanding of some physical processes (e.g. microphysics in clouds), independently of the modelling exercise (we do not know how it works).	We have added a reference to structural uncertainty.



No.	Batch	Page:line		Comment	Notes
		From	To		
				2. the modelling layer : one part of the uncertainty arises from our inability to model well-known physical processes (in other words, "we know how it works but we do not know how to model it", I would say that convection belongs to this category). This last uncertainty can be divided into : (i) one part that can be measured by the spread between models using different parametrizations; (ii) one part that cannot be measured by this spread, because all GCMs have more or less the same structure and therefore the same errors (again the example of convection). [Stéphane Hallegatte]	
9-288	A	9:15	9:27	I think that in chapter 9 other methods for attribution than dynamical modelling should be mentioned. I suggest to insert a sentence at page 9, in the paragraph between lines 15 and 27. "Besides using general circulation models, attribution can be carried out using other approaches, such as neural networks. Such method allows to establish nonlinear relations without relying on the explicit description of the physical process involved. Convergence of attribution studies based on different approaches is very important in order to increase confidence on the results" [Piero Lionello]	We disagree that purely descriptive statistical methods, such as neural networks analysis, can be used for attribution.
9-289	A	9:15	9:16	carry forward to summary [Stephen McIntyre]	Model uncertainty is referred in the ES.
9-290	A	9:17	9:17	Should read 'Chapter 10' here I think. [Robert Colman]	Accepted.
9-291	A	9:18	9:18	"degeneracy" needs explanation: it is the standard term, but it is not a widely-familiar concept [William Ingram]	Explained.
9-292	A	9:27		Replace "robust" with "somewhat less uncertain" [Vincent Gray]	Rejected.
9-293	A	9:27		But the models are imperfect which limits ability to do attribution of things like precipitation. [Kevin Trenberth]	Redrafted.
9-294	A	9:34	9:35	that were ... At the time" -> "then [William Ingram]	Accepted.
9-295	A	9:43	9:43	Is there any rationale as to which words get capitalized and which words are not capitalized in subsection headings? [Andrew Lacis]	This is an editorial detail for consideration by the TSU.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-296	A	9:44	10:56	Effort should be directed toward understanding climate change and climate variability by investigating contributing causes and assessing the climate response. Attribution is mostly a pointless exercise. It is like trying to assign blame after having lost a basketball game 99 to 98. Do you blame the guy who missed the last desperation shot (even though he scored fifty points up to that time). Maybe the blame should go to the coach for calling a bad play. Maybe it was somebody else who screwed up on defense. ... or maybe it was just plain bad luck, or maybe the other team got lucky. The basis from which to begin is the basic observational record that documents the steady increase in CO <sub>2</sub> and other greenhouse gases, and also the fact that the global mean temperature has been increasing over the past century. The Earth has always had a greenhouse effect that keeps the surface temperature some 33 K warmer than it would otherwise be. The added GHGs are simply strengthening the greenhouse effect. If blame needs to be attributed, then explaining the observed increase in GHGs should suffice. [Andrew Lacis]	We disagree.
9-297	A	10:4		Insert after "paleo-reconstructions". However, some recent studies have found evidence that temperatures comparable to or greater than those current today occurred in the 11th and 15th centuries [Vincent Gray]	Rejected. This is a summary of TAR results.
9-298	A	10:11		Insert after "observations"; "after adjustment of model parameters" [Vincent Gray]	Rejected. Models are not tuned in the way suggested.
9-299	A	10:11		Insert after "response to"; "known" [Vincent Gray]	Accepted.
9-300	A	10:11		Insert after "explain"; "all of" [Vincent Gray]	Revised.
9-301	A	10:18		Insert after "was"; "considered to be" [Vincent Gray]	Rejected. These are objective results that are being discussed.
9-302	A	10:19	10:19	In fact, only radiative forcings are discussed [William Ingram]	Accepted.
9-303	A	10:23		Replace "found" by "thought" [Vincent Gray]	Rejected.
9-304	A	10:56		Add at end "There is no mention of human influence in this statement so it may include natural changes in greenhouse gas concentrations" [Vincent Gray]	Rejected.
9-305	A	11:0		Section 9.1.4 is unduly long. [Kevin Trenberth]	Redrafted and shortened.
9-306	A	11:11	11:17	We get our information by making observations. It should be clarified at what point	See 9-235.



No.	Batch	Page:line		Comment	Notes
		From	To		
				"observations" lead to a "detection". Perhaps it may happen when a sufficient baseline of observations has been accumulated and that some sort of "trend" or "change" becomes apparent - one that stands out "significantly" from the "normal" variability of the observational record. This is undoubtedly the case for GHG and temperature trend measurements. Fortunately, CO <sub>2</sub> and other GHG measurements have been made with such precision that even the most strident of greenhouse skeptics are not likely to question the validity of these measurements. The global temperature record is also well established, but there are some legitimate questions of sampling and "heat island" effects that need to be addressed. [Andrew Lacis]	
9-307	A	11:12	11:17	It is one thing to have "detection" of anthropogenic regional changes on the environment include all of the land-use, deforestation, and agricultural activities. Detection and attribution of regional climate change is a totally different problem where current modeling and observational uncertainties severely limit the accuracy of any conclusions that can be drawn. [Andrew Lacis]	Rejected. The objective here is simply to give a road map of the rest of the chapter. Assessment of regional detection will appear farther on.
9-308	A	11:13	11:13	D&A may be exciting to the chapter authors, but I'm not sure it is true in the general case. [Matthew Collins]	Redrafted.
9-309	A	11:14	11:14	Again, in fact, only radiative forcings are discussed [William Ingram]	Noted, but "external" makes clear the point that these forcings do not arise from natural internal mechanisms.
9-310	A	11:14	11:14	I would suggest changing "However" to "In addition" as the two sets of analyses are complementary rather than in conflict. [Michael MacCracken]	Revised along the lines suggested.
9-311	A	11:19	11:28	It would be better to start with the observational record of greenhouse gas trends which have been precisely measured and are not subject to dispute. There is a place for "top-down" methods to infer limits on possible aerosol contributions using the observed temperature record and climate change modeling approaches. [Andrew Lacis]	Rejected. This is a comment on the scope of the chapter, which was determined by a formal scoping process.
9-312	A	11:27	11:27	Again, in fact, only radiative forcings are discussed [William Ingram]	See 9-309.
9-313	A	11:47	11:57	It is pointless and ineffective to be harping so much about anthropogenic influences - just discuss the observational results. [Andrew Lacis]	We do describe observational results.
9-314	A	11:47	11:47	Delete "the following" as redundant with "Section 9.5--there is no other section 9.5." [Michael MacCracken]	Accepted.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-315	A	12:0		Table 9.2.1 3rd column 1st cell (F&al02) - a 2D model can hardly be called a GCM [William Ingram]	revised
9-316	A	12:0		Table 9.2.1 caption - does Suli mean 1st or 1st & and indirect effects, or what? [William Ingram]	revised
9-317	A	12:8	12:29	This section is repetitive and unnecessary. Delete it [Vincent Gray]	revised
9-318	A	12:8	12:30	Statistical detection and attribution analyses can be useful, but there is no substitute for physical modeling of radiative forcings and observations. See for example, Hansen et al. (1997, 2002, 2005) where the effects of observed and inferred changes in radiative forcing constituents are compared to the observed global temperature record. Considerable confidence can be attached to the global mean comparisons between model simulations and the observed temperature record since this is primarily a global energy balance problem. Much less confidence is warranted for regional climate changes since the modeling and understanding of regional climate change is still rather primitive. [Andrew Lacis]	We introduce now the forward calculators that you seem to prefer and discuss them thoroughly in section 9.4
9-319	A	12:8		Obvious overlaps with Chapter 2. There is a need for a "box" relating key material in Chapter 2 to what is in Chapter 9 (and to a lesser extent that in Chapter 6) [Bryant McAvaney]	Put in a table and explicitly mention Ch. 2
9-320	A	12:10	12:17	Repeats. [Kevin Trenberth]	revised
9-321	A	12:19	12:29	Too much description of what will be included. [Kevin Trenberth]	revised
9-322	A	12:19	:22	Another assumption behind this statement that should probably be mentioned is the absence of non-linear effects (it is assumed that the change can be expressed as a linear combination of patterns for all forcings). [Christoph Schar]	Added
9-323	A	12:23	12:23	Define "bottom-up" forcing.(is p 13 line5) [Bryant McAvaney]	Revised to Forward model calculator of forcing
9-324	A	12:31		Section 9.2.1: I find this section confusingly difficult to read. I suggest to add a box, explaining in clear and simple terms the difference between "top-down" and "bottom-up" approaches. I also suggest to add a table or figure (perhaps copying one from Ch 2) showing and summarizing the many different radiative forcings. [Fons Baede]	Revised to Forward/Inverse; table added
9-325	A	12:33		Section 9.2.1.1: it does not make sense to present all these numbers here in the text. Give a table or figure. [Fons Baede]	done



No.	Batch	Page:line		Comment	Notes
		From	To		
9-326	A	12:37	12:40	You say "Radiative forcing change since 1750 is reviewed in detail in Chapter 2." then go on to list a large number of forcing agents. First of all it isn't clear what change means here. Use the term "temporal pattern", which you use elsewhere if not it could be taken to mean a simple difference between forcings at two times. Secondly, I don't agree that the temporal changes have been reviewed in as much detail as you require here. The figure 2.9.3 shows the temporal change for one model (without uncertainty ranges). Your fig 9.4.1 presumably relies on a range of temporal changes that are in each model. To make sense of the arguments presented in your chapter you need to show these patterns and show that they are consistent with the conclusions in Ch2.G20 [Kenneth Carslaw]	Reworded – Figure 2.9.3 now mentioned.
9-327	A	12:37	12:42	While the IPCC paradigm is that one can simply add up radiative forcings across the globe, independent of their spatial distribution (geographically--and perhaps vertically), it is not really clear that this is going to be sustainable as model simulations and analyses improve over time. The spatial distribution of sulfate aerosols changes over time through the 20th century, and this likely means that there have been changing influences on the atmospheric circulation, which will adjust to make up for regions of excess or reduced energy input by changing the atmospheric circulation, and these changes can have large-scale influences. Were the IPCC approach to adding up forcings to be applied to orbital element forcing, there would be roughly zero forcing, yet a very large response--so the seasonal and latitudinal distribution of forcings must matter (calculating the forcing for glacial times by switching what are normally called feedbacks into forcings is a bit of an ad hoc way of trying to sustain the IPCC paradigm--a bit like epicycles). We are also learning that absorbing aerosols may well have, in effect, a different climate sensitivity than GHGs, so again, the IPCC paradigm needs to be tweaked. Given that such tweaking likely lies ahead, I would advise inserting somewhere in this paragraph a statement that this simple summing up of fluxes provides only a rough sense of the amount of forcing and expected response, and perhaps accounting for the uncertainties created by this factor not being well-represented in the estimates of uncertainty.. [Michael MacCracken]	added
9-328	A	12:38		Water vapour is a "long-lived greenhouse gas. Why is it omitted? [Vincent Gray]	Not omitted, also not "long lived"
9-329	A	12:42	12:48	Why repeat this material from Chapter 2? [Bryant McAvaney]	Now in a table.
9-330	A	12:42		Replace "90%" with "95%" and change all the figures to comply. Read Chapter 3 page 3_7, lines 14-24 and adopt their recommended practice of the use of 95% confidence figures. [Vincent Gray]	This is from Ch. 2



No.	Batch	Page:line		Comment	Notes
		From	To		
9-331	A	12:43	12:48	It seems quite perplexing to have the uncertainty of the direct aerosol forcing by 100% and of the indirect forcing only be 30%. Perhaps what is confusing here is that percentages are confusing things, especially when one considers the net direct aerosol forcing from summing up several sources. It would seem to me better to simply give estimates and uncertainties in terms of the fluxes--so "total direct aerosol forcing" would be -0.2 plus or minus 0.2, first indirect aerosol forcing would be -1.2 plus or minus 0.4 or something like that--for one gets somewhat of a misimpression from the percentages (200% for contrails makes it look like a very significant uncertainty, but the magnitude is very small). I also really wonder how there can be 3-figure precision on the uncertainty of surface albedo change--this should say plus or minus 0.07 or something. And having this in a table would make for much easier comparison. [Michael MacCracken]	Changed to absolute values and put in a table
9-332	A	12:43	12:48	the values are difficult to identify in the text. May be a table? [Bernard Seguin]	done
9-333	A	12:43		Replace "(10%)" by "(12%) (multiply by 1.21)" [Vincent Gray]	Not consistent with Ch. 2.
9-334	A	12:43	:48	Please provide the uncertainties in W/m <sup>2</sup> (rather than %). Statements like "aviation induced contrails (0.01 W/m <sup>2</sup> (200%))" just give the wrong impression. You were actually lucky that none of the radiative forcings ended to be zero (otherwise the attached relative uncertainty would be infinite). [Christoph Schar]	done
9-335	A	12:44		Replace (40%) by "(48%)" [Vincent Gray]	Not consistent with Ch. 2
9-336	A	12:44		Replace "(50%)" with (61%)" [Vincent Gray]	Not consistent with Ch. 2
9-337	A	12:44		Replace "(100%)" with (121%)" [Vincent Gray]	Not consistent with Ch. 2
9-338	A	12:46	12:47	What is the confidence in the RF given for aviation induced ice clouds? [Lourdes Maurice]	Full details in Ch. 2 – there is only a range given.
9-339	A	12:46		Replace "(30%)" with "(36%)" [Vincent Gray]	Not consistent with Ch. 2
9-340	A	12:46		Replace "(200%)" with "(242%)" [Vincent Gray]	Not consistent with Ch. 2
9-341	A	12:47		Replace "(235%)" with "(284%)" [Vincent Gray]	Not consistent with Ch. 2
9-342	A	12:48		Replace both " (200%)" with "(242%)"	Not consistent with Ch. 2



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Vincent Gray]	
9-343	A	12:49	12:49	Explain what are the IPCC AR4 models [Bernard Seguin]	Web site referenced now.
9-344	A	12:53	:54	Total net forcing is presented here with great precision, followed by a very confusing clause: "although a forcing close to zero is possible"! What is the probability for the forcing to be close to zero? [Fons Baede]	67% confidence interval given
9-345	A	12:54	12:54	First, the 3 figure precision on this range seems to me absurd. Second, without some prior discussion of context, the breadth of this range seems to me to be misleading. It would really help to have (a) a few sentences more of introduction to this topic up around line 32--so saying there are bottom-up and top-down approaches to getting at the forcing, with uncertainty about bottom-up arising due to limitations in observations of forcings over space and time, thus giving a quite broad range--and uncertainty in top-down arising because of uncertainties in model physics and in understanding paleoclimates. (b) section 9.2.1.2 needs to have a comparable sentence giving the mean and the range via the top-down approach for comparison. (c) And there needs to be some sort of indication that while the range represents the 90% confidence level, the central value is more likely to be near the real answer, because there are other sorts of constraints that apply (e.g., thermodynamic consistency, the record of Earth history, recent climate variations, etc.) and that these are suggestive that the mean values are roughly correct. It really baffles me why the phrase "although a forcing close to zero is possible"--remember that in the IPCC lexicon, "possible" means roughly 50% likelihood, and there is no way that a zero forcing is this likely. I would suggest simply deleting this phrase as the text above already says these are 90% limits. [Michael MacCracken]	The precision is that given in Ch. 2; The introduction is now in first par of 9.2 and in the first paragraaf of 9.2.1.2. Took out offensive sentence and now talk about confidence interval.
9-346	A	12:54	12:56	Another reason to be cautious about simply adding up these fluxes is that they have had different temporal patterns over the 20th century, so their likely climate effect will be different--and just adding them up this way really could be misleading. In addition, their geographic, vertical, and seasonal patterns also likely have influences, so indeed, this totalling up not only provides a weak constraint, but likely presents an over simplified representation of the net influence--and we should only be using these numbers as a rough estimate. [Just a note that the vertical distribution of aerosols has likely varied significantly over the century--originally, the SO2 was emitted near the surface, and so sulfate was mostly at a level below most of the absorbing water vapor, whereas when tall stacks came along (and coal burning in homes was reduced), most of the SO2 was emitted higher up, so the sulfate lifetimes were much longer, the aerosol geographic distribution was greatly changed, and this aerosol was above most of the water vapor and so had a	Ch. 2 , of course did this addition. Now point to fig 2.9.3 for temporal example



No.	Batch	Page:line		Comment	Notes
		From	To		
				somewhat different effect. Similar changes are taking place with respect to the absorbing aerosols.] Overall, I would suggest that a good bit more qualification be expressed about simplifying representation of what happened during the 20th century, with the varying timing, location, and magnitudes of the forcing making it really essential to be using comprehensive models to put everything together. [Michael MacCracken]	
9-347	A	12:54		Replace "likely" with "calculated" [Vincent Gray]	Changed wording
9-348	A	12:55	12:55	forward reference to subsection needs fixing - need substantiation of statement [Bryant McAvaney]	removed
9-349	A	13:4	13:13	There is also the fact that due to the large heat capacity of the ocean there exists about 0.85 W/m <sup>2</sup> of unrealized warming (Hansen et al., 2005). The climate system simply hasn't had time to respond to the accumulated GHG radiative forcing. [Andrew Lacis]	No change
9-350	A	13:5	13:23	These two paragraphs discuss and distinguish "bottom-up" and "top-down" estimates (better, "forward" and "inverse" calculations) of radiative forcing. The definition and discussion of differences should be improved. Borrowing from Anderson et al. (2003, Science, 300, 1103-1104), concise definitions and discussion could be given as follows: "Forward calculations of aerosol climate forcings are based on knowledge of the pertinent aerosol physics and chemistry and, as such, can resolve the separate contributions by various aerosol components and forcing mechanisms. In contrast, inverse calculations infer aerosol forcing from the total forcing required to match climate model simulations with observed climatic changes. Thus, inverse calculations require additional assumptions that are not employed in the forward calculations: specifically, that the observed climatic changes are accurately known and that climate models accurately represent the transient relationship between forcing and climate response." [Theodore Anderson]	accepted
9-351	A	13:5	13:5	After mentioning "bottom-up estimates" (better, "forward calculations"), cross-reference Chap. 2, which is where forward calculations of radiative forcing are comprehensively discussed. [Theodore Anderson]	Now in title of section 9.2.1.1
9-352	A	13:5	13:5	Unclear. Replace by: "Bottom up approaches to estimate AEROSOL forcing ..." [Stéphane Hallegatte]	accepted
9-353	A	13:5	13:13	One issue not mentioned here is the possibility that aerosol's efficacy, that is "a comparative measure of the effectiveness of a given radiative forcing agent at changing the surface temperature, compared to carbon dioxide." (Chapter 2 pp3), is important. So it could still be possible (although unlikely with current knowledge) that a net forcing close	Efficacy mentioned in previous para. And statement qualified here



No.	Batch	Page:line		Comment	Notes
		From	To		
				to zero does not automatically imply high sensitivities, e.g. the climate response to aerosols could be smaller than that for CO <sub>2</sub> . Similarly it could be theoretically possible that a net negative forcing could still cause a warming if the sensitivity to CO <sub>2</sub> is much higher than the sensitivity to aerosols. There may be reasons to rule these situations out, but a reference to chapter 2's discussion about efficacy would be helpful here or at least mentioned. [Gareth S. Jones]	
9-354	A	13:5	14:25	An issue of balance here - this quite a extensive section some tighter summary is called for. [Bryant McAvaney]	Will try
9-355	A	13:5	:13	I find the way of reasoning in this subparagraph unconvincing, perhaps even circular. Model estimates of natural variability may be wrong and therefore zero or negative forcing may not be ruled out on that basis. By ruling out zero or negative forcing, the implicit assumption is made that there is external forcing, but that is what we are trying to prove. [Fons Baede]	The assumption, as stated, is that the natural variability in the model is correct
9-356	A	13:6	:7	What is the meaning of the sentence "These might yield.. etc"? What means "might yield"? They "do yield" or "do not yield", but why "might". [Fons Baede]	removed
9-357	A	13:8	13:13	Even internal variability has to satisfy heat and water budgets and so there has to be sources of heat via redistribution. If the model is constrained by observations, how can it warm unless there is an imbalance at TOA? The role of natural variability is given too much credence, given constraints. [Kevin Trenberth]	Revised wording
9-358	A	13:9	13:13	This sentence states that "net negative forcing would be impossible to reconcile with instrumental observations unless the entire warming were due to natural internal variability, which is effectively ruled out if the natural variability in unforced climate models (or the amount of variability in paleoreconstructions that is not explained by external forcings) is taken as the measure of internal variability (Mitchell et al., 2001; see also Section 9.3.4 and 9.4.1.3)." This is accurate as far as it goes and well stated. However, the "unless" and "if" conditions deserve emphasis. A good way to accomplish this would be to add the following sentence: "This situation shows the importance of continued efforts to validate model-based estimates of natural climate variability." [Theodore Anderson]	We also have observed ocean warming which contributes (now stated)
9-359	A	13:15	13:23	Although detection and attribution methods can obtain useful information about	Added discussion



No.	Batch	Page:line		Comment	Notes
		From	To		
				<p>magnitude of uncertain forcings what also should be mentioned here is the risk of circular reasoning. If a magnitude forcing deduced from D&amp;A methods is put into a climate model to compare with the observations then naturally the result will appear better than it could have been if "bottom up" methods were used to get the forcing magnitude.</p> <p>What the "top down" derived estimates of forcing magnitudes can be used for and what they can't be used for should be stated more clearly here, and two possible references to this issue should be added (TL Anderson et al 2003 &amp; H. Rodhe, R. J. Charlson and T. L. Anderson, "Avoiding Circular Logic in Climate Modeling", CLIMATIC CHANGE 44 (4), 419-422, 2000)</p> <p>[Gareth S. Jones]</p>	
9-360	A	13:15	14:25	<p>There is no real consensus that current climate GCMs are capable of reliably modeling regional climate system responses. So, detection and attribution of radiative forcings and feedback responses on a regional scale is mostly wishful thinking. Any top-down analysis approach must be capable of accurately reproducing all climate feedbacks - otherwise any implicit radiative forcing can not be unambiguously distinguished from overestimated or underestimated feedback contributions. This is a particularly worrisome problem for the simplified 2D and energy balance models used in TABLE 9.2.1. For such simplified models, the feedback processes are inevitably prescribed or "tuned" with significant arbitrariness. Hence, conclusions from such models are necessarily suspect. While the simplified model approach can produce useful results and insights, strong caveats need to be expressed.</p> <p>[Andrew Lacis]</p>	Added caveat to EMIC discussion
9-361	A	13:33		<p>Insert "fairly" before "well known"</p> <p>[Vincent Gray]</p>	Not accepted
9-362	A	13:39	13:39	<p>Columns and lines of table 9.2.1 do not contain the identification of what is reported, so that it is impossible to understand the content</p> <p>[Bernard Seguin]</p>	Caption revised.
9-363	A	13:57	14:5	<p>This sentence might mention that the growth in aerosol forcing during this period was likely partly due to the increase in emissions with the economic development following WW II, but was likely much more dramatically influenced by the switch to tall stacks that occurred, which had the effect of increasing the sulfate lifetimes from a day or two, when SO<sub>2</sub> emissions were at the surface, to a week or two once SO<sub>2</sub> emissions got injected so that they were quickly lofted into the troposphere and were above the fast removal processes operating in the surface boundary layer. This longer lifetime allowed the effects of the aerosols to be felt over a much larger region (including over the very dark ocean surface where they would have a greater effect than over land areas). In addition, when this happened, the dark, sooty component of the coal effluent was typically scrubbed out,</p>	Don't know of any evaluation of the switch to tall stacks.



No.	Batch	Page:line		Comment	Notes
		From	To		
				so the reflective efficiency of the sulfate aerosols likely increased as well. It is not clear that the models are representing all aspects of these changes (so it is not clear that the temporal pattern is really correct), and the limitations of present simulations should be mentioned as an area where there remain significant uncertainties to be worked out. [Michael MacCracken]	
9-364	A	14:0		Figs 9.2.1 & 9.2.2 - combine them, or at least give them the same colour scale [William Ingram]	Tried same scale but this washes out BC changes
9-365	A	14:25	14:25	What is the summary statement? [Bryant McAvaney]	Given in above paragraph.
9-366	A	14:27	15:22	Need to ensure complete consistency with Chapter 6 and remove unnecessary overlap. The Late Maunder Minimum is not discussed in Chapter 6 [Bryant McAvaney]	We agree, done.
9-367	A	14:27		Section 9.2.1.3: Another section made difficult to read due to the many numbers which should preferably be presented in a table. [Fons Baede]	Numbers have been somewhat reduced. Usefulness of a table is discussed with chapter 6, here, however, we feel the discussion in a table would be too short.
9-368	A	14:30	14:30	I would suggest writing the acronym LGM after the words "Last Glacial Maximum" since the acronym is used two paragraphs further on. [Patrick Hamill]	Done.
9-369	A	14:33	14:33	The correct reference is Hays et al (1974) [Gavin Schmidt]	Reference has been clarified (review paper applicable).
9-370	A	14:34	14:35	But what about comment in section 6 that orbital forcings do not achieve permanent snow cover? [Stephen McIntyre]	The sentence is correct as stands.
9-371	A	14:34		Delete "strong" [Vincent Gray]	Sentence reassessed, is correct.
9-372	A	14:37	14:37	"Solar insolation" is repetitive. Replace either by "Insolation" or by "Solar irradiation". [Martin Stendel]	Text has been edited.
9-373	A	14:39	14:39	All GHGs, not just CO2 [Gavin Schmidt]	CO2 is cited as the main greenhouse gas responsible for radiative forcing. We give estimates for it consistent with chapter 6
9-374	A	14:40	14:44	As mentioned in a prior comment, there is some inconsistency across the WG I draft regarding what is forcing and what are feedbacks. During the LGM, the lower CO2 concentration was the result of a feedback--and when we include the carbon cycle in Earth system models, the changes in the apportioning of among the reservoirs of the	Text has been clarified to make the role of CO2 clear. Also, the ice sheets are now termed boundary conditions.

**Comment [GH1]:** Pascale, can you check what he refers to?

**Comment [GH2]:** is this consistent with ch6?

**Comment [GH3]:** pascale, is this correct?



No.	Batch	Page:line		Comment	Notes
		From	To		
				atmosphere, ocean, and biosphere will be treated as a feedback, and not a forcing--it is only because our models are not yet fast enough that this is being made the equivalent of an external forcing. Similarly for vegetation changes and even for snow and ice cover--and ultimately for ice sheets. I would urge that the IPCC come up for some other term to describe the situation when the effect of a feedback is not included in a model, for calling the CO2 variation of the LGM a forcing rather than a feedback will be playing into the confusion of the Skeptics, who will be then doing their temporal correlations and saying that the CO2 is coming after the warming so CO2 changes don't have an effect, etc. [Michael MacCracken]	
9-375	A	14:46	14:48	I would suggest doing a bit of rewording, changing this to: "Because of the differences in the Earth's orbital parameters during the mid-Holocene, summer insolation in the Northern Hemisphere was 5% higher than at present. However, because of the seasonal cycle in forcing was larger by about 27 W/m**2, there was much less insolation during the winter, and so there was only a negligible change in annual solar forcing (0.011 W/m**2). For the Southern Hemisphere, there was a net annual forcing of -1 W/m**2 and for the globe, the annual net forcing was only 0.XXX W/m**2." [Michael MacCracken]	Text has been rephrased.
9-376	A	14:51	14:54	On line 52, change "lead" to "led"--but more important, for what periods are these changes in flux applicable--is this comparing the year 2000 to the year 1000 or what? And is this present minus past, etc.--this really needs to say when is being compared to when--and it might even be better to have the baseline be the same preindustrial period used to reference human induced contributions. [Michael MacCracken]	This has now been clarified.
9-377	A	15:0		Fig 9.2.3 - should say why polar caps omitted [William Ingram]	Explanation added in caption.
9-378	A	15:0		Fig 9.2.3 - particular models cannot be picked out, so omit key [William Ingram]	removed.
9-379	A	15:1	15:1	Move "2" into exponent. [Martin Stendel]	Moved.
9-380	A	15:3	15:3	"global dimming" is a very inappropriate name for a phenomenon which makes the globe brighter! While obviously some physically inappropriate terms must be accepted as having become standard (e.g. "greenhouse effect"), I don't think this is one. If the term has to be used for the sake of readers who expect to see it under that name, I suggest something like 'Sometimes called "global dimming" (despite being a brightening of the globe), this ...' [William Ingram]	Changed to state "sometimes called..." and explain dimming is at surface.
9-381	A	15:3	15:22	The one obvious thing in Fig 9.2.3 is the effect of Pinatubo, but no clue is given the reader	Explanation added.



No.	Batch	Page:line		Comment	Notes
		From	To		
				that that's what it is! It should be mentioned somewhere in the text (or possibly the figure caption) [William Ingram]	
9-382	A	15:12	15:13	The use of "Late Maunder Minimum" is used in this paragraph incorrectly at least once. Maunder Minimum is a term to describe a period of low numbers of sunspots or low solar activity between 1640 and 1720, it is not a climatic term. The sentence could read something like "During the cool period, approximately 1675-1715, sunspots were generally missing (the so called Maunder Minimum) and solar irradiance is believed to have been smaller than before and after". If this correction is made then the use of "Maunder Minimum" for lines 14-22 will be correct, as it will be clearer they refer to the lack of sunspots and not a cool period on Earth. [Gareth S. Jones]	Suggestion has been adopted,
9-383	A	15:18	15:18	Change "having" to "to have" [Michael MacCracken]	Text has been edited.
9-384	A	15:21	15:21	I am confused by the sign here--is not the forcing positive from the Maunder Minimum to the present? Why give this as present to preindustrial as this has time going backwards--and we really want to be comparing with human forcings (so how does human-induced forcing compare to the natural change since preindustrial--the direction of comparison needs to be changed. And should the comparison be to present--or to the preindustrial baseline? [Michael MacCracken]	The direction we look at is from the present time back, to explain relatively cooler conditions. This has been clarified in the text.
9-385	A	15:21	15:21	What is the rationale for doing Maunder minimum to present? It is much cleaner to do MM to the mean pre-industrial. [Gavin Schmidt]	These values are used for an estimate of climate sensitivity between both periods. Hence, this has been kept.
9-386	A	15:24	15:56	While there are characteristic features for the different types of radiative forcings, these characteristics are anything but universal. They depend on GCM treatment of model dynamics, feedback processes, and ocean-atmosphere interactions. Volcanic forcings depend on the geographic location and time of year of the volcanic eruption. Note for example that the Agung and El Chichon eruptions were hemispherically quite different. [Andrew Lacis]	Have emphasized that these are illustrative.
9-387	A	15:24		On "Spatial and Temporal Patterns of the Response to Different Forcings and their Uncertainties."? At least for me, it was very impressive to see the relatively low temperature rises at equatorial band and large rises at certain Northern temperate regions (e. g. Fig. 3.4.4). Is there any model result which can explain such spatial distribution of temperature? If there is not, it should be clearly written that the current state of the models do not reach this level; regional climate changes are the very target of the United Nations Framework Convention on Climate Change.	This is what the detection and attribution studies try to do – compare predicted and observed changes. However, their capability to explain small regional changes is limited. Hence they work on large patterns of change. This is now stated.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Kiminori Itoh]	
9-388	A	15:24		It is not clear how the presented results were obtained. The description of the experiment should be slightly extended to explain what forcing has been applied, what ozone changes have been used for entire 20th century and so on. How was calculated the solar effect? It would be useful also to compare the obtained results with observations (where it is possible). [Eugene Rozanov]	These descriptions are in the original papers. Not added, since these are only illustrative.
9-389	A	15:26	15:51	Results from a single model need downplaying against results confirmed with other models. [Bryant McAvaney]	Agree. Added caveat that these are only useful when a large number of models show such distinction.
9-390	A	15:27	15:32	Discuss direct solar forcing in ocean versus IR effect. [Stephen McIntyre]	Do not understand what is needed. We do discuss the greenhouse gas effect seen in Figure 9.2.1.c and the solar forcing in Figure 9.2.1.a on a zonal average basis.
9-391	A	15:27	15:51	References to the sub-panels of Fig. 9.2.1 are mixed up in this paragraph. [Leon Rotstayn]	Thanks, fixed.
9-392	A	15:28	15:32	This is entirely based on results from a few & imperfect models, & should have some indication of this, & the resulting uncertainty [William Ingram]	Have added a reference for the need for more models.
9-393	A	15:28	15:51	According to Fig 9.2.1, the ozone induced cooling of the NH polar stratosphere is roughly equivalent to the cooling of the SH polar stratosphere. Is this really correct? [David Thompson]	Actually, the SH polar stratosphere is cooled more than the NH as can be seen in the larger extent of the coolest region. But we now reference the need for more models.
9-394	A	15:32	15:32	It should be stated that the CSIRO climate model was "coupled to a mixed-layer (q-flux) ocean". [Leon Rotstayn]	Added.
9-395	A	15:33	15:35	Explain differences: CO2 forcing initiates in the upper troposphere - explain why it affects surface in NH. [Stephen McIntyre]	These are 100 year simulations and the land surface in the NH has a smaller heat capacity so responds more quickly to the warming that is initiated in the mid-troposphere. Added
9-396	A	15:35	:38	The references c,d,e to the various panels of fig 9.2.1 are wrong. [Fons Baede]	fixed
9-397	A	15:49	15:49	9.2.1e should be 9.2.1f	fixed



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Jouni Räisänen]	
9-398	A	16:1	16:1	What does "optimally" mean & how does it differ from "most"? [William Ingram]	First sentence of 9.2.2.2??? See appendices.
9-399	A	16:1	16:13	An inference is not drawn. Rather too much of a review. [Bryant McAvaney]	Added.
9-400	A	16:4	16:18	Chapter 3: Section 3.4.3 and Box 3.1 have comments on dimming that are somewhat at odds with this. There are problems with ISCCP and the trends are wrong. There is no decreasing cloudiness. [Kevin Trenberth]	The downward trend in outgoing SW agrees with the models! But your problem with clouds are noted.
9-401	A	16:5	16:5	An explicit link could be made to the in-depth discussion on global dimming present in chapter 3. [Peter Thome]	Done
9-402	A	16:7	16:7	Omit short & redundant sentence [William Ingram]	done
9-403	A	16:9	16:9	would add "SW" between "outgoing" and "flux". [Sandrine Bony]	done
9-404	A	16:12	16:13	Differences between some model results and observations are significant during the Pinatubo period. So, it might be nice either to present only the model results that include a volcanic forcing in the 20C3M simulation, or to say that some of the AR4 models do not use any volcanic forcing, which explains the large discrepancies between models and observations around 1991-93 (Pinatubo). [Sandrine Bony]	Have added that some models did not include Pinatubo
9-405	A	16:15		Figure 9.2.3; Would it be possible to comment on the large difference between ERBS and ISCCP data which prevent to make definite conclusions from the comparison. [Eugene Rozanov]	Added. Note that ERBS data are considered more accurate.
9-406	A	16:23	16:23	What are "emission factors" & how do they differ from emissions? [William Ingram]	Text has been clarified.
9-407	A	16:26	16:26	Again, what are "emission factors" & how do they differ from emissions? [William Ingram]	Text has been clarified.
9-408	A	16:26	16:39	This is a useful 'tutorial' but again no inference is drawn [Bryant McAvaney]	added
9-409	A	16:27	16:27	Replace by : "This difference arises from the fact that the numerous climate processes (e.g. global circulation, convection...) operate a permanent redistribution of energy over the globe." I found dangerously misleading to say that this difference occurs because feedbacks vary spatially, as it may make people think that we are talking about local phenomena (one	Disagree – polar amplification causes higher temperature change there. Nevertheless, I added the fact that climate processes redistribute energy. Feedbacks do not need to be local.



No.	Batch	Page:line		Comment	Notes
		From	To		
				location would be more or less sensitive to changes in GHG concentrations). On the contrary, there are such differences because climate is global. I insist on the fact that a feedback is a process by which a change in one variable (e.g. the temperature in London) is amplified or damped. Here we are rather talking about interactions between changes in one variable at different locations. The word "feedback" should not be used here. [Stéphane Hallegatte]	
9-410	A	16:34	:36	I read this sentence many times but failed to understand it. [Fons Baede]	Removed
9-411	A	16:37	16:37	Just a note that lofted sulfate (and lofted soot over the Indian Ocean) can go a long way and have a large spatial footprint. Lifetimes can be weeks, creating a quite spread out radiative forcing that might well influence some aspects of atmospheric circulation. [Michael MacCracken]	noted
9-412	A	16:41	16:41	The influence of the solar irradiance variability on the surface pressure and temperature via perturbation of the PNJ was illustrated by Tourpali et al, (2003, GRL) and Egorova et al. (2004, GRL) [Eugene Rozanov]	added
9-413	A	16:41	16:48	Stendel et al. (Stendel, M., I.A. Mogensen and J.H. Christensen, 2005a: Influence of various forcings on global climate in historical times using a coupled AOGCM. Clim. Dyn. 25, 10.1007/s00382-005-0041-4) find a tendency towards the negative NAO state in periods of reduced solar input. In addition, the authors find an increase in blocking patterns over Western Europe, in particular in autumn. [Martin Stendel]	added
9-414	A	16:42	16:46	It would seem quite plausible that the regional distribution of sulfate aerosols could also lead to changes in the NAO--that the sulfate aerosol forcing had a global consequence is being found in the records--well, with it all concentrated in a relatively limited area (North America across to Asia), it likely had a bigger effect there--and this is just the region of the NAO. This possibility needs to be mentioned--and further investigated. [Michael MacCracken]	No studies, so not mentioned
9-415	A	16:52	16:56	Obscure. We are told different forcings produce effects with similar sensitivity, & then seem to be told that the patterns being similar is useful. I think the second statement is intended to mean to the extent that each pattern in the model is similar to the corresponding real-world one, but the only reasonable reading in context is to the extent that different forcing's patterns are similar. Make the meaning, whatever it is, clear. [William Ingram]	Revised (refers to 9-18, lines 1 – 4.)
9-416	A	16:57		This section discusses the temporal pattern of forcing responses without discussing any of the statistical techniques for characterizing temporal patterns! Of primary importance is	A discussion of time-scale of response to short time-scale forcing is included.



