IPCC Working Group I Fourth Assessment Report

Expert and Government Review Comments on the Second-Order Draft

Chapter 8

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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 (15 June 2006)

No.	Batch	Page:line From	Page:line To	Comment	Notes
8-1	A	0:0	0:0	The title of Chapter 8 is misleading. No comprehensive description of the structure and the principle of function of the AOGCMs is given. An appropriate title might be "Model development and evaluation", for instance. [Govt. of Finland (Reviewer's comment ID #: 2009-58)]	Rejected. We believe sufficient information on the principles of models is given here.
8-2	A	0:0	0:0	In several figures, a multi-model mean is depicted. In calculating this mean, enhanced weight should be given to "physically developed" models (AOGCMs with a good horizontal and vertical resolution, sophisticated parameterizations etc.). Moreover, when there are several model versions of the same research centre, only the most novel version should be included in the mean; alternatively, if all such model versions are included, a reduced weight should be given to each one. [Govt. of Finland (Reviewer's comment ID #: 2009-59)]	Rejected. The question of weighting of models ('metrics') is discussed in 8.1 and in Ch. 10, and the literature does not support the proposed approach.
8-3	A	0:0	0:0	Repetitive use of footnote "Supplementary material is available at the website serving the chapter drafts" is disturbing. This information might be included in the introduction of the chapter. [Govt. of Finland (Reviewer's comment ID #: 2009-60)]	Accepted. Will be streamlined.
8-4	A	0:0	1:	You have to face it. No model has ever successfully forecast any future climate in quantitative terms. It is surely because they incorporate only one of the many influences on the climate, increases in greenhouse gases. Why should any of us believe them? [VINCENT GRAY (Reviewer's comment ID #: 88-901)]	Rejected. See 8.1 and 8.4.
8-5	A	0:0		This chapter would benefit greatly from a table of robust findings and key uncertainties, similar to the ones that appear in some earlier chapters. [Lenny Bernstein (Reviewer's comment ID #: 20-65)]	Rejected. Authors considered this but it was felt that this role is fulfilled (for the material of this chapter) by the Executive Summary.
8-6	A	0:0		Congratulations to that chapter, an impressive amount of information. I was a bit unhappy in the FOD because I felt that for many of the diagnostics it was not clear whether they would matter for the model to be used for projections. I think the SOD has improved a lot in that sense, and the problems are discussed in several places that we are far away from understanding whether and how control climate matters for projections. It's difficult and I don't see any obvious way to improve the situation further, there are really not many papers on that so far. But I like the detailled discussion of the feedbacks, it seems that understanding and quantifying those helps more than plotting model minus observations for countless fields. [Reto Knutti (Reviewer's comment ID #: 133-10)]	Taken into account. Section on metrics in 8.1 has been modified.
8-7	A	0:0		For most of the figures where model and observations are compared, why are the models not taken over the same period as the data? Warming has been considerable over the last few decades, so for example when the obs. Are 1960-1990 and the models are 1980-2000,	Taken into account. Periods will be harmonised as far as practically possible, but consistency between

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				this might make a difference. It might not be very large, but it seems trivial to use the same period and avoid any problems and criticisms. [Reto Knutti (Reviewer's comment ID #: 133-11)]	periods for different model variables is also important. The likely impact of any differences in periods is taken into account in the evaluation
8-8	A	0:0		This chapter is very comprehensive in discussing model performance of the suite of models as a whole, but I think nowhere in the text is any statement that some models are obviously doing a better job than others. The whole chapter implicitly says that we should trust models more if they perform better for present day climate, and yet no attempt is made to rank models (which might be politically incorrect) or to at least give a distribution of RMS errors. [Reto Knutti (Reviewer's comment ID #: 133-14)]	Noted. Robust metrics do not currently exist. Text on this issue in 8.1 has been revised.
8-9	A	0:0		This chapter should included a table of robust findings and key uncertainties, rather than expecting the reader to extract them from the text. [Jeff Kueter (Reviewer's comment ID #: 137-60)]	Rejected. Authors considered this but it was felt that this role is fulfilled (for the material of this chapter) by the Executive Summary.
8-10	A	0:0		Given that the results of models in polar regions are used elsewhere in this report, for example relying on snowfall rates on Antarctica and Greenland and on their melting/loss of ice sheets, it seems unfortunate that the chapter, at least in the parts I looked at, did not seem to provide a collective summarization of model performance in high latitudes. Traditionally, the situation has been that models do better for temperature than precipitation, do better for rain than snow, and do better in flat terrain than in regions of sharp orography--so one would think the results atop Greenland and Antarctica might well be suspect--yet those model results provide, in good part, the basis for lowering the projected change in sea level as compared to the TAR. I think it would be an important addition to the IPCC WGI assessment if, for example, a box could be added that covered model performance in the polar regions and so gave an indication of what level of confidence can be placed in the model results (I should note that, as I recall, Richard Alley, in a comment on the TAR finding that warming would lead to more snow on Greenland, found instead a negative correlation between NH temperature anomaly and snowfall on Greenland, so there is a real need to have a summary of the current situation). [Michael MacCracken (Reviewer's comment ID #: 152-265)]	Rejected. Beyond the scope of this chapter to provide detailed regional summaries (if given for one region they would be needed for all). Chapter 11 provides summary information on regionaal scales.
8-11	A	0:0		General comment on section 8.2: since this is really a discussion for IPCC, what is the rationale for spending time discussing model improvements that are not used by the models in their IPCC formulations? It is all well and good to look to the future of modeling, but that's not really the role of this chapter, and in some sense it is misleading - it seems to imply model capabilities that are not actually being utilized. (This comment does not apply to modeling studies that point out the value or deficiency of some	Taken into account. Section 8.2 will be revised with this in mind.

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				modeling component that is not being used in the IPCC simulations.) [David Rind (Reviewer's comment ID #: 214-60)]	
8-12	A	0:0		The whole chapter lacks information on the tropics where the effects of climate change are likely to be most severe and where the models have severe difficulties in simulating the current climate and its inherent variability. The section on mkonsoon variability is unacceptable. There should be a specific sections on Africa, tropical Americas, Asian and Austral monsoons. These could have been provided by the CLIVAR panel - VACS, VAMOS, AAMP, but seem not to have been engaged in the process. As it stands the chapter is very unbalanced and this should be addressed as a matter of urgency. [Julia Slingo (Reviewer's comment ID #: 243-1)]	Monsoons: Accepted. The monsoon subsection has been rewritten and linked to Chapters 3, 9 and 11 which provide additional information.
8-13	A	0:0		The chapter is lacking sufficient information on the ability of the models to capture regional rainfall patterns and their variability. Changes in rainfall will likely constitute a much graver impact of climate change than temperature and we need to be clear about the level of skill of our models in representing regional rainfall behaviour in space AND time, particularly over land. I see this as a major gap in the AR4 which could be addressed with the information we have to hand. [Julia Slingo (Reviewer's comment ID #: 243-2)]	Rejected. See response to 8-10.
8-14	A	0:0		General comment on section 8.2: since this is really a discussion for IPCC, what is the rationale for spending time discussing model improvements that are not used by the models in their IPCC formulations? It is all well and good to look to the future of modeling, but that's not really the role of this chapter, and in some sense it is misleading - it seems to imply model capabilities that are not actually being utilized. This comment does not apply to modeling studies that point out the value or deficiency of some modeling component that is not being used in the IPCC simulations. [Govt. of United States of America (Reviewer's comment ID #: 2023-499)]	Taken into account. Section 8.2 will be revised with this in mind.
8-15	A	0:0		Should include a table that shows what changes are in AR4 models compared to those in the TAR - and perhaps a separate column that indicates what advances are occurring in models not used for IPCC assessments. [Govt. of United States of America (Reviewer's comment ID #: 2023-500)]	Taken into account. Complete traceability from the models used in TAR is not possible for a variety of reasons, but we will endeavour to provide more such information.
8-16	A	0:0		This chapter would benefit greatly from a table of robust findings and key uncertainties, similar to the ones that appear in some earlier chapters. [Govt. of United States of America (Reviewer's comment ID #: 2023-501)]	Rejected. Authors considered this but it was felt that this role is fulfilled (for the material of this chapter) by the Executive Summary.
8-17	A	0:0		This chapter should include a table of robust findings and key uncertainties, rather than expecting the reader to extract them from the text. [Govt. of United States of America (Reviewer's comment ID #: 2023-502)]	Rejected. Authors considered this but it was felt that this role is fulfilled (for the material of this chapter) by the