No.	Batch	Page:line		Comment	Notes
		From	To		
				the issue of persistency versus antipersistency, since this directly maps onto the concept of feedbacks, both instantaneous and cumulative. The following sentences should be added. "The response of the climate over time to random forcings will indicate whether feedbacks in the climate system are cumulative over time or whether they tend to dampen out. If anthropogenic forcing by greenhouse gases exerts a dominating influence on a climate system governed by cumulative positive feedbacks then observational data will exhibit a time series characteristic called persistency. This is a feature of data that resides below the temporal resolution of climate models so it cannot be evaluated by GCM experiments. However direct tests on climatic data series has shown that temperature data are antipersistent on all time scales, which is inconsistent with the conclusion that greenhouse forcing exerts a dominant influence on the global climate (Karnier 2003, 2005)." [Ross McKittrick]	
9-417	A	16:57		References for above cell: see G9. [Ross McKittrick]	References were not provided (but we found them anyway).
9-418	A	17:1	17:2	The space-time pattern of response will always be incorrect, to some extent, as no model is perfect (or perfectly sampled) [William Ingram]	noted
9-419	A	17:2	17:2	"Uncertainty in the spatial pattern of response." is misleading. Wouldn't it be rather "of forcing" ? Uncertainty in the spatial pattern of response deals more with the regionalisation chapter. [Stéphane Hallegatte]	D&A studies deal with response. But forcing uncertainties can cause spatial pattern changes in the response
9-420	A	17:3	17:17	One should mention the uncertainty arising from interactions between the transport by atmospheric circulation and chemical cycles (e.g. O3). [Stéphane Hallegatte]	Transport is mentioned. This complication would take a lot of words to explain.
9-421	A	17:6	17:9	And for aerosols, an important uncertainty is the height of emission. Particularly for SO2, this changed through the 20th century, and this very likely had a very strong influence on the average lifetime of the aerosol in the atmosphere, increasing typical residence time for sulfate from perhaps 2 days to nearly 2 weeks. Too little attention has gone into this. [Michael MacCracken]	You may be correct, but there are no studies to quote.
9-422	A	17:19	17:19	idem than line 2 [Stéphane Hallegatte]	Comment unclear.
9-423	A	17:20	17:24	"These uncertainties depend mainly on the uncertainty in the spatio-temporal expression of emissions, and, for some forcings, fundamental understanding of the possible change over time." : Again, there is interactions between transport and cycles. So, spatial patterns and temporal patterns are closely related.	There are interactions, but the spatial and temporal patterns may differ.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Stéphane Hallegatte]	
9-424	A	17:23	17:33	If not done above, somewhere in this paragraph it would be worth pointing out the change through the century in the typical height of emission of SO <sub>2</sub> , which early in the 20th century was being emitted very near the surface from factories and homes, but by mid-century was being mainly emitted from tall stacks of electric generating plants. This had a very large influence on the lifetime of the sulfate in the atmosphere, creating regional hazes and acid deposition far downwind, so changing the pattern and strength of the sulfate forcing by a very large amount. [Michael MacCracken]	added
9-425	A	17:23	17:32	3 times 'For example' in 9 lines [Bernard Seguin]	fixed
9-426	A	17:30	17:30	are' rather than 'is' [Bernard Seguin]	Time history is singular
9-427	A	17:35	17:35	Not only over the 20th century! (e.g. the Hegerl & al, 2000) [William Ingram]	noted
9-428	A	17:36	17:37	While the time history of the SO <sub>2</sub> emission may be known, it is not clear that the height of emission is sufficiently well-known. [Michael MacCracken]	mentioned
9-429	A	17:39	17:44	ensure consistency with statements in Chapter 6 and 10. [Bryant McAvaney]	Ch. 6: "The current lack of consistency between various data sets makes it difficult, based on current 8 knowledge, to attribute the century and longer time scale climate variations to solar variability, episodes of 9 intense volcanism, or simple modes of variability internal to the climate system."
9-430	A	17:39	:42	Either you need to reference another chapter, or a much more thorough discussion regarding the uncertainties of solar forcing is needed. There are other methods than sun-spot numbers (e.g. isotopes), and the literature is much broadened than apparent from the list of references. Two additional authors that should obtain consideration are Solanki, Beer. [Christoph Schar]	Reference to Ch. 6.4.1 added. And 2.7.1.2
9-431	A	17:43	17:43	What does "estimates of the response of a model to greenhouse gas forcing" mean? My guess is a "perfect-model" study on an "all-forcings" run, but I don't know, & the innocent	clarified



No.	Batch	Page:line		Comment	Notes
		From	To		
				reader would have no hope of guessing that. [William Ingram]	
9-432	A	17:44	17:44	Spelling of "Amman" incorrect, should be "Ammann". [Garth S. Jones]	corrected
9-433	A	17:48	17:53	Hansen et al. (1992) with their GCM simulation of available data, successfully predicted the magnitude and time scale of the global cooling and stratospheric warming of the climate response to the Pinatubo volcanic eruption (see also Hansen et al., 1996). [Andrew Lacis]	noted
9-434	A	17:51	17:53	What is the evidence that the radiative forcing associated with Pinatubo is the "best-quantified"? Surely we know the CO2 forcing with much greater accuracy. Indeed, this is even indicated on the next page (page 18, line 57). [Matthew Collins]	Meant best known volcanic forcing
9-435	A	17:52	17:52	Change "it's" to "its" [Michael MacCracken]	done
9-436	A	17:52	17:52	Pinatubo is very useful for quantifying responses (water vapour feedback, dynamical feedbacks, radiative feedbacks, ozone changes etc.)- the dispute is only whether it is a good test of climate sensitivity or not. [Gavin Schmidt]	noted
9-437	A	17:52	17:52	Replace "it's" by "its". [Martin Stendel]	done
9-438	A	17:55	17:57	This variation in sensitivity is being termed the climate efficacy and a reference to section 2.8.5 may help [Piers Forster]	added
9-439	A	17:55	18:20	If the regional natural variability for unforced climate change is not fully understood, how can there be any real certainty in detection and attribution, particularly when the distribution of radiative forcings (e.g., aerosols) is so uncertain. Do we really know that regional climatic variability is not simply chaotic in nature? Perhaps it is possible to make regional predictions in some statistical sense. But this requires large ensembles of climate experiments to establish the nature of the chaotic variability that is so evident on the regional scale. For a singularly unique climate forcing event such as the Pinatubo eruption, can climate models really be expected to reliably predict the sharp cooling in Canada and the warming in Europe and west coast of North America that actually took place. Would an identical Pinatubo eruption actually produce an identical regional climate response? [Andrew Lacis]	Detection uses large scale changes (i.e. NH vs SH)
9-440	A	17:56	17:56	"to within approximately 40%": 40% is a lot! Can we still consider that the response is	Some particular efficacies are this



No.	Batch	Page:line		Comment	Notes
		From	To		
				"similar"? [Sandrine Bony]	different. Changed to 25% consistent with Ch. 2
9-441	A	17:56	17:56	To say that any temperature response per unit forcing which has an approximate error of 40% is "similar" to any other with an approximate error of 40% seems a little optimistic. Is 0.6 approximately 1.4? [Matthew Collins]	See above response
9-442	A	18:1	18:9	I find this discussion on 'scaling' confusing [Bryant McAvaney]	revised
9-443	A	18:3	18:5	Given the effect of changing height of SO <sub>2</sub> emissions, it is not at all clear that the space-time pattern of the influence is being correctly represented. For aerosols, the pattern of influence may also be strongly dependent on the particular weather pattern, so sulfates injected during fair sky periods will have much longer and more extensive effects than for emissions during precipitation (and in fact the level of emissions may have been correlated with the weather, given that demand can vary with weather). Great care will need to be taken to really get this all adequately represented. [Michael MacCracken]	noted
9-444	A	18:4	:7	Restrict first sentence to detection studies, and second to attribution. [Christoph Schar]	revised
9-445	A	18:10	18:10	Omit "distinct" as the possible distinctness is covered by the proviso at the end of the sentence. [William Ingram]	revised
9-446	A	18:16	18:18	As shown in a figure of Simon Tett showing forcing during a simulation from 1750 to the present, the sulfate aerosol forcing creates strong latitudinal (and likely also longitudinal) gradients due to its localized emission through much of the century (to eastern North America and Europe), and the patterns and gradients of the forcing changed in time. I would suspect that these gradients affected atmospheric circulation (which always responds to try to reduce gradients) and there has so far been little investigation into all of this. Hence, I would do a bit more qualifying of the phrasing here, and be urging more detailed investigations. [Michael MacCracken]	noted
9-447	A	18:18	18:18	"affected by uncertainty" clearer as "less certain"? [William Ingram]	changed
9-448	A	18:22	18:23	Solar & greenhouse gas are not that different (though solar+volcanic, assuming we know their relative size, admittedly is). [William Ingram]	Noted, but there are important temporal differences in solar and greenhouse
9-449	A	18:25	18:27	Again, this is not only over the 20th century (e.g. the Hegerl & al, 2000)	notes



No.	Batch	Page:line		Comment	Notes
		From	To		
				[William Ingram]	
9-450	A	18:27	18:27	Suggest replacing "some model parameters" by "the choice of parameter values used in the description of small-scale physical processes" [Jouni Räisänen]	ok
9-451	A	18:37	:39	Isn't there a similar problem with correlations between solar and greenhouse gas forcing? At least for a part of the 20th century this should be the case. Is this a fundamental problem (as the sentence is proposing), or does it merely reduced the accuracy of the attribution? [Christoph Schar]	Solar forcing in the models has been early in the 20th century compared to greenhouse.
9-452	A	18:41	18:46	As indicated above, the pattern of aerosol forcing influence was likely changing continuously through the 20th century and so really identifying a horizontal pattern will be difficult and require quite a number of further simulations--once there is a better reconstruction of the changing heights and locations of the SO2 emissions. [Michael MacCracken]	The results depend mainly on the NH/SH spatial pattern
9-453	A	19:1	:3	How to reconcile this sentence with page 9-12, lines 53-54, where a forcing "close to zero" is not ruled out? [Fons Baede]	Former is removed
9-454	A	19:2		Is "anomalous forcing" the same as "external forcing"? [Fons Baede]	Forcing change is what is meant.
9-455	A	19:2		Replace "anomalous" with "additional" [Vincent Gray]	See above
9-456	A	19:2		Insert "probably" before "positive" [Vincent Gray]	Statement reflects literature
9-457	A	19:3	19:4	Here it is stated that "Top-down studies which use methods closely related to those used in climate change detection research, indicate that the magnitude of the net aerosol forcing is very likely less than -1.7 W/m2." This statement is important and accurate as far as it goes, but it is woefully incomplete. It fails to acknowledge that "top-down" studies (better term is "inverse calculations") depend upon three assumptions, which are very difficult to validate: (i) that our observations provide accurate knowledge of past climate changes, (ii) that climate models accurately represent the transient relationship between forcing and climate response, and (iii) that the past climate changes were actually caused by the inferred forcings, and not by other, unknown forcings or by unforced natural variability. Moreover, the statement above should be followed, immediately by a reference to the aerosol forcing values derived from forward calculations in Chap. 2, and the implications should be discussed. In other words, the aerosol forcing information from Chap. 2 and the present chapter should be INTEGRATED. These improvements can be accomplished by the following	The combination of observed atmospheric and ocean warming rules out natural variability as the cause of the observed atmospheric T warming; modeled variability also rules this out. Agree that the Ch. 2 information should be integrated, but there are no forcings available from the current AR4 models! Added last sentence thanks!



No.	Batch	Page:line		Comment	Notes
		From	To		
				revised wording: "Inverse calculations, which use methods closely related to those used in climate change detection research, indicate that aerosol forcing cannot be more negative than -1.7 W/m <sup>2</sup> if anthropogenic forcing is the explanation for the observed, 20th century warming (Anderson et al., 2003, Science, 300, 1103-1104). As summarized in Chap. 2, forward calculations of aerosol radiative forcing (which do not depend on knowledge of climate change or the ability of climate models to simulate transient response to forcings) indicate a likelihood that aerosol forcings are indeed less negative than -1.7 W/m <sup>2</sup> (see Table 2.9.1)."  [Theodore Anderson]	
9-458	A	19:4		Replace "is very likely" with "may be" [Vincent Gray]	disagree
9-459	A	19:8	19:9	Prior to the instrumental era, it is necessary to use indirect indicators ("proxy data") to infer past climate variations ". However in the discussion of estimating the scale of "natural variations", only models are discussed. Discuss variations as shown by proxies as well as by models where appropriate (not just on the 1000 year basis). see page 20 line xx. [Stephen McIntyre]	studies mainly refer to the 20th century
9-460	A	19:18		Replace "robust" with "indicative" [Vincent Gray]	Not sure what the reviewer is referring to here.
9-461	A	19:24	19:25	One should add : "Thanks to model-based patterns". More generally, the text may make people think that we are able to distinguish patterns from observations only. On the opposite, any separation of patterns is model-based. [Stéphane Hallegatte]	done
9-462	A	19:29	19:31	This statement is not true in general. The effects of solar and volcanic forcings can be separated using multi-variate regression [Haigh, 2003; Gleisner and Thejll, 2003] since the temporal behaviour of solar irradiance and volcanic emissions are different enough. Tests for this colinearity problem can be devised [e.g., Gleisner and Thejll, 2003]. [Hans Gleisner]	The emphasis here is no low-frequency aspects of solar forcing, which are uncertain.
9-463	A	19:31	19:33	These conclusions are not robust. Disclose the overlapping nature of proxies as possibly creating a systemic bias. [Stephen McIntyre]	The point being made is simply that statistical relationships diagnosed from longer records are generally more reliable.
9-464	A	19:32	19:33	This statement is not true in general. The atmosphere's response to the 11-year solar cycle provides the most efficient and reliable attribution of the climate responses to solar variability. [Hans Gleisner]	An unequivocal determination of specific mechanisms – whether direct or indirect - that involve solar



No.	Batch	Page:line		Comment	Notes
		From	To		
					variability and climate has yet to be accomplished. (from Ch 2 write-up)
9-465	A	19:35	19:40	I found this very difficult to follow. Although I do not have any specific suggestions the authors should consider redrafting for clarity. [Peter Thorne]	revised
9-466	A	19:36	19:36	Should "latter" be "former"? [Jouni Räisänen]	revised
9-467	A	19:39	19:39	"other anthropogenic forcings" is ambiguous. Should this be "non-greenhouse-gas anthropogenic forcings"? [Jouni Räisänen]	revised
9-468	A	19:40	19:40	"further back in time" - "on longer timescales"? [William Ingram]	Not revised
9-469	A	19:42	24:48	Why is it necessary to have this section when there is a whole chapter on palaeo-climates? If the desire is to simply focus on those palaeo-climate studies which make direct inferences about future climate change then this section could be shortened considerably. [Matthew Collins]	Noted, section has been shortened and part on proxy records included in the over subsections
9-470	A	19:42	24:48	Much of the discussion in section 9.3 is redundant (and in some places, inconsistent with) what is provided in more detail in chapter 6. As this material would in large part appear to be the purview of chapter 6, it would seem appropriate to shorten this section significantly, referring to chapter 6 for more detailed discussions. Only the specific implications of paleoclimate studies for conclusions related to detection/attribution would seem to belong in this chapter. [Michael Mann]	Noted, overlap have been clarified
9-471	A	19:43	19:43	"strongly damped hydrological cycle" is not only obscure (& apparently punning) to a non-specialist, but the reverse of the truth if interpreted in the way that seems most natural to me: the time-constant of the system, as the residence time of water vapour in the atmosphere would have been substantially shorter, though precipitation would have been lower (Allen & Ingram, 2002), which I assume is what should have been said. [William Ingram]	Noted. Text modified. Evaporation is also lower at LGM, and water vapour is reduced.
9-472	A	19:44		Section is rather long and overlaps some sections of Chapter 6 (especially reference to proxy temperature records) [Bryant McAvaney]	Noted. They have been minimized Proxy section merged in the other subsections and shortened.
9-473	A	19:46	19:46	"the northern and southern" - why not "both"? [William Ingram]	This comment should not be for line 19:47, 19:46
9-474	A	19:47	19:49	Add : "even though both are permanently active and interactive" [Stéphane Hallegatte]	Noted, but not included



No.	Batch	Page:line		Comment	Notes
		From	To		
9-475	A	19:47	19:49	I would suggest reversing the order of the listing of the factors here--in that the forcing term is likely the larger of the two over the last 1000 years, and the other periods covered in this section also were driven by external factors. [Michael MacCracken]	Done
9-476	A	19:53	19:53	What does "secular" timescales" mean? A dictionary check did not help. [Michael MacCracken]	Replaced with longer.
9-477	A	20:2	20:2	The text is quite inconsistent on whether "Last Glacial Maximum" is upper or lower case, or some mixture of the two. See page 19, line 46; page 5, line 8; etc. [Michael MacCracken]	Taken into account, but not in this paragraph
9-478	A	20:18	20:19	Chapter 6 does not say that the Earth warmed rapidly over the last 1000 years or so. It should be the last 100 years, and to be even more correct, the last few decades. [Lenny Bernstein]	Noted; the text provided was alright, results from a typo error
9-479	A	20:18	20:19	Chapter 6 says that the Earth cooled over most of the last 1000 years, with warming starting only in the last century. [Jeffrey Kueter]	Noted; the text provided was alright, results from a typo error
9-480	A	20:18	20:18	It is quite misleading to say that rapid warming has occurred over the last 1000 years--it has only been rapid over the last 100 years or so. [Michael MacCracken]	Noted; the text provided was alright, results from a typo error
9-481	A	20:18	20:33	This subject matter would appear to be under the purview of chapter 6, and the reader should simply be referred there for a more thorough and balanced discussion of this subject matter. [Michael Mann]	noted
9-482	A	20:18	:19	This is not true: the Earth has slowly cooled over the past 1000 years, only to warm during the 20th century. [Fons Baede]	Noted; the text provided was alright, results from a typo error
9-483	A	20:20	20:21	Delete "a further" [Vincent Gray]	Description of the curve has been revised
9-484	A	20:20		Replace "cooler" with "warmer" [Vincent Gray]	Description of the curve has been revised
9-485	A	20:20		Replace "the first half of the millenium" with "the medieval warm period" [Vincent Gray]	Description of the curve has been revised
9-486	A	20:22	20:22	There needs to be an indication given of how long these "climatic events" lasted--were they long enough to be a change, or were they just a fluctuation? [Michael MacCracken]	Taken into accout
9-487	A	20:22	20:22	What is the sign of these events? Are they warmings or coolings? [Peter Thorne]	Description of the curve has been revised



No.	Batch	Page:line		Comment	Notes
		From	To		
9-488	A	20:23	20:23	What is the reference for the "New reconstructions"--was this von Storch? And is the claim that the reconstructions are showing larger variations really proving out, or turning out to be a problem with the model? [Michael MacCracken]	Noted. Text refers now to chapter 6.
9-489	A	20:23	20:24	The statement "New reconstructions suggest larger variations..." is not defensible. Firstly, the TAR showed several different reconstructions, not just Mann et al (1999), so this sets up a straw man. Chapter 6 provides a much more balanced discussion in this regard, and should simply be referred to. The statement, moreover, is flatly false as a broad-brush claim. A number of reconstructions using entirely independent or partially independent data, and different methods, give results that are quite close to those reconstructions (Mann et al, 1999; Briffa et al, 2001; Jones et al, 1998) that were featured in the TAR. The most recent study, using entirely independent data that is not obviously prone to any underestimation of low-frequency trends--global glacial mass balance changes (Oerlemans, H., Extracting a Climate Signal from 169 Glacier Records, Science, 308, 675-677, 2005) gives a result that is at the lower-end amplitude of variability, similar to Mann et al, 1999 and the other reconstructions shown in the TAR. Rutherford et al (2005) obtain reconstructions that are quite similar to those found in the TAR as well. And several reconstructions suggesting more variability (Moberg et al and Esper et al) agree remarkably poorly with each other. Moreover, the methods used in these latter studies have been called into question: Esper et al because of their overly liberal implementation of the RCS tree-ring standardization method, and Moberg et al because of their use of a statistical scaling approach that can artificially inflate low-frequency variability as shown by Mann et al (2005) [Mann, M.E., Rutherford, S., Wahl, E., Ammann, C., Testing the Fidelity of Methods Used in Proxy-based Reconstructions of Past Climate, Journal of Climate, in press, 2005]. The statement needs to be reworded to more accurately reflect the current state of our knowledge, which is indeed one of uncertainty, but not one which appears to selectively favor reconstructions that exhibit greater variability. [Michael Mann]	Noted. The text didn't intend to say that a reconstruction was better than another. Better reference chapter6 now, and information on proxy records mixed in the other subsections. Thank you for all the explanations.
9-490	A	20:25	20:31	The text about methodological uncertainties in the reconstructions overlaps strongly with Ch. 6 and is in my opinion not needed. [Jouni Räisänen]	Noted. Text has been revised and shortened
9-491	A	20:29	20:29	The reference to Mann et al "2005" is presumably supposed to be to "Mann et al 2005a" [Mann, M.E., Rutherford, S., Wahl, E., Ammann, C., Testing the Fidelity of Methods Used in Proxy-based Reconstructions of Past Climate, Journal of Climate, in press, 2005] rather than to the Mann et al ("2005b") study cited. Mann et al 2005a shows that pattern-based reconstruction approaches faithfully reconstruct low-frequency variability for a	This section has been revised and refers to chapter 6.



No.	Batch	Page:line		Comment	Notes
		From	To		
				wide range of signal-to-noise ratios, and contradicts claims made by von Storch et al (2004). Mann et al 2005b involves modeling of past changes in El Nino, and has little if anything to do with the discussion in question. [Michael Mann]	
9-492	A	20:29	20:31	The statement "although...good agreement" does not survive scrutiny. In fact, published proxy-based Northern Hemisphere temperature reconstructions that suggest the greatest variability [Moberg et al (2005) and Esper et al (2002)] also happen to show the least resemblance to each other,(see figure 6.8). Other reconstructions using techniques that are specifically designed to recover low-frequency variability [see Rutherford, S., Mann, M.E., Osborn, T.J., Bradley, R.S., Briffa, K.R., Hughes, M.K., Jones, P.D., Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Methodology, Predictor Network, Target Season and Target Domain, Journal of Climate, 18, 2308-2329, 2005; see also: Mann, M.E., Rutherford, S., Wahl, E., Ammann, C., Testing the Fidelity of Methods Used in Proxy-based Reconstructions of Past Climate, Journal of Climate, in press, 2005] are in good agreement with the reconstructions shown in the TAR. So are entirely independent reconstructions based on data that are a priori not subject to the loss of low-frequency variability [i.e., global glacial mass balance changes: Oerlemans, H., Extracting a Climate Signal from 169 Glacier Records, Science, 308, 675-677, 2005]. The Oerlemans glacier-based reconstruction is at the lower-end amplitude of variability, similar to Mann et al, 1999 and the other reconstructions shown in the TAR. [Michael Mann]	The section has been revised and all information on proxy records refers to chapter 6
9-493	A	20:31		Insert before "Nonetheless" " McIntyre and McKittrick (2003,2005) have found serious errors in the treatment of Mann et al (1998), which , when corrected, show a distinct medieval warm period in the 15th century which was warmer than any 20th Century figure. It is possible that the same aerrors apply to other compilations. [Vincent Gray]	The section has been revised and refers now to chapter 6 for all the detailed information about the proxies.
9-494	A	20:31		Replace "Nonetheless all" with "Despite this finding several other" [Vincent Gray]	Noted This part is now mixed with model results
9-495	A	20:35	20:43	It is unclear to me why this discussion is here at all. This subject matter would appear to be the purview of chapter 6 where a more complete and better discussion (e.g. page 36 therein) is already provided. [Michael Mann]	Taken into account, the discussion on variability is better included in the other subsections.
9-496	A	20:36	20:38	Surely not really what is meant - the reconstructed changes can't be accurate to 5-10%! [William Ingram]	noted
9-497	A	20:41	20:43	With regard to discussion of forcing of ENSO in past centuries, solar irradiance has been found to play a minor role relative to tropical volcanic forcing. Mann et al ("2005b")	This discussion could be found later in the text. Now all aspects are on the



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Mann, M.E., Cane, M.A., Zebiak, S.E., Clement, A., Volcanic and Solar Forcing of the Tropical Pacific Over the Past 1000 Years, Journal of Climate, 18, 447-456, 2005] find that the low-frequency changes in both amplitude of variability and mean state of El Nino inferred from the Cobb et al (2003) estimates correspond well with the response of the Cane-Zebiak model to tropical volcanic radiative forcing changes over the past 1000 years, with solar forcing playing a secondary role. [Michael Mann]	same paragraph.
9-498	A	20:45	20:45	The BIOME6000 is not global, but terrestrial. The corresponding mapping attempted for the Ocean (GLAMAP and other extension of the pioneering CLIMAP effort) need to be discussed as well [Fortunat Joos]	Taken into account and reference to chapter 6 added
9-499	A	20:45	20:45	What is a "natural" climate fluctuation? Presumably the magnitude of climate fluctuations increases with the time scale under application of no "external" forcings. In some sense, all climate fluctuations, whether it is ice ages occurring on million year times scales or unforced variability of current climate, are all "natural". But are ice ages and inter-glacials really what is meant by "natural climate fluctuations" in the context of current climate change? More relevant may be formulating the question in terms of inter-annual variability of current climate versus past climates. [Andrew Lacis]	Taken into account. We agree that the reference to natural fluctuation was not well put into context
9-500	A	20:50	20:50	Change "zone" to "zones" [Michael MacCracken]	done
9-501	A	21:0		9.3.3 Seems to overlap substantially with chapter 6. [James Annan]	Noted; the point of view is different. Feedbacks are highlighted here, whereas in chapter 6 there is more emphasise on model evaluation and mechanisms of climate change
9-502	A	21:5		The most sophisticated simulation on LGM is no doubt the report of Calov et al. ([1] R. Calov, A. Ganopolski, M. Claussen, V. Petoukhov, R. Greve, Transient simulation of the last glacial inception. Part I: glacial inception as a bifurcation in the climate system, Climate Dynamics (2005) 24: 545-561. DOI 0.1007/s00382-005-0007-6; [2] R. Calov, A. Ganopolski, V. Petoukhov, M. Claussen, V. Brovkin, C. Kubatzki, Transient simulation of the last glacial inception. Part II: sensitivity and feedback analysis, Climate Dynamics (2005) 24: 563-576. DOI 10.1007/s00382-005-0008-5). They show that the insolation change and ice-snow feedback are sufficient to reproduce LGM. The contribution of CO2 became minor when the grid size is decreased. Thus, the conventional results based on large contribution of CO2 are very doubtful and not convincing now. [Kiminori Itoh]	Noted, but not relevant for this section. Information provided to chapter 6 where long term climate fluctuations are discussed.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-503	A	21:12	21:12	What are considered to be "external" forcings in this context? [Andrew Lacis]	This paragraph does not exist any more, and when needed reference has been made to section 9.2, where forcing are defined for these periods.
9-504	A	21:15	21:17	Ice sheet forcing is a feedback - not a direct radiative forcing. Discuss models which proceed from Milankowitch forcing only. [Stephen McIntyre]	Noted, but we do not agree. We consider models that are used for future climate projection and snap shot experiments, for which ice sheet can be considered as a forcing. However, the terminology used has been changed to make it clear that we discuss simulations in which ice sheets are specified.
9-505	A	21:16	21:16	The Maunder Minimum is a solar event, not a climatic one! I appreciate "Little Ice Age" has been given so many meanings as to have no meaning now, but it is essential to distinguish forcing & response or circular thinking is bound to follow [William Ingram]	Taken into account. The vocabulary has been clarified to be more consistent with a forcing and climate response as suggested.
9-506	A	21:32	21:32	and in terrestrial carbon storage (e.g. Kaplan et al, GRL, 2002; Joos et al., GBC, 2004, see also chapter 6). [Fortunat Joos]	Accepted, thank you
9-507	A	21:34	21:35	What does "warmer than the last 800 years" mean - grammatically there are 2 possibilities: that the 20th-century average is warmer than the 8-century average, or that the 20th-century average is warmer than any year in the 8-centuries, but I suspect "warmer than any other century in the last 8" is meant [William Ingram]	Noted, paragraph rewritten
9-508	A	21:34	21:35	Are these values for the Northern Hemisphere or for the global mean? [Jouni Räisänen]	Northern hemisphere, text modified
9-509	A	21:34	:54	Two additional studies that should probably be cited here: Claussen M, Brovkin V, Ganopolski A, Kubatzki C, Petoukhov V, 2003: Climate change in northern Africa: The past is not the future. CLIMATIC CHANGE 57 (1-2): 99-118 Brovkin V, Bendtsen J, Claussen M, Ganopolski A, Kubatzki C, Petoukhov V, Andreev A, 2002: Carbon cycle, vegetation, and climate dynamics in the Holocene: Experiments with the CLIMBER-2 model. GLOBAL BIOGEOCHEMICAL CYCLES 16 (4): Art. No. 1139 [Christoph Schar]	Not considered here, more relevant for chapter 6
9-510	A	21:57	21:57	I assume "ocean" is meant before "initial"?	Yes it is. Text now modified



No.	Batch	Page:line		Comment	Notes
		From	To		
				[William Ingram]	
9-511	A	22:0		Table 9.3.1 caption - the English says that over- & under-estimation are about the paleodata - is that really meant? [William Ingram]	This has been clarified, thanks.
9-512	A	22:0		Table 9.3.1 caption - what does "decadally smoothed for annual" mean? [William Ingram]	Text has been simplified.
9-513	A	22:0		Fig 9.3.1 - last line of caption incomprehensible [William Ingram]	Revised
9-514	A	22:0		replace "thermohaline circulation" with "THC" [Peili Wu]	We now use the agreed standard term (meridional overturning circulation).
9-515	A	22:20	22:20	What is meant by "internal variability" in this context? [Andrew Lacis]	Text has been clarified.
9-516	A	22:21	22:21	Maunder Minimum is a term to describe a period of low numbers of sunspots or low solar activity between 1640 and 1720 (Eddy Science 1976), it is not a climatic term and it is extremely misleading to use it as such. "Late Maunder Minimum" should be removed or replaced with more appropriate term. [Gareth S. Jones]	Taken into account
9-517	A	22:27	22:50	If it were not known for a fact that greenhouse gases had increased during the past century, it would still have been possible to blame any climate change on long-term solar variability. After all, there really are no definitive measurements of potential solar luminosity changes earlier than several decades ago. But there is a clear record of documented GHG increases, and the radiative consequences of these GHG changes (together with some inferred aerosol changes) fully account for the observed trends of global temperature increase. [Andrew Lacis]	We're not clear on what point is actually being made here. We do know more about solar during the period when ghg forcing changes rapidly.
9-518	A	22:27	22:50	Once again, the subject matter would seem to come under the purview of chapter 6, where a more detailed discussion is already provided. For a discussion of the reasons for differences between different simulation studies, the reader should simply be referred there. Only the implications for detection/attribution (e.g. lines 46-50) would seem appropriate for discussion here. [Michael Mann]	Taken into account, but minimum explanation is needed.
9-519	A	22:30	22:30	Replace "Gonzalez-Ruoco" by "Gonzalez-Rouco". [Martin Stendel]	done
9-520	A	22:35	22:37	Figure 6.10 does not show a general cooling of the Northern Hemisphere from the beginning of the millennium until the 19th century. It shows a warming period at the beginning of the millennium that lasts for about 150 years before the onset of cooling.	Noted, paragraph rewritten



No.	Batch	Page:line		Comment	Notes
		From	To		
				While Chapter 6 argues that the Medieval Warm Period was not global in nature, it accepts that it impacted the Northern Hemisphere, and the climate simulations show that warming. [Lenny Bernstein]	
9-521	A	22:35	22:37	My reading of Figure 6.10 shows a warming period at the beginning of the millennium that lasted for about 150 years before the onset of cooling. This warming to about 1150 is the Medieval Warm Period (MWP). While there is a debate as to whether the MWP was global, there is no debate that it affected large portions of the Northern Hemisphere. [Jeffrey Kueter]	Taken into account, paragraph rewritten
9-522	A	22:37	22:40	This sentence needs to be clarified. It is unclear whether the authors mean "Late Maunder Minimum" to be the name of the cold period (1675-1715) or to mean the period of low number of sunspots. If the former then it is an incorrect useage of the term. If the latter it is implying a direct causation of the associated cool period by low solar activity, however it is possible volcanic activity contributed significantly to this cool period (e.g. Tett et al 2005), so just mentioning late Maunder Minimum is misleading and confusing. Maunder Minimum is a term to describe a period of low numbers of sunspots or low solar activity between 1640 and 1720 (Eddy Science 1976), it is not a climatic term and it is extremely misleading to use it as such. "Late Maunder Minimum" should be removed or replaced with more appropriate term. [Gareth S. Jones]	Taken into account, the text has been modified and put less emphasise on the this cold period.
9-523	A	22:37	22:37	The findings in Stendel et al. (Stendel, M., I.A. Mogensen and J.H. Christensen, 2005a: Influence of various forcings on global climate in historical times using a coupled AOGCM. Clim. Dyn. 25, 10.1007/s00382-005-0041-4) corroborate results of the cited authors, with the Late Maunder Minimum being the coldest and the late 20th century being the warmest spell of the integration period, respectively. [Martin Stendel]	Ref. added.
9-524	A	22:46	22:48	Should be stressed more. This is the most important finding. [Stéphane Hallegatte]	Taken into account, there is also a link in the beginning of section 9.4
9-525	A	22:48	22:56	This largely repeats 39-46 on 9-15: harmonize [William Ingram]	Done
9-526	A	22:48	22:50	This point is VERY IMPORTANT and needs to be made prominently in the Executive Summary to overcome the misimpression that human influences only were important after 1970, And I would add that this conclusion is evident even before better accounting for the dramatic change in height of emission of SO <sub>2</sub> , thereby lengthening the lifetime of sulfate aerosols, and before there really being careful analysis of how the localized sulfate forcing may have affected atmospheric circulation (like the NAO). [Michael MacCracken]	Statement added to the ES.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-527	A	22:48	:50	This is new to me. Is it based on one study only? This statement contrasts with the much more careful wording of Page 9-26, lines 8-25, where no reference is made to this study. [Fons Baede]	Noted, and rewritten
9-528	A	22:49	22:56	The volcanic & solar forcings both give a tropospheric cooling but opposite sign effects on stratospheric temperatures, so it is surprising to get the same effect on the NAO (given the effect is generally thought to be via the stratosphere); shouldn't this be noted as an oddity or the explanation given if there is one? [William Ingram]	Will be revised if possible.
9-529	A	22:55	22:55	"which varies by a factor of two between different climate models (chapter 8).": it would be more appropriate to refer to chapter 10 instead of chapter 8, since the range of model estimates of climate sensitivity is given in chapter 10. [Sandrine Bony]	done
9-530	A	23:5	23:5	What is meant by the sentence - what "initial conditions"? [Bryant McAvaney]	Ocean initial conditions
9-531	A	23:10	23:11	It might be worth being explicit that the volcanic effect has been found at the surface. Most of the signal is stratospheric, but this component will have been missed using proxy indicators. [Peter Thorne]	Surface now mentioned.
9-532	A	23:18	23:19	Maunder Minimum is a term to describe a period of low numbers of sunspots or low solar activity between 1640 and 1720 (Eddy Science 1976), it is not the name of a climatic cool period and it is extremely misleading to use it as such. "Late Maunder Minimum" should be removed or replaced with more appropriate term. [Gareth S. Jones]	Taken into account. Text has been clarified
9-533	A	23:18	23:19	Tett et al 2005 could also be added as a reference here. [Gareth S. Jones]	done
9-534	A	23:18	23:23	There is some apparent confusion as to what these different studies are actually looking at. Shindell et al (2003) find that solar forcing plays a larger role in terms of regional anomalies because of dynamical feedbacks which have a large regional projection, but contribute little to global or hemispheric mean temperatures in comparison with volcanic forcing. Andronova et al (2005) look at hemispheric mean temperatures--here volcanic forcing plays a much greater role. There is no contradiction--these studies are looking at very different things. The confusion here stresses the importance of distinguishing between regional and hemispheric/global changes, and recognizing the seasonally-distinct nature of potential dynamical responses to forcing. Once again, the discussion here would best be left to chapter 6. [Michael Mann]	Taken into account
9-535	A	23:19	23:21	Maunder Minimum is a term to describe a period of low numbers of sunspots or low solar	Taken into account



No.	Batch	Page:line		Comment	Notes
		From	To		
				activity between 1640 and 1720 (Eddy Science 1976), it is not a climatic term and it is extremely misleading to use it as such. "Maunder Minimum" should be removed or replaced with more appropriate term. [Gareth S. Jones]	
9-536	A	23:20	23:20	Shindell et al (2003) concluded that both solar and volcanic were important for global temp, though solar was dominant for the regional signal. [Gavin Schmidt]	Taken into account
9-537	A	23:21	23:23	carry forward discussion of uncertainties more clearly in summary [Stephen McIntyre]	done
9-538	A	23:37		Figure 9.3.1. The text and/or figure caption should specify the sensitivity of the model. [Stephen E Schwartz]	Specified.
9-539	A	23:41	23:45	Whilst the annual mean surface temperature response to solar forcing does look similar to GHG forcings, there are differences when seasonal means are examined. The patterns of forcing become quite different depending for the Sun over a year, whilst those from GHGs remain quite similar. The effects of this can be seen in D&A results in Stott et al , Climate Dynamics, 2001. [Gareth S. Jones]	This is a useful comment, but it is not applicable here because of the generally poor resolution of the seasonal cycle in proxy data.
9-540	A	23:45	23:48	The expressions of the two forcings (solar and volcanic) is readily distinguishable by virtue of their distinct seasonal, spatial signatures. This is discussed by Shindell et al (2003) and Shindell et al (2004). [Michael Mann]	Noted, but not enough space here to provide all details
9-541	A	23:47	23:48	Discuss how solar and greenhouse are distinguished with more information about details. [Stephen McIntyre]	Noted, but not enough space here to provide all details
9-542	A	23:49	:51	the probability given here appears low, certainly in comparison with some of the other probabilities (e.g. - the role of anthropogenic forcing). Most scientists probably think that it is certain (or at least virtually certain) that preindustrial temperature series show effects from natural external forcing. [Christoph Schar]	We agree, but prefer our more conservative likelihood assessment in order to account for uncertainty in proxy reconstructions.
9-543	A	23:53	23:53	There is only one "mode of variability" discussed here - NAM/NAO - what about others? [Bryant McAvaney]	Noted, we also consider ENSO in the text.
9-544	A	23:54	23:54	The quoting of a linear trend should be accompanied by a warning that this can be, including for this case, misleading - or, perhaps better, omitted [William Ingram]	Accepted. A linear trend is quoted since a linear trend is quoted in chap 3 but the text is amended to state that a linear trend is not a good approximation to the observed temperature change
9-545	A	23:57	24:1	Stendel et al. (Stendel, M., I.A. Mogensen and J.H. Christensen, 2005a: Influence of	Cited.



No.	Batch	Page:line		Comment	Notes
		From	To		
				various forcings on global climate in historical times using a coupled AOGCM. Clim. Dyn. 25, 10.1007/s00382-005-0041-4) also find a tendency towards the negative NAO state in periods of reduced solar input. In addition, the authors find an increase in blocking patterns over Western Europe, in particular in autumn. [Martin Stendel]	
9-546	A	24:0		Fig 9.4.2. The most obvious thing about this figure is that the grey matches perfectly between model & observations! Say what it is. [William Ingram]	Noted. This figure has been replaced by one showing trends over the two periods used in chap 3
9-547	A	24:1	24:1	As well as Rind et al 2004 the following reference could also be added, as it also shows a good agreement between solar forcing and the north annular mode:- A Ruzmaikin, Feynman J, Jiang X, Noone DC, Waple AM, Yung YL, "The pattern of northern hemispheric surface air temperature during prolonged periods of low solar output", GRL, 2004 doi:10.1029/2004GL019955 [Gareth S. Jones]	Considered.
9-548	A	24:1	24:1	There is one model study that does not see a NAO/NAM/AO circulation pattern associated with solar forcing, despite various sensitivity tests. This should be mentioned, reference- MA Palmer, Gray LJ, Allen MR, Norton WA, "Solar forcing of climate: model results", Advances in space research, 34 (2004) 343-348 [Gareth S. Jones]	Cited.
9-549	A	24:4	24:4	Maunder Minimum is a term to describe a period of low numbers of sunspots or low solar activity between 1640 and 1720 (Eddy Science 1976), it is not the name of a climatic cool period and it is extremely misleading to use it as such. "Late Maunder Minimum" should be removed or replaced with more appropriate term. [Gareth S. Jones]	Taken into account
9-550	A	24:8	24:16	Problem with land cover change experiments is the 'strength of coupling' issue - Koster et al (GLACE experiment) [Bryant McAvaney]	Noted, but not enough space here to discuss all the details. Chapter 6 is the right place.
9-551	A	24:14		I question this statement Several studies suggest that the continental-scale deforestation over Europe has led to a warming. For instance, Heck et al (2001) find a warming effect that is comparable with the greenhouse gas warming of the last decades. During the spring season and in Spain, the effect is as large as +2 K. The effect is related to changes in evapotranspiration. Heck, P., D. Lüthi, H. Wernli and C. Schär, 2001. Climate impacts of European-scale anthropogenic vegetation changes: A study with a regional climate model. J. Geophys. Res. - Atmos., 106 (D8): 7817-7835 [Christoph Schar]	Not relevant for this chapter, but for chapter6.
9-552	A	24:26	24:26	The complete lack of similarity for 1925-44 leaves me entirely uncompeled.	Noted. This figure has been replaced by



No.	Batch	Page:line		Comment	Notes
		From	To		
				[William Ingram]	one showing trends over the two periods, 1901-2004 and 1979-2004, the two periods used in chap 3.
9-553	A	24:32	24:38	Include discussion of uncertainties. [Stephen McIntyre]	Discussion added.
9-554	A	24:33		Replace "likely" with "unlikely" [Vincent Gray]	Not clear what text the reviewer refers to.
9-555	A	24:35		Delete "robust" [Vincent Gray]	Rejected.
9-556	A	24:39		Delete "substantially" [Vincent Gray]	Accepted.
9-557	A	24:40	24:42	It would really help if some indication could be given of the magnitude being considered--for example, are running decadal variations larger than, say 0.5 C? [Michael MacCracken]	That level of detail would not be appropriate for a summary. Fig 9.3.1 gives a good indication of the magnitude.
9-558	A	24:44	24:44	Is it really only "possibly"--meaning, in the IPCC lexicon, about 50-50? On the next page, line 6, it gives a likelihood of "likely", which seems more reasonable to me, given what we have been learning. Consistency is needed. [Michael MacCracken]	This is based Ch 6. The FOD of Ch 6 did not give a likelihood assessment for the last 2000 years.
9-559	A	24:44		Replace "and possibly" with "or even" [Vincent Gray]	Rejected. No justification given.
9-560	A	24:48		Replace "detected" by "surmised" [Vincent Gray]	Rejected.
9-561	A	24:50	41:25	It would be better to incorporate some of this discussion in Chapter 3. [Andrew Lacis]	Rejected since it is appropriate to start off each subsection of 9.4 with a short summary of observational evidence
9-562	A	24:56		Insert after "planet" "since 1980" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-563	A	25:1	25:1	"a greater degree of uncertainty" -> "less certainty"? [William Ingram]	26:8 Replaced with "more uncertainty"
9-564	A	25:2		Wrong again. The 0.75K refers to 1860-2004 (see Chapter 3, page 3_3 line 8.) Replace "0.75K" with "between 0.57 and 0.62K" [Vincent Gray]	Taken into account. Text has been modified to correctly refer to warming by 2004 relative to 1860-1900
9-565	A	25:2		Delete the first "approximately" [Vincent Gray]	Rejected since it is appropriate to start off each subsection of 9.4 with a short



No.	Batch	Page:line		Comment	Notes
		From	To		
					summary of observational evidence
9-566	A	25:5	25:6	This is a misquote of Chapter 6. Chapter 6's finding (Pg. 6-4, lines 8-11) reads: "Indeed, it is very likely that average Northern Hemisphere temperatures during the second half of the 20th century were warmer than any other 50-year period in the last 500. It is also likely that this was the warmest period in the past 1000 years and unusually warm compared with the last 2000 years." Chapter 6's assessment should be quoted correctly. There is a significant difference between saying that it was unusually warm compared with the last 2000 years and saying that the mean temperatures were unprecedented in the last 2000 years. [Lenny Bernstein]	Accepted. Text has been altered to correctly reflect chapter 6.
9-567	A	25:5	25:6	Chapter 6's conclusion (Pg. 6-4, lines 8-11) states: "Indeed, it is very likely that average Northern Hemisphere temperatures during the second half of the 20th century were warmer than any other 50-year period in the last 500. It is also likely that this was the warmest period in the past 1000 years and unusually warm compared with the last 2000 years." Chapter 6's conclusion that the last 50 years were "unusually warm compared to the last 2000 years" does not support the use of the adjective "unprecedented" in describing this period. Replace the current text with a direct quote from Chapter 6 to ensure consistency between the chapters. [Jeffrey Kueter]	Accepted. Text has been altered to correctly reflect chapter 6.
9-568	A	25:6		Replace "and likely" by "but not" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-569	A	25:8	25:8	"find" is too conclusive for just one study - "conclude", perhaps [William Ingram]	26:15. Accepted.
9-570	A	25:10	25:12	What implication is being drawn here? [Bryant McAvaney]	Text amended to avoid direct comparison of rates of rise at this point.
9-571	A	25:16		Insert here a paragraph which summarises the other temperature records: reanalysis, proxy, borehole (both since 1900, radiosondes and MSU satellite readings [Vincent Gray]	Rejected. Section 9.4.1 is dealing with surface temperature change during the industrial era. Radiosondes and MSU are discussed in 9.4.4 and proxy and borehole records in 9.3.
9-572	A	25:18		Insert "surface" before "instrumental" [Vincent Gray]	Accepted. Done.
9-573	A	25:20		These simulations used models with different climate sensitivities, rates of ocean heat uptake and magnitudes and types of forcings. Figure 9.4.1 shows that simulations that include increasing greenhouse gases, the effects of aerosols and natural external forcings provide a consistent explanation of the observed temperature record, whereas simulations	Taken into account. The point is made in the text that a good fit of modelled and observed temperature could have been obtained with smaller