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					Executive Summary.
8-18	A	0:0		This is a well written chapter that generally covers its subject comprehensively. One minor general comment is that there could be more cross links to other chapters, particularly the observational chapters. Also, there are probably bits of nomenclature that should be standardized across WG1. For example, we should probably agree to use AOGCM to designate a coupled model, rather than OAGCM or CGCM. Also, we should standardize on some designation for the collection of AOGCM experiments at PCMDI that was developed under the auspices of WGCM and CMIP. Ch 8 often refers to them as CMIP models (which is slightly confusing because there is more than one versoin of CMIP), while Ch 9 usually refers to them as IPCC AR4 models (which doesn't recognize CMIP). Perhaps Gerry Meehl can tells us? Another one I noted - should we refer to AMIPII or AMIP2? [Francis Zwiers (Reviewer's comment ID #: 305-33)]	Models referred to as 'Multi-model dataset at PCMDI' or 'multi-model dataset'. Definition to be given in 8.3
8-19	A	1:0	1:	Contributing author's name: T. Yakemura --> T. Takemura [Masahide Kimoto (Reviewer's comment ID #: 127-1)]	Name corrected.
8-20	A	1:10	1:23	There are only 2 models from developing countries(both from China) in the whole 23 global models involved in this chapter. In order to emphasize the contributions from developing countries, two main calculators of the two Chinese models in contribution autors, Yu Yongqiang and Xu Ying, should be added. [Govt. of China (Reviewer's comment ID #: 2006-58)]	Rejected. Contributing authors are those who have made specific contributions to the drafting of the chapter.
8-21	A	1:23	1:23	"T. Yakemura" should be "T. Takemura" [Seita Emori (Reviewer's comment ID #: 62-1)]	Name corrected.
8-22	A	3:0		Throughout this chapter, & maybe the others I haven't read, the word "model" is used with appalling carelessness. Sometimes it does just mean "model", more often it means "GCM", & sometimes I don't know what is meant. So what hope is there for policy-makers trying to understand it? The Executive Summary is particularly bad, with the first page generally saying "model" but meaning "GCM", & subsequent pages generally saying "GCM" (or even more specifically, "AGCM" &c). The opening sentence of the Executive Summary should make it plain that the chapter is almost all about GCMs, with a bit about simpler models at the end, & every use of "model" before Section 8.8 should be checked & replaced by "GCM" if that is what is actually meant. [William Ingram (Reviewer's comment ID #: 114-1)]	Accepted. Will be reviewd throughout chapter.
8-23	A	3:0		The executive summary must include a specific statement about the skill of models in representing regional rainfall. [Julia Slingo (Reviewer's comment ID #: 243-3)]	Reject. Too much detail for ES. Regional simulation is discussed by Ch 11. Statement on global precip will be added.

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8-24	A	3:1	6:18	The exec summary highlights progress, which is appropriate. But it would also be appropriate to note some areas with lack of progress such as soil moisture (page 15) which is a relevant ecological and societal variable. [Michael Manton (Reviewer's comment ID #: 157-40)]	Reject. ES highlights areras of progres. Space constraints mean this is not possible.
8-25	A	3:3	3:5	These models are also used in Chapters 6, 9 and 7 (to the extent that the same models have hosted carbon cycle components). [Francis Zwiers (Reviewer's comment ID #: 305-34)]	Rejject. Too much detail for this point in ES.
8-26	A	3:4	3:4	Insert after "climate change" the following "and in the attribution of observed climate change" to more accurately describe the content of the chapter. [Govt. of Australia (Reviewer's comment ID #: 2001-338)]	Rejject. Too much detail for this point in ES.
8-27	A	3:4	3:4	Replace "climate change" with "change of climate" [VINCENT GRAY (Reviewer's comment ID #: 88-885)]	Reject. Climate Change is defined in the Glossary.
8-28	A	3:4	3:4	Insert before "Confidence" "Despite the total absence of any succedssful future climate prediction" [VINCENT GRAY (Reviewer's comment ID #: 88-886)]	Reject. See text in chapter.
8-29	A	3:4	3:4	Use of the term "confidence" is not appropriate or supported by the evidence. The "confidence" at the time of the TAR was unjustifiably high. Due to considerable improvements in the physical realism of the models, "confidence" might be more justified today, however, there is no objective evidence that it is higher today. In fact, with advances in the evaluation of the models, we have evidence that we should be less confident in model estimates of future climate evolution than at the time of the TAR. For example, we now know there are positive biases in the albedo in all the models, on the order of 0.016 (20+watts/m^2) (Roesch 2006). Therefore models must have obtained their good reproduction of historical data by means of compensating biases in other components. It is likely this bias is in increased climate sensitivity to the greenhouse gasses, the variables that are most prominent in our future scenerios. Until this albedo bias is corrected, and the models are reparameterized, the models are of limited usefulness for predictions, attribution of past warming and in climate commitment studies. Therefore, confidence is decreased, despite significant improvement in the models, because we now have specific evidence of the model limitations and biases. I recommend we refrain from discussing "confidence" and instead provide a sobering summary of current model limitations. I recommend the second sentence be replaced with this text: "Models have been enhanced by a range of advances since the TAR, but will be of limited usefulness for attribution of past warming and the making of future predictions, until significant identified biases have been corrected." [Martin Lewitt (Reviewer's comment ID #: 146-1)]	Reject. Errors in albedo do not imply climate sensitivity errors. Confidence is based on expert judgement of LA teaam.
8-30	A	3:7	3:8	The term "plausible" is very ambiguous when used with "quantitative". In light of the	Taken into account. Text modified.

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				evidence of significant model biases, the phrase "plausible qualitative" should probably be used instead. The realistic climate behaviors captured by advances in the model science is gratifying. These hold out the promise of interesting and useful quantitative results with future advances and corrections of model biases, but at this time it is ambiguous and incorrect to use the phrase "plausible quantitative". Perhaps you can make a case for "plausible quantitative" results under certain conditions or for certain phenomena rather than globally or related to the future? [Martin Lewitt (Reviewer's comment ID #: 146-2)]	
8-31	A	3:8	3:8	Insert after "above" "but no evidence of any actual succesful prediction" [VINCENT GRAY (Reviewer's comment ID #: 88-887)]	Reject. See text in 8.1 and Ch 1 concerning projections in FAR and SAR.
8-32	A	3:11	3:11	Insert after ."(see Chapter 8)" "but not from a single successful forecast" [VINCENT GRAY (Reviewer's comment ID #: 88-888)]	Reject. See text in 8.1 and Ch 1 concerning projections in FAR and SAR.
8-33	A	3:14	3:14	"numerics" will be meaningless to policymakers: "computational methods"? [William Ingram (Reviewer's comment ID #: 114-2)]	Accepted. Text modified.
8-34	A	3:16	3:24	It should be noted that systematic biases in coupled models remain a serious issue because they have non-linear impacts on modes of variability (e.g. El Nino) and global teleconnections (Ref: Turner, A. G., P. M. Inness and J. M. Slingo, 2005: The Role of the Basic State in Monsoon Prediction. Q. J. R. Meteorol. Soc., 131, 781-804) [Julia Slingo (Reviewer's comment ID #: 243-4)]	Reject. Concluded that this was too detailed for ES.
8-35	A	3:18	3:18	The first time AOGCM is used in the chapter it should be defined. [Govt. of Australia (Reviewer's comment ID #: 2001-339)]	Accept. RW/DR to resolve editorial issue
8-36	A	3:19	3:20	Suggest deleting the end of the sentence : "despite the fact that flux adjustments have been eliminated in most models."It may look contradictory for a policy makers to read on lines 16 to 19 that flux adjustment suppression is a progress and then that "improvements in the simulation of many aspects of present climate" have been achieved, "despite" this progress. [Govt. of France (Reviewer's comment ID #: 2010-53)]	Accepted. Text modified.
8-37	A	3:19	3:20	Clearer to replace "the fact ... in" by "this elimination of flux adjustment from" [William Ingram (Reviewer's comment ID #: 114-3)]	Taken into account. Text modified.
8-38	A	3:22	3:22	"Some" is misleading - many & major problems remain. This should be honestly acknowledged - & this Executive Summary should generally make it plain that some things are easier to simulate than others, & that ENSO, as an alternation between different quasi-equilibria, is by its very nature a very hard one. [William Ingram (Reviewer's comment ID #: 114-4)]	Taken into account. Will ensure text in Chapter and ES will be modified to highlight variable performance among models. The ENSO subsection plainly acknowledges that "serious systematic errors persist", which is not inconsistent