Confidential, Do Not Cite or Quote

Chapter 9: Batch AB (11/16/05)

Page 77 of 186



No.	Batch	Page:line		Comment	Notes
		From	To		
				<p>that include only natural forcings do not simulate the warming observed over the last three decades.</p> <p>These sentences are among the most important in the chapter and perhaps in the report. That said it seems essential that the properties of the models be explicitly stated, namely at minimum the sensitivities, and that the figure show the time series of the forcings and the time series of the rate of ocean heat transport from mixed layer to deep ocean (permitting comparison with the measurements of Levitus and Willis). To the extent that the argument is made that present understanding of temperature change over the industrial period derives from the ability to match this change in models.</p> <p>It has been noted (Schwartz, 2004) that different models with widely differing sensitivity can yield similar trends in global mean temperature anomaly. Similarly Hansen et al (2005) noted good agreement between their model runs and observed temperature anomaly but expressed the caveat "A good fit of observed and modeled temperatures also could be attained with smaller forcing and larger climate sensitivity, or with the converse. Schwartz S. E., Uncertainty requirements in radiative forcing of climate change. <i>J. Air Waste Management Assoc.</i> 54, 1351-1359 (2004).</p> <p>Hansen, J., L. Nazarenko, R. Ruedy, M. Sato, J. Willis, A. Del Genio, D. Koch, A. Lacis, K. Lo, S. Menon, T. Tsvetkov, Ju. Perlwitz, G. Russell, G.A. Schmidt, and N. Tausnev 2005. Earth's energy imbalance: Confirmation and implications. <i>Science</i> 308, 1431-1435, doi:10.1126/science.1110252.</p> <p>My hunch is that the model results derive from models with rather widely differing sensitivities.</p> <p>Figure 9.4.1 c at least shows the results of the individual models. Figures a and b do not and consideration should be given to finding a way to do so in these panels also. It certainly appears in panels a and b as if more than 13 points are given for each year. So it is not clear whether some models have more replicates than others, potentially misleading.</p> <p>I find it rather astonishing that such little discussion is given to Figure 9.4.1. Especially given the extensive discussion of similar figures in AR3.</p> <p>[Stephen E Schwartz]</p>	<p>forcing/larger sensitivity or the converse is made in the FOD text in the para starting 26:27. The references suggested have been added to the text at this point.</p> <p>Model properties from climate model runs made for AR4 are given in chap 8 (Table 8.2.1)</p> <p>The FOD paragraph starting at 3:25 summarises the arguments that the confidence in attribution of anthropogenic warming is increased as a result of such model studies.</p>
9-574	A	25:21	25:23	<p>At least one (if not most) of the simulations also included tropospheric and stratospheric ozone changes as well as GHGs and aerosols, whilst some of the simulations do not include aerosol indirect effects. This does not seem to make a big difference in distinguishing them from the natural only runs. This should be mentioned here.</p> <p>[Gareth S. Jones]</p>	Accepted. Text has been modified to reflect this fact.
9-575	A	25:21	25:23	<p>It is worth adding that the simulations that included natural forcings sampled a range of possible solar and volcanic forcing datasets. i.e. they don't just sample model uncertainty.</p> <p>[Gareth S. Jones]</p>	Accepted. Additional text inserted to make this point.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-576	A	25:22	25:22	I would suggest changing "gases" to "gas concentrations" in order to make sure there is no confusion with the associated situation involving an increase in the number of gases being considered. [Michael MacCracken]	Accepted. Done.
9-577	A	25:22		Replace "a consistent" with "one" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-578	A	25:23		Insert "known" before "natural" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-579	A	25:23		Insert "readily" before "simulate" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-580	A	25:26	25:26	Omit "possible" - redundant given start of line [William Ingram]	26:32 Accepted. Done.
9-581	A	25:26	25:26	Reference to Figure 9.4.1: Add the simulation by Stendel et al. (Stendel, M., I.A. Mogensen and J.H. Christensen, 2005a: Influence of various forcings on global climate in historical times using a coupled AOGCM. Clim. Dyn. 25, 10.1007/s00382-005-0041-4). [Martin Stendel]	Will be added if possible.
9-582	A	25:30	:32	This is an overstatement. The panels in Fig.9.4.2 actually show that there are serious problems. For instance, for 1925-44, the observed warming is over the continents, while in the simulation it is over the pacific (where the observations show a pronounced cooling). For 1945-64, the substantial warming over North America and the Atlantic are missed; and for 1965-84 the one over Eurasia. A more balanced and careful discussion is needed. It is possible that a fraction of the model shows more reasonable results, but not the ensemble mean. [Christoph Schar]	Noted. This figure has been replaced by one showing trends over the two periods, 1901-2005 and 1979-2005.
9-583	A	25:32	25:32	Apparently the similarities between model simulations and observed spatial patterns of climate change are compelling. I do not find them so! For example, the patterns shown for the period 1925-1944 are almost orthogonal in the N Pacific-N American region. Perhaps the similarity could be quantified in terms of a simple correlation coefficient between the relevant spatial maps. [Matthew Collins]	Noted. This figure has been replaced by one showing trends over the two periods, 1901-2005 and 1979-2005, the two periods used in chap 3.
9-584	A	25:32		Replace "compelling" with "interesting" [Vincent Gray]	Noted. This figure has been replaced by one showing trends over the two periods, 1901-2005 and 1979-2005, the two periods used in chap 3.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-585	A	25:32		Larger inter-decadal variations are seen in the observations than in the ensemble mean model simulation of the 20th century because the ensemble averaging process filters out much of the natural internal inter-decadal variability that is simulated by models. If the figure is misleading, as the above text suggests, then some fix should be sought, rather than trying to explain it away. Perhaps some (or all) of the individual model results should be shown, permitting an assessment of the accuracy of the explanation given. [Stephen E Schwartz]	Noted. This figure has been replaced by one showing trends over the two periods, 1901-2005 and 1979-2005, the two periods used in chap 3.
9-586	A	25:34	:36	Most models show a quite dramatic overestimation of the short-term effects of Pinatubo and other volcanic eruptions. Is it possible that the scaling of the volcanic forcing is overestimated such as to better match the long-term temperature series? Please discuss! [Christoph Schar]	D+A analyses estimate whether models systematically overestimate or underestimate the contributions from different forcings including greenhouse gases and volcanoes and they do not find that models have been systematically tuned to have a bigger vol trend and a smaller ghg trend than the observationally constrained estimates. The issue of fortuitous agreement is dealt with in the para starting at page 26 line 27 of the fod and the fact that d+a analyses do not rely on fortuitous (or tuned) agreement of this sort.
9-587	A	25:37	25:38	Short sentence needs some justification [William Ingram]	26:44-45 Accepted. Text modified to link statement to paper by Meehl et al.
9-588	A	25:38	25:38	Figure 9.4.2, because of the averaging, is inadequate. It only shows that the averaged model does not reproduce well interdecadal variability... [Stéphane Hallegatte]	This figure has been replaced by one showing trends over the two periods, 1901-2005 and 1979-2005, the two periods used in chap 3.
9-589	A	25:38		Fig 9.4.2: many small scale details in the grey shading are identical in the "observed" and "simulated" panels, suggesting to the reader that these are not independent. These grey areas may be caused by the plotting programme, but they should be removed. [Fons Baede]	This figure has been replaced by one showing trends over the two periods, 1901-2005 and 1979-2005, the two periods used in chap 3.
9-590	A	25:38		Fig 9.4.2: the caption should explain which forcings were included in the simulations. [Fons Baede]	This figure has been replaced by one showing trends over the two periods, 1901-2005 and 1979-2005, the two periods used in chap 3. The caption to the new figure describes the forcings



No.	Batch	Page:line		Comment	Notes
		From	To		
					included in the simulations.
9-591	A	25:38		Figure 9.4.2: The difference between simulations and observations are rather substantial over Eurasia. For example for 1965-1984 decade the simulated positive anomalies are close to 0, while the observed are rather noticeable. During the next decade the observed warming is again more pronounced. Could you, please, comment on this. [Eugene Rozanov]	This figure has been replaced by one showing trends over the two periods, 1901-2005 and 1979-2005, the two periods used in chap 3.
9-592	A	25:40	25:47	A single model result - should be downplayed (unless supported both other models). [Bryant McAvaney]	Rejected. This result is also seen in other models. References to such studies have been added to the text.
9-593	A	25:40		Global mean and hemispheric scale temperatures are controlled by external forcings on multi-decadal time scales. The sentence seems poorly constructed. Perhaps what is meant is: Global-mean and hemispheric-scale temperature trends on multi-decadal time scales are controlled by external forcings. yes? [Stephen E Schwartz]	Accepted. Changed.
9-594	A	25:49	26:6	It seems imperative to quantitatively summarize the results stated in this para. I suggest a table that includes forcing over the period, temperature change over the period, model sensitivity, and heat flux from ocean mixed layer to deep ocean. [Stephen E Schwartz]	See response to 9-573
9-595	A	25:50		Insert "some" after "include" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-596	A	25:52	25:52	Relevance of linear trend unclear without context - "cannot be reliably removed", perhaps? [William Ingram]	27:1 Accepted.
9-597	A	25:53	25:53	Reference 20C3M? [William Ingram]	27:2 Rejected. Not aware of a reference.
9-598	A	26:1		Replace "good" by "plausible" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-599	A	26:4		Insert "so readily" after "not" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-600	A	26:5	26:6	This conclusion does not follow. Replace "much more likely" on line 5 to "origin" on line 6 with "possibly contains an anthropogenic component"	Rejected. The reviewer provides no evidence or reasoning for suggesting



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Vincent Gray]	this change.
9-601	A	26:8	:25	<p>The discussion of decadal internal variability (and in particular that of the AMO) is far too short. It represents one of the key issues to be considered as it could potentially explain some of the observed warming. Some relevant references are:</p> <p>Latif M, 2001: Tropical Pacific/Atlantic Ocean interactions at multi-decadal time scales, GEOPHYSICAL RESEARCH LETTERS 28 (3): 539-542</p> <p>Latif M, Roeckner E, Botzet M, Esch M, Haak H, Hagemann S, Jungclaus J, Legutke S, Marsland S, Mikolajewicz U, Mitchell J, 2004: Reconstructing, monitoring, and predicting multidecadal-scale changes in the North Atlantic thermohaline circulation with sea surface temperature. JOURNAL OF CLIMATE 17 (7): 1605-1614</p> <p>Pohlmann H, Botzet M, Latif M, Roesch A, Wild M, Tschuck P, 2004: Estimating the decadal predictability of a coupled AOGCM. JOURNAL OF CLIMATE 17 (22): 4463-4472</p> <p>[Christoph Schar]</p>	We feel that this is primarily an issue for Chapter's 3 and 8.
9-602	A	26:11	26:11	<p>The use of uncertainty lexicon is generally very good in the chapter, but there are a few uses of "likely" that need to be changed to reduce confusion.</p> <p>[David Easterling]</p>	Accepted. Text changed here.
9-603	A	26:13	26:14	<p>Nozawa et al 2005 are not the only ones to attribute some of the early century warming to natural causes, so do Tett et al 2002 &amp; Stott et al 2003b to name but two. These (and other?) references should be included.</p> <p>[Gareth S. Jones]</p>	Accepted. Text amended.
9-604	A	26:15		<p>Page 26, line 15. Differences between simulations including increases in greenhouse gases only and runs also including the cooling effects of sulphate aerosols (e.g., Tett et al., 2002) indicate that the cooling effects of sulphate aerosols could account for some of the lack of observational warming between 1950 and 1970, despite increasing greenhouse gas concentrations.</p> <p>The text might note that this explanation has been offered previously:</p> <p>Seasonal, latitudinal, and secular variations in temperature trend: Evidence for influence of anthropogenic sulfate. Hunter, D. E., Schwartz, S. E., Wagener, R., and Benkovitz, C. M., Geophys. Res. Lett. 20, 2455-2458 (1993).</p> <p>[Stephen E Schwartz]</p>	Accepted. Text amended
9-605	A	26:18	26:22	<p>Somewhere here it needs to be made clear that the AMO is still rather poorly defined due to the relatively short record. Mention likely also should be made that there may well be some human influences occurring here, as at least the land cover change and sulfate aerosol forcings have been predominantly in this region and changes in their strength and pattern over time (and changes in the gradient of forcing with GHGs) might well be having some influence. There is really a need for much more work on this.</p>	See also response to 9-601



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Michael MacCracken]	
9-606	A	26:18	26:22	It is unclear why Delworth and Mann (2000) is not cited here: Delworth, T.L., and Mann, M.E., Observed and Simulated Multidecadal Variability in the Northern Hemisphere, Climate Dynamics, 16, 661-676, 2000. [Michael Mann]	Accepted. Text amended.
9-607	A	26:18	26:23	These aspects are considered in depth in chapter 3 so this discussion would benefit from explicit cross-referencing. [Peter Thome]	Accepted. Text amended.
9-608	A	26:23		Nagashima et al. (2005) find that carbonaceous aerosols are required for the MIROC model to provide a statistically consistent representation of observed changes in near-surface temperature in the middle part of the 20th century. Such a statement seems to require a quantitative statement including the magnitude of both the positive and the negative forcing of the aerosol. A large negative forcing would require an offsetting positive forcing. [Stephen E Schwartz]	Noted. The text has been amended to include some explanatory text, noting the regional nature of the BC and OC negative surface forcing.
9-609	A	26:27	26:33	The authors rightly note that the ability of climate models to reproduce observed temperature trends in the 20th century may be a function of improved models, or a fortuitous occurrence caused by compounding errors. This important point is not further discussed in the summaries. The implications to policy makers should be discussed in the summary. [Lourdes Maurice]	The fact that models are only able to reproduce observed temperature change when they include anthropogenic forcings and their failure to do so when they exclude anthropogenic forcings is the key point that is true even if the level of agreement is to some extent a fortuitous combination of sensitivity and forcing. Text has been amended to make clear that it is the close level of agreement that could be fortuitous.
9-610	A	26:27	26:30	Why "persuasive"? Only if you assume the models are essentially flawless in their depiction of the global climate system. In no other area of science would this kind of counterfactual model simulation be considered "persuasive" evidence of cause. The sentence should read: "The ability of climate models to reproduce some observed temperature changes over the 20th century when they include anthropogenic forcings and their failure to do so when they exclude anthropogenic forcings is suggestive of a human influence on global climate." [Ross McKittrick]	Rejected. None of the model simulations is able to simulate recent global warming when they omit anthropogenic forcings.
9-611	A	26:27	:36	This argumentation should be changed, as it is not fully honest in its current form. In essence, the approximate agreement of models with the global mean temperature (cf. Fig.9.4.1) is at least partly an effect of tuning. Some parameters in the parameterization	Accepted. Text amended to reflect this with discussion already in place of possible tuning.



No.	Batch	Page:line		Comment	Notes
		From	To		
				schemes have been adjusted by all modeling groups, at least partly (implicitly or explicitly) driven by the desire to match the observed warming since 1960. This is an appropriate procedure, given that there are indeed uncertainties in the respective parameterizations. The important point - however - is that there appears to be no way to match the GCM simulations with observations, unless some net GHG-induced warming is accounted for. [Christoph Schar]	
9-612	A	26:28		Replace "anthropogenic" with "greenhouse gas" both times [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-613	A	26:29		Insert after "climate" "But urbanization and land-use change which are also "anthropogenic" might be involved" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-614	A	26:29		... given the large uncertainties in aerosol forcings, agreement could have been obtained fortuitously as a result of, for example, balancing too much (or too little) greenhouse gas warming by too much (or too little) aerosol cooling, and there is some evidence for a possible negative correlation between models' sensitivity and their total forcing over the century. This sentence is very important. It is not stated well. There is not such a thing as a "greenhouse gas warming" or an "aerosol cooling". There is only a "warming" in response to a total forcing that is the sum of the forcings by the several agents. I propose a restatement: ... given the large uncertainties in aerosol forcings, agreement could have been obtained fortuitously as a result of, for example, balancing too great (or too small) a model sensitivity by too large (or too small) negative aerosol forcing, and there is some evidence for a possible negative correlation between models' sensitivity and their total forcing over the century. Some reference should be given to the statement of the "evidence of possible negative correlation" or alternatively, if that is a new finding, the evidence should be presented. Jeff Kiehl has presented such evidence in lectures. See also Schwartz S. E., Uncertainty requirements in radiative forcing of climate change. J. Air Waste Management Assoc. 54, 1351-1359 (2004). [Stephen E Schwartz]	Accepted. Text amended including references to Schwartz et al and Hansen et al as suggested.  The evidence for negative correlation has been removed as there is no published evidence for this.
9-615	A	26:31	26:33	It is not clear what is meant by 'there is some evidence for a possible negative correlation ...' [Robert Colman]	Accepted. Text deleted.
9-616	A	26:38	26:41	Should be stated much earlier in the chapter, as it is the basis of most of the work!	Rejected. This paragraph occurs before



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Stéphane Hallegatte]	the detection work is discussed and it is the detection work which assumes the additivity relationship to hold.
9-617	A	26:38	26:48	The proportionality of response to forcing can be expected to apply only globally in view of heat flows. Cox et al (1995) found that global mean temperature response was proportional to global mean forcing even for highly nonuniform spatial distribution of forcing (Northern Hemisphere aerosol forcing) but that there was substantial response in the SH to the NH aerosol forcing. Climate response to radiative forcings by aerosols and greenhouse gases. Cox S. J., Wang W.-C. and Schwartz S. E., Geophys. Res. Ltrrs., 22, 2509-2512 (1995). [Stephen E Schwartz]	Noted but the point of this paragraph is to discuss additivity of responses to different forcings which is discussed both globally (where it holds well) and regionally (where it holds less well).
9-618	A	26:50	27:6	I think that this subsection is probably unacceptable in its current form. The ability of models to simulated natural variability is a key aspect of the whole chapter, but the validation limits attention to one single aspect. A more thorough and critical assessment is needed. The discussion of interannual variability and its simulation should go beyond global mean surface temperature, and include an element of "understanding" (consistent with the theme of this chapter). It should also reflect discussions of interannual variability in other chapters (e.g. chapter 8) (continued in next comment) [Christoph Schar]	We agree that this is desirable, and will consider this possibility. However, we note that the observational record is very short in relation to the multi-decadal time scales considered in D&A work.
9-619	A	26:51	26:57	Also p 9-27, line 4: ENSO should dominate natural variability and does not compare well with observations. Fig 9.4.1c is not plotted to show equal variance power vs frequency. This should discuss ENSO. See also p 9-44 lines 20-27 where some of this is discussed. [Kevin Trenberth]	ENSO is evident in Fig 9.4.3, but is not dominant, at least in global mean surface temperature. We plot the spectrum as we do to be consistent with the TAR, and to emphasise the lower frequency variability that is important for D&A work.
9-620	A	26:51	27:6	(continued from previous comment) Personally, I have some doubts about the ability of models to properly represent natural variability. I am not convinced by the kind of data shown in Fig.9.4.3. There are many reasons for concern: First, we know that global mean temperature is strongly affected by El Nino. Yet we know rather well that most GCMs have large biases regarding El Nino (both in terms of frequency and amplitude), and in general underestimate El Nino variability. Second, it is well known that most models have a tendency to overestimate surface-temperature variability over land. Third, there are profound difficulties to simulated atmospheric blocking and NAO-like variability. Fourth, very little systematic validation has been performed regarding Atlantic multi-decadal variability. In summary: it	See 9-618 and 9-619.



No.	Batch	Page:line		Comment	Notes
		From	To		
				appears possible that much of the agreement seen in Fig.9.4.3 is the result of compensating errors and tuning. Please comment and discuss! May be more detailed reference to other chapters could help, along with a brief summary. [Christoph Schar]	
9-621	A	26:52	26:52	"could lead" too weak: it *does* bias low, though the effect is not necessarily large enough to matter [William Ingram]	28:3 Accepted
9-622	A	27:0		Fig 9.4.4 1st sentence of caption compressed into incomprehensibility: explain top fully & then the next 2 using "as before but" language [William Ingram]	Figure redrawn with new caption.
9-623	A	27:0		Fig 9.4.4: I can't work out what the 3rd & 4th sentences mean: clarify [William Ingram]	Figure redrawn with new caption.
9-624	A	27:1	27:21	The term "low bias" is confusing. I suspect it means "bias towards low values" of the scaling factors, whereas it may be interpreted as a small bias. This should be clarified. [Matthew Collins]	28:3 Accepted. Text modified to clarify.
9-625	A	27:3		"similar variance": but the width of the band of variances is a factor of 10 or more! [Fons Baede]	Accepted. Text modified to stress consistency.
9-626	A	27:9	35:	An important factor is how do we distinguish the aerosol signal from the GHGs signal. It has been shown (e.g. See Krishnan and Ramanathan, Vol 29, No. 9, P. 54-1, 2002 GRL, ) that the aerosol signal (at least in the tropics) peaks during the dry season (October to May in Northern Hemisphere tropics).while it is lot weaker during the wet season and thus GHGs show up clearly during the summer in S. Asia. May be the lesson is, we have to start looking in detail regionally and seasonally to identify the human imprint unambiguously. [Veerabhadran Ramanathan]	Noted. Sentence added to end of 9.4.1.4 to reflect this point.
9-627	A	27:23		Replace "robust" by "suggestive" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-628	A	27:28	27:32	Fomby and Vogelsang do not do any attribution, so their paper should not be cited in this paragraph. Kaufmann and Stern's conclusions were refuted by Umberto Triacca, "On the use of Granger Causality to investigate the human influence on climate." Theor. Appl. Climatol. 69, 137-138 (2001). He showed that the structure of their VAR could not, in principle, discriminate between the conclusion of human influence on climate and the conclusion of no human influence on climate. Their estimated results are consistent with either conclusion. [Ross McKittrick]	Rejected – Fomby and Vogelsang is cited as an example of a statistical analysis of the global mean temperature series. Triacca is a critique of Kaufmann and Stern, 1997 not Kaufmann and Stern, 2002.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-629	A	27:31	27:31	What does "likely" mean? Not what IPCC say it means, I think [William Ingram]	28:39 Accepted. Text changed.
9-630	A	27:31		Insert "claimed they had" after "(2002)" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-631	A	27:32	27:32	In accordance with the previous comment, I would suggest to add in chapter 9, page 27, at line 32 "Human influence has been detected also using independent methods. A neural network model applied to the analysis of global temperature records from 1860 to 2000 shows that observed increased and variability can be explained only accounting for anthropogenic emissions superimposed to natural forcings (Pasini and al. 2005)" ref is: Pasini A., M. Lore', F. Ameli 2005 Neural network modelling for the analysis of forcings/temperatures relationships at different scales in the climate system, Ecological Modelling (in press, available online 30 September 2005), doi:10.1016/j.ecolmodel.2005.08.012 [Piero Lionello]	Taken into account. A reference to the paper is now included.
9-632	A	27:34	27:34	Although presented as an example this still is a single model result - consider reducing amount of text. [Bryant McAvaney]	Accepted. Text has been shortened.
9-633	A	27:43		Replace "found" by "claimed" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-634	A	27:50		Delete "best" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-635	A	27:52		Insert " possible" before "substantial" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-636	A	27:53		Replace "found" by "considered" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-637	A	28:3		Delete "low" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-638	A	28:4		scaling factors It should be pointed out that as soon as scaling factors, the factor by which	Rejected. It is still possible to draw



No.	Batch	Page:line		Comment	Notes
		From	To		
				the amplitude of the response is scaled to account for possible errors in the amplitude of the forcing or of the climate model's response (definition from appendix 9A1 page 86, line 27) are introduced, it is no longer possible to draw inferences about sensitivity. The definition of scaling factor should not be buried in the appendix but should be explicitly given in the text. These scaling factors can be substantial, Figure 9.4.6, implying large uncertainty either in model sensitivity or forcing. [Stephen E Schwartz]	inferences about sensitivity since the scaling factor is known as is the un-scaled model's climate sensitivity.
9-639	A	28:5		Delete "low" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-640	A	28:6	28:8	Whilst Crooks 2004 did find that HadCM3 may be underestimating the observed free atmosphere response to solar forcing, Jones et al 2003 found that the HadCM3 response to solar forcing was consistent with the observed free atmosphere and surface temperatures combined. This unfortunate conflicting evidence should be noted. [Gareth S. Jones]	Rejected. In Jones et al, regression is done with ols (meaning results for SOL biased low) and SOL has a much higher amplitude in the observations than in ALL. However there is a caveat on this result provided by Crooks et al as shown in 5 <sup>th</sup> bar in revised fig 9.4.4 which has been added.
9-641	A	28:11	28:11	... only relatively a small ..." should be "... only a relatively small ..." [Hans Gleisner]	Accepted.
9-642	A	28:12	28:13	Therefore, the evidence for ... at present is mixed." should be "Therefore, at present the evidence for ... is mixed." [Hans Gleisner]	Accepted.
9-643	A	28:12	28:12	As for 50-51 of 9-2, this is nonsensical to the innocent reader - or, worse, will be read as indicating that the observations underestimate what actually happened. It simply doesn't make sense to most people to attribute a cause to something that didn't happen. The best way I can see to get the meaning across is "... that greenhouse gases acting alone would likely have driven more warming than has been observed ..." or "that the warming due to greenhouse gases, if no other forcings had been acting, would likely have exceeded that observed ..." [William Ingram]	29:20 Accepted. Text changed.
9-644	A	28:13	28:15	On a solar-cycle time scale, the effects of solar forcing have been quantified using multi-variate regression [Haigh, 2003; Gleisner and Thejll, 2003]. The uncertainties on a longer, decadal to century, time scale are due to uncertainties in past solar irradiance, and not to difficulties in discriminating solar forcing from volcanic forcing. [Hans Gleisner]	Accepted. Text changed.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-645	A	28:17		Insert "possibly" before "attributable" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-646	A	28:19	28:20	As for 50-51 of 9-2 & 12 above, it is nonsensical to the innocent reader to attribute a cause to something that didn't happen. Actually, this text is so close to that at 12 that maybe a reference to it being the same result is appropriate. [William Ingram]	29:27-28 Accepted. Text changed.
9-647	A	28:24		Replace "found" by "estimated" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-648	A	28:24		consistent estimates for the greenhouse gas attributable warming of 0.7 to 1.3 C offset by cooling from other anthropogenic factors (associated mainly with cooling from aerosols) of 0.2 to 1 C This finding (and similar findings reported later in the same para and in the next several paras) has enormous implications which, it would seem, must be stated. Namely, at the high end of the range greenhouse warming of 1.3 C offset by aerosol cooling of 1.0 C, it means that 77% of the positive greenhouse forcing resulting from some 40 years worth of CO2 emissions (the e-folding time of anthro CO2 emissions) is being offset by the cooling forcing of a week's worth of aerosol forcing (atmospheric residence time of the aerosol). Such a possibility was noted by Schwartz, 1993: Does fossil fuel combustion lead to global warming? Schwartz, S. E., Energy Internatl. J. 18, 1229-1248 (1993). [Stephen E Schwartz]	Accepted. Sentence inserted.
9-649	A	28:37	28:37	fig 9.4.4 the indications for each graph are impossible to read (too small) [Bernard Seguin]	Accepted. Figure amended.
9-650	A	28:37		Fig 9.4.4 is hard to understand. [Kevin Trenberth]	Accepted. Figure amended.
9-651	A	28:39	28:57	This use of AR4 models is important and does indeed 'increase confidence' [Bryant McAvaney]	Noted.
9-652	A	28:44	28:57	This should take into account covariability with precipitation. [Kevin Trenberth]	Noted.
9-653	A	28:49		Replace "has been" by "might have been" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-654	A	28:51	9:28	Where exactly in Fig. 9.4.4. should the reader look at at this point? [Jouni Räisänen]	Noted. Figure has been amended to be clearer.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-655	A	28:51	28:53	What do "the model error structure" & "the model uncertainty" mean? If you think you're making it easier for the casual reader by using language that nobody could guess the actual meaning of in place of standard precise but obscure language, you're wrong [William Ingram]	30:2-4 Noted. Text uses terminology adopted by the authors of the paper but has been clarified.
9-656	A	28:51		Replace "show" by "indicate" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-657	A	29:0		Fig 9.4.5: caption: "in some cases" - indicate which with an asterisk or something [William Ingram]	Accepted. Done.
9-658	A	29:5		Insert "considered they had" before "detected" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-659	A	29:7	29:7	It should be added as a caveat that the Stone and allen 2005b study only used global means of the temperatures and no spatial information. This does increase the risk of degeneracy, so whilst interesting the conclusions about the lack of detecting natural changes should be treated with a little caution. [Gareth S. Jones]	Accepted. Text modified.
9-660	A	29:8	29:12	This says that Gillett & al showed that the sulphate aerosol response determines whether a space-time or space-only analysis is used. I guess a sentence has been lost? [William Ingram]	30:18-20 This is not what was meant. Sentence changed.
9-661	A	29:20		Replace "have likely" with "might have" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-662	A	29:23		Replace "detection" withj "estimation" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-663	A	29:23		Delete "robust" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-664	A	29:24		Replace "detection" with "estimation" twice [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-665	A	29:25	29:25	the early and latter" -> "both [William Ingram]	30:33 Accepted.
9-666	A	29:25		Replace "likely" by "possible" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting



No.	Batch	Page:line		Comment	Notes
		From	To		
					this change.
9-667	A	29:27	29:29	Again, these indications of human influences going back well over the 30-50 years often talked about needs to be made prominently in the opening summary. [Michael MacCracken]	Agreed. These conclusions are discussed prominently in the ES.
9-668	A	29:27		Replace "likely" by "may have" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-669	A	29:38		Use of Bayesian approach is indeed a significant step forward [Bryant McAvaney]	Noted.
9-670	A	29:43		There seems to be acceptance of "scaling factors" but this seems to be easy to criticize. Can it be justified? [Kevin Trenberth]	Noted. This is the basis of methodology used in detection and attribution which is discussed in some detail earlier in 9.4.1. and in appendix 9.A.1.
9-671	A	29:56	29:56	Much as I like the phrase, should "ensembles of opportunity" be explained or at least put in quotation marks? [William Ingram]	31:7 Accepted. Reference added.
9-672	A	30:8		Reanalysis is not reliable for this purpose. [Kevin Trenberth]	Noted.
9-673	A	30:21		strong evidence for an anthropogenic influence Do the authors mean "strong evidence for an anthropogenic greenhouse gas influence"? The aerosol influence is also anthropogenic, so the contrast seems inappropriate. [Stephen E Schwartz]	Rejected. The sentence is correct but has been clarified.
9-674	A	30:26	30:27	The second cause given is largely the cause of the 1st - swap & add "therefore"? [William Ingram]	31:33-34 Accepted.
9-675	A	30:44	30:44	Why assume it's possible - Jones & al (2000) suggest not. [William Ingram]	31:52 Taken into account. "Will" changed to "would"
9-676	A	30:51		Section 9.4.1.6: why this section in this chapter? It seems more appropriate to have this section on future warming rates in Chapter 10. [Fons Baede]	The purpose of this section is to discuss observational constraints on transient climate response, to complement the section in this chapter on observational constraints on climate sensitivity. The section has been re-titled to reflect this. However there may be a case for deleting this section.
9-677	A	30:56		Assuming... Dangling participle, apparently resulting from trying to sweep under the carpet who is	The dangling participle has been removed. This approach does not



No.	Batch	Page:line		Comment	Notes
		From	To		
				<p>doing the assuming. Be explicit about who is doing the assuming.</p> <p>The applicability of the assumption depends on the assumption that the mix of aerosol forcing and greenhouse gas forcing remains constant into the future. This should be explicitly stated and discussed. I am not at all sanguine about the statement, attributed to Kettleborough that</p> <p>This linear relationship between past and future fractional error in temperature change is sufficiently robust over a number of realistic forcing scenarios to introduce little additional uncertainty (Kettleborough et al., 2005).</p> <p>The assumption that aerosols will continue to offset the same fraction of GHG forcing in the future as until now rests on a proportionality between these forcings. Aerosols reside in the atmosphere a short time and hence their concentration is proportional to emissions. CO<sub>2</sub> remains in the atmosphere a long time and hence its concentration is proportional to the integral of emissions. There is only one mathematical function (describing the time dependence of emissions) that is proportional to its own integral. Thus aerosol forcing will continue to offset the same fraction of GHG forcing in the future as in the past only if emissions continue to rise exponentially. Of course this cannot continue indefinitely, at which time the ratio of GHG forcing to aerosol forcing will increase dramatically. The implications of this are enormous and need to be discussed. See also</p> <p>Does fossil fuel combustion lead to global warming? Schwartz, S. E., Energy Internatl. J. 18, 1229-1248 (1993).</p> <p>[Stephen E Schwartz]</p>	assume that the mix of aerosol forcing and greenhouse gas forcing must remain constant in future since separate scalings are applied to the greenhouse gas response and to the aerosol response. This has now been made clear in the text.
9-678	A	30:57	30:57	<p>"do not include dynamical ... processes in the stratosphere" vague &amp; misleading (sounds stronger than intended: no relevant effects are omitted, but resolution may not be adequate for them to emerge). "2005), and chemical and dynamical processes associated with the atmosphere's response to solar irradiance are omitted or not adequately resolved in many climate models used in detection studies" is clear &amp; hardly longer</p> <p>[William Ingram]</p>	32:8-9 Accepted.
9-679	A	31:0		<p>change sentence to: Variability in freshwater storage and the THC could also be associated with the Atlantic Multidecadal Oscillation (AMO) (Vellinga and Wu 2004, Kerr 2005, Knight et al. 2005)</p> <p>[Peili Wu]</p>	Refers to 43:31 Agreed to refer to Vellinga and Wu, 2004.
9-680	A	31:0		<p>ref: Vellinga, M. and P. Wu, 2004: Low-latitude freshwater influence on centennial variability of the thermohaline circulation. J. Climate, 17(23), 4498-4511.</p> <p>[Peili Wu]</p>	Refers to 43:31 See response to 9-680
9-681	A	31:0		<p>Atlantic Climate Pacemaker for Millennia Past, Decades Hence?</p> <p>[Peili Wu]</p>	Refers to 43:31 See response to 9-680



No.	Batch	Page:line		Comment	Notes
		From	To		
9-682	A	31:0		Richard A. Kerr, Science 1 July 2005; 309: 41-43 [DOI: 10.1126/science.309.5731.41] (in News Focus) [Peili Wu]	Refers to 43:31 See response to 9-680
9-683	A	31:1	31:2	A reference to this discussion of MOS should be given so as to tie the climate change studies to this field. [Michael MacCracken]	Done.
9-684	A	31:3		Replace "robust" with "acceptable" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-685	A	31:6	31:7	Replace "are likely to" with "may" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-686	A	31:20	31:20	Not just short but unreliable on longer timescales [William Ingram]	32:27-28 Text altered.
9-687	A	31:21	31:23	This sentence just seems a bit out of context here--it is just not clear what the point is for this chapter. [Michael MacCracken]	Rejected. The point is that forecasts are staid and do not depend to first order on which model is used.
9-688	A	31:22	31:22	Isn't it figure 9.6.2 ? [Stéphane Hallegatte]	Accepted.
9-689	A	31:22	31:22	Factor of 2 in sd or variance? [William Ingram]	32:30 2 in sd. Text altered.
9-690	A	31:22	31:22	Clearer with "even" before "for" [William Ingram]	32:30 Accepted.
9-691	A	31:22	31:22	Figure 9.6.2, not 9.6.1. [Jouni Räisänen]	Accepted.
9-692	A	31:32	31:32	"estimated" is unjustifiably vague: "calculated"? [William Ingram]	32:40 Accepted.
9-693	A	31:32	31:32	relatively small" - *not* generally true on the city scale, the obvious one - add something like "on large scales" [William Ingram]	32:40 Accepted.
9-694	A	31:35	31:39	Land use and land use change uncertainties are probably "underdone" in light of published material. [Bryant McAvaney]	Noted.
9-695	A	31:38	31:39	"attributable warming" is ambiguous. Should this be "warming attributable to increasing greenhouse gases"? [Jouni Räisänen]	Taken into account. Text changed to reflect warming attributable to other anthropogenic forcings



No.	Batch	Page:line		Comment	Notes
		From	To		
9-696	A	31:49	31:52	In that sulfates and black carbon have opposite sign effects, a bit better explanation is needed to understand why their influences will be difficult to separate. Apparently, the problem is because their patterns of influence are so similar that all one really gets is the net aerosol forcing--but their temporal history of emissions (and heights of emission) are different, so there should be opportunities to do so. [Michael MacCracken]	Noted. The text contains some discussion of separation of sulfates and black carbon
9-697	A	31:52	31:54	As well as India there could be greater differences in patterns of response to BC in China (Roberts and Jones 2004) and over the Arctic from changes in snow ice albedos (Hansen and Nazarenko 2004). These should be added. [Gareth S. Jones]	Accepted.
9-698	A	31:53		Replace "are likely to" with "may" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-699	A	32:0		Fig 9.4.6: caption: was sd or variance doubled? [William Ingram]	It was variance – caption revised.
9-700	A	32:11	32:11	Spelling of "Amman" incorrect, should be "Ammann". [Gareth S. Jones]	Accepted.
9-701	A	32:18	32:25	The argumentation in this paragraph needs to be reconsidered. It basically says that because climate models reproduce observed temperature changes we can conclude that missing physics can't be important. There is missing physics and it is manifested in the huge uncertainties related to the aerosol indirect forcing. This is sufficient evidence that the 'fortuitous' [your word] agreement of model and observation may indeed be that. This report must endeavour to present the opposite viewpoint: it should present the evidence that demonstrates that we understand the physics well enough to understand temperature changes, not simply assert that model-observation agreements mean we have got the problem licked. [Kenneth Carslaw]	This paragraph was misleading and has been rephrased. The point is that detection and attribution analyses do take account of systematic errors through scaling factors – this approach appears to work well for attribution of global mean temperature as evidenced by the consistency of results from models with different sensitivities and forcings.
9-702	A	32:18	32:19	Missing physics ought to discuss feedbacks, clouds, water vapor etc. [Kevin Trenberth]	Accepted. Included in revised text.
9-703	A	32:18	:25	The reasoning here is questionable. The fact that we are able to simulate temperature changes with missing or imperfectly understood physics does not indicate that such errors are relatively small. At best it suggests that that is the case. Suggestion: replace "indicate" by "suggest". [Fons Baede]	Accepted. Paragraph was misleading. See response to 9-701
9-704	A	32:27		Natural variability ought to include ENSO. [Kevin Trenberth]	Accepted.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-705	A	32:30		Do any models simulate the Atlantic Multidecadal Oscillation? Isn't an error of a factor of two possible? [Kevin Trenberth]	Noted
9-706	A	32:32		Replace "is likely to" with "may" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-707	A	32:34	32:34	Again, does "variability" mean sd or variance? [William Ingram]	33:40 Standard deviation. Text amended.
9-708	A	32:40		Insert "usually" before "estimated" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-709	A	32:41		Replace "effects appear to" with "has been shown to have significant" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-710	A	32:43	32:43	And because of greater modelling uncertainty [William Ingram]	33:51 Accepted.
9-711	A	32:45	35:33	A relevant recent piece of published attribution research that is not yet included in this chapter is the paper on "joint attribution" by Root et al. (citation provided at end of comment). In this paper, the authors present a meta-analysis demonstrating a statistically significant linkage between anthropogenic forcing of the climate system and observed changes in species phenology during the 20th century. This is important to detection and attribution research because species phenology can serve as a proxy for springtime temperatures, independent of thermometer records. The paper compares modeled historical temperature datasets produced by natural forcings only, anthropogenic forcings only, and their combination to species data, finding that anthropogenic forcings are the primary driver of the observed species changes considered in the paper. This additional piece of evidence showing statistically that anthropogenic climate change is having a discernable influence on species around the world would be a very useful addition to this chapter. In the current structure of the chapter, the most suitable location for its addition seems to be section 9.4.2, Regional Surface Temperature Change, as the changes in species are linked specifically to changes in regional temperature. Further, the analysis shows no statistical difference in the strength of correlation between modeled temperatures and species data when considering temperature data from the individual GCM grid box closest to each species' location or when considering temperature data from a more regional average of GCM grid boxes (the 9 grid boxes around each species' location). [Michael Mastrandrea]	This paper is dealt with at length in WGII so we do not discuss it here.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-712	A	32:49		Replace "observed" with "since the 15th Century" [Vincent Gray]	Noted. Insert "for at least 250 years" with a reference to fig 6.8.
9-713	A	32:49		Insert after "been" ; "sporadic" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-714	A	32:53		Insert after "America"; "and for Antarctica" [Vincent Gray]	Rejected. Reasonable observational data coverage in fig 3.2.9 does not extend to Antarctica
9-715	A	33:0		Fig 9.4.7: caption untrue: the warming in Southern Africa was plainly not steady. [William Ingram]	Figure and caption changed.
9-716	A	33:0		Fig 9.4.7: caption untrue: Western N America plainly does warm early in the century in the model. [William Ingram]	Figure and caption changed.
9-717	A	33:13		Delete "robustly" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-718	A	33:14		Insert at the beginning "have claimed to have" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-719	A	33:14		Delete "Given this success" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-720	A	33:25	33:25	This sentence needs rewriting to make it decipherable. [Bryant McAvaney]	Accepted. Sentence re-written.
9-721	A	33:34	33:37	That the NAO may be influenced by anthropogenic forcing needs much greater exposure than this limited mention. For example, in that hurricane number is tied to NAO state, this would seem to be an indication that hurricane number may well have been influenced by human influences. Yet all the various analyses of hurricane number that I have seen look only for a linear change in their number and make the presumption that all the variations are of natural origin, and so it is not surprising that they can so far identify no human influence. Also, the word "may" should be changed to some level of likelihood from the IPCC lexicon. [Michael MacCracken]	"May" changed to "could" to avoid likelihood statement. The issue of anthropogenic influence on the NAO is discussed in 9.5.2.2.
9-722	A	33:34		Replace "is robust to the" with "involves" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-723	A	33:48		Insert "thought to be" " before "detected" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-724	A	33:54	33:54	Add after (Knutson et al. 2005): including a century-scale sea surface warming in the "Main Development Region" for Atlantic hurricanes. (A figure can be provided if desired.) [Thomas Knutson]	Rejected. We prefer not to call out specific regions and impacts here (it would be difficult to do so comprehensively).
9-725	A	34:3	34:3	"failing to capture" sounds as if they should, but AFAIK it could be due to internal variability [William Ingram]	35:12 Replace "failing to capture" by "underestimating"
9-726	A	34:6	34:18	It should be noted in this paragraph that observed regional trends and variations may be wholly or partly caused by natural internal variability and hence there is no way models would be able to reproduce them. The paragraph is presented as if these are fundamental problems with models but it is quite possible that the models are entirely consistent with the forced component of the change (remember the observations are a 1-member initial condition ensemble). [Matthew Collins]	Accepted. Text changed to reflect this.
9-727	A	34:6	34:7	The conclusion that taken together there is "compelling evidence" of human influence on regional climates suggest a stronger degree of them is reflected in the executive summary (that "regional surface temperatures have likely been affected..."). [Robert Colman]	Rejected. The "compelling" applies to human influence on regional climate (in some regions) whereas the "likely" applies to human influence on every single continent except Antarctica
9-728	A	34:6	34:11	Somewhere near here it needs to be said that land cover change can have significant effects on regional to local climates--and that these models, at best, are only starting to include rough reconstructions of land cover change on a fine regional basis. In addition, it is not clear that these models are fully and properly treating air pollution influences in these regions--such as the height of SO2 emission, the amounts of ash, treating air quality buildup during particular weather regimes, etc.--so it should not be too surprising that there are some discrepancies here. Indeed, doing all of this is going to be very hard--but it should also be mentioned that not being able to replicate regional changes in all regions is unlikely to be an important factor in estimating the coming influences of GHG increases, etc. as these effects are typically much larger and less spatially dependent than due to these other terms. [Michael MacCracken]	Accepted. Text changed.
9-729	A	34:6		Delete "compelling" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-730	A	34:7		Replace "evidence" by "an indication" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-731	A	34:12		It is not correct to say the central US has cooled: see Fig 3.2.9, for 1901 to 2004 the region that cooled is the southeast, although even there, there is a modest warming since 1979. [Kevin Trenberth]	Accepted. Central US warmed between 1901-1940 and cooled between 1940 and 1979. This is what the paper discusses.
9-732	A	34:13	34:16	After "... North and South China in the second half of the 20th century", please add "Barely any model except the GFDL-CM2.1 could reproduce the cooling trend of southwestern China in recent decades against the warming trend elsewhere". The title of the reference "Zhou and Yu (2005) " has been revised as: Zhou Tianjun, and Rucong Yu, 2005, 20th Century Surface Air Temperature over China and the Globe Simulated by Coupled Climate Models, Journal of Climate, conditionally accepted [Tianjun ZHOU]	Text changed.
9-733	A	34:20	34:20	fig 9.4.7 the indications for each graph are impossible to read (too small) [Bernard Seguin]	This figure has been replaced
9-734	A	34:20		Figure 9.4.7: It is necessary to define the regions in this plot, giving a reference for them is not sufficient. [Kevin Trenberth]	This figure has been replaced.
9-735	A	34:22	34:32	But is it right for the right reason? That needs to explore precipitation and drought as well as temperature change. Usually dry conditions precede the major heat waves. [Kevin Trenberth]	Noted.
9-736	A	34:34	34:47	Surely detection studies with atmosphere only model are at a lower level of "confidence" than full coupled model results especially as noted the potential problem of 'double counting' the forcings through the SST imposed. [Bryant McAvaney]	Noted
9-737	A	34:43	34:43	What does "indices scanning the transition from mean to extreme" mean? [William Ingram]	35:53 Text clarified.
9-738	A	35:3	35:3	Again (9-3 57 to 9-4 1) I don't think anyone is underestimating the simulated changes, i.e. thinking they are smaller than they actually are. I assume "underestimated" should be "underestimates", i.e. they are actually smaller than they should be. [William Ingram]	36:13 Text clarified
9-739	A	35:4	35:6	There is a potential conflict here with the chapter 3 analysis of extremes which paints a more complex picture than that given here. Checking required for consistency between the chapters. [Peter Thorne]	Can't see any obvious inconsistency. DTR has not decreased uniformly, but is has decreased and the change is larger than simulated by models under



No.	Batch	Page:line		Comment	Notes
		From	To		
					control conditions.
9-740	A	35:4	35:14	This deals with DTR but should be a more global land issue (why is it in this section?), and the associated changes in cloud cover are a critical issue in getting this right. [Kevin Trenberth]	Noted. Paragraph moved.
9-741	A	35:7	35:10	The Stone and Weaver papers found that aerosols were not needed in their model simulations to cause the decrease in DTR (albeit underestimated decrease). Perhaps this should be noted. [Gareth S. Jones]	Rejected. Space limitations mean there is not space to go into this in more detail.
9-742	A	35:10	35:10	"naturally" (i.e. from internal variability and/or natural external forcings) seems conceptually out of place [William Ingram]	36:19 Rejected. Don't agree.
9-743	A	35:19	35:19	"predictable" sounds as if we *can* do it, rather than we could if we knew more - "potentially predictable"? [William Ingram]	36:28 Accepted. Done.
9-744	A	35:24	35:33	Much belatedly there is finally a mention of the role of rainfall with temperature and the fact that high temperatures go with dry conditions over land in summer and the tropics. This should be a global land issue and given much more prominence as it relates to how well models do precipitation and drought, and this is a major outstanding issue. See for instance Trenberth and Shea (2005) GRL 32, L14703, doi:10.1029/2005GL022760 "Relationships between precipitation and surface temperature". [Kevin Trenberth]	Citations included to Trenberth and Shea and to Douville.
9-745	A	35:24		Replace "seen" by "indicated" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-746	A	35:31		Insert "claimed to have" before "detected" " [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-747	A	35:44	35:44	"warmest in 500 years" could mean "warmest in at least 500 years (which is as far back as we know)" or "we know that the last time it was as warm was 500 years ago" & a reader might take either reading for granted - clarify! [William Ingram]	36:53. Agreed. Text clarified.
9-748	A	35:48	35:48	Give usual (but not totally intuitive) abbreviation: THC? [William Ingram]	36:56. Rejected. MOC is the accepted term in the AR4.
9-749	A	35:54	35:54	Or a decrease! This looks totally biased! (Though of course some impacts of extreme events are positive.) [William Ingram]	37:8. Agreed. Use "change". William's incorrect page and line numbering makes this process of responding much



No.	Batch	Page:line		Comment	Notes
		From	To		
					more difficult than it should be.
9-750	A	36:0		Fig 9.4.8: caption 1st line: "**the* threshold"? [William Ingram]	Caption improved.
9-751	A	36:11		Delete "robust" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-752	A	36:13		Replace "robustly" with "convincingly" [Vincent Gray]	Agreed.
9-753	A	36:23	36:28	Should be at the beginning of the section of regional changes [Stéphane Hallegatte]	Agreed.
9-754	A	36:24	36:26	The first point is very important and deserves wide and prominent mention—namely that "most of the variability of global mean temperature on multi-decadal timescales is externally driven"!!! I also think we should be indicating that we have somewhat less understanding of the second point, and how much is due to internal forcing, and how much might be contributed by various types of errors (e.g., the WW II records may be seriously limited by limited and changing station coverage, instrument calibration problems, etc.). [Michael MacCracken]	Noted. Amended text refers back to section 9.4.1
9-755	A	36:30		The introduction of "fraction of attributable risk" is an important step. It is unfortunate that not more studies have used the concept (or something similar). [Bryant McAvaney]	Noted.
9-756	A	36:39		I have not checked the reference (Allen, 2003) but I do not understand the formula. If $P_0 > P_1$ , then the attributable fraction is $> 1$ . [Fons Baede]	The formula is correct. If the risk doubles under climate change from 10 events/year to 20 events/year then 10 out of every 20 events are anthropogenic and the FAR=0.5. If on the other hand the risk halves from 10 events/yr to 5 events/yr due to human influence, there are 5 events that are not happening due to human influence and FAR = -1 meaning that the cost of the number of events that has happened has also been "saved" as a result of human influence. If the risk decreases from 10 events/yr to 1 event/yr the FAR = -9 consistent with the cost of 9 times the cost of the event that has



No.	Batch	Page:line		Comment	Notes
		From	To		
					happened being "saved" as a result of human influence. This is why FAR becomes negative in these circumstances.
9-757	A	36:52		Replace "likely" with "possibly" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-758	A	36:53	36:54	This conclusion needs a more cautious formulation. First, the 50% increase in standard deviation assumed by Schär et al. (2004, p. 335) to demonstrate the potential significance of increased variability in 1991-2002 appears too large even for a model that simulates a doubling of variability by the end of the 21st century. Second, by no means all models simulate as large increases in variability as the one used by Schär et al. [Jouni Räisänen]	Agreed. Used "might be consistent" instead of "was consistent"
9-759	A	37:0		Fig 9.4.9: caption towards end: "forced to have the same mean" obscure to some: "shifted vertically to match in the mean over 1960-99"? [William Ingram]	Agreed.
9-760	A	37:1	37:1	What on earth does "real" mean? [William Ingram]	38:11 Text amended.
9-761	A	37:4	37:6	There are not many studies on changes in the distribution of observed seasonal surface temperature and hence the available one should be included. SUGGESTION: add the following sentence after "variance expected from natural variability.": Scherrer et al. showed that estimates for temperature variability changes in Europe show a weak increase (decrease) in summer (winter) for the time period 1961 to 2004, but these changes are not statistically significant at the 90% level. Reference: Scherrer, S.C., C. Appenzeller, M. A. Liniger and C. Schär, 2005: European temperature distribution changes in observations and climate change scenarios. GEOPHYSICAL RESEARCH LETTERS, VOL. 32, L19705, doi:10.1029/2005GL024108. [Christof Appenzeller]	Agreed.
9-762	A	37:10	37:12	This contradiction may be associated with differences between models rather than between scales. Vidale et al. (2005; "European climate variability in a heterogeneous multi-model ensemble", submitted to the PRUDENCE special issue of Climatic Change) show that even regional climate models with the same GCM forcing simulate substantially different changes in summertime temperature variability in Europe. See Section 11.3.3.3.2. [Jouni Räisänen]	Agreed. Text amended to refer to chap 11.
9-763	A	37:21		Replace "very likely" with "possible"	Rejected. The reviewer provides no



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Vincent Gray]	evidence or reasoning for suggesting this change.
9-764	A	37:21		Insert "considered" before "a better" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-765	A	37:27		You should mention that the Stott et al. study involves a spatial averaging over a large European/Mediterranean region. Thus, the implications of this results for heat-waves (such as in 2003) is still only partly understood. [Christoph Schar]	Taken into account. Sentence added.
9-766	A	37:29	27:45	This paragraph concerns projections so should either appear in chapter 10 or 11. [Matthew Collins]	Agreed. Text passed to Ch 11.
9-767	A	37:30		Insert "thought to be" after "was" [Vincent Gray]	See response to 9-766
9-768	A	37:32		Insert "speculative" before "SRES A2" [Vincent Gray]	See response to 9-766
9-769	A	37:34	37:36	The difference between the Stott et al. results and the other results may be associated with differences between models rather than between scales. [Jouni Räisänen]	See response to 9-766
9-770	A	37:41	37:43	Kjellström et al. (2005; reference in Ch. 11) show the same result for the RCM simulations participating in PRUDENCE. [Jouni Räisänen]	See response to 9-766
9-771	A	37:47	37:49	Warm night attribution needs to be re-examined in light of new results on DTR in Chapter 3 [Thomas Karl]	Changes or lack of changes in DTR do not mean warm nights are not increasing. Decreasing DTR has been associated with daytime temperatures increasing faster than night-time temperatures in recent times. As Christidis shows there is a clear signal of increasing maximum nighttime temperatures.
9-772	A	37:47	37:47	Tebaldi et al (2005) not listed in references [Bernard Seguin]	Noted. Thanks. Added to references.
9-773	A	37:52	37:52	"due to" seems too strong [William Ingram]	39:5. Accepted. Text changed to emphasies main point of Thorne et al



No.	Batch	Page:line		Comment	Notes
		From	To		
					2002 conclusion.
9-774	A	37:56	38:26	Much of his discussion seemd misplaced and is a mixture on comments about data quality and trends. [Bryant McAvancy]	Agreed. Chapter 3 discussion is now summarised and cited.
9-775	A	37:56		Insert after "warmed" "after 1997, due mainly to the 1998 El Niño event" [Vincent Gray]	Section 9.4.4.1 has been completely rewritten to better reflect assessment made in chap 3 and in CCSP report.
9-776	A	37:57		Replace "shorter" with "1979-2004 period" [Vincent Gray]	See response to 9-775
9-777	A	38:4		Insert "almost" before "consistent" [Vincent Gray]	See response to 9-775
9-778	A	38:4		Delete "or larger than" [Vincent Gray]	See response to 9-775
9-779	A	38:4		Replace "as expected from models" with All models, however, expect a higher warming in the atmosphere than on the surface" [Vincent Gray]	See response to 9-775
9-780	A	38:6	38:6	Crooks reference can be replaced by Crooks & Gray (2005, J.Clim., 18, 996-1015), I think [William Ingram]	39:16. Rejected. No the correct reference is given in the text.
This is 39:16. 9-781	A	38:6	38:26	This description is overly simplified. There remain important differences in the tropics in both lower tropospheric temps and T2 temps among all data sets that remain unexplained. The work of Santer et al suggests only one data set is consistent with the models, but even here the rate of warming in the LT is still smaller that would be expected if the normal amplification between the sfc and the troposphere we in place. The CCSP report should be available by the time these comments are addressed to help resolve [Thomas Karl]	See response to 9-775
9-782	A	38:6	38:18	This paragraph heavily overlaps with the discussion in Chapter 3, some rewriting can be considered. [Eugene Rozanov]	Agreed. See response to 9-775.
9-783	A	38:6		Replace "Record lengths of 25 years are now possible from satellites" with "Temperature records from satellites now extend to 25 years" are now [Vincent Gray]	See response to 9-775
9-784	A	38:10		Delete "greatly" [Vincent Gray]	See response to 9-775.
9-785	A	38:11	38:11	This should be "dataset construction methodology choices" rather than "retrieval methods". Retrieval methods are how to go from the retrieved parameter (radiation) to a	See response to 9-775.



No.	Batch	Page:line		Comment	Notes
		From	To		
				geophysical parameter (temperature). All the groups use the same retrieval parameter. The differences arise through the homogenisation process applied to the retrieved values. [Peter Thorne]	
9-786	A	38:11		Delete from "If" to "large" [Vincent Gray]	See response to 9-775.
9-787	A	38:11		"If these differences are real" what does this mean: there are real differences, why is it an "if"? [Kevin Trenberth]	See response to 9-775.
9-788	A	38:16	38:18	Though it might be awkward, I would think somewhat greater explanation should be given here--backing up IPCC's decisions in previous assessments to be restrained in drawing conclusions from the Christy MS data sets, etc. as they had not been duplicated. I also commend the author for not citing the papers based on flawed science--in my opinion the papers having the mathematical sign errors should have been formally withdrawn by the authors so they could no longer be cited and some sort of "mea culpa" should have been published. [Michael MacCracken]	See response to 9-775
9-789	A	38:16	38:18	What does "internally consistent" mean? Both differ and there remain substantial issues due to orbit changes, drift in equator crossing time, correction for diurnal cycle, etc. That is also true in channel 2: see Fu and Johanson 2005 GRL (also Chapter 3). [Kevin Trenberth]	See response to 9-775.
9-790	A	38:17	38:18	mention version numbers for the UAH products otherwise people will lose track of which version is which [Gavin Schmidt]	See response to 9-775.
9-791	A	38:18	38:18	Add the following sentences at the end of this paragraph: "Vinnikov et al. (2005) have shown that when T2 data are correctly processed, the global average tropospheric temperature trend is the same as the surface trend, and that the upward tropospheric trend is larger than the surface trend in the tropics, but smaller in higher latitudes. These latitudinal variations of surface and tropospheric trends are virtually the same as those simulated by the GFDL model for the same period when forced by observed GHG and aerosol variations." ref: Vinnikov, Konstantin Y., Norman C. Grody, Alan Robock, Ronald J. Stouffer, Philip D. Jones, and Mitchell D. Goldberg, 2005: Temperature trends at the surface and in the troposphere. J. Geophys. Res., in press. [Alan Robock]	See response to 9-775.
9-792	A	38:18		Add at end "Despite these corrections the MSU record, with a rise of 0.123K per decade, is significantly lower than the surface figures of 0.154- 0.176K for the same period. [Vincent Gray]	See response to 9-775.
9-793	A	38:26		Add at end "After these corrections have been applied, the radiosonde record confirms the	See response to 9-775.



No.	Batch	Page:line		Comment	Notes
		From	To		
				lower temperature record of the MSU satellites"? [Vincent Gray]	
9-794	A	38:29	38:43	In view of comments about quality of radiosonde obs in section above inferences about tropopause height need to be more carefully constrained from obs quality. (Inf from re-analyses while useful - has the problem (potential) of "too much model" [Bryant McAvaney]	Agreed. Comment inserted about quality problems and their possible influence on this result.
9-795	A	38:40	38:41	It should be noted that observation-based estimates agree much better with the GCMs outside this specific area, so that this is an example of the selection problems raised on 9-7, & a posteriori "local" significance tests inflate the real significance [William Ingram]	39:50 Noted. Text of this section heavily rewritten to reflect CCSP report.
9-796	A	38:41		Delete "robustly" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-797	A	38:42		Insert before "Solar" ; "They considered that" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-798	A	38:44	38:45	It should be noted that this disagreement on longer timescales only is precisely what one would expect if the disagreement were due to inhomogeneity in the observation-based estimates [William Ingram]	Agreed. Text amended. This is line 39:54-55.
9-799	A	38:49		Replace "detected" by "thought that this showed" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-800	A	38:50		How do you know it was overestimated by the model? Recent work by Sherwood et al (2005) and Randle and Wu (2005) show that the observations greatly underestimate the trends (negative bias); see also Chapter 3, Section 3.4.1. [Kevin Trenberth]	Agreed. Text amended and reference made to 3.4.1.
9-801	A	38:53		Delete "robustly" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-802	A	38:54		Replace "robustly" with "convincingly" [Vincent Gray]	Rejected. Robust was word used by authors since the detection is robust to



No.	Batch	Page:line		Comment	Notes
		From	To		
					truncation choice in their analysis.
9-803	A	38:56		Insert after "changes" "could be made to" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-804	A	39:1	39:15	All of this should be qualified by the increased understanding that the observations are seriously flawed (Sherwood et al, Randel and Wu). [Kevin Trenberth]	Agreed. Text rewritten to reflect this.
9-805	A	39:3		Delete "robustly" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-806	A	39:3		Replace "detected" by "indicated" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-807	A	39:9		Delete "robust" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-808	A	39:9		Replace "detection" by "indication" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-809	A	39:12		Delete "unambiguously" [Vincent Gray]	"Unambiguously" was the word used by the authors of Thorne et al in their paper. However have deleted it to avoid confusion.
9-810	A	39:14	39:14	As for 50-51 of 9-2, it is nonsensical to most people to attribute a cause to something that didn't happen & I suggest "greenhouse gases acting alone would have caused more warming than actually observed over ..." or "greenhouse gases, if no other forcings had been acting, would have been responsible for a warming larger than actually observed over ..." [William Ingram]	40-24-25. The wording in the para has been changed. Also to be consistent with TS. Note first reference to this statement has been removed. See response to 9-815
9-811	A	39:14		Delete "by far" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting



No.	Batch	Page:line		Comment	Notes
		From	To		
					this change.
9-812	A	39:16	39:16	HadRT or HadAT? [Bernard Seguin]	This is HadRT2.1s.
9-813	A	39:16		Insert "claimed to have" before "detected" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-814	A	39:23	39:31	Reservations about use of atmosphere only models in this context. [Bryant McAvaney]	Noted – we don't rely much on the conclusions from this work but it is important to cite I think since this work supports work from coupled models.
9-815	A	39:24	39:24	Again (as for 50-51 of 9-2 & 14 of this page), it is nonsensical to most people to attribute a cause to something that didn't happen - but this is so repetitive of 14 they should be combined anyway [William Ingram]	Agreed. See response to 9-810. This is 40:33-34. Sentence changed and to be consistent with TS.
9-816	A	39:27		Replace " It is highly unlikely" with "their modeld indicate" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-817	A	39:28		Insert "not" after "could" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-818	A	39:34	39:35	Again, known bad trends in radiosonde data should be taken into account. [Kevin Trenberth]	Agreed. The discussion has been revised accordingly.
9-819	A	39:39		Replace "helps" with "may help" [Vincent Gray]	Agreed.
9-820	A	39:40	39:40	I would suggest changing "may have" to "likely" to make this consistent with the IPCC lexicon [Michael MacCracken]	Agreed.
9-821	A	39:50	39:57	In view the major conentionius issue of surface versus upper air - ths section seems a little "underdone". [Bryant McAvaney]	Noted. This issue has been extensively reviewed by CCSP, 2006 and it doesn't seem to make sense to duplicate such an extensive review here. The section has been revised to draw more on the



No.	Batch	Page:line		Comment	Notes
		From	To		
					CCSP review.
9-822	A	39:50	39:57	Inclusion of the Figure 4 from this paper in modified form (as available in Chapter 5 of the public review CCSP report version) would greatly aid this discussion and make the point about explicit consideration of observational uncertainty far more forcefully. [Peter Thome]	Reference made to CCSP report where figure is contained.
9-823	A	39:56	39:57	Do you know that Vinnikov and Grody values are not consistent: they show even more warming than RSS? Also, given that trends in radiosondes have been discredited and UAH agree with sondes (so they claim), doesn't that also discredit UAH? [Kevin Trenberth]	Text revised to be consistent with CCSP, 2006 which contains latest datasets.
9-824	A	39:57	39:57	Add at end of paragraph: "Vinnikov et al. (2005) have also shown that their analysis of surface and tropospheric temperatures agrees with model simulations." [Alan Robock]	Taken into account. An extra sentence has been included.
9-825	A	40:0		Section 9.4.5. This should be removed and accommodated in the executive summary. [Kevin Trenberth]	We disagree. This section provides a useful summing up of the evidence for evidence of human influence on temperature changes.
9-826	A	40:1	40:1	observational dataset" confusing - in the field used to mean "data as processed" but to an outsider sounds like "collection of observations" [William Ingram]	Agreed. This is 41:11.
9-827	A	40:4		Replace "the evidence has" with "model results have" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-828	A	40:4		Insert after "strengthened" "the possibility" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-829	A	40:4		Summary needs careful attention to likelihood words versus subjective "clearly identified" "compelling evidence" etc etc. [Bryant McAvaney]	Agreed. Compelling is justified because it applies to detection on all regions providing compelling evidence for global human influence. Instances of use of clear or clearly identified removed.
9-830	A	40:6		Insert "surface" after "instrumental" [Vincent Gray]	Accepted.
9-831	A	40:6		Insert "many of" before "which" [Vincent Gray]	Accepted.
9-832	A	40:6		Add at end "can be adjusted to"	Rejected. The reviewer provides no



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Vincent Gray]	evidence or reasoning for suggesting this change.
9-833	A	40:8		Insert before "able" ; "easily" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-834	A	40:9		Insert "known" before "natural" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-835	A	40:10	40:10	I would suggest changing "demonstrate" to "lead to the conclusion" as the this is more what is meant. [Michael MacCracken]	Accepted.
9-836	A	40:10		Replace "These studies demonstrate" with "Climate models, therefore, indicate" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-837	A	40:10		Insert after "forcings" "(which include urbanization and land chnages" [Vincent Gray]	Rejected. Sentence modified to discuss role of greenhouse gas forcings.
9-838	A	40:10		Replace "dominate" by "play a large part in " [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-839	A	40:12		Delete from "This conclusion" to available". This sentence. Is unnecesary [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-840	A	40:14		Insert after "have" ; "claimed to have" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-841	A	40:14	:19	See my comment regarding variability (p26,150) [Christoph Schar]	Noted. An assessment of variability on regional scales has been made and support this statement.
9-842	A	40:16		Insert "often" after "models" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-843	A	40:18		Replace "found" by "claimed" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-844	A	40:24		Replace "were" with "seemed to be" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting



No.	Batch	Page:line		Comment	Notes
		From	To		
					this change. Sentence re-jigged to be consistent with statement in body of text.
9-845	A	40:25	40:25	I would suggest changing this to read "there has been a significant cooling influence from aerosols counter-acting some of the warming influence of the increasing concentrations of greenhouse gases." Note the use of the word "influences" as this is really what is going on. [Michael MacCracken]	Accepted.
9-846	A	40:25		Replace "They show" with "The models indicate" [Vincent Gray]	Sentences revised consistent with suggestion.
9-847	A	40:25		Replace "has" with "may have" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-848	A	40:26	40:26	I would suggest changing "is evidently" to "has proven" [Michael MacCracken]	Taken into account. "evidently" deleted.
9-849	A	40:27	40:29	It is likely that multi-decadal variations in the thermohaline circulation would cause differential hemispheric heating/cooling so there is a possible degeneracy here. [Matthew Collins]	Noted. Text clarified and "can" inserted to stress that this fingerprint helps in some studies.
9-850	A	40:28		Delete "fingerprint" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-851	A	40:30		Delete "very" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-852	A	40:30		Replace "evidence for" with "indication of" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-853	A	40:31		Replace "weaker evidence" with "a weaker indication". [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-854	A	40:31		Delete "clear" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-855	A	40:32	40:32	Change "cooling" to "cooling influence" as the temperature has actually been rising. [Michael MacCracken]	Accepted.
9-856	A	40:33		Replace "likely" by "possible"	Rejected. The reviewer provides no



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Vincent Gray]	evidence or reasoning for suggesting this change.
9-857	A	40:36	40:43	The findings here that there is "compelling evidence" of human influence on regional climates, and that human influence "has been clearly identified" suggest a stronger degree of them is reflected in the executive summary (that "regional surface temperatures have likely been affected..."). [Robert Colman]	Sentence adjusted to make consistent, the compelling referring to human effect on climate as a whole, the fact that there is detectable change on all continents provides compelling evidence for a human influence on global climate.
9-858	A	40:36		Replace "identification" by "indication" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-859	A	40:36		Replace "signal" by "effect" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-860	A	40:39		Delete "significant" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-861	A	40:39		Delete "compelling" [Vincent Gray]	See response to 9-857.
9-862	A	40:39		Add at end "a possible" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-863	A	40:42		Replace "clearly identified" with "indicated" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-864	A	40:43	40:43	Should there be a cross reference to the previously discussed "selection effect" here? [Matthew Collins]	Accepted. Together with new sentence inserted drawing together a conclusion that the chance of all results being spurious is very small.
9-865	A	40:45		Insert at beginning "Model" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-866	A	40:47		Replace "significant" by "possible" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-867	A	40:47		Replace “robust” with “convincing” [Vincent Gray]	Noted. Sentence changed.
9-868	A	40:48		Replace “detection” with “indication” [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-869	A	40:53		Replace “very likely” with “possible” [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-870	A	40:53		Insert “considered to be” before “batter” [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-871	A	40:53		Replace “has” by “may have” [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-872	A	41:2		Replace “has” with “might have been” [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-873	A	41:2		Replace “a significant” by “an” [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-874	A	41:4		Replace “detected” by “postulated” [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-875	A	41:6		Replace “has” by “seems to have” [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-876	A	41:7		Replace “show” by “indicate” [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-877	A	41:7		Replace “are Likely” with “may be” [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for suggesting this change.
9-878	A	41:9	41:16	This discussion is inconsistent with discussion on page 38, and I believe this discussion better captures the current state of affairs [Thomas Karl]	Noted. The discussion on page 28 has been substantially revised to be consistent with the CCSP report as is this summary paragraph.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-879	A	41:9	41:16	The inconsistencies noted here are now known to be due to bad radiosonde data. There is not "only one observational estimate" because Vinnikov and Grody also exists. Since this is the summary, why doesn't it also deal with stratospheric cooling? [Kevin Trenberth]	Noted. Sentence altered to reflect that not just one estimate agrees. A discussion of stratospheric trends seems outside the scope of a brief summary.
9-880	A	41:18	43:32	It would be better to move this discussion to Chapter 5. [Andrew Lacis]	Rejected. Quantitative comparison between modelled and observed estimates of oceanic climate change are made here.
9-881	A	41:18		Section 9.5: there are many overlaps between this section and chapters 3 - 5 [Fons Baede]	Noted. One overlap addressed by moving sea level discussion to chap 5.
9-882	A	41:30	41:31	Shouldn't this also mention mass gain by the oceans from melting glaciers, etc? [Kevin Trenberth]	Agreed. Done.
9-883	A	41:34	41:40	I don't follow this argument. The change in ocean heat content could easily arise from some process of internal climate variability which involved a change in the planetary albedo. I wouldn't call that a 'net positive radiative forcing of the climate system'. Suggest omit this argument (it doesn't detract from the formal D&A results later). [Richard Wood]	Disagree. This argument was suggested by a reviewer of ZOD and appears in the literature (Hansen et al, 2005). No known process can actually do this.
9-884	A	41:34		Replace "±5" with "±10". Chapter 5 uses only one standard error. It should be doubled to give 95% confidence [Vincent Gray]	This part of the sentence has been replaced since these numbers are not quoted by the Chap 5 ES.
9-885	A	41:35		Replace "during the latter half of the 20th century" with "since 1975" [Vincent Gray]	Noted but correct to last 50 years (since increase is from 1955 as quoted in the chap 5 ES)
9-886	A	41:40		Insert after "source" "However, it remains a difficulty that the ocean began to heat from 1970, whereas the surface began to heat only from 1980, and the lower troposphere has only begun to heat since 1997" [Vincent Gray]	Rejected. These assertions are incorrect.
9-887	A	41:41		Add at the end "but they have a difficult task" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for his proposed change.
9-888	A	41:45		Replace "in the second half of the 20th century" with "since 1975" [Vincent Gray]	See response to 9-885.
9-889	A	41:48		I believe Section 5.5.3 and associated figures are much better than 5.2.2; it embraces the studies of Ishii and Willis as well as Levitus (this is a Chapter 5 problem). [Kevin Trenberth]	Rejected. Observational model comparisons have been made with ocean heat content not with sea level.
9-890	A	42:1	42:31	More reference to Chapter 5 might be useful here. On line 24 the reference to Levitus et	Additional references made to Ch 5 –



No.	Batch	Page:line		Comment	Notes
		From	To		
				al decadal variability raises questions about how real it is: it is not replicated in Church et al 2004 sea level reconstruction, for instance. [Kevin Trenberth]	but note that THIS ISSUE WASN'T RESOLVED BY CHAP 5 PEOPLE at time this response was prepared.
9-891	A	42:5	42:5	Change "that cooling from" to "the cooling influence of" [Michael MacCracken]	Agreed. Done.
9-892	A	42:5		Replace "likely" with "possible" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for his proposed change.
9-893	A	42:15	42:18	Suggest "..., simulated ocean warming due to anthropogenic factors ... is consistent ... and reproduces ..." [Richard Wood]	Agreed. Done.
9-894	A	42:17		Insert "more or less" before "consistent" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for his proposed change.
9-895	A	42:19		Insert "possible" before "human" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for his proposed change.
9-896	A	42:23	42:31	add in this paragraph- Natural variability in ocean circulation is responsible for decadal shifts in north Pacific heat transport, which may drive decadal atmospheric oscillations (Schneider et al. 1999, 2002). Full citations- Schneider, N., A. J. Miller and D. W. Pierce, 2002: Anatomy of North Pacific decadal variability. J. Climate, 15 586-605. Schneider, N., A. J. Miller, M. A. Alexander and C. Deser. 1999. Subduction of decadal north Pacific temperature anomalies: Observations and dynamics. J. Phys. Oceanogr., 29 1056-1070. [Franklin Schwing]	Rejected. This discussion is not sufficiently relevant here.
9-897	A	42:23		Delete "well" [Vincent Gray]	Rejected. The reviewer provides no evidence or reasoning for his proposed change.
9-898	A	42:28	42:28	I don't believe the PDO to be any more than a projection of ENSO onto decadal means (with advection downstream), so they should not be listed together without comment as if they are independent phenomema. [William Ingram]	This is 43:48. Rejected. This is not a consensus view.
9-899	A	42:31	42:31	Considering the point of this paragraph, suggest "...observationally-based estimates of..." rather than "...observed...". [Richard Wood]	Agreed. Done.
9-900	A	42:34	42:34	At start of paragraph, change to read "This section presents a brief synopsis" to get away from this being a personal view.	This section completely rewritten to incorporate material from chap 5.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Michael MacCracken]	
9-901	A	42:35	42:45	I found this paragraph too short to understand. I think the discussion of 5.5.7 (about which I have some reservations, sent to Chapter 5) should be in chapter 9 – it is about understanding recent climate change. Moving the section to chapter 9 would avoid the need for a compressed summary. [Richard Wood]	The 5.5.7 material has been revised and moved to chap 9.
9-902	A	42:35		Replace “significant” with “possible” [Vincent Gray]	See response to 9-900
9-903	A	42:41	42:42	What is the basis for the statement “for which there is no observational evidence”? The TOPEX/Poseidon derived rates of rise beginning in the 1990s are considerably higher than the rates of rise for the century on average, indicating an acceleration in the rate of rise at a time when the natural forcing would suggest the rate should be slowing. There is also evidence of accelerated glacial melting (and even of some acceleration of Greenland ice sheet melting) to justify suggestions that this is a human influence. [Michael MacCracken]	See response to 9-900.
9-904	A	42:42		It is said here that there is no observational evidence for acceleration. This is in contrast with Section 5.5.7, page 5-34, line 41 where I read: “The increase in rate of rise over recent decades is consistent with observations and with rising anthropogenic forcing.” [Fons Baede]	See response to 9-900.
9-905	A	42:42		This is wrong: There is clear evidence for acceleration of sea level rise: the rate is 3 mm/year since 1992 when TOPEX etc data have been available (see sec 5.5), the question is whether it will continue or maybe whether it is a rebound from Pinatubo. But don’t deny that it has occurred. [Kevin Trenberth]	See response to 9-900.
9-906	A	42:43	42:44	This is a very strange sentence—given that for several thousand years prior to human contributions, the sea level had been essentially constant. It would really seem that anthropogenic forcings are the only significant contributor to overall sea level rise during the 20th century. And by using the superlative “largest” there is an implication that there are at least two other potential contributors that are also contributing when in reality, none of the others (isostasy, for example) are quite small. [Michael MacCracken]	See response to 9-900.
9-907	A	42:43	42:43	Chapter 5 discusses at length observational evidence for acceleration. [Richard Wood]	See response to 9-900.
9-908	A	42:46		On the sentence, “One possible oceanic consequence of climate change is a slowing down or even halting of the thermohaline circulation.”, I think a statement of Carl Wunsch is important (C. Wunsch, “What Is the Thermohaline Circulation?” Science, Vol 298, Issue 5596, 1179-1181, 8 November 2002). He points out the following: “The conclusion from	Rejected as not relevant.



No.	Batch	Page:line		Comment	Notes
		From	To		
				this and other lines of evidence is that the ocean's mass flux is sustained primarily by the wind, and secondarily by tidal forcing." Since broad readers know his statement, there should be a comment on his idea here in this section. [Kiminori Itoh]	
9-909	A	42:49	42:49	Recently, the data assimilation community starts focusing on long-term changes in 3-D water mass properties/pathways (e.g., Stammer et al. 2002; Masuda et al. 2003). Such activities could be referred in this sub-section. (Stammer D, Wunsch C, Giering R, et al.: Global ocean circulation during 1992-1997, estimated from ocean observations and a general circulation model, JOURNAL OF GEOPHYSICAL RESEARCH-OCEANS 107 (C9): Art. No. 3118 SEP 2002; Masuda S, Awaji T, Sugiura N, et al.: Improved estimates of the dynamical state of the North Pacific Ocean from a 4 dimensional variational data assimilation, GEOPHYSICAL RESEARCH LETTERS 30 (16): Art. No. 1868 AUG 29 2003 ) [Toshiyuki Awaji]	Noted
9-910	A	42:52	42:56	This is not quite right, recommend seeing Chapter 5 and Fig 5.6.1. The freshening is only at high latitudes and salinity has increased in the subtropics. It is misleading to say "global increase in the hydrological cycle" but should say that with warming and increased water holding capacity, increased precipitation at higher latitudes is favored. See also Section 5.5.3 for changes in salinity. [Kevin Trenberth]	Agreed. Corrected to be consistent with 5.6.
9-911	A	43:13	43:13	Suggest add ", but this fingerprint has not been formally detected in changes to date". [Richard Wood]	Agreed. Done.
9-912	A	43:15	43:16	This needs a cross-reference to the section of the IPCC report where this is discussed. [Michael MacCracken]	Agreed. Cross-referenced Box 5.1.
9-913	A	43:15	43:15	Suggest replace "or even halting of the thermohaline circulation" with "of the meridional overturning circulation (MOC) (see 10.3.4)". And change "THC" to "MOC" throughout this paragraph (for consistency with other chapters). [Richard Wood]	OK. MOC is the agreed IPCC term.
9-914	A	43:15	:32	The following should be added as observational evidence: Dickson et al. (2003, Philosophical Transactions) don't find "convincing evidence yet of any significant, converted slowdown in the Atlantic overturning circulation". The Curry and Mauritzen (2005) paper should be mentioned here. They don't find any weakening of the MOC. It is noted in ref. (11) of Curry and Mauritzen (2005) that the 20% reduction was not persistent, may have stopped or even reversed. The changes in the Labrador Sea freshening should also be mentioned somewhere here. The recent freshening trend has stopped or even reversed (Wu et al. 2005b). [Peili Wu]	Rejected. This is an issue for Chap 5.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-915	A	43:17		Suggest do not use the term "enhanced hydrological cycle" as it is not clear what it means. Refer to changes in precipitation and evaporation. Also line 26. L 20: why does freshening correspond to upward THC? Do thermal effects dominate? Is this dependent on definition of THC? Do winds change? [Kevin Trenberth]	Agreed. Text rewritten to clarify.
9-916	A	43:20	43:20	It might be appropriate to mention here the Kerry Emanuel hypothesis (I think from JGR, 1999) that warming will lead to an intensification of the THC because increased mixing of warm tropical near surface waters by increased tropical cyclone activity (e.g., energy dissipation over the oceans) will increase the bringing up of cold deep waters in the tropics, making downwelling in higher latitudes easier (and so greater). [Michael MacCracken]	Rejected since not aware of more recent (post TAR) work supporting this hypothesis.
9-917	A	43:20		What is an "upward" or "downward" trend in the THC? [Fons Baede]	Noted. Re-written. Change to increase and decrease, the terms used in box 5.1.
9-918	A	43:22	43:24	The Arctic origins of the freshening in HadCM3 are not clearcut. Suggest replace this sentence with. "Dickson et al (2002) propose a possible role for the Arctic in driving the observed freshening of the subpolar North Atlantic." Then continue as before. [Richard Wood]	Agreed
9-919	A	43:26	43:32	This is not right and is not consistent with observed changes. The Atlantic Multidecadal Oscillation has barely one cycle and the latest change (warming) is as likely not related to that at all, as it is global. To say that things are natural, still means that the energy has to come from somewhere. [Kevin Trenberth]	Noted. The reference is to results that show that natural externally forced HadCM3 runs show freshening. The text has been amended to clarify and refer to box 5.1.
9-920	A	43:29	43:30	But earlier in this chapter it is noted that anthropogenic factors could be influencing the natural oscillations--and is it the case that Wu et al. allowed for this--or are they simply making the assumption that all NAO and other variation must be natural because it is not changing in a straight line? [Michael MacCracken]	The text has been clarified to avoid confusion about Wu et al.
9-921	A	43:34	47:23	There is no section about the "Hadley-Walker circulation". Yet, several papers have discussed its decadal change over the last two decades (e.g. Chen et al. 2002, Mitas and Clement 2005). Is it because it is difficult, at the present time, to predict the response of the Hadley Cell? (cf lines 42-46 of the same page). [Sandrine Bony]	Yes, we presently rely on the Risbey et al. assessment.
9-922	A	43:34	51:16	This discussion would be more at home in Chapter 3. [Andrew Lacis]	Reject. We include references to chapter 3 for observed changes, but discuss mechanisms and the consistency of simulated and observed changes.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-923	A	43:34		This section is extraordinary long with some circulation changes supported much more strongly than others but no relative ranking of how they rate in an overall context is presented. [Bryant McAvaney]	Noted
9-924	A	43:39	43:39	This is the third time this has been said in this chapter! Harmonize! [William Ingram]	Don't know what this refers to.
9-925	A	43:41	43:41	A more pertinent example than greenhouse forcing would be the gradients created by the contrast of sulfate aerosol forcing and GHG forcing. During the mid-20th century, these contrasts grew to be quite strong (GHG warming influence at high latitudes and sulfate aerosol cooling influence over North America, the North Atlantic and Europe)--there just had to be a regional response (just as there is for orbital element forcing). [Michael MacCracken]	Noted
9-926	A	43:42	43:42	The reference Risbey et al. (2002) is not in the references (unless it is Risbey and Kandlikar 2002). [Sandrine Bony]	Accepted, reference has been added.
9-927	A	43:49	43:57	This paragraph discusses projections so should appear in chapter 10. [Matthew Collins]	Taken into account, the first lines (49-52) are evidence of variability and change, and the rest of the paragraph is needed on the context of the discussions.
9-928	A	43:56	43:57	What about the NCAR studies of Meehl et al? [Kevin Trenberth]	Apart from an early study (1993) the studies we found appeared to deal with the issue of model formulation and ENSO. Which study is meant here?
9-929	A	43:57	43:57	modify sentence to end: have shown little or no change (e.g., Collins, 2000b) with greenhouse gas-induced changes obscured by pronounced internally generated multi-decadal variations in ENSO amplitude (Knutson et al. 1997). Reference to add: Knutson, T. R., S. Manabe, and D. Gu, 1997: Simulated ENSO in a global coupled ocean-atmosphere model: Multidecadal amplitude modulation and CO2 sensitivity. Journal of Climate, 10(1), 138-161. [Thomas Knutson]	Reference added.
9-930	A	44:0		Fig 9.5.2 How do you get 50-year trends from a 40-year reanalysis? [William Ingram]	Accepted, correction has been made in Fig. 9.5.2
9-931	A	44:1	44:6	But this needs to recognize that models do ENSO poorly. Also it relates to the measure of ENSO: is it SST anomalies, SLP, or precipitation. Some models show that even as SLP (Southern Oscillation) changes weaken, the precipitation changes are increased. [Kevin Trenberth]	This is what our results show – at least that models are inconsistent in terms of the simulated response to anthropogenic forcing. No reference is