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					with the corresponding statements in the ES and FAQ.
8-39	A	3:23	3:24	The first time ENSO and MJO are used in the chapter they should be defined. [Govt. of Australia (Reviewer's comment ID #: 2001-340)]	
8-40	A	3:25	3:25	Add conclusions on quality of mean sea level pressure fields. See comments on section 8.3.1.3 [Govt. of Netherlands (Reviewer's comment ID #: 2016-43)]	Reject. Too much detail for ES.
8-41	A	3:25	3:27	The ability of climate models to represent extreme preciitation events is due to lack of resolution and this should be stated. [Julia Slingo (Reviewer's comment ID #: 243-5)]	Reject. Mentioned later in ES.
8-42	A	3:25	3:27	There is not much assessment in the section on precipitation extremes to support this conclusion. Simulated extremes should become more intense with increasing resolution, but nontheless, it is generally reasonable to expect that the extremes of model simulated precipitation should be smaller than observed because models cannot simulate the high spatial variability in precipitation intensity that is observed in nature. This is not a judgement on whether precipitation producing processes are correctly represented at the grid scale - but simply an observation that rain gages do not measure grid-square mean precipitation, which is all that models can simulate. A model that produces extremes as intense as observed at rain gages is therefore probably suspect (at least, for the most extreme kinds of events). [Francis Zwiers (Reviewer's comment ID #: 305-35)]	Noted. However our assessment is for scales resolved by the models.
8-43	A	3:26	3:27	Would read better removing "generally … falling" & adding "generally being underestimated" at end of sentence [William Ingram (Reviewer's comment ID #: 114-5)]	Accepted. Text modified.
8-44	A	3:26		"Suggest changing ""remains variable"" to ""differs between models""" [Govt. of Canada (Reviewer's comment ID #: 2004-152)]	Taken into account. Will modify 8.5 and ES text for clarity.
8-45	A	3:30	3:35	The uncertainty in climate sensitivity associaed with ocean heat uptake processes should be acknowledged. [Julia Slingo (Reviewer's comment ID #: 243-6)]	Reject. Ocean heat uptake is a separate issue – see next bullet.
8-46	A	3:31	3:31	"found in different" -> "between", & "inter-model" -> "these" [William Ingram (Reviewer's comment ID #: 114-6)]	Accepted. Text modified.
8-47	A	3:32		With low cloud as the largest contributor. [Govt. of Austria (Reviewer's comment ID #: 2002-46)]	Taken into account. Text modified.
8-48	A	3:39	3:42	Is this bullet indeed based on discussion presented later in the chapter? [Govt. of Finland (Reviewer's comment ID #: 2009-61)]	Taken into account. The supporting text has been modified in 8.1.
8-49	A	3:39	3:39	"historical" confusing - will sound to the innocent reader as if past only, or perhaps past	Accept 'Historical' removed.

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				observed changes - I simply can't guess whether the latter is meant, or just "existing observations". Clarify by replacing with "existing", or removing & adding "of historical climate change" after "observations" instead. [William Ingram (Reviewer's comment ID #: 114-7)]	
8-50	A	3:43	3:43	Omit "a" & "set of" for better flow - & "diagnostic" as meaningless & confusing to policymakers. [William Ingram (Reviewer's comment ID #: 114-8)]	Taken into account. Text modified.
8-51	A	3:44		What are "impact-relevant" surface temperatures? And how are they different to other types of surface temperatures? [Martin Manning (Reviewer's comment ID #: 155-40)]	Comment appears misplaced. Cannot find these words in chapter.
8-52	A	3:45	3:45	"The" -> "This" [William Ingram (Reviewer's comment ID #: 114-9)]	Accepted.
8-53	A	3:50	3:50	"Intercomparison exercises" -> "Comparisons" [William Ingram (Reviewer's comment ID #: 114-10)]	Rejected. Intercomparisons is a widely used term.
8-54	A	3:53	3:53	behavior -> behaviour [Govt. of Finland (Reviewer's comment ID #: 2009-62)]	Editorial
8-55	A	4:2	4:2	In the executive summary text it would be useful for a brief dot point outlining that the AR4 suite of models were used in previous assessment reports and that further information about them can be found in the TAR. [Govt. of Australia (Reviewer's comment ID #: 2001-341)]	Reject. Comment is false.
8-56	A	4:7	4:7	indirect effects (plural?). [Francis Zwiers (Reviewer's comment ID #: 305-36)]	Accepted.
8-57	A	4:10	4:10	Omit "a" [William Ingram (Reviewer's comment ID #: 114-11)]	Accepted.
8-58	A	4:11	4:11	"over the next few decades". Why does terrestrial processes affect simulation of climate just on this time-scale? Why not on centennial scale, for instance? Or the present-day climate? [Govt. of Finland (Reviewer's comment ID #: 2009-63)]	Taken into accout. Text added.
8-59	A	4:15	4:15	Omit "so-called" as the quotation marks do the job [William Ingram (Reviewer's comment ID #: 114-12)]	Accepted
8-60	A	4:24	4:24	"tiling" meaningless to policymakers: add reference or brief phrase of explanation [William Ingram (Reviewer's comment ID #: 114-13)]	Taken into account. Text deleted.
8-61	A	4:27	4:27	So it should, if there were such a thing! Of course no ice is really permanent on Earth, & in this context this word, longer, incorrect & meaningless to policymakers, should be replaced by "land", which is what is meant. [William Ingram (Reviewer's comment ID #: 114-14)]	Accepted. Text changed

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8-62	A	4:27	4:27	Also chapters 6, 9 and 7 (to the extent that the same models have hosted carbon cycle components). [Francis Zwiers (Reviewer's comment ID #: 305-37)]	Rejected. Too much detail for ES.
8-63	A	4:34	4:35	I can't find anywhere in Chapter 8 where evidence that the "simulation of marine low-level clouds … has improved" is presented - it only seems to appear in the Executive Summary. To my knowledge, the only published study showing an improvement in marine stratocumulus is in Martin et al. (2006) for HadGEM1 vs HadCM3. It is also fairly well known that there has been an improvement in the GFDL model due to the Lock scheme, however this result hasn't been published. Whilst it would be interesting to include a statement about these two models in the chapter, similar results really needs to have been published for several models if it is to appear as a statement in the Executive Summary of the chapter and in the Technical Summary of the report. I've also spotted a cross-reference to this in Chapter 10 (Comment 10), however the rest of the report should be checked for further references. [Keith Williams (Reviewer's comment ID #: 290-3)]	Accepted. Results from error in underlying chapter text, which will be corrected.
8-64	A	4:42		which formulation? [David Rind (Reviewer's comment ID #: 214-61)]	Taken into account. Sentence deleted.
8-65	A	4:42		Which formulation? [Govt. of United States of America (Reviewer's comment ID #: 2023-503)]	Taken into account. Sentence deleted.
8-66	A	4:44	4:46	Going from ".. notable progress .." to ".. only modest improvement …" in the same sentence seems to create an internal inconsistency. [Martin Manning (Reviewer's comment ID #: 155-41)]	Taken into account. Text clarified.
8-67	A	4:52	4:52	The reference to the Pacific Decadal Oscillation makes it sound as if it were a mode of variability in its own right. Since it is in fact no more than the projection of ENSO onto decadal means, I suggest the reference is changed to something like "extratropical effects of ENSO" - which will also include other important effects. [William Ingram (Reviewer's comment ID #: 114-15)]	Reject. View of the LAs is that consideration of the PDO in its own right is warranted by the literature and is useful.
8-68	A	4:56	5:1	Again, there are a lot more problems with the simulation of ENSO than phase locking & EN/LN symmetry! All GCM simulations of ENSO have errors of a size that would be totally unacceptable for an "easy" quantity like pmsl. This should be honestly acknowledged - & it should be made it plain that some things are easier to simulate than others, & that ENSO, as an alternation between different quasi-equilibria, is by its very nature a very hard one. [William Ingram (Reviewer's comment ID #: 114-16)]	See comment 8-38.
8-69	A	5:1	5:1	Again (though certainly not as grossly), over-optimistic phrasing. Add "some" after "with", or re-write as "Variability resembling the"? [William Ingram (Reviewer's comment ID #: 114-17)]	Accepted.

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8-70	A	5:2	5:2	"with insufficient strength" -> "too weakly" [William Ingram (Reviewer's comment ID #: 114-18)]	Accepted.
8-72	A	5:4		what about blocking? [David Rind (Reviewer's comment ID #: 214-62)]	Rejected. There is a scarcity of literature on how well the AR4 models as a group represent blocking.
8-73	A	5:4		Is this also to imply duration of extreme events, and in that case what about blocking, which is indicated later to be underestimated in duration? [Govt. of United States of America (Reviewer's comment ID #: 2023-504)]	See comment 8-72.
8-67	A	4:52	4:52	The reference to the Pacific Decadal Oscillation makes it sound as if it were a mode of variability in its own right. Since it is in fact no more than the projection of ENSO onto decadal means, I suggest the reference is changed to something like "extratropical effects of ENSO" - which will also include other important effects. [William Ingram (Reviewer's comment ID #: 114-15)]	Reject. View of the LAs is that consideration of the PDO in its own right is warranted by the literature and is useful.
8-74	A	5:8	5:9	This sentence is saying no more than what ought to occur (i.e. would with a perfect model) if point observations are compared with grid-box means from a model. If that is all that's going on it's too trivial to mention - if that has been properly allowed for as I trust is the case, this should be mentioned explicitly to avoid any possibility of confusion. [William Ingram (Reviewer's comment ID #: 114-19)]	Noted. However our assessment is for scales resolved by the models. Will consider again after review of Sun paper – Shukla
8-75	A	5:14		it would seem that the simplification of processes (water vapor feedback, clouds, atmospheric dynamics, even ocean dynamics) in EMICs would affect overall climate sensitivity and large-scale patterns - prohibiting quantitative inferences even on large scales. Good comparisons with observations and GCMs can be obtained by tuning to the known results. It is very risky to utilize these models for quantitative assessments in climate situations removed from the present day. By using these models for long-term climate change projections, IPCC is leaving itself open to justified criticism from informed critics (if such critics really exist). EMICs are potentially useful for exploring concepts, however. [David Rind (Reviewer's comment ID #: 214-63)]	Rejected. The Authors consider that the limits of applicability of EMICs are clearly mentioned in the executive summary as well as in the main text and do not need to be further underlined.
8-76	A	5:14		qualitative inferences about... [Govt. of United States of America (Reviewer's comment ID #: 2023-505)]	See answer to comment 8-75.
8-77	A	5:15	5:15	Omit "organized" (or, if it is intended to mean something, replace with something that does) [William Ingram (Reviewer's comment ID #: 114-20)]	Accepted. "Organized" has been replaced by "coordinated".
8-78	A	5:15	5:15	"intercomparisons -> "comparisons" [William Ingram (Reviewer's comment ID #: 114-21)]	See answer to comment 8-53.
8-79	A	5:31	14:34	Should be deleted. Refers to model capabilities outside the IPCC framework. Could be	Rejected. This is relevant to the