Confidential, Do Not Cite or Quote

Chapter 9: Batch AB (11/16/05)

Page 118 of 186



No.	Batch	Page:line		Comment	Notes
		From	To		
					given to studies showing an increasing precip response to ENSO.
9-932	A	44:4	44:6	Is not the occurrence of more intense precipitation events (seen on most continents) likely an indication that some of the consequences of ENSO changes are occurring--if ENSO is the cause of the more intense types of rains, is not an increase in their intensity a suggestion that something is happening? [Michael MacCracken]	No reference given here.
9-933	A	44:8	44:18	This material is covered in Chapter 3, but it is not fully consistent here. [Kevin Trenberth]	We include a reference to chapter 3 here. The reviewer doesn't say what is inconsistent with chapter 3 – it appears generally consistent to us, though we have modified the text slightly.
9-934	A	44:17	44:17	...from tropical Pacific variability in El Nino (e.g., Knutson and Manabe 1998), and also.... Reference to add: Knutson, T. R., and S. Manabe, 1998: Model assessment of decadal variability and trends in the Tropical Pacific Ocean. Journal of Climate, 11(9), 2273-2296. [Thomas Knutson]	Reference not needed here.
9-935	A	44:20	44:27	If this is correct, then why isn't it dealt with in all of the section dealing with temperature changes (and so-called natural variability)? [Kevin Trenberth]	Noted
9-936	A	44:27	44:27	In what sense is the identified anthropogenic influence on the PDO? [Matthew Collins]	The positive phase – this has been added.
9-937	A	44:29		To understand the large decadal fluctuation of AO and NAO, the nature of AO should be clarified at first. A recent study on the AO has revealed that the AO can be excited by various kinds of external forces (H. L. Tanaka & M. Matsueda, J. Meteorol. Soc. Jpn., 83, 611-619 (2005)). Thus, there is a possibility that the solar magnetic activity affects the climate through interactions with AO (or NAO) (D. R. Palamara and E. A. Bryant (2004) "Geomagnetic activity forcing of the Northern Annular Mode via the stratosphere," Annales Geophysicae 22: 725–731) or NAO (F. Boberg and H. Lundstedt (2002) "Solar Wind Variations Related to Fluctuations of the North Atlantic Oscillation," Geophys. Res. Lett., VOL. 29, NO. 15, 1718, 10.1029/2002GL014903). [Kiminori Itoh]	There is a good discussion of the AO and NAO in section 3.6.4, which is referenced here.
9-938	A	44:30	44:40	While NAO variability as seen in recent decades is not the norm in model control runs, Cooper and Gordon (2002) note a period in the HadCM3 control run with increasing NAO and a number of associated aspects of ocean variability, very similar to observations since 1960. Thus it is plausible that the observed NAO variability is largely internal in	Cooper and Gordon (2002) conclude that the magnitude of the trend is not consistent with the observations, however. Since this study does not do a



No.	Batch	Page:line		Comment	Notes
		From	To		
				origin. Reference: Cooper, C. and C. Gordon, 2002: North Atlantic Ocean decadal variability in the Hadley Centre coupled model. J. Climate, 15, 45-72.  [Richard Wood]	quantitative comparison of the simulated and observed trends we choose not to cite it here.
9-939	A	44:42	44:42	I am not sure that "Most climate models simulated an increase in the NAM". For simulations of 20th Century climate, some may but the majority do not. Also it should be made clear what an "increase in the NAM" is. Is it a shift in the mean SLP and geopotential height, a change in variance, etc. [Matthew Collins]	We have clarified that this means an increase in the NAM index in the text, and there is a reference to Thompson and Wallace and Chapter 3 for definitions in the previous paragraph. We stand by the assertion that most models simulate an increase in the NAM index.
9-940	A	44:42	44:45	This statement could be made stronger were it followed by this sentence: 'While a few of the 14 IPCC AR4 coupled models analyzed by Miller et al. (2005, revised) exhibit no annular trend, none of the models exhibit a trend toward a lower NAM index and higher Arctic SLP.' [Ron Miller]	We are discussing the response to GHGs only here. Miller et al includes all the major forcings, and is cited later in the paragraph.
9-941	A	44:44		Most model studies that examine trends in, say, the NAM focus on trends in sea-level pressure over the Arctic. This is problematic, because pressures could fall dramatically over the Arctic due solely to a monsoonal-like response to rapid warming in that region (ie, due to ice melt), and not to true changes in the atmospheric annular mode (which changes SLP through mechanical not thermal forcing). The only way to check if a model really has a trend in the NAM is if the pressure falls are shown to be barotropic, and many of the cited studies do not do this. I think the text should clarify this caveat on the model studies. [David Thompson]	We now clarify that we are dealing with the NAM 'index', and mention that this is defined based on SLP. We accept that any monsoon-like response driven by SST melting would therefore project onto this index, but we are unaware of any studies exploring this mechanism.
9-942	A	44:50	44:53	Miller et al. (2005, revised) reach the same conclusion by explicitly calculating the change in the annular PC of SLP following a volcano. (In comparison, Stenchikov et al composite SLP following an eruption, and note a qualitative resemblance between the composite and the NAM pattern). The multi-model ensemble average is of the correct sign, but is smaller and statistically distinct from the observed annular anomaly. [Ron Miller]	Miller et al. is not citable.
9-943	A	44:51	44:51	The term "20C3M" seems a bit jargony to be used, seemingly without a nearby explanation of what is meant. [Michael MacCracken]	Accepted – explanation added.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-944	A	44:55	45:2	From the sound of this, it seems as if the analysis is looking for a linear trend in the NAM over the 20th century from anthropogenic forcing. But why should this be the type of effect to be looking for, particularly in the NH where the human influences were positive, then negative, and then strongly positive again (and there were also regional gradient effects resulting from the counterbalancing forcings over the globe as a whole). So, it does seem that the wrong type of response is being looked for, especially as the NAM is likely to be most influenced right at the nexus of where solar and GHG forcing are causing the sharpest changes in forcing and gradients of forcing. [Michael MacCracken]	We can only review the available literature here. Gillett et al. (2003) looked at decadal means of SLP in four models and still found the observed and simulated trends to be inconsistent.
9-945	A	45:4		I strongly recommend Panels a) and b) in Figure 9.5.2 not be shown. The NCEP/NCAR and ERA 40 Reanalyses are not the correct tool for assessing SLP trends, particularly back to 1955 in the SH. Relying on such trends could invite substantial criticism ... [David Thompson]	We also show the station-based record from HadSLP2.0 for comparison. A caveat has been added to the figure caption regarding the reanalysis trends in the high latitudes of the Southern Hemisphere.
9-946	A	45:6		It should be noted that the observed anomalies of SST and stratospheric wind may result from greenhouse forcing. [Ron Miller]	Accepted – text changed to note that SST changes may be externally forced.
9-947	A	45:6		Suggest delete first sentence. The subsequent material discusses the mechanisms. [Kevin Trenberth]	We think the sentence is helpful in introducing the discussion of mechanisms.
9-948	A	45:13		Schwing et al. (2003) have identified a teleconnection of decadal climate variability between the north Atlantic and north Pacific, although this coupling appears to have multiple spatial modes over time. Full citation- Schwing, F.B., J. Jiang, and R. Mendelsohn. 2003. Coherency of regime shifts between the NAO, NPI, and PDO. Geophys. Res. Lett. 30: 1406, doi:10.1029/2002GL016535. [Franklin Schwing]	Not relevant to discussion of trends.
9-949	A	45:15	45:19	I am not convinced about this much-quoted statement (here I see actually watered down compared to the usual exclaimed direct causal relationship) that some percentage of the winter surface warming in Eurasia is due to the increase of the NAM/NAO. How is it possible, using purely analysis of observations (the cited papers), to separate the influence of a trend in that NAM and a contemporaneous trend in greenhouse gases? It seems at odds with the previously discussed (regional) D&A results in which none of the models capture the circulation trends but do simulate the warming trend (see Figure 9.4.7 noting the caveat that this is for annual averages). Does the surface air from the Atlantic really make it all the way to Siberia without being influenced by surface fluxes? It would seem unlikely to me.	These papers separate the influence of the NAM by examining regression patterns based on monthly detrended anomalies. We think these conclusions are clearly demonstrated in the cited reference, and stand by them.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Matthew Collins]	
9-950	A	45:15	45:19	Please refer to Chapter 3, Section 3.6.4 for more complete discussion on NAO/NAM. [Kevin Trenberth]	Accepted. Reference to 3.6.4 added.
9-951	A	45:20	45:20	Should say briefly, even if Box 3.4 says it in detail, that we can be almost certain physically that the maximum possible hurricane intensity will increase - but few even approach this maximum [William Ingram]	We think the text as it stands already reflects the balance of evidence on this.
9-952	A	45:22	45:49	Please see Section 3.6.5 for SAM observations discussion. The NCEP and ERA-40 reanalyses greatly overestimate changes in SAM (see also Trenberth et al 2005, J Climate 2812-2825.) [Kevin Trenberth]	Accepted,, there is a reference to Section 3.6.5. We refer to Marshall (2003) for observations of the SAM trend, and have now added a caveat on the reanalysis trends to the caption of figure 9.5.2.
9-953	A	45:28	45:32	Again, it reads as if the authors are expecting to see a linear response to the GHG increase--but there are multiple forcings of different strengths, and so one should clearly be looking for a more complex human influence--so to some extent up, then down, then up--and since the ocean-atmosphere system is all interconnected, one has to be looking at the pattern of forcing, gradients, etc. I would think the IPCC authors should be pretty cautious about jumping to conclusions that the types of changes we are seeing are natural or anthropogenic, given the very limited analyses that seem to have been done. While there is almost surely a natural influence, there is also quite likely some sort of human influence as well--and we need to work to separate them out. [Michael MacCracken]	The studies we cite compare observed changes with climate model simulations which should take into account the effects the reviewer describes.
9-954	A	45:35		Partly induced? [Kevin Trenberth]	We now say 'largely induced'.
9-955	A	45:37	45:37	Sexton et al used an incorrect ozone forcing field that produced too large high latitude forcing. So the published result may be too large a trend in this diagnostic. [Peter Thome]	We accept the reviewer's point, but we are only discussing the sign of the response here, not its magnitude.
9-956	A	45:38		I believe Gillett/Thompson showed the simulated trends are largest in summer. [David Thompson]	Several of the cited studies show that the changes are largest in summer - we don't think it is necessary to repeat the references at the end of the sentence.
9-957	A	45:42	45:45	Perhaps append this sentence with: 'The effect of greenhouse gases is seen in the upward SAM trends projected by IPCC AR4 models during early winter (May-July), when ozone forcing is absent (Figure 12 from Miller et al. 2005, revised).' [Ron Miller]	We prefer to discuss studies with GHG changes only.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-958	A	45:43	45:43	"destructiveness" misleading in that it sounds relevant to land whereas it's a mostly ocean-based measure with little relation to destruction, as I understand Emanuel & Pielke will be agreeing in print soon [William Ingram]	Accepted. 'Destructiveness' replaced with 'intensity'.
9-959	A	45:46	45:49	Please see Section 3.6.5 and associated figures for changes with SAM. [Kevin Trenberth]	Accepted. Reference to 3.6.5 added.
9-960	A	46:0		Section 9.5.2.6 needs to discuss problems with climate models (related to subgrid scale parameterization of convection which onsets prematurely and with insufficient intensity in all climate models so that it prejudices the environment against organized convection, such as hurricanes) and simulations of tropical storms. See Trenberth (2005 Science) for an assessment of this issue. Also numbers of storms are a red herring unless intensity is tied to the numbers. The storms leave behind a cold wake and thus a strong storm can inhibit the next one from developing. There is no expectation of increase in numbers. There is expectation that intensity will increase and that total activity will increase. [Kevin Trenberth]	Proviso regarding spatial resolution added, and a reference to 8.5.3.
9-961	A	46:6	46:6	fig 9.5.2 already appearing p45, line 4 [Bernard Seguin]	Deleted.
9-962	A	46:10	46:13	First, what is it about the Asian monsoon that decreases by 15%? Is it the amount of air being moved? Chapter 10 (page 3, line 35) says that monsoon precipitation (which is really what matters) generally increases, but with decreases in some regions. This needs to be further clarified. [Michael MacCracken]	This is based on the velocity streamfunction, therefore we think 'circulation' is a good description. It is clear that we are not talking about precipitation here.
9-963	A	46:16	46:21	There needs to be some physical explanation here. The traditional view is that warming over land leads to the monsoon, and what occurred through the 20th century (and is projected for the future) is a warming of the land relative to the ocean. So, what is then the physical mechanism that leads to a diminished monsoon as this seems quite counterintuitive? [Michael MacCracken]	A physical explanation for this weakening is not readily available in the literature.
9-964	A	46:22	46:22	"precipitable" is a totally confusing word - it sounds as if it means condensate! Just say "column-integrated" as that's what's meant [William Ingram]	Rejected. We explained what it means.
9-965	A	46:23	47:6	I read this discussion with great interest, as it had been written prior to the 2005 hurricane season. I believe that text is well balanced. [Christoph Schar]	Thank you.
9-966	A	46:24	46:24	hurricane seasons of 2004 and 2005, [Thomas Knutson]	Accepted



No.	Batch	Page:line		Comment	Notes
		From	To		
9-967	A	46:24	46:26	I suggest to replace 'The active North Atlantic hurricane season of 2004,... Simmonds, 2005),' by 'The active North Atlantic hurricane seasons of 2004 and 2005,...Simmonds, 2005), and close to Iberian Peninsula, in Madeira surroundings, in October 2005.' <p>[Javier Martin-Vide]</p>	Change accepted.
9-968	A	46:24	46:24	The Brazilian cyclone was unprecedented in the South Atlantic during the instrumental/observational period (see Ch.3, p. 72, l. 31); thus wording "unusual" should be replaced. <p>[Axel Michaelowa]</p>	Taken into account. There is no historical data on statistics of this phenomenon in the region., and it is hard to say that is "unprecedented", there may have been others in the past. The word "unusual" seems to be the best choice.
9-969	A	46:24	46:43	The results of projection of climate change should be described in Chapter 10, and the interpretation and understanding the mechanism of the projected climate change should be described here based on the conclusion of the projection in Chapter 10. Recent model results seem to have reached some consensus: reduction of total number of tropical cyclones and intensification of tropical cyclones (increasing the number of intense tropical cyclones) in the future warmer climate. <p>[Masato Sugi]</p>	Accepted – suggested change made.
9-970	A	46:24	46:23	The reduction of total number of tropical cyclones and intensification of tropical cyclones seems somewhat contradictory. A possible explanation of these results may be as follows. The reduction of the total number of tropical cyclones may be explained by an increase in dry static stability of the atmosphere and a relatively little increase in precipitation (Sugi et al. 2002). A large increase in dry static stability and a relatively little increase in precipitation (convective heating) lead to a weakening of tropical circulation and a reduction of total generation of kinetic energy for tropical cyclones. With the same amount of condensation heating, a tropical cyclone should be weaker in the warmer climate where the dry static stability is larger. However, further intensification of tropical cyclone is possible if more water vapor is available, even with increased dry static stability of the atmosphere. A few intense tropical cyclones can develop instead of many relatively weak tropical cyclones with the same amount of kinetic energy generation. <p>[Masato Sugi]</p>	Accepted – suggested change made.
9-971	A	46:24	46:23	Yoshimura and Sugi (2005) investigated the impact on tropical cyclone frequency of the SST increase and CO2 increase separately. They found that the CO2 increase leads to a reduction of precipitation and have a large impact on tropical cyclone frequency, while the SST increase have little impacts, because when the SST is increased precipitation increases but also dry static stability of the atmosphere increases.	Accepted – suggested change made.



No.	Batch	Page:line		Comment	Notes
		From	To		
				Sugi and Yoshimura (2004) have pointed out that the reason for the reduction of precipitation associated with the CO <sub>2</sub> increase is a reduction of radiative cooling due to overlap effect of CO <sub>2</sub> and water vapor absorption bands, suggesting that the overlap effect of CO <sub>2</sub> and water vapor absorption bands is the basic reason for the reduction of tropical cyclone frequency in a warmer climate.  [Masato Sugi]	
9-972	A	46:24	46:24	add "...seasons of 2004 and 2005...". [Kevin Walsh]	Accepted
9-973	A	46:24		Delete "unusual" [Vincent Gray] What about atypical?/	Noted, we prefer to keep "unusual"
9-974	A	46:27	46:27	Bit too strong (& inconsistent with Ch 8) as the climate change signal is small in 2 decades [William Ingram]	Wrong lines and page.
9-975	A	46:27	46:27	That the TAR used the phrase "no compelling evidence" is really unfortunate--it should have been using the IPCC lexicon rather than seeming to require some sort of 2-standard deviation test to say that the null hypothesis was not being met--or whatever. Hopefully, this assessment round will not get hung up in absolutely strict statistical testing--and will give indications of the relative likelihood of changes. Further, the tests that were done were looking generally for a linear change in hurricane number, and this is not what should have been looked for given the combinations of forcings at work and their spatial gradients in the NH that may well have affected the NAO. So, I would suggest that ARS should not really be so much emphasizing what was done in that they were looking for the wrong indication of a human influence. [Michael MacCracken]	Dont' accept this. We are expected to build on what was reported in the TAR.
9-976	A	46:31	46:33	Suggest this modification: Knutson and Tuleya (2004) suggest that maximum surface wind speeds may increase by about 6% over 80-years, assuming a 1%/yr compounded increase in CO <sub>2</sub> levels, with larger percentage increases in near-storm rainfall. Michaels et al. (2005) argue that the future rate of change will be smaller than Knutson and Tuleya's (2004) result, since the future radiative forcing is likely to be less than the +1%/yr CO <sub>2</sub> scenario. Knutson et al. (2001) find that such tropical cyclone intensification is robust to the inclusion of ocean coupling beneath the simulated hurricanes, while Knutson and Tuleya (2004) demonstrate that their simulation results are robust across a range of choices....(and leave lines 34-35 as is). [Thomas Knutson]	Discussion of simulated future trends deferred to chapter 10.
9-977	A	46:31	46:31	This sentence does not make clear which aspects of the preceding sentence the "this	Deleted.



No.	Batch	Page:line		Comment	Notes
		From	To		
				conclusion" applies to. Indeed, it really only applies to the projection of increased windfall and rain, but that is not the main thrust of the preceding sentence, so clarification is needed. [Michael MacCracken]	
9-978	A	46:32	46:32	I understand that Knutson now has a response to Michaels et al., so hopefully that will also be referred to here. [Michael MacCracken]	Now deferred to chapter 10.
9-979	A	46:35	46:35	"Global terrestrial annual mean" obscure - I eventually guessed "mean over all the land in the world" was meant, but I first read it as meaning "the distribution over the earthly globe". While "terrestrial" means "land when in clear opposition to "marine", otherwise it sounds like a specification that Planet Earth is meant. Drop complicating "annual" (saying "mm/year/decade" in next line) & use clear short "global land mean". [William Ingram]	Accepted.
9-980	A	46:35	46:35	Replace "However" with this: Oouchi et al. (2005), using a very high resolution (20 km grid) global atmospheric model with specified SST warming and increased CO2 levels, report a substantial decrease in the overall frequency of tropical cyclones in all basins except the North Atlantic, which showed a substantial increase. They also report a global increase in the number of intense tropical cyclones and a global increase in the intensities of the strongest storms in their simulations, although such increases were not found in all basins. [Thomas Knutson]	Deferred to chapter 10.
9-981	A	46:35	46:37	This sentence really needs to give a physical reason to explain why one might expect a diminution in number, especially given the trends shown in Webster et al of a tendency for an increase in the intense tropical cyclones. Given that we know not all initial disturbances develop into tropical cyclones and at the same time overall convective precipitation is going up, it is a bit hard to understand how some of those disturbances that did not quite make it to cyclone status will not cross over that barrier. The sentence needs to make clear whether this study had prescribed or interactive SST, whether the layer of warmed water was allowed to have deepened, etc. as there are many factors that likely affect tropical cyclone development, and we need to know what was free to change and what was held fixed in this high resolution run (it would, for example, be nice to know if running this model with prescribed SST for past years gave a good indication of the year to year variations in frequency, etc.). [Michael MacCracken]	Explanation now given.
9-982	A	46:35	46:39	Time slice experiments (especailly application in tropics) has recently been called into question by Douville (Climate Dynamics 24,373-391,2005. Hence speculation on cyclone changes from such relatively low resolution time slice experiments is questionable.	Discussion of projected trends deferred to chapter 10.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Bryant McAvaney]	
9-983	A	46:36	46:36	T106 (approximately 1 degree grid spacing) atmospheric model ... frequency may decrease *substantially* in response to future greenhouse gas forcing, although with an increase in the North Atlantic. McDonald et al. (2005) using a similar approach with another ~1 degree grid spacing atmospheric model, find a smaller global change of frequency in response greenhouse warming, with a large variation in response among different basins. [Thomas Knutson]	Deferred to chapter 10.
9-984	A	46:41	46:41	fine resolution *regional* model study by Walsh et al. [Thomas Knutson]	Now deferred to chapter 10.
9-985	A	46:45	46:47	Why use only 5 of 9 models - or did they use all, but 4 failed to show detectability? And were the 2 dodgy ones amongst the 5 or the 4? [William Ingram]	Wrong page and line numbers.
9-986	A	46:45	46:46	Again, these analyses were generally looking for a long-term linear trend rather than a variation characteristic of the human influence on, for example, NH surface temperature, and the analyses generally considered all of the variations off of the linear trend (or baseline) to be natural in origin, when in fact some of the variation was similar in a sense to the multidecadal swings in NH temperature over this period that are recognized as due to human activities. In addition, all variations in the NAO, for example, one of the drivers of Atlantic hurricane frequency, is presumed to be all of natural origin when this chapter makes clear that some of it could well be due to human influences. In my view, the IPCC should be much more cautious than it is here (or was in the TAR) about human influences on tropical cyclones--we still have a lot of analysis to do. [Michael MacCracken]	Don't accept. I think we are sufficiently cautious about human influences on cyclones.
9-987	A	46:45	46:46	The Pielke et al (2005) paper has been completely rewritten and in any case is still wrong. [Kevin Trenberth]	Noted
9-988	A	46:50	46:52	This is very narrow selection --- Trenberth had no data, but Groisman et al in several papers in the early 2000s have tested this with data and find no response -- these should be cited [Thomas Karl]	Accepted -- Reference to increases in hurricane rainfall removed.
9-989	A	46:50	46:50	Gettleman et al. (2002) find substantial increases in CAPE (Convective Available Potential Energy) at a number of tropical radiosonde locations, with mostly positive trends among the stations analyzed. DeMott and Randall (2004) report a more mixed pattern of increases and decreases of CAPE in the tropics using a larger sample of stations. (Note: There is additional discussion of this issue in Section 3.8.3.1 (3-69). References are also available in Chapt. 3.)	This discussion focuses on potential intensity, therefore we have not included the suggested references here. However, we now cite 3.8.3.1 where these are discussed.

Confidential, Do Not Cite or Quote

Chapter 9: Batch AB (11/16/05)

Page 127 of 186



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Thomas Knutson]	
9-990	A	46:55	46:55	"terrestrial" again confusing: I assume "land-only" is meant, & so should be said [William Ingram]	Done.
9-991	A	46:55	46:56	suggested modified wording: ... including oscillations such as ENSO, the NAO, and the Atlantic Multi-decadal Oscillation (AMO -- Section 3.6.6.1), as well as the strong tropical mean warming since the mid-1970s... [Thomas Knutson]	Accepted
9-992	A	47:0		Again, "terrestrial mean" sounds like a mean over Planet Earth, but I think "global land-only mean" is meant [William Ingram]	Accepted.
9-993	A	47:0		Section 9.5.3.2. There are major conceptual problems in dealing with precipitation. I suggest that Section 9.5.3 should be redone based on expectations of why precipitation should change? See Trenberth et al (2003). With increased GHGs there is a small acceleration of E and P and thus the hydrological cycle, but that can easily vanish with aerosols in the scenario. Good heavens look at the vertical scale of Fig 9.5.4! But robust changes in precipitation intensity and frequency should be sought and are strongly evident in the observational record (Chapter 3, sec 3.8.2.2), in contrast to what is stated here, and also are found in models where they are properly analyzed. This is robust and it is quite misleading to focus on precipitation amount where there is no clear expectation. This problem carries over into Question 9.1. [Kevin Trenberth]	Discussion of expectations added. Q9.1 extensively redrafted.
9-994	A	47:1	47:6	I disagree with this assessment. The expectations (Trenberth 2005 Science) and findings (Emanuel, Webster et al) along with model expectations (in spite of their likely underestimates) (Knutson and Tuleya) surely make a strong case for more intense storms but little change in numbers. This problem carries over to Question 9.1. [Kevin Trenberth]	Noted. No detection and attribution studies of hurricanes exist in the literature.
9-995	A	47:2	47:6	Again, most of the analyses have been looking for a linear trend over the century, and there is really no basis for expecting that this should be the signal. With NH temperature going up, down, up due to human activities, with the NAO possibly being affected by human activities, and so on, there is likely to be a rather non-monotonic and even nonlinear response, and the analyses to date seem only very limited and preliminary. IPCC should be more cautious in drawing conclusions about this, especially given the types of new results coming from Emanuel and from Webster et al. [Michael MacCracken]	Noted. There are no detection and attribution studies in the literature. We believe we are sufficiently cautious here.
9-996	A	47:6	47:6	Add TAR (Box 10.2) as another reference [Thomas Knutson]	Rejected. We are expected to assess what is new since the TAR, and cannot reference every relevant section of the



No.	Batch	Page:line		Comment	Notes
		From	To		
					previous assessment.
9-997	A	47:8		On the trajectory of extra-tropic cyclones. Tinsley pointed out that solar magnetic activity could affect the trajectory of extra-tropic cyclones through the global electric circuit (B. A. Tinsley, Influence of Solar Wind on the Global Electric Circuit, and Inferred Effects on Cloud Microphysics, Temperature, and Dynamics in the Troposphere, Space Science Reviews 94, 231-258 (2000).)?Although this mechanism may not be welcome by the climate community, it cannot be neglected. [Kiminori Itoh]	Noted
9-998	A	47:9	47:22	The discussion p 9-48 lines 52-55 is relevant here. I know there has been a lot of new work on analysis of models, (e.g. by Yin at NCAR, Soden and Held at GFDL) and I hope their studies can be referenced. [Kevin Trenberth]	Yin results now reported. Soden and Held is not citable, since it has only just been submitted.
9-999	A	47:9		There needs to be some discussion also of energy transport by these eddies. [Bryant McAvaney]	We are not aware of any comparison of simulated and observed changes in 20 <sup>th</sup> century eddy energy transports in the literature.
9-1000	A	47:10	47:17	Reads as if this explanation were new: it is in Allen & Ingram (2002), & I think Mitchell & al (1987), if not a Manabe paper earlier still [William Ingram]	We don't understand this comment. The text does discuss the history of this explanation.
9-1001	A	47:20	47:22	"However, the reanalyses...detected.": It could be emphasized that although the reanalyses might have problems (and even if they have, it is not yet demonstrated that changes in observing systems actually affect the trends in extra-tropical cyclones derived from the reanalyses), the trends in extra-tropical cyclones derived from observations and coupled models are qualitatively consistent. [Sandrine Bony]	Accepted – text revised accordingly.
9-1002	A	47:20	47:22	This seems a one-sided conclusion. While we have not detected it, we also have not at all ruled it out. [Michael MacCracken]	Text revised.
9-1003	A	47:24		Chapter 9, p47, line 24 Section 9.5.3 Precipitation Precipitation is a difficult parameter for any climate model simulation. This is especially true for high impact events where the precipitation rate is large. Weather forecasting models do a poor job in this area (e.g Ebert et al., 2003) and it would be difficult to believe that climate models can do better with their coarser resolution. In terms of energy, one mm/day translates to a latent heat release of 25 w/m2 (see Hallett and Isaac, 2001). Precipitation amounts go from less than one mm/day near the poles to over 8 mm/day in the tropics. So the latent heat associated with precipitation is quite large and a	Not relevant for this chapter. An issue for chapter 8.



No.	Batch	Page:line		Comment	Notes
		From	To		
				<p>small error in the prediction of precipitation might lead to significant errors in climate simulations.</p> <p>Ebert, E., U. Damrath, W. Wergen and M.E. Baldwin, 2003: The WGNE assessment of short-term quantitative precipitation forecasts. Bull. American Met. Soc., 481-492.</p> <p>Hallett, J., and G.A. Isaac, 2001: Perspectives in cloud physics. Bull. A.M.S., 82, 2259-2263.</p> <p>FOR THE AUTHORS ATTENTION – SECOND PRIORITY FOR INCLUSION</p> <p>Precipitation and surface temperature are highly correlated in latitudes above 45N (Isaac and Stuart, 1992, 1996) with more precipitation occurring with warmer temperatures during most seasons. During the summer, however, often the relationship reverses with less precipitation occurring when the surface temperature is warmer. Climate models (e.g. Stuart and Isaac, 1994) can have a hard time simulating this relationship, especially the trend towards less precipitation when the temperatures are the warmest in the summer. More effort should be placed on evaluating climate models and their ability to correctly handle this precipitation temperature relationship.</p> <p>Isaac, G.A. and R.A. Stuart, 1992: Temperature-precipitation relationships for Canadian stations. J. Climate, 5, 822-830.</p> <p>Isaac, G.A., and R.A. Stuart, 1996: Relationships between cloud type and amount, precipitation and surface temperature in the Mackenzie River valley - Beaufort Sea area. J. of Climate, 9, 1921-1941.</p> <p>Stuart, R.A. and G.A. Isaac, 1994: A comparison of temperature-precipitation relationships from observations and as modelled by the General Circulation Model of the Canadian Climate Centre. J. Climate, 7, 277-282.</p> <p>[George Isaac]</p>	
9-1004	A	47:27	47:27	<p>Please remove the citation (Trenberth et al. 2005) in this sentence; which is not necessary: the suggestion that atmospheric moisture increases with temperature is much older than that (end of the 19th century).</p> <p>[Sandrine Bony]</p>	Rejected – I think it is still useful to have a recent, easily available reference to this point, even if Trenberth was not the first to say it.
9-1005	A	47:29	47:30	<p>The observational records are so uncertain that this statement cannot be proven true. I suggest that it is deleted. As much as anything else it relates to entirely raw radiosonde data and there are very obvious discontinuities at the station scale.</p> <p>[Peter Thome]</p>	Deleted. Chapter 3 has also changed its discussion of the radiosonde record to indicate more uncertainty.
9-1006	A	47:32	47:32	<p>I don't believe "droughtiness" exists: "drought" will do</p> <p>[William Ingram]</p>	Accepted.
9-1007	A	47:37	47:37	<p>"... indicating that the water vapour feedback is realistically simulated" is too strong of finding from the Soden et al paper. In any case, water vapour feedback is discussed at</p>	Agreed.



No.	Batch	Page:line		Comment	Notes
		From	To		
				length in section 8.6, and should not be discussed further here. I suggest the comment on water vapour feedback here be dropped. [Robert Colman]	
9-1440	B	47:41	47:48	Section 9.5.3.2 . This rise observed is not justified in West Africa, because the 20th century is characterized by a tendency persistence in the fall of decadal and monthly rainfall (Tapsoba, 1997; Vissin and al, 2003; Houssou and al, 2004 et Houndénou and al, 2005). [Expédit Wilfrid VISSIN]	This statement refers to global precip, not regional.
9-1008	A	47:41	48:50	There is a fundamental issue which the authors may have overlooked here. As shown by Hulme et al (2000), the global average land precip showed an overall increase from 1900 to 1950( consistent with the surface warming) but went on a declining trend since the 1950s and still have not recovered to the peak values in 1950. This trend is clearly inconsistent with the GHGs signal. As pointed out by Ramanathan et al (Science, Vol 294, P.2119, 2001), the large reduction in surface solar radiation by absorbing aerosols may help explain this negative trend. Some where here in this chapter, it should be mentioned that there is now a large body of evidence from field observations (INDOEX, Ace-Asia) that aerosols have led to a large reduction of solar radiation at the surface (typically factor of 4 to 10 larger than the TOA aerosol forcing) and its effect on evaporation and precipitation may help explain several regional anomalies in trends and models have not yet captured these new findings (see Ramanathan et al, 2001). [Veerabhadran Ramanathan]	Revised to mention the effects of aerosols. Ramanathan et al. (2001) is also now cited in 9.5.3.2.1. Note that the effects of aerosols on Indian monsoon rainfall are already discussed in 9.5.3.3.3, where Ramanathan et al. (2005) is cited.
9-1009	A	47:42	47:43	Why should there be a causality between the increased atmospheric moisture content and the increased global mean precipitation? As explained page 48 (lines 15-16), global precipitation is controlled by the global radiative cooling and surface fluxes. [Sandrine Bony]	This sentence only says 'might be expected', and forms a link with the previous section. The following sentence has been revised to make it clear that global mean precipitation is controlled by the tropospheric energy budget.
9-1010	A	47:42	47:45	The first two sentences in this section are likely to cause confusion. The first sentence is wrong in that it confuses the total amount of water in the atmosphere, with its rate of change. The second sentence confusingly leaps from global changes to patterns of change. Furthermore, the wording of sentence two implies that sentence one does provide a pointer to changes in precipitation. I suggest these to be replaced with a clear statement that for the MEAN precipitation, it is the availability of energy, rather than precipitable water which controls changes in precipitation (referencing Allen and Ingram, 2002) [Robert Colman]	The first sentence says 'might be expected', and the second argues why this is not the case. The second sentence has been revised to focus on the global mean. A reference to Allen and Ingram has been added.
9-1011	A	47:43	47:56	The 3 sentences on 52-56 logically belong earlier: insert in 43.	I can't tell which sentences are referred



No.	Batch	Page:line		Comment	Notes
		From	To		
				[William Ingram]	to here.
9-1012	A	47:45	47:48	Here is another example of seeming to search for a linear variation (since 1940 in this case) when there is good reason to suspect that the human-induced signal will not be monotonic or even linear--what is said to be "interdecadal variability" could indeed be a variation consistent with the human-induced changes in large-scale surface temperature. [Michael MacCracken]	Although we do briefly discuss linear trends here, all the detection studies cited in 9.5.3.2.1 include temporal variations in precipitation – they search for nonlinear variations in precip. These temporal variations are also shown in Figure 9.5.4.
9-1013	A	47:49		Due to complexity in the spatio-temporal patterns and their changes, one approach has been to build trend fitting methods using a flexible probability model. Thus one may assume that the data have been generated by a stochastic process that allows for arbitrary changes in the underlying probability distribution function over time (Ghosh et al. 1997) and space. Assessing changes in the probability distribution function is equivalent to assessing changes in quantiles, estimation of these functions, in particular extreme quantile functions become relevant. These allow one also to assess extreme events such as heavy precipitation events or droughts. Also, changes in the quantiles can hint at changes in the variability (e.g. via inter-quartile range). For references, see Ghosh & Draghicescu (2002a, b). [Sucharita Ghosh]	Noted.
9-1014	A	47:53	47:54	Delete from "thereby" in line 53 to "precipitation" in line 54. It does not follow. [Vincent Gray]	'significant' in this context means inconsistent with internal variability, and therefore the second part of this sentence does follow from the first.
9-1015	A	47:53	47:54	The final phrase of the sentence really is not needed--and is a bit confusing. [Michael MacCracken]	The detection of external influence on precipitation was a key conclusion of all three studies cited.
9-1016	A	47:54	47:54	Reword clumsy sentence which contains both "simulated" and "simulations". [Robert Colman]	Agreed
9-1017	A	48:6		Fig 9.5.2. This figure contains errors as the NCEP and ERA reanalyses overestimate observed changes, see Section 3.6.5 and Trenberth et al 2005. [Kevin Trenberth]	See 9-930 and 9-945.
9-1018	A	48:6		How can this be known when global precipitation estimates are not good enough to say this? [Kevin Trenberth]	The reference to observations has been removed here.
9-1019	A	48:7	48:11	It would seem it should be mentioned that there are, I believe, great differences in the variance of terrestrial precipitation with model resolution. With the typical resolution of climate models, storms are quite spread out, and so one would not expect to get the peak	This sentence refers to the variance of global mean precipitation. Whether or not resolution affects the variance of

Confidential, Do Not Cite or Quote

Chapter 9: Batch AB (11/16/05)

Page 132 of 186



No.	Batch	Page:line		Comment	Notes
		From	To		
				rainfalls, whereas with models with high resolution, there is the chance of getting much higher rainfall rates. Despite this, resolution is not one of the mentioned shortcomings in this sentence--quite strange. [Michael MacCracken]	global mean precip has not been shown to my knowledge. The sentence says that it is possible that climate models underestimate internal variability.
9-1020	A	48:9	48:21	Changes in the Asian monsoon are critically dependent on aerosol assumptions, and how they are introduced into models (are they interactive, and is the aerosol washed out by rains?) The material, lines 14-19, is wrong. The paper by Chase et al (2003) is wrong and this material is discussed more fully in Section 3.7.1: please see especially page 3-60, lines 10-12. [Kevin Trenberth]	Noted. References to chapter 3 added.
9-1021	A	48:15	48:29	There is a misunderstanding in the sentence "this may be partly offset by a decrease in the efficiency of cooling due to greenhouse gas increase". The radiative cooling in the atmosphere generally increase when greenhouse gas is increased. As an exceptional case, the cooling decreases when CO2 is increased, because of the "overlap effect" of CO2 and water vapor absorption bands, as pointed out by Sugi and Yoshimura (2004). They further pointed out that the CO2 and other greenhouse gas are quite different in this regard. This difference must be carefully considered in the detection of precipitation change due to greenhouse gas increase. [Masato Sugi]	Accepted. Greenhouse gases changed to CO <sub>2</sub> .
9-1441	B	48:15	48:29	Section 9.5.3.2.1. The role of the oceanic temperature of surface in the rechauffement current one is not shown everywhere. It will thus have to be moderated. [Expédit Wilfrid VISSIN]	Don't understand the relevance of this comment.
9-1022	A	48:15		Please see Trenberth et al (2003). It is not the energy budget of the troposphere but the energy budget of the surface, where evaporation occurs, that is critical. Granted that latent heat provides a way to transport heat upwards where it can be radiated to space, but it is the evaporation side that is the main driver. [Kevin Trenberth]	We disagree with this assessment.
9-1023	A	48:21	48:21	I would really favor changing "CO2" to "CO2 concentration"--the shorter phrase is really an unfortunate convention. [Michael MacCracken]	I don't think the way this is used here could lead to any misunderstanding.
9-1024	A	48:26		Global mean precipitation is not well known, see Section 3.3; nor is it clear that there ought to be signal in global mean precipitation owing to aerosol effects. [Kevin Trenberth]	'global mean precipitation' replaced by 'global land mean precipitation'. A comment about aerosols has also been added to the end of the paragraph.
9-1025	A	48:27	48:27	The fact that the "coupling strength" between land and atmosphere varies so strongly between models (Koster et al 2005) makes even more problematic the issue of moisture availability.	This sentence deals with the energy budget argument, rather than with moisture availability. The cited



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Bryant McAvaney]	reference does not seem relevant here.
9-1026	A	48:29	48:29	Trends should not be used without warning [William Ingram]	Revised.
9-1027	A	48:31	48:33	Have not the changes in vegetation and in stream channeling for flood control also likely had some influences? [Michael MacCracken]	They have presumably had some influence, but this paper concentrates on the climatic and stomatal resistance effects.
9-1028	A	48:31	48:33	Our studies at NCAR – not yet published- are at odds with this, see Qian et al (2005 submitted, in Chapter 3 references). Such studies need to deal not just with changes in precipitation amount but also changes in intensity (which increases runoff). We can indeed explain changes in runoff and evaporation (and pan evaporation) without any consideration of stomatal effects. I urge major caution on this finding. [Kevin Trenberth]	Qian et al. and Milly et al. are now cited, and we mention that observed runoff trends can also be simulated in response to observed climate, or climate forcing alone.
9-1029	A	48:36	48:37	Again, trends should not be used without warning - but particularly in this case! [William Ingram]	We simply describe what is shown in the figure from ch 3 that is cited. It would be inappropriate to call out the limitations of trend calculations each time we refer to a trend, and anticipate that the appropriate caveats will be discussed in Ch 3.
9-1030	A	48:45		Section 9.5.3.2.2: a large part of this section deals with future changes. This overlaps with Ch 10. [Fons Baede]	References to simulations of future changes replaced with a reference to chapter 10.
9-1031	A	48:49	48:51	Is this also not the case during monsoon conditions? [Michael MacCracken]	Don't understand relevance.
9-1032	A	48:54	48:54	"demonstrated" should be "confirmed" - this was reported about 20 years ago by Folland & colleagues! Of course I accept the value of new studies with better GCMs & more data, but these should not be presented as if they were fundamentally new [William Ingram]	This is 50:7. Agreed. Early work by Folland and Rowell cited.
9-1033	A	49:0		Fig 9.5.5 caption: should say explicitly what the red & grey are [William Ingram]	Caption revised.
9-1034	A	49:1	49:15	Mostly on future projection. Should be moved to or coordinated with Ch. 10. Actually, the discussion of faster increase of precipitation extremes than the mean in a warmed climate is summarized very well here in Ch. 9, while it is almost lacking in Ch. 10, where it should be. [Seita Emori]	Several references have been replaced with a reference to chapter 10. The discussion of mechanisms is also relevant to the 20 <sup>th</sup> century, and belongs in our chapter.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1035	A	49:1		Replace "will likely" with "may" Replace "will likely" with "may" [Vincent Gray]	Not needed, since we already have "suggest" in this sentence. Hardly a strong statement.
9-1036	A	49:3	49:3	I think it would be important to add a sentence before 'Simulated...' such as 'Like a counterpoint in some subtropical areas, as Mediterranean countries, a few days account for much of the annual total, so a small variation in the frequency of the rainiest days produces a big effect on annual amount (Martin-Vide, 2004). Reference: Martin-Vide, J., 2004: Spatial distribution of daily precipitation concentration index in Peninsular Spain. International Journal of Climatology, 24, 959-971. [Javier Martin-Vide]	This regional study is not relevant here.
9-1037	A	49:5	49:8	This sentence sums up our theoretical understanding of what controls extreme compared to mean precipitation. This is a critically important point, which is misplaced. It needs to be made at the beginning of the section, before any discussion of what models or observations say. [Robert Colman]	Moved to the beginning of the section, as suggested.
9-1038	A	49:5		This suggestion comes from Trenberth, K. E., 1998: Atmospheric moisture residence times and cycling: Implications for rainfall rates with climate change. Climatic Change, 39, 667-694. Trenberth, K. E., 1999: Conceptual framework for changes of extremes of the hydrological cycle with climate change. Climatic Change, 42, 327-339. And see also Trenberth et al (2003). [Kevin Trenberth]	We do not need to cite the first reference to every statement (otherwise we would have to drop other Trenberth references elsewhere).
9-1039	A	49:6	49:6	What does "its" refer to? It is not clear. [Michael MacCracken]	Troposphere but agree clarification needed. 'the atmospheric' inserted before 'moisture content'.
9-1040	A	49:12	49:12	What exactly is the 'circulation change'? [Bryant McAvaney]	Describing the simulated circulation changes is beyond the scope of this section.
9-1041	A	49:18	49:18	Change "may" to "are likely to" to conform with IPCC lexicon. [Michael MacCracken]	This is not a probabilistic statement.
9-1042	A	49:20		It is also model dependent as the change depends on the climatology of where precipitation occurs in the control run. [Kevin Trenberth]	Noted.
9-1043	A	49:32	49:32	Does this address changes in intensity or are we referring to actual and absolute values -- please clarify and if patterns indicate whether overall magnitude was considered. [Thomas Karl]	It considers trends in uncertainty using Frich et al. indices as stated. We think this is clear enough.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1044	A	49:36		As mentioned earlier (see page 47), changes in extreme precipitation can be handled by modeling changes in the probability distribution function and more specifically, in extreme quantiles (e.g. Ghosh & Draghicescu 2002a, b). [Sucharita Ghosh]	Noted.
9-1045	A	49:38		Section 9.5.3.3: this overlaps with or repeats parts of Chapter 3 [Fons Baede]	Agreed. Reduced descriptive portions.
9-1046	A	49:39		"even if global precipitation increases" is inappropriate. [Kevin Trenberth]	Agreed. Deleted.
9-1047	A	49:41	49:41	To add 'and on geographical factors' after 'circulation' [Javier Martin-Vide]	Agreed. Added.
9-1048	A	49:44	49:46	What is the reference for this observation? ("Over Europe, precipitation...southwest."). Is it Marengo et al. (2004) too? Please clarify. [Sandrine Bony]	This section deleted. See 9-1045.
9-1049	A	49:44	49:44	But there's more to decrease in winter - is the *fractional* decrease more? [William Ingram]	Not clear what this refers to.
9-1050	A	49:48	49:48	Why presume so? Globally, precipitation is strongly energetically constrained, with its distribution having much more freedom - that increases both signal & noise but I see no reason to assume the latter always more. [William Ingram]	Not clear what this refers to.
9-1051	A	49:50	49:52	Please use a find all command and identify all the 'may' 's used in this report and change to the probability scale identified with this IPCC report [Thomas Karl]	Replaced "may" with "possibly" in this instance.
9-1052	A	49:50	49:50	The work done within IPSL (Javal and collaborators) on the contribution of land water to sea level is also of relevance here. [Bryant McAvaney]	Don't understand relevance.
9-1053	A	49:54		On the Salel drought. According to a report of Hansen's group, black carbon from China and India might be related with the high temperature and low precipitation in the Sahara/Sahel region (S. Menon, J. Hansen, L. Nazarenko, Y. Luo, "Climate Effects of Black Carbon Aerosols in China and India," Science, 297, 2250-2253 (2002)). It also suggests the high temperature at Canada and Siberia. This paper looks so important that its negligence will be not justified. [Kiminori Itoh]	Added.
9-1054	A	49:55	50:3	Is global climate change being left off as a possible factor simply because a linear change is not seen? As noted earlier, one should not be looking for a linear effect to see a human-influence, or even a global warming consequence. The global and hemispheric changes did not change linearly, there were changes in land-ocean gradients, there were likely	Rewritten to indicate that these hypotheses were noted by Zeng, not us.