No.	Batch	Page:line From	Page:line To	Comment	Notes
				reformulated to say that IPCC models in general have not been assessed for these purposes (and assessment itself might be difficult - perfect models do not imply perfect skill). [Govt. of United States of America (Reviewer's comment ID #: 2023-506)]	assessment. See text in 8.4.
8-80	A	5:40	5:40	Recall in a footnote, for the non-specialist reading this Executive Summary only, the definition of the lapse rate [Govt. of France (Reviewer's comment ID #: 2010-54)]	Rejected. See glossary
8-81	A	5:42		"Suggest changing ""volcanic"" to ""volcanically""" [Govt. of Canada (Reviewer's comment ID #: 2004-153)]	Accepted.
8-82	A	5:42		Presumably needs to be: " ... in a way that is consistent ..." [Martin Manning (Reviewer's comment ID #: 155-42)]	Accepted.
8-83	A	6:1	6:1	"there is growing evidence that cryospheric feedbacks are only partly responsible for polar amplification" is totally misleading: it suggests this is a new idea & still not certain, whereas it has been well known since before the 1st Assessment Report. I assume what is meant is something like "much recent work suggests that cryospheric feedbacks are responsible for less of the polar amplification"? [William Ingram (Reviewer's comment ID #: 114-22)]	Takene into account. Text modified.
8-84	A	6:2	6:3	"with ... of" -> "and varies between models much less than" for clarity [William Ingram (Reviewer's comment ID #: 114-23)]	Taken into account. Text modified.
8-85	A	6:5	6:5	"estimate" ambiguous - from obs or models or both? Clarify [William Ingram (Reviewer's comment ID #: 114-24)]	Need to clarify what part of Chapter text this refers to Kattsov to advise.
8-86	A	7:10	7:10	Replace ."can often" by "might" [VINCENT GRAY (Reviewer's comment ID #: 88-889)]	The text has been changed.
8-87	A	7:10	7:10	This is a strange statement. I would say that no prediction of any model is ever perfectly right, models always have errors, so it seems one can talk about skill using some metric, but not about a binary right/wrong. In other words, if the prediction has an uncertainty (or has a PDF) a single event can never prove or falsify the prediction, there is always a non-zero probability for an event being very far away from the prediction. [Reto Knutti (Reviewer's comment ID #: 133-2)]	The text has been changed.
8-88	A	7:10	7:18	Editorial point: This section seems too short and too closely linked to the following one to merit a separate section heading and number. Why not merge with the following section and use a combined heading. [Martin Manning (Reviewer's comment ID #: 155-43)]	Taken into account. Section has been restructured DAVE: OK? I think this para can be merged with what follows (in the revised version I sent)
8-89	A	7:12	7:12	Delete "quickly" [VINCENT GRAY (Reviewer's comment ID #: 88-890)]	Rejected. No reason given for suggestion.