No.	Batch	Page:line		Comment	Notes
		From	To		
				changes to atmospheric circulation induced by the gradients in the forcing 9and their changes over seasons and over decades, etc.). How can one rule out a global climate influence of some amount? And there can be lags and leads as this is all connected to various changes of varying amounts in varying locations. And then on line 22 to 24 there is specific mention of how human activities could be causing an influence. [Michael MacCracken]	
9-1055	A	50:0		Figure: 9.5.5: The observations in this figure should be updated to at least 2004. If possible I would also prefer to incorporate simulations of other models. [Reindert Haarsma]	We chose to reproduce a published diagram in this instance. This diagram serves adequately to support the points being made in the text.
9-1056	A	50:1	50:3	2 of the 3 hypotheses have a reference; perhaps one should be added for the global SST hypothesis too? [Dave Rowell]	There are several. Have added Rowell 1996.
9-1057	A	50:3	50:3	I am perplexed why there is only one reference here--does this cover just internal variability or all of the various possible contributions? I would suggest, for example, adding a reference to Charney and then some of the later papers as well. [Michael MacCracken]	Zeng is cited also. Covers all three theories.
9-1058	A	50:10	50:10	Of the key "earlier findings", Rowell (1996) should perhaps also be included, as the first study to simulate multi-decadal variations of Sahel rainfall with observed SSTs. Rowell, D.P., 1996: Reply to comments by Y.C. Sud and W.K.-M. Lau on 'Variability of summer rainfall over tropical north Africa (1906-92): Observations and modelling' by D.P. Rowell, C.K. Folland, K. Maskell and M.N. Ward (April 4, 1995, 121, 669-704): Further analysis of simulated interdecadal and interannual variability of summer rainfall over tropical north Africa. Q. J. R. Meteorol. Soc., 122, 1007-1013 [Dave Rowell]	Agreed. Done.
9-1059	A	50:13	50:14	Rowell (2003) has been a little misquoted here; I suggest changing this phrase to read "and Rowell (2003) finds that Mediterranean SSTs are an additional important contributor to decadal variations of Sahel rainfall." (The paper claims that the Mediterranean has an influence of similar magnitude to that of other ocean basins, rather than a dominant one.) [Dave Rowell]	Agreed. Done.
9-1060	A	50:20	50:23	This is not the right way to phrase this. It is clear that model results reveal the dominant role of SST changes, but also that land changes provide an important feedback. The issue of changes in SSTs is not posed right either: clearly SSTs are rising with global warming, but here the issue is why the 3 tropical oceans are not all rising in the same way and so what are the differential changes in SSTs is the critical question. The answer relates to the role of ENSO in the Pacific, the THC in the Atlantic, and monsoons in the Indian oceans and the fact is that models do not simulate well the differences. Models do	Agreed. Both suggested changes made.



No.	Batch	Page:line		Comment	Notes
		From	To		
				well with specified SSTs, however. That highlights the need to be able to simulate patterns of SST change in coupled models, not just global values. [Kevin Trenberth]	
9-1442	B	50:20	50:37	Section 9.5.3.3.1. I doubt this result which establishes a bond between the oceanic temperatures of surface and the rains in the Sahel, because a study currently in hand in the north of Benin in zone soudano-sahelian, shows contrary results. It will be necessary to re-examine the model for better results representative of the field. [Expédit Wilfrid VISSIN]	Revised. We now discuss Horel et al 2005 who discuss simulations with both AGCMs and a number of IPCC AR4 CGCMs.
9-1061	A	50:20		Replace “are likely to” with “may” [Vincent Gray]	No reason or evidence provided for change.
9-1062	A	50:24	50:27	Given this type of influence in low latitudes (at the latitude of the gradient in sulfate and GHG forcing), plus the suggested impact on the NAO due to human activities, why would one not suspect some sort of influence on tropical cyclones/hurricanes? At least in discussing the hurricane studies, IPCC should be taking a more open view of things, not jumping to conclusions when studies are mainly limited to looking for a linear trend--which is likely not the signal to be searching for. [Michael MacCracken]	This could be correct, but there is no available published research to support it, at this time.
9-1063	A	50:35	50:37	The reference to Haarsma et al. (2005) is out of place here. Haarsma et al. investigated a large ensemble of a single model (CCM3) and found an increase in Sahel rainfall due to anthropogenic forcing. I assume that the study of Vizi and Cook (2005) is meant. [Reindert Haarsma]	Replaced with reference to Hoerling et al (2005a)
9-1064	A	50:35	50:37	Now, the Sahel rainfall is a monsoonal rainfall, and here there is a suggestion of a small increase due to global warming, in contrast to earlier statements about monsoons (at least the Asian monsoon) becoming weaker. [Michael MacCracken]	Yes, the monsoons are complicated.
9-1065	A	50:38	50:38	Why not both? [William Ingram]	Not clear what this refers to.
9-1066	A	50:55	51:2	I am aware of a paper submitted to GRL recently which argues that beyond the large-scale atmospheric forcing, local land clearance might have enhanced the rainfall decline ("Land cover change as an additional forcing to explain the rainfall decline in South West of Australia" by, B. Timbal and J. Arblaster, submitted to Geo. Res. Letters) [Robert Colman]	Has this paper been accepted? It was submitted after the deadline.
9-1067	A	50:57	51:2	I think this point is wrong. Given the nature of precipitation it seems quite plausible (in fact I would have thought quite likely) that some regional changes might be quite large, and relatively easily detected, whereas global mean changes might well be quite small and difficult to detect, because of offsetting regional increases and decreases. Global changes are likely to be controlled by energy constraints and therefore may be modest. Regional	Agreed. Sentence has been deleted.



No.	Batch	Page:line		Comment	Notes
		From	To		
				changes would be caused by circulation or local energy changes etc and could be quite large. In fact the findings in section 9.5.3.3 point out regional changes which exhibit a large signal driven by circulation changes. [Robert Colman]	
9-1068	A	51:8	51:8	"wastage" sounds teleological & directly anthropogenic: I assume either "retreat" or "melting" is actually meant [William Ingram]	Removed word.
9-1069	A	51:9	51:13	These results depend critically on how aerosols are put into models and most specify them and do not rain them out. Those issues ought to be discussed. The Ramanathan et al (2005) result is not correct as the changes in monsoon rains are part of the 1976 climate shift that is almost global in scope. Please see chapter 3.7.1.1 which should probably be cross referenced here. [Kevin Trenberth]	This comment demonstrates that a great deal of work is needed, before we can say anything strong about the causes of monsoon changes. I see no reason for changing the short discussion we have on this topic.
9-1070	A	51:14	51:17	In contrast, at 6kBP, there are strong indications that the warmer summer conditions led to (much) stronger monsoons. We really need some physical explanations here rather than simply referring to the results of various model simulations without follow-up explanations. [Michael MacCracken]	I thought we had long ago discarded the paleo-analog approach to estimating what might happen with climate change.
9-1071	A	51:18	52:49	This discussion should be moved to Chapter 4. [Andrew Lacis]	Discussed with Chapter 4 (Phillip Mote). We will reduce description of observed changes and concentrate more on their understanding.
9-1072	A	51:18		Section 9.5.4. This section contains almost exclusively observational information which is available elsewhere in the report. As there is minimal discussion of model results or of model-obs intercomparisons I suggest this section could be dropped for expediency. [Peter Thome]	See 9-1071
9-1073	A	51:23	51:41	This section really needs a summary statement that is, at least for the Arctic, expressed in positive terms instead of the sort of double negative--something like "Human activities are very likely the cause of the sharp reduction in Arctic sea ice over the past several decades." [Michael MacCracken]	Current description is clear
9-1074	A	51:23		On the sea ice. In relation to this part, Chapter 4 cites two important reports: [1] G. Holloway and T. Sou, Has Arctic Sea Ice Rapidly Thinned? J. Climate, 15, 1691-1701 (2002); [2] I. G. Rigor, J. M. Wallace, and R. L. Colony, Response of Sea Ice to the Arctic Oscillation, J. Climate, 15, 2648-2663 (2002). The report by Holloway & Sou shows the importance of sea ice distribution, and that by Rigor et al. shows the importance of NAM. Similar discussions were made by Polyakov et al. (2003). These	Rigor et al cited



No.	Batch	Page:line		Comment	Notes
		From	To		
				mechanisms seem plausible, and should be discussed here. [Kiminori Itoh]	
9-1075	A	51:35	51:40	To simulate southern ocean sea ice would require proper simulation of changes in SAM: see Chapter 3 for this and Fig 3.6.7. This should be discussed. [Kevin Trenberth]	This discussion would require considerable space, and not add much understanding.
9-1076	A	51:37	51:37	Here and in other places the terms realistic or unrealistic are used. This is in the eyes of the beholder --- can you please define more objectively (note this applies to all chapters I have read) [Thomas Karl]	Only use "unrealistic" once, in an appropriate context
9-1077	A	51:48		Change "or" to "and". [Kevin Trenberth]	This section has been re-written in collaboration with Chapter 4 (Mote).
9-1078	A	52:0		Section 9.5.5. Should this summary be here or adopted in the executive summary? The problems pointed out earlier apply here also. Major revisions desirable. [Kevin Trenberth]	We prefer to retain section summaries in the text as an aid for the reader.
9-1079	A	52:2	52:2	What does "significant" mean? If "statistically significant", meaningless without significance level [William Ingram]	53:10 - Revised as appropriate.
9-1080	A	52:5	52:26	Sentence on 24-26, as it refers to opposite changes being seen in opposite hemispheres, would fit better above: insert in line 5. [William Ingram]	53:32-35 - Disagree that this fits better above.
9-1081	A	52:6	52:8	Much too strong - Emanuel (2005) finds the SST increase can only account for 10% of the increase in hurricane power [William Ingram]	53:13-16 - Disagree - we think the wording has been couched adequately.
9-1082	A	52:15	52:15	Physical understanding as well as, & more convincingly than, just model simulations! [William Ingram]	Comment not clear.
9-1083	A	52:20	52:22	First, change "assumed" to "indicated"--this was not an assumed result. Second, Lonnie Thompson offers an alternative explanation to what is amplifying the rate of glacial melting, and that is basically having enough melting (or a long enough season) that meltwater runs off rather than having to evaporate for the glacier to lose mass. This leads to a given amount of heat being able to melt much more ice and cause a much faster loss of mass. The notion of the changing albedo is fine, but may well not be the predominant factor. [Michael MacCracken]	Section substantially re-written in collaboration with Chapter 4 (Mote).
9-1084	A	52:31	52:31	Sine the "little ice age" may not be real, it would be better here to either include the term in quotes, or better yet, give the years when this was occurring. [Michael MacCracken]	Term capitalized and put in quotes. Ch 6 identifies term. Possible candidate for the glossary.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1085	A	52:34	52:34	What is meant by "accelerating" needs to be explained. [Michael MacCracken]	Seems clear in context.
9-1086	A	52:41	52:42	As noted above, Lonnie Thompson notes that one can get much greater loss of mass if the meltwater can flow off the glacier than if it has to be evaporated (for remaining meltwater refreezes in the subsequent cold season). Looking for a correlation with temperature seems a bit strange--one can get faster melting with no temperature change in a glass of water, and on Greenland. [Michael MacCracken]	Section substantially re-written with Chapter 4 (Mote).
9-1087	A	52:42	52:43	And the melt extent in 2005 was even bigger than in 2004 and 2005--and 2002. [Michael MacCracken]	See 9-1086
9-1088	A	52:44	52:44	"greenhouse gases" should be "CO2", shouldn't it? [William Ingram]	Text has been revised as suggested.
9-1089	A	52:47	52:49	Very obscure: I can't suggest specific changes as I'm not sure what is meant [William Ingram]	Text has been clarified.
9-1090	A	52:51		Summary states that models have reproduced the tendency of NAM but could not give reasonable magnitude. Although this may not be an incorrect representation, it is more appropriate to point out that natural variations may be larger than anthropogenic ones as for NAM. In this regard, Prof. Akasofu of Alaska University gives a presentation at American Parliament to state that it is difficult to characterize the changes occurring in the Arctic regions ( <a href="http://appropriations.senate.gov/hearmarkups/record.cfm?id=223302">http://appropriations.senate.gov/hearmarkups/record.cfm?id=223302</a> ). If the temperature rise at the Arctic regions is, as Prof. Akasofu points out, due to the changes in the ocean currents, the mechanism suggested by conventional models (ice-snow feedback) is neither plausible nor convincing. [Kiminori Itoh]	This comment refers mainly to Arctic warming and not to the trend in the NAM. Not relevant.
9-1091	A	52:52	52:53	may ... to some extent" too weak: "will ... to some extent" or just "may" [William Ingram]	Accepted
9-1092	A	52:53		Delete "quantitative" [Vincent Gray]	Reject. Quantitative evidence came mainly from temperature. Evidence from other variables was more qualitative.
9-1093	A	52:53		Delete "almost" [Vincent Gray]	Reject.
9-1094	A	52:53		Insert after "on" "model simulations of" [Vincent Gray]	Not correct – also used data.
9-1095	A	52:54		Replace "identified" by "simulated by models" [Vincent Gray]	Not correct – based on models and observations as well as physical



No.	Batch	Page:line		Comment	Notes
		From	To		
					understanding.
9-1096	A	52:56	52:57	Delete from “and on line 56 to “models” on line 57 [Vincent Gray]	Rejected – no justification given.
9-1097	A	52:56		Replace “an” by “ a poaaible” [Vincent Gray]	What evidence that “an” is incorrect summary of the published literature?
9-1098	A	53:0		Section 9.6 This section is interesting and well-written, but I wonder whether it belongs at all in this chapter, rather than chapter 10 which in fact covers substantially the same ground. It does not seem to be a detection and attribution issue much at all. Perhaps there is no harm in some repetition, but amalgamating this section into chapter 10 would seem logical. [James Annan]	Chapter 10 has a box summarizing information on climate sensitivity from several chapters. The coverage of 9.6 has been determined by cross chapter meetings.
9-1099	A	53:2		Insert “known” before “natiral” [Vincent Gray]	‘simulated’ inserted before ‘natural’
9-1100	A	53:3	53:3	Change “influence” to “influences” [Michael MacCracken]	We prefer ‘influence’.
9-1101	A	53:4		Insert after “basins “ It remains difficult to explain how the ocean could warm from 1970 when the atmosphere only began to warm in 1997” [Vincent Gray]	Don’t understand suggestion, and reject the assertion that the atmosphere only began to warm in 1997.
9-1102	A	53:7	53:10	Good clear statement of important point [William Ingram]	Thanks.
9-1103	A	53:12	53:13	This seems strong based on a single study that was quoted earlier [Thomas Karl]	Although only one formal D&A study is cited, two other studies have demonstrated that simulated and observed trends are consistent.
9-1104	A	53:13	53:16	Anytime the term “compelling evidence” is used, it should be explained that this is (as I understand it) statistical jargon for meaning that one has greater than 95% confidence in a result--so that one has jumped to a level of likelihood in the IPCC lexicon of something that would be very, very likely (note that very likely only requires 90% confidence). I would really favor sticking to the IPCC lexicon and giving relative likelihood instead of using this term. In any case, this sentence needs to be updated in view of the papers of Webster et al. and Emanuel--and mention should also be made that most studies to date have been looking for a linear trend which is arguably not what the signal is likely to be--sort of like saying there are no roses in Arctic forests--well, so what, they are elsewhere. [Michael MacCracken]	We are making a statement about the evidence here, not a direct likelihood statement about the anthropogenic influence on cyclones. ‘no compelling evidence’ is not calibrated language.
9-1105	A	53:13		Delete “compelling” [Vincent Gray]	Reject – no justification.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1106	A	53:18	53:20	Is it really appropriate to be expressing such low confidence as is indicated by the phrase "suggesting ... has possibly ..."? First, there has been an increase observed in atmospheric water vapor, and second, there is really no other comparably plausible explanation to that this has been a result of human-induced global warming. The human influence here, in my view should be stated as at least "likely" and more probably "very likely". [Michael MacCracken]	'possibly' deleted.
9-1107	A	53:22	53:22	I do not understand what "This explains" is referring to here, especially as I thought there had been an increase in global precipitation detected (see, for example, TAR, page 4, first bullet under second title). Is this chapter going back on that? [Michael MacCracken]	Anthropogenic influence on global precipitation has not been detected.
9-1108	A	53:25	53:26	Uncertainties always remain -- please use the lexicon "likely" very likely etc. [Thomas Karl]	Sentence revised to used calibrated language.
9-1109	A	53:27	53:27	What does "have not been clearly" mean here--there is, I thought, widespread agreement that human activities are causing the increase of surface temperature for land and ocean--so what is being asked for here--99% certainty? This statement needs to reflect the many indications that human activities are causing the SST increase--there are no other plausible explanations with any real support. [Michael MacCracken]	Revised to use calibrated IPCC language.
9-1110	A	53:32	53:32	Change "but" to "and" [Michael MacCracken]	Done
9-1111	A	53:32		Insert after "shown" "by models" [Vincent Gray]	'natural variability' replaced by 'simulated internal variability'
9-1112	A	53:32		Insert "known" before "natural" [Vincent Gray]	See 9-1111
9-1113	A	53:33	53:34	Do the models really predict that SH sea ice should be decreasing--is this based on an up-to-date model result? In any case, there should be a cross-reference to where in this report changes in sea ice are being simulated. [Michael MacCracken]	This is based on Gregory et al. (2002), which is cited in 9.5.4.1. This section being a summary does not include references.
9-1114	A	53:34	53:34	Again lapse in use of uncertainty measures --- is this very likely or likely or virtually certain? [Thomas Karl]	Revised to use calibrated language.
9-1115	A	53:37	53:52	Climate sensitivity is really a discussion about climate feedback processes. The first quantitative discussion of radiative forcings and climate sensitivity is that given by Hansen et al. (1984) (see also Hansen et al., 1997). The first step is to precisely define the radiative forcing that is going to drive the climate change. Radiative forcings are	Noted.



No.	Batch	Page:line		Comment	Notes
		From	To		
				typically expressed in terms of W/m <sup>2</sup> , while climate change is measured in terms of degrees global temperature change - hence a conversion factor is required. Hansen et al. (1984) express their radiative forcing for doubled CO <sub>2</sub> (and 2% solar irradiance increase) in terms of Delta-T-zero, which is the equivalent of adjusted forcing, but expressed in terms of a global surface temperature change with no feedbacks allowed to operate. [Andrew Lacis]	
9-1116	A	53:37	61:3	The whole section 9.6 is very well written and well organized. [Sandrine Bony]	Noted, thanks
9-1117	A	53:37		Section 9.6: Chapters 8, 9 and 10 all contain information on Climate Sensitivity. I suggest to bring all this info together in one chapter, f.e. ch 10. [Fons Baede]	Rejected. There is a box in chapter 10, summarizing model and observational results on climate sensitivity, but the observational estimates are best kept in ch 9 since based on related techniques.
9-1118	A	53:37		Section 9.6: The terminology in this section is sloppy. Often the words "equilibrium" or "effective", to specify the type of climate sensitivity we are talking about, are missing. [Fons Baede]	Text has been revised.
9-1119	A	53:37		The title of this section should, I suppose, be: "Observational Constraints on Equilibrium Climate Sensitivity". [Fons Baede]	Rejected, the section talks both about ECS and TCR. This is more clarified in text, though.
9-1120	A	53:37		In this section, the author declares not to deal with regional sensitivity. Although I understand difficulties associated with the regional sensitivity, we should recall that the aim of the United Nations Framework Convention on Climate Change, that is, regional climate changes are more important than average temperature changes. If the AR4 states only the rise in the average temperature, it is not so different from TAR. So, it should be important to stress (or to point out) the need of researches on regional climate changes, and hence, regional climate sensitivity. [Kiminori Itoh]	Rejected. Research needs are not part of the assessment. Also, the definition of climate sensitivity is based on global mean changes, regional changes result from it.
9-1121	A	53:37		There is overlap with both Chapter 6 and 10 here - some consolidation is called for and some place where all the available information is synthesised. [Bryant McAvaney]	Overlap with chapter 10 is minimal, apart from the summary box. Cross chapter boundaries on sensitivity have been clarified.
9-1122	A	53:39	53:52	I should be pointed out here that not all the estimates are based just on observations. Some of the estimates do include assumptions about, and information from, models of varying degrees of complexity. Likewise, the forward reference to the Chapter 10 studies should be expanded to say that these are based on a combination of models (actually GCMs) and observations. Perhaps the real distinction here is that the estimates discussed in chapter 9 are based on simplified models and observations and those in chapter 10 are based on	Noted. Text has been edited to clarify the distinction.



No.	Batch	Page:line		Comment	Notes
		From	To		
				complex models and observations. [Matthew Collins]	
9-1123	A	53:39	53:48	<p>This § summarizes the definition, dependencies and limitations of climate sensitivity. While lines 39-40 give a very clear operational definition, how to determine climate sensitivity (CS), several readers might be confused why there should be more definitions necessary and to what “cautions” (line 45) refers. To my understanding, 9.6 refers to equilibrium climate sensitivity, and in that sense, there is no ambiguity in definition – there is nothing to be added to lines 39-40. The “caution” may rather refer to the fact that different models give diverging results, according to that definition. In lines 46-47, then it says: “CS to large negative forcing in the LGM” – obviously, here CS is used in a wider sense. I think, this § remains unclear on how CS shall be defined. I suggest to stick to the definition of lines 39-40 &amp; Section 8.6.2.1 and mention to the reader that a linear extrapolation of that context (i.e. that from so defined CS one can infer on temperature answers to other types of CO<sub>2</sub> changes only in rather limited ways, as the climate system employs a couple of rather nonlinear feedbacks).</p> <p>[Hermann Held]</p>	Text has been edited.
9-1124	A	53:39		<p>Climate sensitivity definition. climate sensitivity (defined as the equilibrium global mean temperature response to a doubling of CO<sub>2</sub> from preindustrial levels) Consideration should be given to abandoning the definition of sensitivity based on a doubling of CO<sub>2</sub> concentration. The reason for this is that sensitivity will not shift every time further research refines the forcing per doubling of CO<sub>2</sub>; this has happened several times during the lifetime of IPCC and will surely occur again. In fact doubling implies an initial concentration, which also changes. Chapter 2 noted (page 2-12) a substantial range of forcing associated with doubled CO<sub>2</sub> in different models: A recent comparison of line-by-line and GCM radiation schemes found that clear sky instantaneous RF and surface forcing agreed very well (better than 10%) among the 5 line-by-line models investigated, using the same single atmospheric background profile. The GCM radiation schemes were less accurate, with ~20% errors in the CO<sub>2</sub> RF ... (Collins et al., 2005 and Chapter 10). Nevertheless, the current set of Atmosphere and Ocean GCMs (AOGCMs) used in Chapter 10 of this report found values for RF, for a doubling of CO<sub>2</sub> that ranged between 3.5 and 4.2 W m<sup>-2</sup>, in good agreement with the TAR RF value of 3.7 W m<sup>-2</sup> (see Chapter 10 and Forster, 2005). Webb et al (2005) compare forcing for doubled CO<sub>2</sub> in 9 models, with that forcing ranging from 3 to 4 W m<sup>-2</sup>. Webb, M. J., C. A. Senior, D. M. H. Sexton, K. D. Williams, M. A. Ringer, B. J.</p>	Rejected. The present definition is well established in the literature. Caution on uncertainty in forcing has been discussed.



No.	Batch	Page:line		Comment	Notes
		From	To		
				<p>McAvaney, R. Colman, B. J. Soden, R. Gudgel, T. Knutson, S. Emori, T. Ogura, Y. Tsushima, N. Andronova, B. Li, I. Musat, S. Bony, and K. Taylor, 2005: On uncertainty in feedback mechanisms controlling climate sensitivity in two GCM ensembles. Clim. Dyn., in revision.</p> <p>A similar conclusion is reached in Table 10.2.1, for which the average and standard deviation forcing for doubled CO<sub>2</sub> for 9 models is <math>3.71 \pm 0.48 \text{ W m}^{-2}</math>, or <math>\pm 13\%</math> (range 2.99 to 4.23).</p> <p>If the basis for the expression of sensitivity (that is forcing associated with doubled CO<sub>2</sub>) is itself uncertain to 20%, it will be impossible to ascertain whether reports of different sensitivities in different AOGCM's are due to different model sensitivities or different forcings associated with doubled CO<sub>2</sub>.</p> <p>The Webb paper shows a strong correlation among different models of the increase in equilibrium warming for doubled CO<sub>2</sub> with the increase in forcing for doubled CO<sub>2</sub>. How much of this is due to a difference in forcing versus a difference in sensitivity? All the more reason to define sensitivity as K per W m<sup>-2</sup>.</p> <p>The use of systematic units is to be encouraged throughout science. Use of sensitivity referenced to CO<sub>2</sub> doubling is akin to measuring the density of substances relative to water, and then having to change the density of various substances if further research refines the density of water. Except of course that the forcing due to doubling of CO<sub>2</sub> is a lot less well known than the density of water.</p> <p>I therefore urge that sensitivity be defined as change in global mean surface temperature in response to a radiative forcing of 1 W m<sup>-2</sup>. Sooner or later as the science is refined so that differences of 20% are important that decision will be made. I urge that it be made sooner in order to advance the science.</p> <p>[Stephen E Schwartz]</p>	
9-1125	A	53:45	53:47	<p>Confusing - appears to say that equiprobable climate sensitivity gives a flat curve but equiprobable climate feedback doesn't! Clearest to separate out an explanation of "flat" &amp; then say that assuming flatness of different things has different meanings, so they're not neutral as they might seem.</p> <p>[William Ingram]</p>	Text has been edited corresponding to suggestions.
9-1126	A	53:49	53:51	<p>I think this needs a bit more justification/motivation/explanation.</p> <p>[William Ingram]</p>	Text has been edited.
9-1127	A	53:53	53:53	<p>For estimating global climate change, this is actually a more robust quantity than adjusted forcing. Lacis and Mishchenko (1995) show that for a globally uniform forcing, such as doubled CO<sub>2</sub>, Delta-T-zero is essentially independent of latitude while the adjusted flux has a significant latitudinal dependence because it depends directly on the magnitude of</p>	Noted. The importance of feedbacks for sensitivity uncertainty has been clarified.



No.	Batch	Page:line		Comment	Notes
		From	To		
				the local Planck radiation, whereas Delta-T-zero has already taken that into account. Hansen et al. (1984) showed that while the feedback efficiencies of the different feedback processes can be obtained from 1D model calculations and can be compared in linear fashion, the feedback effects on the global surface temperature are multiplicative in nature and do not combine linearly. Thus, while the radiative effects of atmospheric constituents can be evaluated with good accuracy, the model physics that are involved in producing the different feedback processes, which are necessarily more complex, are really the source of climate sensitivity differences between different GCMs. [Andrew Lacis]	
9-1128	A	53:54	53:54	I am not sure that it is really possible to divide the studies into three basic approaches. The approach is the same in each case; it is just that different data is used in the implementation. [Matthew Collins]	Text has been edited.
9-1129	A	53:54	54:5	In reality, the observational constraints on climate sensitivity consist of observational verification that the atmospheric water vapor is governed by the Clausius-Clapeyron relation - i.e., does a warmer atmosphere really contain more water vapor than a colder atmosphere. If it does, water vapor produces a positive feedback. There are many observational studies that show this to be true. If there is less snow and sea ice in a warmer climate, then snow-ice albedo is also a positive feedback. The nature of cloud feedback is more difficult to establish. An increase in cloud cover and optical depth (in response to increased temperature) may be a positive feedback in the case of cirrus clouds, but a negative feedback for low-level clouds. The actual relationship may also be a function of season and latitude, and there is no guarantee that the same relationship applies to longer time scales. Establishing how clouds respond to temperature change addresses the principal uncertainty affecting climate sensitivity. [Andrew Lacis]	An explicit discussion of the role of feedback uncertainty for climate sensitivity has been added. Process-level studies of feedbacks are discussed in chapter 8.
9-1130	A	53:57	54:2	several studies have used observed surface temperature changes over the last 150 years (see Chapter 3), the estimated ocean heat uptake since 1955 based on Levitus et al. (2000, 2005), and changes in atmospheric temperatures (Forest et al., 2002, 2005, Lindzen and Giannitsis, 2002). The sentence should note that this approach also requires knowledge of the forcing over the time period under examination, and is highly sensitive to uncertainty in that forcing. Uncertainty requirements in radiative forcing of climate change. Schwartz S. E., J. Air Waste Management Assoc. 54, 1351-1359 (2004). [Stephen E Schwartz]	Text has been edited.
9-1131	A	54:2	54:3	The list of citations appears incomplete, given the list of authors on page 9-55 (e.g., why is Knutti et al. not mentioned?).	References to all studies this applies to have been added.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Hermann Held]	
9-1132	A	54:3	54:4	The conclusions of studies that have used radiosonde data should be qualified by the findings of inaccurate trends. [Kevin Trenberth]	The effect of this uncertainty has been qualified (is small since study in question uses also surface T).
9-1133	A	54:5	54:6	It is wishful thinking to believe that climate sensitivity can actually be observed. [Andrew Lacis]	Rejected, this is not said in the text.
9-1134	A	54:9	54:11	Please characterize this one "further variant" in a couple of words; the fact that "uncertain parameters in coupled climate models.....10.5.4.5." are varied does not distinguish from the work by Forest et al., cited above (page 9-54, line 3). [Hermann Held]	Text has been edited.
9-1135	A	54:13	54:13	I suggest to add "above" before "studies" (otherwise one would not understand the following §) and "(compared to 3D GCMs)" after "simplified". [Hermann Held]	The sentence seems clear enough.
9-1136	A	54:15	54:17	It should also be noted that methods which ignore vast tranches of observational information (which in practice means all of them) will of course overestimate the uncertainty as a result. Much research is focussed on variants of the question "what does data set Y tell us about climate sensitivity" for different Y, but in fact the important question is surely "what does all the available evidence tell us about climate sensitivity" (or in simpler form "what is climate sensitivity"). This point is also relevant in respect of later comments. [James Annan]	A caution to this effect has been added.
9-1137	A	54:16	54:17	I very much appreciate that this delicate point is mentioned. However, "...treat as fixed..." is an unclear term: do you mean that (1) ocean diffusivity is not varied in the uncertainty analysis or (2) that ocean diffusivity does not vary with position within that model? I guess, you intend (1) and suggest to write "is not varied in the course of the uncertainty analysis". [Hermann Held]	Thanks, comment incorporated.
9-1138	A	54:16	54:18	What exactly is the definition of "climate sensitivity"? Presumably, the time scale that is needed to reach equilibrium (hence interaction with the ocean heat capacity) should not be a factor - only the magnitude of the temperature change in response to the applied forcing should be used to define climate sensitivity. The climate response to, say doubled CO <sub>2</sub> , consists of two parts. Part one is the direct forcing due to the doubled CO <sub>2</sub> , i.e., how accurate is the radiative transfer modeling of CO <sub>2</sub> absorption and emission. Part two is the additional increase in temperature due to feedback interactions. It would seem logical to confine the definition of climate (feedback) sensitivity to the temperature change contributed by feedback processes. This is the part that is really at the center of understanding the magnitude of climate change in response to any applied climate	A longstanding definition of climate change has been used in this chapter, in agreement with terminology with other chapters, so change of definition is rejected. The importance of feedbacks has been clarified. The timescale is not part of the definition of equilibrium sensitivity, but of TCR, which is made clear in the text.



No.	Batch	Page:line		Comment	Notes
		From	To		
				forcing. Regarding part one (the direct contribution), there should be little uncertainty - this part is only a question of radiation model accuracy. The time scale to actually reach equilibrium is a totally separate kind of concern. [Andrew Lacis]	
9-1139	A	54:24	54:24	Maunder Minimum is a term to describe a period of low numbers of sunspots or low solar activity between 1640 and 1720 (Eddy Science 1976), it is not the name of a climatic cool period and it is extremely misleading to use it as such. "Maunder Minimum" should be removed or replaced with more appropriate term.  [Gareth S. Jones]	Accepted.
9-1140	A	54:25	54:25	In case the related publications get accepted (Annan et al. (2005), Schneider von Deimling et al. (2005)), they should be included in some way in Table 9.2.1. [Hermann Held]	Accepted
9-1141	A	54:27	55:2	The three key parameters, or categories, for assessing climate change should be (1) direct radiative forcings, (2) feedbacks, and (3) time scale to reach equilibrium. The direct radiative forcings are the best understood and can be accurately computed (given the changes in radiative forcing constituents). The equilibrium climate sensitivity "alpha" is not really a parameter or a "constant" in any real sense. Rather it is the sum total of all feedback processes, so it varies geographically, with season, and with climate regime. The time scale factor involves the heat capacities of the atmosphere, ground, and ocean, and the energy exchange between the heat reservoirs. Thus, the transient climate response and heat uptake by the ocean are really part of the time scale problem. Any discussion of observational constraints should refer to the specific physical processes involved whether they be (1) changes in aerosols and GHG concentrations, (2) changes in cloud and water vapor distributions, or (3) transient temperature changes and heat uptake by the ocean. [Andrew Lacis]	See 9-1138. Equilibrium sensitivity is defined as annual and global mean change due to CO2 doubling, this has been clarified.
9-1142	A	54:29		Delete "rigorously" [Vincent Gray]	Text has been edited.
9-1143	A	54:32	54:32	I suggest to add "(whereby the term "parameter" shall cover both "tuning parameter" as well as "system property" in the following)" behind "parameters". [Hermann Held]	Text has been edited as suggested.
9-1144	A	54:46	54:48	Replace "earth system models....such models." by "most earth system models of intermediate complexity (the so-called EMICs; see Chapter 8). Most studies to date have been performed with such models, for reasons of demand in CPU time." The EMIC CLIMBER-2 (by PIK) displays climate sensitivity as an emerging property such as GCMs do, yet studies on climate sensitivity have been performed with it (Schneider von Deimling et al., 2005).	Text has been edited similar to suggestion.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Hermann Held]	
9-1145	A	54:50	54:55	Too careful : if observational constraints cannot overcome the differences in priors, it means that we cannot assess yet the climate sensitivity from observations. [Stéphane Hallegatte]	Noted. Our assessment is limited, as has been made clear in the text.
9-1146	A	54:50	55:2	I wholeheartedly agree with this paragraph up to "these prior assumptions matter". Indeed, it is very important to point out that results are sensitive to choices of prior distribution. However, from "and it is therefore..." onwards to "are to be constrained", presents only one of at least two arguments about how to deal with this issue. A better balance is needed. The argument that is presented seems to provide a pragmatic or objective strategy for choosing the prior. However, this seems to contradict the very nature of a Bayesian analysis, which is the cornerstone of allowing for subjectivity in scientific analysis. The alternative strategy that I am aware of is to test the sensitivity of the results to a number of choices of prior distribution, just as Tol and De Vos (1998) have done on p.55 line 15-16. It should be noted that in the statistical literature on how computer models can be used to make statements about the real world (e.g. Kennedy and O'Hagan 2001; Goldstein and Rougier 2004), priors are formulated in terms of input parameters, but my statistical colleagues are not aware of any statistical studies that advocate an uninformative prior in the target estimate. Priors in input parameters are done for several good reasons (input parameters are an uncertain entity in the prediction problem, to account for statistical interdependence of models in parameter space, because model inadequacy is formulated in terms of the best choice of input parameters (e.g. Craig et al 2001)). So please reword this last sentence to present both strategies and therefore a better balanced statement about the current state of knowledge. For example "...assumptions matter. Therefore it is important to test the sensitivity of the results to a number of prior choices, for instance, in assumptions (e.g. Tol and De Vos 1998) or input parameters (Frame et al 2005). Frame et al go on to advocate an alternative strategy, which is to sample the flat ("uninformative") prior in equilibrium sensitivity if this is the target of the estimate, or in transient climate response if the future temperature trends are to be constrained." Kennedy MC, O'Hagan A Bayesian calibration of computer models JOURNAL OF THE ROYAL STATISTICAL SOCIETY SERIES B-STATISTICAL METHODOLOGY 63: 425-450 Part 3 2001 Goldstein M, Rougier J Probabilistic formulations for transferring inferences from mathematical models to physical systems SIAM JOURNAL ON SCIENTIFIC COMPUTING 26 (2): 467-487 2004 Craig PS, Goldstein M, Rougier JC, and A. Scheult. Bayesian forecasting for complex systems using computer simulators	The text has been edited, discussing the importance of the prior in a more balanced manner.



No.	Batch	Page:line		Comment	Notes
		From	To		
				JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION 96 (454): 717-729 JUN 2001  [David Sexton]	
9-1147	A	54:52	55:2	A related point to the previous one. Dave Frame's analysis is based on the premise that one is asking the question of the form: "what does this study tell us about property X of the climate system, given no other information" (3rd page of his paper). However, the answer to this is not necessarily (or even perhaps likely) to actually be a useful overall estimate of X. More likely, a policy-maker or other scientist is interested in the answer to the question "what is X" (even acknowledging a probabilistic answer). When the goal is to use some observations to forming an estimate of X, the prior is not an arbitrary choice, but should actually represent the researcher's prior belief! I realise that this may seem like a rather abstruse debate that should properly take place within the peer-reviewed literature, but on the other hand, you wouldn't want to accidentally misrepresent Dave's work, and to say that we should choose our prior according to what answer we are looking for is likely to be (IMO rightly) viewed as an odd suggestion. I have also emailed Dave (but not yet received a reply) and maybe you would like to do the same. [James Annan]	The discussion of the importance of the prior has been revised, referring to several possible approaches and the role of prior belief, thanks.
9-1148	A	54:57	55:2	While I fully agree with the preceding lines of this §, I somewhat disagree with the conclusions drawn from that. These conclusions represent a direct transfer from the conclusions of Frame et al., 2005. The Frame et al., 2005 - paper has a lot of merits as it is the first paper that bravely addresses a very delicate, century-old problem of Bayesian inference within the climate community, at least to my knowledge. The effects demonstrated for an EBM and correctly represented in lines 50-56 are extremely inspiring. However, the suggestion made by Frame et al., 2005, on how to solve the problem should be fought through in a statistical journal first before given as a general recipe by IPCC. (Frame et al., 2005 was published in GRL. While Geophys. Res. Lett.'s reputation is for geophysics, it is certainly not so much for Bayesian statistics.) My impression is that their suggestion would work only in a limited sense, both w.r.t. the model as well as the decision context (their statistical result shall be used in) at hand. My impression got substantiated in discussions with trained statisticians. For illustration, here I would like to mention just 3 items that I see as problematic: (1) Bayesian statistics (as against classical statistics) offers the advantage to include subjective knowledge (the knowledge must be included in those parameters for which it is at hand, in that sense I agree with Frame et al.). This is at the same time a burden if such knowledge is not at hand. A subset of Bayesians aims at "objective Bayesianism" by requiring "non-informative", mostly uniform priors for those cases. In my view, Frame et al. attempt a modification of objective Bayesianism, yet subscribe to it. I personally find the other	See 9-1147 response



No.	Batch	Page:line		Comment	Notes
		From	To		
				<p>group more convincing that claims that there is no such thing as objective Bayesian updating, and that one should rather look at the set of posteriors as a whole when having used various priors (e.g., the seminal work by P. Walley, 1991).</p> <p>However, a non-unique prior in the <math>n</math> parameters leaves space for non-uniqueness of the posterior. (3) In general, a flat prior on CS will be hard to obtain if CS STRONGLY depends on more than one parameter: it is rather easy to construct examples that show that if <math>CS=a+b</math>, <math>a</math>, <math>b</math>, being 2 parameters, one cannot construct a flat prior on CS. This becomes intuitively clear as <math>n</math> becomes large: then the center limit theorem predicts a Gaussian on CS, no matter how the priors on <math>(a, b, \dots)</math> are chosen.</p> <p>At the moment, such a more ambitious approach will be hard to establish for the 4AR. However, there still remains the (technically also demanding, yet better known) possibility of classical statistics. If there is no subjective knowledge at hand or if one does not want to use it in order to be most objective, one should simply abstain from Bayesian statistics and use classical statistics, as was done in the brilliant article Allen &amp; Tett, 1999, on the fingerprinting method. (2) Yet, even if one followed the claims of objective Bayesianism, the recipe by Frame et al. runs into trouble, once the "parameter of interest" (e.g., CS) depends on more than one ("n") tuning parameter (as) it is generically the case for more complex models such as GCMs. Then one is basically dealing with a <math>nD \rightarrow ID</math> transfer function, and one cannot expect a unique result on the prior distribution for the <math>n</math> parameters, given the prescription that the prior for CS shall be flat.</p> <p>Hence, I do not feel that at stage the recipe in Frame et al. shall be recommended as a kind of "gold standard for Bayesian inference" as one could read this §. However, they nevertheless shall be cited as they fuel a desperately needed discussion. Hence I recommend to replace "...assumptions matter and it is.....constrained." by "assumptions matter. The approach by Frame et al. should be vividly discussed as it may serve as a standardizing recipe in the future, at least for special applications. As alternatives for situations in which no or little subjective input is wanted or available, there still remains the possibility of classical inference or generalized Bayesian approaches according to P. Walley (P. Walley, Statistical Reasoning with Imprecise Probabilities, Chapman &amp; Hall, London, 1991), respectively. Both latter approaches would require numerical schemes beyond simple Monte-Carlo type methods, however. For the time being we shall stay with the folder of Bayesian schemes applied - see below."</p> <p>[Hermann Held]</p>	
9-1149	A	55:0	56:	<p>Section 9.6.1.1</p> <p>This section has a recitation of a series of 9 studies in each paragraph and is lacking a synthesis and assessment. Rather than talk about the studies, I suggest talking about the science and referencing the appropriate studies where required.</p> <p>[Kevin Trenberth]</p>	Text has been edited.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1150	A	55:0		Fig 9.6.1 Again, the grey dashed line contradicts the text [William Ingram]	Figure has been edited and text clarified, model does not produce low sensitivities, so the lower limit of the grey range is not a constraint.
9-1151	A	55:0		Fig 9.6.1 The difference between the 2 Forest & als makes one wonder how much of the rest of the variation is due to whether natural forcings are included, but one has to refer back to the text to check - please dash all the ones without (& distinguish the 2 LGM ranges by colour - they're different authors after all). [William Ingram]	Impact of natural forcing more discussed, different LGM studies are distinguishable as are, so color changes rejected.
9-1152	A	55:1	55:2	The flat/uniform prior approach is just one of many. It may be possible to accept some prior assumptions for some climate variables. There may be dynamical or thermodynamical constraints or other theoretical considerations. [Matthew Collins]	See response 9-1147
9-1153	A	55:4	55:40	The increased temperature in the 20th century is less than one would have expected from many models based on increased CO2 levels to date - it would be desirable to discuss these issues. [Stephen McIntyre]	Rejected. The response to 20th century forcing between simulations and observations is assessed in 9.4
9-1154	A	55:7		and Table 9.2.1 The table should give not just the uncertainty in sensitivity, but also the uncertainties in forcing, temperature change, and heat flux into the ocean. An equation should be explicitly given by which sensitivity is derived from these quantities: Sensitivity (Kelvin per W m-2) = Delta Temperature / (Forcing - ocean heat flux) Such an equation permits uncertainty in sensitivity to be explicitly calculated from uncertainty in input quantities. Uncertainty requirements in radiative forcing of climate change. Schwartz S. E., J. Air Waste Management Assoc. 54, 1351-1359 (2004) [Stephen E Schwartz]	All these uncertainties are listed in the table. The equation is a bit of a simplification indirectly accounted for by the models, therefore rejected.
9-1155	A	55:13	55:15	Same point - but expansion of this text not needed if done earlier [William Ingram]	Not clear what comment refers to.
9-1156	A	55:14	55:14	Change "beyond" to "more tightly than" to make sentence clearer [Michael MacCracken]	Text edited, thanks.
9-1157	A	55:19	55:25	The Andronova and Schlesinger result is an outlier compared to the others, as it has such a prominent peak at low sensitivity. Without this finding it is possible that stronger conclusions could be drawn overall. I would like to see discussed whether this study or its assumptions can be distinguished from the other studies, or whether there is some ready explanation for why it produced such a prominent low sensitivity. I note that it was one of the earliest studies. This might help in an assessment of the reliability of these results compared with the others.	Noted, text has been edited. While we agree in its uncertainties, we do not feel that this result is an outlier.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Robert Colman]	
9-1158	A	55:25	55:25	It would be helpful here to indicate that the 50% likelihood here outside this range is strongly biased to the high side--perhaps also given a median value (and, if possible, a bit more information on the range). [Michael MacCracken]	Text has been edited.
9-1159	A	55:25		The latter avoids sulphate aerosol forcing uncertainty that affects parameter estimates, but adds uncertainty associated with separating the model's responses the various forcings (see discussion in Section 9.2.3). sounds like a preposition is missing. should it read: The latter avoids sulphate aerosol forcing uncertainty that affects parameter estimates, but adds uncertainty associated with separating the model's responses to the various forcings (see discussion in Section 9.2.3). [Stephen E Schwartz]	Changed, thanks.
9-1160	A	55:27	55:27	Remove 'ocean heat uptake' from that line. Ocean heat uptake is varied by changing mixing parameters, so this is twice the same. [Reto Knutti]	Changed, thanks.
9-1161	A	55:29	55:29	"four standard deviation confidence intervals" is ambiguous (is this +/-2 or +/-4?). [Jouni Räisänen]	Text has been clarified.
9-1162	A	55:32	55:32	It seems to me a bit irresponsible to say "no reliable upper limit" in that Earth history does seem to offer some constraints. The result cited here would thus be likely to have some shortcomings rather than suggest that any climate sensitivity is possible. [Michael MacCracken]	Text has been revised.
9-1163	A	55:34	55:34	"lack of an explanation" is far too weak - the idea makes no physical sense at all: the troposphere has negligible heat capacity to provide any lag: on longer (not necessarily very long!) timescales it would be expected to follow the surface. Of course things may occur although we had no physical idea why, but this one is not supported by data either. [William Ingram]	Text has been revised.
9-1164	A	55:34	55:34	I would suggest changing "negative, but" to "to exert cooling influences, and" [Michael MacCracken]	Change adopted, thanks.
9-1165	A	55:46	55:47	I am not sure of the logic in the sentence that begins "Since uncertainties in forcing...". If the Gaussian distribution does follow from this some proof is required. [Matthew Collins]	Text has been revised.
9-1166	A	55:48	55:48	What is a "very long upper tail"? This should be quantified. What is the value of the 95%? [Matthew Collins]	Values are given in table 9.2.1.
9-1167	A	55:50	55:52	Over-strong statement: Soden & al (2002)'s results are very dependent on things like their	Text has been edited.



No.	Batch	Page:line		Comment	Notes
		From	To		
				choices of reference periods [William Ingram]	
9-1168	A	55:50	55:50	Suggest removing the word 'using' in this line. [Reto Knutti]	Change adopted.
9-1169	A	56:2	56:2	Again, it would help to indicate that most of this 30% was on the high side. [Michael MacCracken]	Text edited, but uncertainties go both ways, as discussed in introductory paragraph.
9-1170	A	56:4	56:32	Throughout these paragraphs, it would be helpful to give both the median value and the ranges, both to better inform and to try to not set up the taking of the ratio of the high to the low value to give an uncertainty range (e.g., saying 1.5 to 4.5 C has led to some people, even in IPCC, that there is a factor of 3 range in the climate sensitivity, when this is clearly a wrong thing to do--just imagine if the range were 0 to 0.000001 and one would get an error of infinity). So, I would recommend giving the mean and then the range, so say, for example a median or mean sensitivity of 3 with the 5-95 values being 1.5 and 4.5 or something similar (and maybe do in a table). [Michael MacCracken]	Text has been revised, summary statement gives most likely value and range.
9-1171	A	56:7	56:7	With 2.4 as a minimum, I would think one needs to include an explanation of what causes this to be the case. For the high values, the uncertainty arises due to uncertainties in ocean heat uptake rates--but what causes the lower bound to change (go from what is typically 1 C to 2.4 C)? [Michael MacCracken]	The range has changed due to the use of updated Levitus data. Lower boundary has been discussed.
9-1172	A	56:14	56:26	Same comment as above. [James Annan]	Text revised.
9-1173	A	56:22		Do you mean "an estimated effective climate sensitivity"? [Fons Baede]	No, text has been clarified.
9-1174	A	56:24	56:26	The Frame et al method does not avoid sulphate aerosol uncertainties, it simply puts them in another part of the problem, i.e. in the multi-fingerprint analysis. [Matthew Collins]	This is a valid point, although separation is affected by smaller uncertainty then approaches making less use of the space-time signature of aerosols. Text edited.
9-1175	A	56:28	56:29	Just a comment: Colman et al (2003) and Soden and Held (2005) estimate that the total feedback parameter from GCMs ranges from about 1.5 to 2.5 W/m <sup>2</sup> /K (compared to a blackbody response), cf Fig. 8.6.1. Therefore, the observational range of estimate of the climate feedback parameter derived by Forster and Gregory (2005) is larger than that derived from GCMs. [Sandrine Bony]	Noted, this is now referred to in the text.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1176	A	56:28	56:32	It is wishful thinking to claim determination of climate sensitivity from ERBE measurements. It is first of all necessary to identify all contributing radiative forcings prior and during the time period in question (there are significant uncertainties in this). Then it is necessary to know how the climate system has responded to the applied forcing. Note also that if the applied forcing were GHG in nature, and the climate had reached equilibrium in response to this forcing, there would be zero flux change at TOA, i.e., ERBE would see no change at all in either the SW or LW flux. As it is, the current climate is about 0.85 W/m <sup>2</sup> out of global energy balance (Hansen et al., 2005), and this is beyond ERBE precision to verify. Furthermore, even if ERBE could measure this flux change, it would be a measurement related to ocean heat capacity and the rate of heat transport into the ocean, not climate sensitivity. [Andrew Lacis]	Noted.
9-1177	A	56:36	56:36	Possibly worth reminding the reader what sort of increases are thought plausible for the next century? [William Ingram]	Reader has been referred to ch 10.
9-1178	A	56:36	56:40	This interpretation is incorrect. Hansen et al. (1984) in GCM climate change simulations showed that for doubled CO <sub>2</sub> , and 2% increase in solar constant, the upper troposphere warms more than the surface temperature (in the tropics through midlatitudes) because of moist adiabatic lapse rate changes - a negative feedback component of the overall positive water vapor feedback. [Andrew Lacis]	Noted. Text has been revised.
9-1179	A	56:36	56:44	Need to resolve this controversy (and the one with Douglass and Knox) [Bryant McAvaney]	Text has been edited to more clearly assess our understanding.
9-1180	A	56:38	56:40	This has a big "if the observed..." and it is not correct. This study should be given less space. [Kevin Trenberth]	Text has been revised, and reference to this paper shortened.
9-1181	A	56:43	56:44	It would be helpful to restate their conclusion or to point the reader back several paragraphs. [Michael MacCracken]	Text revised.
9-1182	A	56:46	56:52	The term "5% limit" and "95% limit" may be a little confusing. I am worried they may be interpreted as hard limits when there is still a finite (in fact 1 in 20) chance of being above and below these limits respectively. 5%-tile and 95%-tile are adequate descriptions. [Matthew Collins]	Suggestion adopted.
9-1183	A	56:46	56:47	Median would be a more meaningful value for the "best estimate" than the mode, which is in some cases very close to the lower tail of the distribution. [Jouni Räisänen]	The median is quite affected by the long tail, and more affected by the prior and the sampled range than the mode.
9-1184	A	56:47	56:47	"best estimates ... range between 1.2C and 4C" seems to contradict the statement above	Statement rephrased similar to

**Comment [Gh4]:** Check if this is a concern



No.	Batch	Page:line		Comment	Notes
		From	To		
				on p56, line 40 that Lindzen and Giannitis who use observations to constrain their prediction to have a best estimate that is less than 1C. So this needs to be reworded. A similar statement is made on p.60, line 40: "Most studies find a lower 5% limit of 1C or greater" which is, in my opinion, a fairer reflection of the results from the various studies. [David Sexton]	suggestion.
9-1185	A	56:47		After many different numbers from many different studies, this "range between 1.2 and 4 grC" comes as a surprise. Where does this range suddenly come from? [Fons Baede]	From figure 9.6.1, which is now referred to.
9-1186	A	56:52	56:52	I would suggest saying "sensitivity significantly exceeds 4.5 C" as the upper bounds are indeed pretty high. [Michael MacCracken]	Suggestion adopted.
9-1187	A	56:54		I understand, in several aspects, the criticism of the Douglass & Knox paper which uses volcanic eruption. But, what about their paper on the annual insolation change? : e. g., D. H. Douglass, E. G. Blackman, and R. S. Knox, Phys. Lett A 323, 3/10/04, 315-322 (2004) and its erratum, "Temperature response of Earth to the annual solar irradiance cycle." Also, what about the report of Forster & Gregory (2005) ("The climate sensitivity and its components diagnosed from Earth radiation budget data," J. Climate, submitted), which is cited in Chapter 8 (Section 8.6)? This paper looks scientifically sound. It shows several important points; 1) the climate sensitivity of models tend to be too large; 2) among models in TAR only one out of ten gives sensitivity patterns similar to their estimation; 3) the volcanic eruption might not be appropriate to estimate general climate sensitivity. [Kiminori Itoh]	Comment considered
9-1188	A	56:57	57:1	Note that Figure 8.6.2 does not show results from Soden et al (2002) but from Forster and Collins (2004). [Sandrine Bony]	Reference changed, thanks.
9-1189	A	56:57	57:5	This is a quite long and confusing sentence--how does a model have a sensitivity of 4 without having water vapor feedback? Is this saying that water vapor feedback alone adds 3 C to the sensitivity? And is "that eruption" Pinatubo? [Michael MacCracken]	Text clarified.
9-1116	A	53:37	61:3	The whole section 9.6 is very well written and well organized. [Sandrine Bony]	Noted, thanks
9-1190	A	57:1	57:17	Forster and Collins, 2004, also examine the water-vapour feedback following Pinatubo. Forster, PMD; Collins, M., 2004: Quantifying the water vapour feedback associated with post-Pinatubo global cooling. CLIMATE DYNAMICS, 23 (2): 207-214. [Matthew Collins]	Now cited.
9-1191	A	57:10		However, Wigley et al. (2005b) demonstrate that the analysis method of Douglass and Knox (2005) severely underestimates climate sensitivity (by a factor of 3) if applied to the	This has been reassessed.



No.	Batch	Page:line		Comment	Notes
		From	To		
				volcanic response in a climate model with known climate sensitivity. Both Robock et al. (2005) and Wigley et al. (2005b) question the analysis method of Douglass and Knox. The argument of Wigley rests heavily on his assertion that Levitus (2000) reported heat flux ocean to atmosphere of 2 W m <sup>-2</sup> following Pinatubo. Of course Levitus heat flow in that time region was net positive, though there is a dip following Pinatubo, which is probably what Wigley is referring to. The accuracy of Wigley's estimate of sensitivity and time constant appear to rest on the inference of the magnitude of that delta, though Wigley's paper is not clear on this point. Robock adduces different arguments and does not adduce the Levitus heat flux. It would seem that Douglas and Knox should be given opportunity to comment. [Stephen E Schwartz]	
9-1192	A	57:12	57:13	Correct reference is Robock (2005) [not et al.]: Robock, Alan, 2005: Comment on "Climate forcing by the volcanic eruption of Mount Pinatubo" by David H. Douglass and Robert S. Knox. Geophys. Res. Lett., 32, L20711, doi:10.1029/2005GL023287. The correct Wigley et al. (2005b) reference is: Wigley, T. M. L., C. M. Ammann, B. D. Santer, and K. E. Taylor, 2005b: Comment on "Climate forcing by the volcanic eruption of Mount Pinatubo" by David H. Douglass and Robert S. Knox. Geophys. Res. Lett., 32, L20709, doi:10.1029/2005GL023312. [Alan Robock]	Thanks, fixed.
9-1193	A	57:13	57:13	I would suggest changing "question" to "explain problems with" to give a better indication of the merit of the criticisms. [Michael MacCracken]	Suggestion has been adopted.
9-1194	A	57:13	57:13	Change "question the analysis method" to "point out logical inconsistencies in the analysis method" [Alan Robock]	Text has been edited, see 1193.
9-1195	A	57:15	57:21	This seems a bit pessimistic especially given Wigley et al's results. It would be helpful to actually quote some numbers to give some context to "reliable upper limit", since it seems to me that Wigley claims substantially tighter limits than some of the largely negative results of the previous section. A limit of say ~5C (or even 6C) at the 95% level might not be useful in the context of an existing 1.5-4.5 range, but it certainly is compared to some other results! [James Annan]	Paper has been reassessed (I dont think it gives a 95% limit).
9-1196	A	57:16	57:17	The statement that the temperature response to a single volcanic eruption cannot establish a reliable upper limit on sensitivity seems at odds with the statement in the previous paragraph that a 6.3 degC sensitivity model shows a feedback which is inconsistent with observations (and by implication is rejected). This should be rectified (the more cautious statement would seem appropriate).	It has been further clarified that the rejection of 6.3oC may be a feature of the feedbacks in one particular model.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Matthew Collins]	
9-1197	A	57:22		Need clarifying synthesis statement on what can and can't be learnt from the use of volcanoes (in so far as climate sensitivity is concerned). [Bryant McAvancy]	Statement added.
9-1198	A	57:23	57:45	This is really an apples and oranges comparison. The transient response is a atmosphere-ocean heat capacity and heat exchange problem. Climate sensitivity is a climate feedback problem. Mixing the two processes does not lead to improved understanding of either physical problem. [Andrew Lacis]	The goal is to provide constraints for future climate change on observed changes, so this statement is not applicable. However, header has been changed to not set these two concepts against each other, and physical properties have been better explained.
9-1199	A	57:23		Should the title of this section be: "Transient climate response vs. equilibrium climate sensitivity"? [Fons Baede]	Title has been revised.
9-1200	A	57:24	57:34	The TCR is not "well constrained" by the observed warming trend, rather is constrained by that warming attributable to CO2 in papers discussed. [Matthew Collins]	Good point, text has been edited.
9-1201	A	57:24	57:45	Two definitions of TCR (per 70 and per 100 years) are confusing. Please always express the 100-year trends e.g. as 3C / century rather than as plain numbers. [Jouni Räisänen]	Units have been revised and better explained.
9-1202	A	57:27		The transient climate response is indicative of the temperature trend associated with external forcing, and as such it is well constrained by an observable quantity, the observed warming trend. Estimation of the transient climate response depends on knowledge not only of the temperature change but also of the forcing and its uncertainty depends on the uncertainty in each (it's a quotient). Given that the total forcing is highly uncertain (e.g., page 60, line 51: a high sensitivity cannot be ruled out because it is possible that a high aerosol forcing could nearly cancel greenhouse gas forcing) it seems on face that the transient uncertainty cannot be anywhere nearly so constrained as indicated in Figure 9.6.2. [Stephen E Schwartz]	The constraint is based on warming attributable to greenhouse gases. This is clarified, and a caveat about uncertainty in separation of greenhouse gas to aerosol signal has been added.
9-1203	A	57:38	57:38	I would suggest changing it to read "and are then reduced rather than kept constant" or something similar as present wording is not very clear. [Michael MacCracken]	Text edited, thanks.
9-1204	A	57:42	57:42	Range OF 2.2 to 4 degree (typo error). [Sandrine Bony]	Fixed, thanks.
9-1205	A	57:42	57:42	? Remove a typo "F" ?	Fixed, thanks.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Hermann Held]	
9-1206	A	57:42	57:42	There should be consistency in the precision here--it should therefore be 2.2 to 4.0 C if that is the appropriate value. [Michael MacCracken]	Text edited.
9-1207	A	57:53	57:56	This statement actually brings into question the whole IPCC paradigm of just adding up forcings independent of latitudinal and seasonal distribution. Clearly, if applied to the glacial cycling and orbital elements, the IPCC forcing would be near zero, yet the response was a major glacial cycle--so there is a problem somewhere. So, should IPCC not be much more cautious when applying its paradigm to aerosols, which has a strong seasonal and latitudinal influence? I think somewhere there needs to be an explanation to resolve all of this (and redoing the glacial calculation by converting the radiative influences of vegetation, CO2 and glaciers from feedbacks into external forcing seems to me a quite problematic way to go as climate models get better and better). [Michael MacCracken]	We agree that backing out response to forcing ratio is problematic (but doesn't question the concept of sensitivity to CO2 doubling as defined). We have clarified this in the text and have also downweighted results from direct estimates due to this problem.
9-1208	A	58:6	58:7	How do we know that the climate state of the LGM was at near equilibrium, especially when accounting for the ocean and cryosphere that have long time constants to adjust? [Michael MacCracken]	This statement has been removed.
9-1209	A	58:16	58:16	Maunder Minimum is a term to describe a period of low numbers of sunspots or low solar activity between 1640 and 1720 (Eddy Science 1976), it is not the name of a climatic cool period and it is extremely misleading to use it as such. "Late Maunder Minimum" should be removed or replaced with more appropriate term.  [Gareth S. Jones]	Terminology has been changed.
9-1210	A	58:18	58:20	Hegerl's proxy data is not available, making this study impossible to evaluate. [Stephen McIntyre]	References to studies are based on peer reviewed literature.
9-1211	A	58:20	58:20	To be literally correct, I would suggest changing "constrain the climate sensitivity" to "constrain estimates of the climate sensitivity" [Michael MacCracken]	Adopted.
9-1212	A	58:24	58:24	Change "distribution" to "distribution function" [Michael MacCracken]	Adopted.
9-1213	A	58:28	58:32	Modes and medians are not comparable. Prefer medians if possible. [Jouni Räisänen]	Noted.
9-1214	A	58:32	58:34	This point should receive a bit more prominence--given The Skeptics seem to think the opposite. [Michael MacCracken]	Accepted, this has been added to the section summary.
9-1215	A	58:37	58:39	This contradicts the figure: I suspect the latter is wrong?	Not clear what the comment refers to.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[William Ingram]	Figure 9.6.1 has been clarified.
9-1216	A	58:42	58:43	I would suggest changing "analyses on" to analyses using data describing" and then to delete "information". And what is meant by "a prior probability distribution"? [Michael MacCracken]	Suggestion adopted. Section on combining evidence has been better explained.
9-1217	A	58:50	58:50	"support" seems too strong given all the trouble we have with instrumental data, & the far greater uncertainty for paleodata - "are consistent with", perhaps? [William Ingram]	Text revised.
9-1218	A	58:51	58:51	Maunder Minimum is a term to describe a period of low numbers of sunspots or low solar activity between 1640 and 1720 (Eddy Science 1976), it is not the name of a climatic cool period and it is extremely misleading to use it as such. "Late Maunder Minimum" should be removed or replaced with more appropriate term.  [Gareth S. Jones]	Terminology revised.
9-1219	A	58:51	58:55	This is probably more a case of reverse engineering than actual information on climate sensitivity. [Andrew Lacis]	We agree and have drastically shortened the description of this result.
9-1220	A	58:51		Section 9.6.2.2: this section suffers from too many different numbers, which make the section unreadable. This is a general problem of the whole of Section 9.6 [Fons Baede]	We disagree that numbers are a problem. However, we agree that subsection 9.6.2.2 was problematic, it has been removed.
9-1221	A	58:51		Suppose that the temperature change from the Maunder Minimum is 1 degree and that the forcing change due to the insolation change is 0.2 W/m <sup>2</sup> . This combination gives too high a sensitivity value of 5 K/(W/m <sup>2</sup> ). Thus, it should be necessary to investigate the cause of the centennial and millennial changes of the temperature. The contribution of natural fluctuation in NAM will be important. [Kiminori Itoh]	This section has been revised, introducing caveats.
9-1222	A	58:52	58:53	Maunder Minimum is a term to describe a period of low numbers of sunspots or low solar activity between 1640 and 1720 (Eddy Science 1976), it is not the name of a climatic cool period and it is extremely misleading to use it as such. "Late Maunder Minimum" should be removed or replaced with more appropriate term.  [Gareth S. Jones]	Terminology revised.
9-1223	A	58:52	58:55	It needs to be mentioned early on that these comparisons are limited because we are likely not at equilibrium during any of these periods, given the long time constants of the oceans, vegetation, and ice. [Michael MacCracken]	This subsection has been drastically shortened due to similar concerns.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1224	A	58:54	59:5	Which of the two values -1.8 and -1.63 W/m <sup>2</sup> is correct? [Jouni Räisänen]	This section has been revised.
9-1225	A	59:6	59:6	Maunder Minimum is a term to describe a period of low numbers of sunspots or low solar activity between 1640 and 1720 (Eddy Science 1976), it is not the name of a climatic cool period and it is extremely misleading to use it as such. "Late Maunder Minimum" should be removed or replaced with more appropriate term.  [Gareth S. Jones]	Terminology revised.
9-1226	A	59:7	59:7	Change "may" to "are likely to"--though I do wonder if this is the case. [Michael MacCracken]	We have concerns about this section and have strongly shortened and caveated it.
9-1227	A	59:9	59:9	I don't know what is meant here [William Ingram]	Not clear what comment refers to.
9-1228	A	59:10	59:10	Maunder Minimum is a term to describe a period of low numbers of sunspots or low solar activity between 1640 and 1720 (Eddy Science 1976), it is not the name of a climatic cool period and it is extremely misleading to use it as such. "Late Maunder Minimum" should be removed or replaced with more appropriate term.  [Gareth S. Jones]	Terminology revised.
9-1229	A	59:14	59:14	For parallelism, change it to read "1.0 C would result in an estimate of climate sensitivity of up to 4.7 C" [Michael MacCracken]	Text revised.
9-1230	A	59:14	59:15	The statement "broadly consistent with estimates obtained from the full millennial record", seems to stand in contradiction to the findings (which should be cited) of Andronova et al [Andronova, N.G., Schlesinger, M.E., Mann, M.E., Are Reconstructed Pre-Instrumental Hemispheric Temperatures Consistent With Instrumental Hemispheric Temperatures?, Geophysical Research Letters, 31, L12202, doi: 10.1029/2004GL019658, 2004]. These authors find that the Northern Hemisphere mean temperature reconstruction of Mann and Jones (2003) is consistent with the estimated EBM response to forcing in past centuries given a mid-range sensitivity which is chosen by a best fit to the more recent instrumental interval and combined natural+anthropogenic forcing. Their study does not support such a high sensitivity as is argued for here. [Michael Mann]	This section has been rewritten. We have assessed the cited paper.
9-1231	A	59:15	59:16	Perhaps there is something I'm missing here? I sure thought that the best guess sensitivity in the TAR was the canonical range of 1.5 to 4.5 C/2xCO <sub>2</sub> . By this standard, 4.7C is an outlier on the high end, hardly a typical or central estimate. 2.2 C is much closer to being a mid-range sensitivity estimate than is 4.7C. Either I'm missing something, or what is	This section has been revised. However, 4.7 as an upper end being broadly consistent with 4.5 is hardly wrong.



No.	Batch	Page:line		Comment	Notes
		From	To		
				said here is simply just wrong. [Michael Mann]	
9-1232	A	59:18	59:18	Change "this time period" to "this comparison of time periods" [Michael MacCracken]	Text has been edited.
9-1233	A	59:20	60:3	It would be highly desirable if a GCM with the same model physics would be able to model both current climate and the climate of the LGM. [Andrew Lacis]	Not clear why the reviewer thinks this is not the case.
9-1234	A	59:21	59:21	Change "gases" to "gas concentrations" [Michael MacCracken]	Text edited.
9-1235	A	59:21	59:24	Ice sheets are a feedback, not a forcing in the forcing definition. If ice sheets are a feedback, climate sensitivity is very different. [Stephen McIntyre]	Ice sheets at peak LGM conditions are a boundary condition. This has been clarified.
9-1236	A	59:22	59:23	Need to say that insolation change was about zero over the annual cycle, but make clear that there were strong seasonal and latitudinal variations. [Michael MacCracken]	Text edited.
9-1237	A	59:24	59:24	This estimate of flux changes includes a number of factors that some models treat as feedbacks, and that we indeed want to call feedbacks in describing, for example, the effect of the CO <sub>2</sub> changes on the glacial cycling. As noted in other comments, I think some clarifications are needed on this. [Michael MacCracken]	The terminology on feedbacks, forcings and boundary conditions during the LGM has been clarified addressing the reviewer's concern.
9-1238	A	59:26	59:26	Presumably this title shouldn't match the one at the level above? [William Ingram]	Title has been changed.
9-1239	A	59:26	59:34	Since the COAGCMs already have climate sensitivities within the standard range, it is completely unexceptional that the LGM forcings and response fall within the same range. This cannot be considered a constraint, especially since many of those models did not use all the forcings. However, if an estimated global mean temp change (consistent with multiple proxies) was used, then that will be an independent constraint. Given values of around 5 to 6 deg C, that gives roughly 0.5 to 1 deg C/W/m <sup>2</sup> with a mean value around 0.75 i.e. close to 3 deg C for 2xCO <sub>2</sub> . This has been used in multiple studies going back to the 1980s (hansen et al, 1985; lorius et al 1990, etc.). More importantly, there is no way to reconcile estimate of climate sensitivities > 6 deg C with the forcing estimates and response at the LGM. This should be made much clearer. [Gavin Schmidt]	Text has been edited, referring to the earlier work also.
9-1240	A	59:27	59:28	It would be clearer to say "changes in greenhouse gas concentrations and in the extent and height of ice sheets produced cooling estimates from 3.1 to 5.2 C (Masson ..." [Michael MacCracken]	Text edited.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1241	A	59:29	59:29	I would suggest changing "since" to "because" here so as to avoid possible confusion about the meaning of "since". [Michael MacCracken]	Text has been edited.
9-1242	A	59:32	59:33	Shouldn't these "very likely" be "virtually certain"? [William Ingram]	Lower limit on sensitivity has been reassessed.
9-1243	A	59:36	59:57	Both the Annan et al. and von Deimling et al. (reference apparently missing) studies employ mixed layer oceans and then examine constraints based on tropical SSTs. Ocean dynamics are known to play a large role in determining tropical SSTs so there may be a "structural" limitation of both the studies. This caveat should be pointed out. [Matthew Collins]	Caveat has been added.
9-1244	A	59:37	59:37	Change "determine" to "develop" [Michael MacCracken]	Text has been edited
9-1245	A	59:38	59:38	Clearer with "equilibrating to" for "with"? [William Ingram]	Accepted.
9-1246	A	59:38	59:51	Good clear summary overall, but perhaps over-does the cancellation between aerosol forcing & climate sensitivity - it's very true if one only looks at the global mean, less so if one uses all the data [William Ingram]	Thanks, text has been revised.
9-1247	A	59:46	59:48	This is inconsistent with Fig. 9.6.1., which suggests a range of 4-7C for the Annan et al. study. [Jouni Räisänen]	Figure has been revised.
9-1248	A	59:47	59:47	6C is better - (revised version of paper, also full ref given below) [James Annan]	Accepted.
9-1249	A	59:54	59:54	Replace "LGM." by "LGM. Almost identical results are obtained when they correlate climate sensitivity and Antarctic paleo data. " [Hermann Held]	A similar comment has been added.
9-1250	A	60:3	60:3	It would be highly desirable to figure out the underlying reason for that discrepancy, before the IPCC report is submitted. [Hermann Held]	Noted.
9-1251	A	60:7		9.3.6.1. There are two important points which you seem to overlook. Firstly, previous estimates have generally failed to attempt to account for model inadequacy in any formal manner. Steps are being taken in this direction, but as yet it is very tentative. Chapter 10 contains a brief pointer to this (its p48 135-42, although this will change as different reference(s) will have to be used). A second important point is that although ignoring sources of uncertainty will artificially underestimate the uncertainty as you say, ignoring sources of data will artificially inflate it. Since each study typically uses a very small	We agree. This has been further discussed in the text by adding a section discussing corroborating lines of evidence and multi evidence papers such as the one by Annan et al. 2006



No.	Batch	Page:line		Comment	Notes
		From	To		
				subset of available data, it would be reasonable to conclude that a more comprehensive viewpoint could narrow the range - at least, rule out the extremes that some of the methods suggest. A simplistic look at the results (which almost invariably suggest a maximum likelihood around 3-4C) would be to ask what is the probability that all the different methods have a bias of the same sign, relative to the true value. Of course the underlying assumptions are not truly independent in many cases, so this is not a formal estimate, but it must be considered of some indicative value. [James Annan]	
9-1252	A	60:10	60:12	Seems to wishy-washy - clearer as "According to an analysis of ... temperatures, human influences more than doubled" [William Ingram]	Text edited.
9-1253	A	60:10	60:20	Again, there should be clear differentiation between (1) direct radiative forcings, (2) climate (feedback) sensitivity, and (3) timescale related process - including "effective diffusivity" for heat transport into the deep ocean. [Andrew Lacis]	Noted. Definition of sensitivity agreed on between different chapters.
9-1254	A	60:22	60:33	Since feedback contributions are rather arbitrarily "prescribed" in the "simplified" climate model simulations, the conclusions reached based on such simulations are likewise questionable. [Andrew Lacis]	This shortcomings of simple climate models have been discussed and assessed.
9-1255	A	60:24	60:24	Some EMICs or simple models may be more elaborate in terms of non-linearity than GCMs. E.g., the EMIC CLIMBER-2 has a more elaborate scheme of Greenland ice melt down than many GCMs. Furthermore, the box model in Zickfeld et al. (GRL, 32, L15070, DOE: 10.C29/2005GL022771) is potentially more non-linear on the dynamics of the Indian monsoon than most GCMs. Hence I recommend to simply omit "simple or intermediate complexity". [Hermann Held]	A cautionary statement has been made
9-1256	A	60:25	60:25	Is "ill-posed" meaningful to the intended readership? [William Ingram]	Question extensively re-drafted.
9-1257	A	60:31	60:33	In Schneider von Deimling et al., virtually identical results are obtained for Antarctic paleo data. Hence I find this concluding sentence very problematic & would completely replace it. My own opinion is that the Schneider von Deimling et al. results are more likely than the Annan et al.-results, for that very reason, leading to very different conclusions. Please modify the conclusions of Chapter 9 accordingly. [Hermann Held]	Text has been revised
9-1258	A	60:35	60:45	Climate sensitivity is not really an observable quantity. Rather, discussion of observational constraints should be directed by their relevance to (1) direct radiative forcings, i.e., as represented by changes in distribution and concentration of GHGs,	Noted. While recognizing that this is important, we disagree with narrowing the approach to that.



No.	Batch	Page:line		Comment	Notes
		From	To		
				aerosols, surface albedos, (2) feedbacks, i.e., as represented by changes in water vapor and cloud distributions in response to temperature changes, and (3) transient and time scale processes and rate of heat transport into the deep ocean. [Andrew Lacis]	
9-1259	A	60:37	60:38	Delete from “Thus inceases” on line 37 to “findings” on line 38 [Vincent Gray]	Rejected. Similar findings from different lines of evidence do increase confidence. However, text has been clarified.
9-1260	A	60:37	60:38	"This increases confidence in individual findings" : These different studies are based on the same methodology and used more or less the same data. They fact that they reaches the same results can hardly be considered as increasing confidence into this methodology. The validation of the methodology would require that its results could be validated against some observations, which is impossible with subjective pdf : even when the climate will have changed, and the climate sensitivity will have been measured, we will not know if these results were correct or not... This is a major drawback of this methodology, that should be stressed in the IPCC Regardless, the high dependency of the results on priors suggests that observations are not sufficient to constraint climate sensitivity. This should be stated clearly. [Stéphane Hallegatte]	There are several independent lines of evidence. This has been clarified in the text. We have also added a iscussion of priors.
9-1261	A	60:39	60:41	Repeats 10-12 above [William Ingram]	First paragraph intended as a summary of the full response.
9-1262	A	60:40		Delete “further increasing confidence” [Vincent Gray]	Further has been removed.
9-1263	A	60:41	60:42	I would think that "very likely" should in both cases be "virtually certain"--there is really no other plausible explanation for Earth history. [Michael MacCracken]	Lower boundary has been reassessed.
9-1264	A	60:41		Replace “very likely with ”probable” [Vincent Gray]	Rejected. No evidence is given for this change.
9-1265	A	60:42		Replace “very likely” with “probably” [Vincent Gray]	See 9-1265
9-1266	A	60:42		Replace “very likely” with “probably” [Vincent Gray]	See 9-1265
9-1267	A	60:42		Replace “ the most likely” by “find a” [Vincent Gray]	Rejected, not consistent with evidence
9-1268	A	60:43		Delete “is”	Rejected, makes sentence



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Vincent Gray]	ungrammatical
9-1269	A	60:43		Delete "on the order of" [Vincent Gray]	Accepted.
9-1270	A	60:44		leading credibility to mainstream climate models used to simulate future climate change. typo. should be lending. But perhaps the phrase is in appropriate altogether. Given the weakness of the constraints based on these observational studies, if it is stated that the credibility of the models rests on these observational constraints, does the sentence diminish rather than strengthen the credibility of the models. [Stephen E Schwartz]	Accepted.
9-1271	A	60:51		a high sensitivity cannot be ruled out because it is possible that a high aerosol forcing could nearly cancel greenhouse gas forcing. this conclusion is enormously important and needs greater emphasis. It means that the greater fraction of the positive greenhouse forcing resulting from some 40 years worth of CO2 emissions (the e-folding time of anthro CO2 emissions) is being offset by the cooling forcing of a week's worth of aerosol forcing (atmospheric residence time of the aerosol). Such a possibility was noted by Schwartz, 1993: Does fossil fuel combustion lead to global warming? Schwartz, S. E., Energy Internatl. J. 18, 1229-1248 (1993). [Stephen E Schwartz]	Noted. This section has been further clarified.
9-1272	A	60:55	60:55	again this is circular. The climate sensitivity in these few PMIP models cannot be used as a constraint. [Gavin Schmidt]	Sentence revised.
9-1273	A	61:1	61:1	Should "noise" be in brackets? [William Ingram]	Question redrafted.
9-1274	A	61:4		After the juggling with so many different numbers from so many different studies, there should be a strong and clear bottom line here saying what is the present range of the most likely equilibrium climate sensitivity. That final conclusion is missing. [Fons Baede]	Text has been revised, as summary statement has been added.
9-1275	A	61:10	62:35	Should say briefly somewhere here (or reference back to something in the main text if it's too cumbersome here) that "unlikely" is not absolute - we Bayesians would say that one should not think it "unlikely" if one had a strong enough a priori reason for thinking it was natural variability, & some people do, for religious or other reasons. [William Ingram]	Rejected. This seems a bit too subtle a point to address in a FAQ. There will be a box explaining the uncertainty language in the TS.
9-1276	A	61:43	61:43	"solar output was likely increasing" - the longer-than-decadal timescales are presumably meant, but not stated. Anyway, is "likely" really justified without circular logic - if we had no evidence of a solar effect on climate, why would we expect more solar variation than, say, Lean Wang & Sheeley's model gives? I don't see the 2-1 confidence that "likely"	Reassessed.

Comment [GH5]: Peter Neville  
CHECK



No.	Batch	Page:line		Comment	Notes
		From	To		
				requires - even 50-50 - *in the absence of circular thinking* - seems to violate Occam's Razor. [William Ingram]	
9-1277	A	61:47	62:8	8 better followed by statement that it would be unlikely that the internal variations happened to match the modelled patterns of climate change response - this is not explicitly stated in 47-52 which is where the text comes closest - shift 47-52 after 8 & make it explicitly refer to the unlikeliness [William Ingram]	Reassessed.
9-1278	A	62:1		Question 9.1: Strongly suggest the wording of this answer be reconsidered to avoid explicitly raising another question that we label as 'ill-posed' and then posing what we think is a better one. A present the answer is dealing with three questions, which makes it very complex. The short answer paragraph would be better starting out with more direct and simpler answer. [David & David Wratt & Fahey]	Agreed. Re-written to avoid this.
9-1279	A	62:3	63:6	By definition, individual events require individual explanations. But if the card deck is being deliberately stacked with either high or low cards, that information should be of practical use to individual playing the game. So, the actual physical changes that take place need to be clearly pointed out. If the sea level rises, then the wave that the levee was built to withstand, may now go over the top. A warmer atmosphere is going to contain more water vapor. So, a stronger hydrological cycle is a likely expectation. Hurricanes have warm ocean water as their basic source of energy. So, warmer ocean water and stronger hurricanes are again a likely expectation. [Andrew Lacis]	Agreed, but this is what we say (when you look at the answer in its entirety).
9-1280	A	62:3	64:33	These questions should be part of the Key Findings at the beginning of the text, not buried in back of the report. [Thomas Karl]	I don't think they will be buried in the final assessment.
9-1281	A	62:3		Question 9.1 I find it odd that you focus exclusively on harmful extreme events that will probably/possibly increase in frequency. Perhaps most extremes are damaging, but what about the likelihood of reduction in cold extremes? [James Annan]	Agreed. We now mention decrease in frosts.
9-1282	A	62:5	62:6	The statement that "Almost any weather event might occur by chance" is simply untrue and not at all helpful--one can name all sorts of things that cannot occur in particular locations, etc. I would suggest just deleting this sentence [Michael MacCracken]	Sentence deleted and paragraph rewritten.
9-1283	A	62:5	62:15	This paragraph (the "headline answer") should be in italics. [David & David Wratt & Fahey]	Agreed and done.
9-1284	A	62:6	62:7	I would suggest starting the paragraph with the second sentence, revised to read: "Human	Paragraph has been rewritten, so

Comment [GH6]: Peter Neville look at this for the questions!



No.	Batch	Page:line		Comment	Notes
		From	To		
				influences on climate of the size already underway will change the likelihood and locations of the occurrence of such specific events as heat waves, flooding, and storms." Note that I use the word "will" here as it is certain that there will be changes--even if we do not know precisely what they will be. [Michael MacCracken]	comment is not relevant.
9-1285	A	62:8	62:8	I really think that instead of the word "risk" the text should be using "likelihood" or "chance of occurrence"--let's keep "risk" for describing changes in the likelihood of potential impacts. [Michael MacCracken]	Have reduced the use of "risk" but it is a well-accepted term in the context it is used here.
9-1286	A	62:9		Replace "evidence" by "indications" [Vincent Gray]	Rejected – the published studies do provide "evidence".
9-1287	A	62:10	62:10	I believe saying "suggesting a possible increase in the risk of heat waves" is much too cautious--according to the IPCC lexicon this means a 50-50 chance, and this seems to me very understated. I would suggest that it is at least very likely that there will be an increased likelihood of heat waves. [Michael MacCracken]	The use of "possible" here is meant to suggest a theoretical possibility, rather than an estimate of the likelihood of an increase..
9-1288	A	62:10	62:12	This sentence is redundant with l. 39-41, same page [Martine Rebetez]	Agreed, but this comes from the structure of the answer, with a one-paragraph summary followed by an elaboration in several paragraphs.
9-1289	A	62:11	62:11	I would suggest deleting "could"--perhaps say "very likely" but when one has something like a 5-sigma occurrence, this is a lot more than "could" [Michael MacCracken]	"Could" seems appropriate here – given that only one study has been done.
9-1290	A	62:11	62:12	Heat wave missing? [Jouni Räisänen]	No.
9-1291	A	62:12	62:12	Again, this seems very cautious. An increase in intense rainfall is already very clear in many places, just as one example. [Michael MacCracken]	But not everywhere. And the data are poor.
9-1292	A	62:13		Replace "is likely" by May" [Vincent Gray]	I don't understand why the reviewer suggests this change.
9-1293	A	62:21	62:21	hurricane seasons of 2004 and 2005. [Thomas Knutson]	Agreed. Done.
9-1294	A	62:21	62:21	Now need to say the "hurricane seasons of 2004 and 2005." [Michael MacCracken]	Agreed. Done
9-1295	A	62:21	62:21	Replace 'season of 2004' by 'seasons of 2004 and 2005' [Javier Martin-Vide]	Agreed. Done.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1296	A	62:25	62:27	This sentence is not really very clear, with its double negative. [Michael MacCracken]	Agreed. Sentence has been rewritten.
9-1297	A	62:25		Suggest omitting or defining the terms and their relevance: persistent blocking high pressure system, anticyclonic subsidence, and vertical mixing. [David & David Wratt & Fahey]	Terms are not too technical for the audience intended.
9-1298	A	62:30	62:31	This sentence is again trying to cover all possible bases and insisting on very high statistical certainty. While it might be possible to get a 5-sigma variation (se have no record of such an occurrence), it is very, very unlikely--so use the IPCC lexicon here and alter the sentence, saying perhaps, given the statistics we do have, that it is "exceptionally unlikely that such factors would arise in the absence of a strong human influence." [Michael MacCracken]	Paragraph has been rewritten and sentence deleted.
9-1299	A	62:33		Replace "probability" by "chances" [Vincent Gray]	Don't understand rationale for reviewer's suggestion.
9-1300	A	62:34	62:35	Clearer wording would be "For example, for the European heat wave this question can be addressed by studying ..." [Michael MacCracken]	Reviewer's suggested rewording is more ambiguous and clumsy than current text.
9-1301	A	62:37		Replace "very likely" by "possible" [Vincent Gray]	"very likely" is, if anything, conservative.
9-1302	A	62:39	62:41	This sentence is redundant with l. 10-12, same page [Martine Rebetez]	See 9-1288.
9-1303	A	62:39		Replace "likelihood" by "possibility" [Vincent Gray]	Why?
9-1304	A	62:40	62:40	Again, change "risk" to "likelihood" and also say "temperature being as hot as" [Michael MacCracken]	Prefer current text.
9-1305	A	62:41	62:41	Change "modelling work" to "analysis and modelling" [Michael MacCracken]	Why?
9-1306	A	62:45	62:45	Change "risk" to "likelihood"--also on line 48 [Michael MacCracken]	Done.
9-1307	A	62:45		I am not convinced that this question is correctly posed. Under a significantly changing climate, it is not clear that "is there an increase" is appropriate - firstly, there may be a decrease, and the null hypothesis of no change has no privileged status, and may lead to a rather sterile debate over detection and level of proof. Instead, it seems that "how much is the probability changing" will often be more appropriate, and an estimate of a modest change, even if not formally distinguishable from zero via observations, is still a valid and potentially useful answer. [James Annan]	Agreed. Text changed to reflect both points.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1308	A	62:48	62:52	It would be useful here to be calling for more examination of the weather in the model simulations--after all, we live the weather, and climate is a mental artifact. We should be checking to ensure the models are getting the weather variability right. [Michael MacCracken]	The paragraph does say this.
9-1309	A	62:55		Please discuss rainfall characteristics, not just amount, but also frequency, intensity, duration, type. [Kevin Trenberth]	This sentence is about the characteristics, ie intensity, rather than total rainfall.
9-1310	A	63:1	63:6	This is weak. Dealing more with changes in intensity that are robustly observed and simulated would be a better focus. I disagree with the last sentence. [Kevin Trenberth]	Where is the "convincing evidence", if the reviewer thinks this sentence is wrong?
9-1311	A	63:2	63:2	Need to change "Predict" to "project" [Michael MacCracken]	"Project" is horrible in this context.
9-1312	A	63:4	63:4	Is in not the case that the increased intensity of precipitation has been detected on most continents, which is a good bit more than "in at least some regions" [Michael MacCracken]	The increased intensity is very variable between regions and continents.
9-1313	A	63:4	63:6	Again, we have jargon used to disguise a very tight statistical test when any normal evaluation of the information would indicate that there is a very strong circumstantial, at least, case of a linkage--and there are no other suspects. This statement is simply much too cautious. [Michael MacCracken]	There are other suspects – stochastic variability could explain why there have been increases in some (but far from all) regions, even in the absence of climate change.
9-1314	A	63:12	63:14	Question 9.2: Can the Warming of the 20th Century be Explained by Natural Variability? Unlikely. I respectfully suggest that the tone of this response is too casual; one wishes to convey the feeling that the response is based on a thoughtful and considered judgment that rests on a preponderance of much evidence. Better something like: The warming over the last century that is very unusual in both magnitude and rate relative to other periods in the last millennium according to estimates deduced from proxy records of temperature such as tree rings and ice cores. This rapid warming is consistent with our physical understanding of how the climate would respond to a rapid increase in greenhouse gas concentrations. ... [Stephen E Schwartz]	Agreed. Paragraph has been rewritten to adopt this approach.
9-1315	A	63:12	64:31	There may well be legitimate questions of just how the climate system may vary under "no applied radiative forcings", or how quickly and by how much it may respond to a given radiative forcing. Not all of the forcings and feedbacks are known with unquestionable accuracy. But the documented changes in CO <sub>2</sub> , CH <sub>4</sub> , N <sub>2</sub> O, and CFCs are very accurately known - their precision is such that no greenhouse skeptic would really	Agreed. Paragraph has been rewritten to adopt this approach.



No.	Batch	Page:line		Comment	Notes
		From	To		
				question their accuracy. But these documented changes in GHGs have equally well defined radiative consequences. These are based on laboratory measurements that are again beyond question. As a result, the greenhouse effect of the Earth's atmosphere has been made significantly stronger, and there is no alternative but for the global surface temperature to warm in order to re-establish energy balance with the absorbed solar radiation. All this has to be accounted for in the observed temperature record, as of course it has been in many climate change simulations conducted with climate GCMs that include all of the relevant physics and climate forcings. [Andrew Lacis]	
9-1316	A	63:12		Replace "probablity" with "chances" [Vincent Gray]	Why?
9-1317	A	63:14	63:14	"Unlikely": this is too weak when compared with the findings in the rest of the chapter. Table 9.1.1 (and elsewhere) suggests that it are should either be "very unlikely" or "extremely unlikely", depending on what is meant by 'natural variability'. [Robert Colman]	Agreed. Word deleted. Replaced by "very unlikely" in final sentence of paragraph.
9-1318	A	63:14	63:14	Alxexander" -> "Alexander [William Ingram]	Corrected.
9-1319	A	63:14	63:14	While I favor full sentences, if there is going to be a brief answer, it needs to be "Very unlikely" rather than just "unlikely". With the additional warming since the TAR and the lack of changes in natural forcing that can explain the ongoing warming, and with the TAR already having said "very unlikely" about internal forcing, the answer is quite justifiably "very unlikely" [Michael MacCracken]	Agreed. See 9-1317.
9-1320	A	63:14		Not "very unlikely"? [James Annan]	Agreed. See 9-1317.
9-1321	A	63:14		Replace "a rapid" by "an intermittent" [Vincent Gray]	Why?
9-1322	A	63:14		Replace "very" by "rather" [Vincent Gray]	On what evidence should this change of terminology be based?
9-1323	A	63:14		Last half-century rather than century? [Kevin Trenberth]	Some of the warming started before 1950.
9-1324	A	63:14		Suggest that this intro paragraph be shortened and simplified. Suggest omitting terms 'dominant external forcings' and 'proxy'. Suggest comparing closely with 6.2 to make consistent and avoid overlap. [David & David Wratt & Fahey]	Difficult to shorten and still retain essential aspects of the answer, in a single paragraph. Have replaced "proxy".
9-1325	A	63:15	63:15	Change to read "reconstructions of temperature deduced from proxy records" to be clearer	Agreed. Sentence rewritten.



No.	Batch	Page:line		Comment	Notes
		From	To		
				about what is being used--they are not just "estimates" [Michael MacCracken]	
9-1326	A	63:16		Insert "more or less" after "is" [Vincent Gray]	Why?
9-1327	A	63:17	63:17	Add to the end of the sentence "and not consistent with a response to natural forcings." [Michael MacCracken]	Agreed. Done
9-1328	A	63:20		Insert "reasonably" before "good" [Vincent Gray]	Why?
9-1329	A	63:28	63:28	I would suggest "human-induced" instead of "man-made" [Michael MacCracken]	Agreed. Done.
9-1330	A	63:30	63:30	Change "stable" to "near stable" [Michael MacCracken]	Agreed. Done.
9-1331	A	63:31		Delete "of sulphur and other chemicals" Aerosols are mostly water, carbon, or salt [Vincent Gray]	Don't understand reviewer's point or relevance.
9-1332	A	63:37	63:38	Delete "of sulphur and other chemicals" Aerosols are mostly water, carbon, or salt [Vincent Gray]	See 9-1331.
9-1333	A	63:37	63:37	Change "much" to "a large fraction" and also "changes" to "change" and "are" to "is" [Michael MacCracken]	Agreed. Done.
9-1334	A	63:37		Is this true? Look at Atlantic multidecadal oscillation and lack of explanation for it. [Kevin Trenberth]	Not for global temperatures.
9-1335	A	63:43	63:43	After "started to rise" add "and before tall stacks dramatically increased lifetimes of SO2 and sulfate" [Michael MacCracken]	Agreed. Done.
9-1336	A	63:48		Insert after the troposphere) "starting in 1997, because of the 1998 El Niño event" [Vincent Gray]	This is incorrect.
9-1337	A	63:50		Replace "only" by "entirely" [Vincent Gray]	This would invert the meaning of the sentence and be incorrect.
9-1338	A	63:51		Insert "they" after "but" [Vincent Gray]	Agreed. Done.
9-1339	A	64:3		Delete "and" [Vincent Gray]	Agreed. Done.
9-1340	A	64:3		Insert after "aerosols" "urbanization and land-use changes" [Vincent Gray]	These are less important on a global-scale, even though they are important in some locations.
9-1341	A	64:7	64:8	"... are very unlikely to have produced the observed warming." Is too weak when	Agreed. Replaced "very" with



No.	Batch	Page:line		Comment	Notes
		From	To		
				compared with the findings and the rest of the chapter. Table 9.1.1 (and elsewhere) suggests that it should be "extremely unlikely". [Robert Colman]	"exceptionally"
9-1342	A	64:7		Delete "very" [Vincent Gray]	See 9-1341
9-1343	A	64:22		Insert "entirely" before "explain" [Vincent Gray]	This would suggest, incorrectly, that it could explain a large part of the observed warming.
9-1344	A	64:26	64:26	Change "the last 1000 years" to "at least the last 1000 years" as the record is going back further. [Michael MacCracken]	Agreed. Done.
9-1345	A	64:27	64:27	Add phrase at end of sentence saying, "forcing, which have been much smaller than the changes in human-induced forcing over the last 100 years." [Michael MacCracken]	Agreed. Done.
9-1346	A	64:29	64:31	This needs to be a stand alone, clear sentence--right now it seems overly cautious and use of the word "they" makes it all rather unclear what is being said. [Michael MacCracken]	Deleted last sentence of this paragraph – it seemed to be a summary sentence and the first paragraph of the answer does this.
9-1347	A	64:31		Add at beginning "more or less" [Vincent Gray]	See 9-1346
9-1348	A	65:0		Missing ref: J. D. Annan, J. C. Hargreaves, R. Ohgaito, A. Abe-Ouchi, S. Emori. Efficiently constraining climate sensitivity with paleoclimate simulations. SOLA Vol 1 pages 181-184, 2005 (probably) [James Annan]	Noted.
9-1349	A	65:0		ADDITIONAL REFERENCES for CHAPTER 9: (To be inserted appropriately); See above remarks. * Beran, J., 1994: Statistics for long-memory processes. Chapman & Hall, New York. * Ghosh, S., Beran, J., Innes, J., 1997: Nonparametric conditional quantile estimation in the presence of long memory. Student 2: 109-117. * Beran, J., Ghosh, S., 1998: Root-n-consistent estimation in partial linear models with long-memory errors. Scandinavian Journal of Statistics, 25, 345-357. * Ghosh, S., Draghicescu, D., 2002a: Predicting the distribution function for long-memory processes. International Journal of Forecasting, 18, 283-290. * Ghosh, S., Draghicescu, D., 2002b: An algorithm for optimal bandwidth selection for smooth nonparametric quantiles and distribution functions. In, Statistics in Industry and Technology: Statistical Data Analysis based on the L1-norm and related methods, Birkhäuser Verlag, Basel, Switzerland, pp. 161-168.	Noted.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Sucharita Ghosh]	
9-1350	A	65:0		With regard to referencing whenever possible, out-dated papers should not be included or flagged with a "see 'Doc et al.' (20NN) for revised results". Known errors or inaccuracies should be flagged in a reference. For example relative to von Storch (2004), a highly publicized critique of Mann et al., Zorita and von Storch have lately submitted to Memorie della Societa Astronomica Italiana, July 31 2005 "Methodical Aspects of reconstructing non-local historical temperatures". In this paper they admit, that while purporting to follow the Mann et al procedures, they in fact used calibration scale factors qualitatively differing from those used by Mann et al, a step invalidating their conclusion relative to Mann et al. Attached to such references should be a warning of the type "see later work of (Zorita and von Storch ...) for modifications to this paper's conclusions". [David Ritson]	Noted. The discussion about the validity of the von Storch criticism is dealt with by chapter 6., the paper is no longer cited.
9-1351	A	71:14	71:14	"G Jones" should be "G S Jones", surely? [William Ingram]	Noted.
9-1352	A	71:24	71:25	The complete reference to Haarsma et al is: Haarsma, R.J., F.M. Selten, S.L. Weber, M. Kliphuis, 2005: Sahel rainfall variability and response to greenhouse warming. Geophys. Res. Lett., 32, L17702, doi:10.1029/2005GL023232. [Reindert Haarsma]	Noted.
9-1353	A	73:6	73:8	Correction to name and paper is now published. Reference should be:- Jones, G.S., A. Jones, D.L. Roberts, P.A. Stott, K.D. Williams, 2005: Sensitivity of global scale attribution results to inclusion of climatic response to black carbon. Geophysical Research Letters, 32, L14701, doi:10.1029/2005GL023370  [Gareth S. Jones]	Noted.
9-1354	A	76:1		Insert References: McIntyre, S., and McKittrick, R., 2003. Corrections to the Mann et al. (1998) proxy data base and Northern Hemisphere average temperature series" Energy and Environment 14, 751-771. Also McIntyre, S., and McKittrick, R., Hockey Sticks, principal components, and spurious significance . 2005. Geophysical Research Letters 32 L03710.doi:10.1029/2004GL021750 [Vincent Gray]	Noted.
9-1355	A	77:55	77:55	The correct spelling is "Räisänen", not "Raisanen" [Jouni Räisänen]	Noted.
9-1356	A	78:17		The full bibliographic details of this publication are: Pezza, A. B., and I. Simmonds,	Noted.



No.	Batch	Page:line		Comment	Notes
		From	To		
				2005: The first South Atlantic hurricane: Unprecedented blocking, low shear and climate change. Geophysical Research Letters, 32, L15712, doi: 10.1029/2005GL023390. [Ian Simmonds]	
9-1357	A	80:14	80:15	add reference "Schneider von Deimling, T., H. Held, A. Ganopolski, and S. Rahmstorf, 2005: Climate sensitivity estimated from ensemble simulations of glacial climate. Clim. Dyn., submitted." [Hermann Held]	Noted.
9-1358	A	81:24	81:24	Add reference to Stendel et al. (2005a) after line 23: Stendel, M., I.A. Mogensen and J.H. Christensen, 2005a: Influence of various forcings on global climate in historical times using a coupled AOGCM. Clim. Dyn. 25, 10.1007/s00382-005-0041-4. [Martin Stendel]	Noted.
9-1359	A	81:34	81:35	It appear that this paper has now been published in Geophysical Research Letters, not Journal of climate. [Gareth S. Jones]	Noted.
9-1360	A	84:1	84:1	The second half of the line seems to have got slightly corrupted. [William Ingram]	Not clear what this refers to.
9-1361	A	84:29		The reference for Wu et al. (2005a) is wrong. It should be: Wu, P., R. Wood and P. Stott, 2005: Human influence on increasing Arctic river discharges Geophys. Res. Lett., 32, L02703, doi:10.1029/2004GL021570. [Peili Wu]	Noted.
9-1362	A	84:31	84:31	More accurately, from the unforced variation of CGCMs, which may be from control runs or from comparing (& scaling) the intra-ensemble variation of runs with the same forcing. Perhaps this is too detailed though. [William Ingram]	Revised – thank you.
9-1363	A	84:35	84:35	The bias is not intuitive: a reference (Hegerl?) should be given as it can't be quickly explained [William Ingram]	Reference added.
9-1364	A	84:37	84:37	As previous comment but one [William Ingram]	Revised – thank you
9-1365	A	84:46	84:48	I don't know what is meant by "deductive ... change)" [William Ingram]	Revised.
9-1366	A	84:54	85:1	Usually it is required to be non-zero in the physically possible direction (unless either sign is physically possible, obviously) [William Ingram]	Revised – thank you.
9-1367	A	86:10	86:12	True as written, but surely worth mentioning climateprediction.net's use of a full GCM, & Murphy et al's use of a linear emulator of a full GCM	Not clear what this refers to.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[William Ingram]	
9-1368	A	86:10	86:42	Turning to the methodology, line 16 says "u is a realization of internal climate variability", which, based on the material on page 8, I assume means it is an output of a climate model. But then line 17 says it is a "Gaussian random vector with a covariance matrix C," which implies it is a vector of white noise. These statements are contradictory to each other, and are further contradicted by the GLS formula in line 18, which is derived assuming u is a regression residual. In these 3 lines you have given 3 conflicting definitions of the vector u. Is it a model-generated internal variability vector, or a vector of white noise, or a regression residual? I consider each possibility in turn in the next 3 cells. [Ross McKittrick]	Disagree. Why should the Gaussian assumption imply that the elements of u form a white noise time series? Typically vector u is composed of information in both space and time. We agree that there was some impression in our language that has been corrected.
9-1369	A	86:10	86:42	If u is a model-generated estimate of internal variability, then in order to solve for the coefficient vector a there must either be a further residual vector in the regression equation or the equation is a perfect fit. But if it's a perfect fit then there would not be any error bars around the signal coefficients. So that's not it. Hence there must be an unstated residual vector. But the formula given for the coefficient vector a is derived by minimizing the sum of squared u terms ( $u'u$ ), not the sum of squared residuals. This makes no sense as presented. The other possibility is that u is in the regression equation and its coefficient is assumed to be 1, so the regression equation is $y=u+Xa+e$ , where e is a residual. But then the formula for a should be the usual restricted least squares formula, $a = \text{inv}(XX)X'y + \text{inv}(XX)R'[R.\text{inv}(XX)R'](r-Ra)$ where R is a matrix with 1 in the top left corner and 0 everywhere else and r is a vector with first element 1 and 0 everywhere else (see any econometrics text under the heading "linear restrictions", e.g. Johnson 1984 pp 204-05). [Ross McKittrick]	Disagree. U is not model generated. As in all regression problems, U is not observable, but can be estimated by calculating the residual that remains after fitting the signal (the independent variables) to the observations contained in y. The covariance matrix that is required to estimate the regression coefficients is estimated from a sample of control run segments, each processed and masked in the same way as the observations. These samples (pseudo residuals if you like) are free of the effects of forced signals and thus can be treated as realizations of U. A residual consistency test is usually applied to compare the internal variability estimated from the climate model in this way with the variability of the residuals obtained from the regression analysis.
9-1370	A	86:10	86:42	If u is a Gaussian noise vector then C is an identity matrix, or an identity matrix multiplied by a constant (sigma) if the noise process is not scaled to a unit variance. And if this is the case there also has to be a further residual term in the regression unless there's a perfect fit. Then the a vector should follow the restricted least squares formula as above. [Ross McKittrick]	See responses to 9-1368 and 9-1369.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1371	A	86:10	86:42	But actually it appears to me that $u$ is a regression residual, which would explain why $a$ is presented herein as the standard GLS estimator. But this means that the "attribution" model works by minimizing the internal variability estimate! This makes the test worthless, just as would be a model that worked by minimizing the 'GHG' signal. [Ross McKittrick]	Disagree. One would then dismiss all applications of regression analysis for the same reasons.
9-1372	A	86:10	86:42	Also, and equally worrisome, this is a simple GLS model being applied to time series data, guaranteeing that the standard errors are going to be biased too small. But most problematic of all is the fact that the regression model includes endogenous variables on the right hand side. The signal series are jointly determined with the temperature series, unless you assume, implausibly, that the temperature response to forcing is independent of temperature itself. All the usual statistical properties of least-squares coefficients, including those properties necessary to support the hypothesis tests as stated in Appendix 9.A.2 assume that an endogenous variable is regressed on exogenous variables. This is not the case in the attribution model, and standard regression theory shows that the coefficients $a$ are inconsistent (see Kmenta, Elements of Econometrics 1986, p 339-341, for example). [Ross McKittrick]	Disagree. The forcing, and thus the response, is exogenous (at least on time scales that have been considered in detection and attribution research).
9-1373	A	86:31	86:31	Only a reader who already knows all this stuff know what "optimally" means. I suggest a parenthesis "(i.e. the test is designed to maximize its power - the chance of detection if there genuinely is something to detect - on standard statistical approximations, without introducing any bias towards detection)" [William Ingram]	Optimal fingerprinting is explained in Appendix 9.A.1, so we feel there is no need to explain again in Appendix 9.B
9-1374	A	86:52	86:52	"subjective" yes - and it can matter, which should also be said! [William Ingram]	Text not revised. The purpose of the Appendix is simply to describe the methods rather than to assess them. We agree that it can matter, and think that is implicit in pointing out that priors can be based on subjective information.
9-1375	A	87:12	87:14	"Also, Bayesian inferences are probabilistic (i.e., based on the posterior likelihoods of detection and attribution), which means that they can better feed into decision making processes that balance risks and benefits." : I do not think, and I know no reason to say, that probabilistic results can better feed into decision making process, in particular because it creates insolvable problems to aggregate risk-aversions of individuals into a "global" risk-aversion. Only a "political" decision-making framework (e.g. democracy) can use such results in a satisfying manner. This sentence should be removed. [Stéphane Hallegatte]	Text deleted.
9-1376	A	87:26	87:29	Make it explicit why this matters - the result of the expert "prior" & the information added	Noted. We feel the text is sufficiently



No.	Batch	Page:line		Comment	Notes
		From	To		
				having basically the same information content is that the narrowing (from an "uninformative" or "true" prior) which that information can give is used twice, tending to spuriously halve the resulting apparent uncertainty [William Ingram]	clear as written.
9-1377	A	87:26		Two approaches to assess and quantify climate change involve nonparametric curve fitting methods for (a) a trend function, that is simply the average of a stochastic process over time (or more generally in space and time) and (b) (space-) time dependent estimation and prediction of the underlying probability distribution function (pdf). Consider for instance the time series case. In this case, typically, stationarity would be assumed. Meanwhile, however there have been efforts to model nonstationary processes where the pdf may change arbitrarily with time, without falling into the Gaussian or any other known distributional categories. Moreover, the correlations may decay hyperbolically and not exponentially as is typically assumed, i.e. there may be long-memory (Beran 1994). The idea of long-memory is related to self-similarity and fractals and may create spurious trend like phenomenon where there may be no trend at all. Beran & Ghosh (1998), Ghosh et al. (1997), and Ghosh & Draghicescu (2002 a & b ) take these issues into account. [Sucharita Ghosh]	Noted. However, the purpose of this Appendix is not to explain detection techniques in general, but rather to explain the specific optimal detection technique that has been used extensively in detection and <i>attribution</i> research.
9-1378	A	89:11	89:22	see my comments #9. [Hermann Held]	See response there, discussion of prior has been revised according to several comments.
9-1379	A	90:0	90:0	Upper-right cell in Table 9.1.1: "Anthropogenic change" should apparently be "Forced change". [Jouni Räisänen]	Rejected. The intent here is to talk specifically about the response to anthropogenic forcing.
9-1380	A	90:0	91:	The "Likelihood" estimates are all exaggerated and purely subjective. Actual figures should be removed. [Vincent Gray]	Likely assessments have been reconsidered. These assessments are the result of carefully considered expert judgement, as is appropriate in an assessment.
9-1381	A	90:0		Table 9.1.1 Are you not prepared to drop "known" in the first two boxes? What do you estimate is the likelihood of some presently unknown factor overturning these conclusions? Your readers would surely be more interested in the version with "known" removed, even if the confidence level is reduced. Indeed, if the confidence level is reduced as a result (ie there is significant belief that as yet unknown factors will overturn these conclusions) then it would in my opinion be misleading not to state that belief clearly. [James Annan]	Assessment in first box revised. Known retained in second box.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1382	A	90:4	90:4	Suggest removing the word "conservative" here and in the underlying text and replacing with whatever is meant by this undefined term. All attribution studies are contingent on poorly defined sets of assumptions, and also omit some constraints and knowledge. It is important for this chapter and this table to have a traceable account of how the judgments were formed based on implicit assumptions. [Haroon Kheshgi]	Caption revised.
9-1383	A	90:8	90:8	In Table 9.1.1(a), several of the statements seems unduly cautious. For example, it is not only "virtually certain" that internal climate variability was solely the cause of the warming during the past half century, it is physically impossible given the increase in CO <sub>2</sub> , just as it is more than "very likely" that the warming of the last half century is solely due to known natural causes. As long as "solely" is in there, there is no question. I would really urge rephrasing these in terms of the likelihood of human influences being larger than natural influences--so make the statements comparative--and then one can reasonably use the IPCC lexicon. With respect to the fourth comparison, it should be clarified to say something like "observed warming during the latter half of the 20th century was less than would have been expected were GHGs the sole influence"--but one does have to have some clarification because there is no such thing as simply "greenhouse warming"--there is an influence, but not just this warming. In the seventh row, what does "significantly affected" mean--the variations in global average temperature were likely about 0.5 C or less, so if this is a significant effect, then GHG warming is very, very significant. With respect to the bottom row on this page, was there a change in the likelihood of the warmest day, even if one did not detect an increase in the maximum? [Michael MacCracken]	Likelihood assessments have been carefully reviewed and in some cases revised.
9-1384	A	92:0		Table: I strongly disagree with several items in this table (but there is no easy way to refer to them): items on tropical cyclones, rainfall changes, heavy rainfall changes, and drought need major revisions. [Kevin Trenberth]	See 9-1383.
9-1385	A	92:2	92:2	In Table 9.1.1(b), the third row on tropical cyclones needs to be updated with the papers by Webster et al. and Emanuel, as there seem to be clear indications of increasing intensity and overall intensity. Regarding the fourth row, the phrase "latter half 20th century" is too broad--the detection, if any, is in the year or so after the volcano--not across the latter half of the century. And in the reasoning for this point, there seems to be much more latitude here than for GHGs--allowing justification based on "theoretical understanding" for volcanoes, which are a much smaller influence than GHGs, seems really to be allowing alternative possibilities much more favor than is the case for GHGs. Then in the next row, the statistical rigor seems to have gone way up--justification by theoretical understanding is apparently not allowed (though all models show it, as I	See 9-1383.



No.	Batch	Page:line		Comment	Notes
		From	To		
				understand it, although their resolution is limited). In my view, there is a bias in the table explanations that is requiring a higher level of evidence for GHGs than for other types of forcing. Then in the next to last row, this goes way too far, suggesting that a change in NH sea ice extent has been found in only one study--and saying that the reduction in sea ice is only "likely" is really absurd--it is very, very evident; is this again requiring some very high statistical test or something--this seems very understated compared to the next row about glacier retreat, for which there is a positive link and "very likely" result. In my view, there needs to be a serious re-consideration of this table. [Michael MacCracken]	
9-1386	A	92:8	92:8	Table entry #3 on tropical cyclone intensity. Add following: Observed trends have additional uncertainties due to substantial inhomogeneities in tropical cyclone data sets, for which corrections have been attempted. [Thomas Knutson]	Accepted.
9-1387	A	93:0	93:0	Median would be more practically relevant and more difficult to misinterpret than mode. [Jouni Räisänen]	Median is more sensitive to choice of prior.
9-1388	A	95:7	95:7	I think this would work better if the table axes were reversed, so list the studies vertically. [Michael MacCracken]	Axes reversed.
9-1389	A	96:0		General comment about figures: many of the figures have incomplete legends [Christoph Schar]	Figures improved.
9-1390	A	97:0		Figure 9.2.1. Is the temperature change calculated as a linear trend or the difference between 2 time periods? The caption describes the plot as extending from 75S to 75N but the figure seems to indicate temperatures from pole-to-pole. [Matthew Collins]	Clarified.
9-1391	A	97:0		Fig.9.2.1 Please clarify in caption what this figure shows. The caption states that the figure shows "Zonal mean temperature change during the 20th century (deg. C/century) ..." What is the change compared to, since the change itself is an average value across the 100 years from 1900 to 2000? Is the near surface air temp? Also, please add labels and units to X and Y axes and to color bars. [Melinda Marquis]	Revised.
9-1392	A	97:0		Fig. 9.2.1. Y axes need labels, p (mb) on the left and z (km) on the right. [Alan Robock]	See 9-1391
9-1393	A	97:0		Figures 9.2.1 and 9.2.2. I am concerned over this mixing of models and forcings. There are GCMs which have been run with the suite of forcings. I am unsure whether any single model has had the whole suite of single forcings experiments applied. However, if it has then runs from this should be used. Also, could very small departures from zero be masked in these diagrams otherwise they are potentially misleading as to where forcings have important impacts.	Single model not available.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Peter Thome]	
9-1394	A	98:0	98:	Caption of Fig. 9.2.2: It should be stated that the units are degrees C (and that this is the difference at equilibrium of a present-day minus a preindustrial simulation of the CSIRO atmospheric model coupled to a q-flux ocean model). [Leon Rotstayn]	Revised – thanks.
9-1395	A	98:0		Figure 9.2.2. What time period over which the model is forced? Is this a trend, a zonal mean? More information please. [Matthew Collins]	See 9-1394.
9-1396	A	98:0		Fig.9.2.2 Add labels a and b to graphics. On right-hand graphic, Y axis label is "height." Is this height above Earth, e.g., in atmosphere? Please clarify. Also, please add labels and units to color bars. [Melinda Marquis]	Revised.
9-1397	A	98:0		Caption to Fig.9.2.1: use same contour interval as in Fig.9.2.1 [Christoph Schar]	See 9-364.
9-1398	A	99:0		Figure 9.2.3. What are the anomalies calculated with respect to? What forcing scenarios are used in the model simulations? Are their errors associated with the observations? [Matthew Collins]	Caption revised. Text discusses uncertainties.
9-1399	A	99:0		Fig. 9.2.3 Please explain abbreviations. [Melinda Marquis]	Revised.
9-1400	A	99:0		Provide units [Christoph Schar]	Done.
9-1401	A	99:5	99:6	Figure 9.2.3: It might be worth noting in the caption that some of the AR4 models of the figure do not use any volcanic forcing, which explains the large discrepancies between models and observations around 1991-93 (Pinatubo). Also, please add an horizontal line on the figure showing the "zero anomaly" line. Finally, please correct the typo ("ISCCP", not ISSCP). [Sandrine Bony]	Revised.
9-1402	A	99:5	99:6	It would be helpful to the less sophisticated reader to indicate that the large blip is due to the Pinatubo eruption. [Michael MacCracken]	This is mentioned in the text.
9-1403	A	100:0		Fig. 9.3.1 In first sentence of caption, consider writing "1000 year" instead of "1k year." [Melinda Marquis]	Revised.
9-1404	A	100:0		Please explain all curves, including CH-blend. Also, ECGI-G and EBM are barely distinguishable [Christoph Schar]	Noted and revised
9-1405	A	100:0		Figure 9.3.1 What is CH-blend? It is not covered in the caption or the accompanying text	Revised.



No.	Batch	Page:line		Comment	Notes
		From	To		
				as far as I can work out. [Peter Thorne]	
9-1406	A	101:0		Figure 9.4.1. I suspect these are global mean temperature anomalies, not temperatures as described in the caption. [Matthew Collins]	Revised.
9-1407	A	101:0		Figure 9.4.1 caption line 4 The quantity plotted is global mean temperature anomaly, not Global mean temperature, as stated. It is not clear what the anomaly is relative to. Presumably a base case over, perhaps, 1900 to 1920 in the observations, and perhaps relative to a model run or runs at the mean conditions of the unperturbed system. This must be specified. [Stephen E Schwartz]	Revised.
9-1408	A	101:1	101:8	Figure 9.4.1: Add simulation by Stendel et al. (2005a). [Martin Stendel]	Added.
9-1409	A	102:0		Figure 9.4.2. These figures may be clearer if block-filling is used rather than contouring. It is obvious to me that the observational data mask has been put over the model results but it should probably be pointed out in the caption. [Matthew Collins]	Figure replaced.
9-1410	A	102:0		Fig. 9.4.2 In caption, refer to observations (left) and simulations (right). Please add label and units to color bar. [Melinda Marquis]	Figure replaced. Caption adjusted appropriately.
9-1411	A	102:0		Stott (2000) provided spatio-temporal maps showing 'Observed', 'Simulated' and 'Simulated-Observed' data. Figure 9.4.2 would be much more informative if a third column of 'Simulated-Observed' maps, additional to the columns containing observed and simulated results, was provided. [David Ritson]	Figure replaced.
9-1412	A	103:0		Figure 9.4.3. Were annual mean values used? How were trends removed from the observed record? [Matthew Collins]	Revised.
9-1413	A	103:0		Fig.9.4.3 Does more than one (i.e., the GISS-EH) significantly over- or under-estimate ...? If not, why is this stated in the plural? [Melinda Marquis]	Revised.
9-1414	A	103:5	103:5	Only one GCM is marked with an asterisk. [Michael MacCracken]	Revised.
9-1415	A	103:6		Replace "10%" with "5%". Increase all the error bars in the figure to comply with 95% accuracy [Vincent Gray]	We now show 95% confidence bands.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1416	A	104:0		Figure 9.4.4. Obvious I know, but the uncertainty bars cross the graph boundaries in the penultimate column (obscuring the axis label). [Matthew Collins]	Figure replaced.
9-1417	A	104:0		Figure 9.4.4: This potentially very useful diagram is, unfortunately, a real "mess"! Having bars that cross into other graphs is not at all helpful. I am not sure what the authors had hoped to achieve with this rather poor graphic. This must be tidied up. [Gareth S. Jones]	Figure replaced.
9-1418	A	104:0		Fig.9.4.4 Label X axes. [Melinda Marquis]	Axes labeled.
9-1419	A	104:0		This is a useful figure. Some effort should be made to improve the labels. [Christoph Schar]	Figure replaced, labelling improved.
9-1420	A	105:0		Fig.9.4.5 Please add units (deg C) to X axis. [Melinda Marquis]	Units added.
9-1421	A	106:0		Define the meaning of "All" in caption. [Christoph Schar]	Done.
9-1422	A	106:8		Replace (90%) with (95%) and increase all the error bars on the figure to comply with 95% accuracy [Vincent Gray]	Rejected, based on standard practice in detection and attribution literature.
9-1423	A	107:0		Figure 9.4.7. Are the regions the same as those adopted in chapter 11? It would be good to cross reference. [Matthew Collins]	Yes. The revised figure developed in collaboration with Ch 11
9-1424	A	107:0		Figure 9.4.7: The names of the domains whose 3 letter acronym is given in the various panels should be included in the figure caption. [Patrick Hamill]	Caption improved.
9-1425	A	107:0		Fig.9.4.7 The 3-letter abbreviations don't add much; consider removing them. [Melinda Marquis]	We prefer to keep the abbreviations.
9-1426	A	107:0		Fig. 9.4.7. Y axes need labels, delta T (K) [Alan Robock]	Axes are labeled.
9-1427	A	107:0		This is an impressive result. Please state explicitly in the caption that it has been obtained from an coupled GCM driven by observed atmospheric forcings. Based on these results, I suspect that the HadCM3 behaves much better than the ensemble mean shown in Fig.9.4.2. See also my corresponding comment (p.25, 130). Please consider showing this model (rather than the ensemble mean) in Fig.9.4.2. [Christoph Schar]	Caption improved. We prefer to show all models in 9.4.2.
9-1428	A	108:0		Fig.9.4.8 In first sentence of caption, it is necessary for correct meaning to keep both "means": "Time series of global mean monthly mean ..."	This is fig 9.4.9. Caption appears to be correct.



No.	Batch	Page:line		Comment	Notes
		From	To		
				[Melinda Marquis]	
9-1429	A	108:3		In figure 9.4.8a and b, though the y scale is normalised, it is better to give a linear scale (from 0 to 1.0) otherwise people may misunderstand and think that it could be logarithmic scale. [Dr. Bundit Limmeechokchai]	Axes have been labelled.
9-1430	A	110:9		Replace "90%" with "95%" and increase all the error bars on the diagrams to comply with 95% accuracy [Vincent Gray]	Rejected. This figure is reproduced from the published literature.
9-1431	A	111:0		Fig. 9.5.2 Add label and unit to color bars. [Melinda Marquis]	Revised.
9-1432	A	112:0		Fig.9.5.3 Explain abbreviation GFDL. Add Y axis label. [Melinda Marquis]	Revised.
9-1443	B	112:0		Figure 9.5.3. This result is not very bad, but it will have to be improved not to also remain in the approximation. To move the legend to reveal correctly the graphs [Expédit Wilfrid VISSIN]	Noted.
9-1433	A	113:0	6:	Fig.9.5.4 "Data set" is two words. [Melinda Marquis]	Noted.
9-1444	B	113:0		Figure 9.5.4. This figure is illegible and difficult to include/understand. It will be necessary to make of them 2 or 3 figures. [Expédit Wilfrid VISSIN]	Rejected. We feel the figure adequately communicates the message that it is intended to send.
9-1434	A	114:0		Fig.9.5.5 Explain abbreviation CRU and SST (sea surface temp?) [Melinda Marquis]	Revised.
9-1445	B	114:0		Figure 9.5.5. The figure results are still very approximate, because simulations of certain years are very bad. It will be necessary to improve the model for more acceptable results [Expédit Wilfrid VISSIN]	Noted.
9-1435	A	115:0		Fig 9.6.1 The Annan et al (2005) results are not from an EMIC but a GCM (the text in 9.6.2.3 is correct). There should be a dot (max likelihood estimate) at 4.5C. The lower limit is undefined, and should not terminate at 4C. It would be incorrect to represent our results as implying a high confidence that climate sensitivity is greater than 4C. It is not clear how to best show this on the figure, though (maybe extend the left down to zero with dots: .....*-----). Strictly speaking it is not a pdf at all, although the top end seems likely to be robust. [James Annan]	Caption and figure revised.
9-1436	A	115:0		Fig.9.6.1 Fix spelling of y axis label: Likelihood. Fix also in caption. [Melinda Marquis]	Fixed.



No.	Batch	Page:line		Comment	Notes
		From	To		
9-1437	A	116:0		Fig. 9.6.2: Can the green star be correct? Why is it so low considering the distribution? [Alan Robock]	The green star is correct.
9-1438	A	116:0		Are the point values in the Figure right? The pdfs are quite different but the point values very close to each other. [Ilkka Savolainen]	Yes, we double checked.