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8-90	A	7:15	7:15	Add at end. "As a result we have found it impossible to carry out a successful test on any prediction" [VINCENT GRAY (Reviewer's comment ID #: 88-891)]	Rejected. Authors consider meaning is clearer as written.
8-91	A	7:22	7:26	Considerable progress has been made in model evaluation has been achieved by focusing on specific processes (e.g. diurnal cycle) and phenomena (e.g. tropical weather systems, MJO, storm tracks) and this should be acknowledged. In particular the increasing emphasis on looking at weather in our climate models has been a major advance since AR3. This focus on phenomena is potentially the most powerful method we have for evaluating our models because it provides an interface to numerical weather prediction and exploits reanalyses and satellite observations. Relevant references include: (i) Bernie, D., S. J. Woolnough, J. M. Slingo and E. Guilyardi, 2005: Modelling diurnal and intraseasonal variability of the ocean mixed layer. J. Clim., 15, 1190-1202. (ii) Inness P. M. and J. M. Slingo 2003: Simulation of the MJO in a coupled GCM. I: Comparison with observations and an atmosphere-only GCM. J. Clim., 16, 345-364. (iii) Inness P. M., J. M. Slingo, E. Guilyardi and J. Cole 2003: Simulation of the MJO in a coupled GCM. II: The role of the basic state. J. Clim., 16, 365-382. (iv) Slingo, J. M., P. M. Inness, R. B. Neale, S. J. Woolnough and G-Y. Yang, 2003: Scale interactions on diurnal to seasonal timescales and their relevance to model systematic errors. Annales Geophysicae, 46, 139-155. (v) Neale, R. B. and J. M. Slingo, 2003: The Maritime Continent and its role in the global circulation: A GCM study. J. Clim. 16, 834-848. (v) Inness, P. M., J. M. Slingo, S. J. Woolnough, R. B. Neale and V. D. Pope, 2001: Organization of tropical convection in a GCM with varying vertical resolution: Implications for the simulation of the Madden-Julian Oscillation. Climate Dynamics, 17, 777-793. (vi) Yang, G-Y. and J. M. Slingo, 2001: The diurnal cycle in the tropics. Mon. Weath. Rev., 129, 784-801. (vii) Spencer, H., R. T. Sutton, J. M. Slingo, M. Roberts and E. Black, 2005: Indian Ocean climate and dipole variability in Hadley Centre coupled GCMs. J. Clim., 18, 2286-2307. (viii) Slingo, J. M., P. M. Inness and K. R. Sperber, 2005: Modelling the MJO. Chapter in 'Intraseasonal variability of the atmosphere-ocean climate system'. Editors W. K-M. Lau and D. E. Waliser, Springer/Praxis Book Company, pp. 361-383. [Julia Slingo (Reviewer's comment ID #: 243-7)]	Taken into account. However space constraints preclude discussion of detailed studies of processes in individual models. Authors have attempted to bring out generic conclusions within the chapter. The topics mentioned in the comment will be revisited during revision and text modified where this fits with the overall goals of the chapter and is possible within space constraints.
8-92	A	7:24	7:24	"reveal" - yes, but+H40 also conceal! [William Ingram (Reviewer's comment ID #: 114-25)]	Reject. Agree with statement but believe this is clear from the etxt as it stands.
8-93	A	7:36	7:37	Difference between "present climate" & "instrumental record" unclear - add "of climate change" after "record"? [William Ingram (Reviewer's comment ID #: 114-26)]	Taken into account. Text has benn restructured and modified.

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8-94	A	7:46		within the envelope of internal variability [Govt. of United States of America (Reviewer's comment ID #: 2023-507)]	Rejected. This is already stated.
8-95	A	7:48	7:52	Observations for the 1990s etc. should never be compared with a 'preindustrial' model control run. Observations and a model simulation should represent the same period. [Govt. of Finland (Reviewer's comment ID #: 2009-64)]	Noted. However the issues involved are discussed i the text.
8-96	A	8:1	8:22	I found aspects of this paragraph confusing. The notion of a 'perfect model" approach is mentioned, and this is then followed with a reference to "observational constraints". I think "perfect model" studies are absolutely essential, the idea, as I understand it, being to select one model and treat it as the observations, and then see if one can predict some aspect of the 21st century of that model, given a means of weighting the models in ones ensemble according to their distance from the chosen model using a norm that measures what one deems to be important for that prediction. Predicting the future of a model should be easier than predicting the future of the world. From this persepective true observations do not come into play until one has tested the methodology in this perfect model setting. Have any of the studies used this "perfect mode" approach systematically? [Isaac Held (Reviewer's comment ID #: 105-33)]	Taken into account. Text has been rewritten.
8-97	A	8:2	8:2	Replace "A full answer to this question remains elusive, but" by "Frankly, nothing" [VINCENT GRAY (Reviewer's comment ID #: 88-892)]	Rejected. Progress in this area is discussed in the text (which has been rewritten for clarity).
8-98	A	8:2	8:2	"A … elusive" misleading in that it suggests there is a full answer waiting to be uncovered. How about "There is no simple answer" instead? [William Ingram (Reviewer's comment ID #: 114-27)]	Taken into account. Text has been rewritten.
8-99	A	8:3	8:3	What does "generating" mean - it's not a standard term? Not "forcing", apparently. "contributing to" clearer? [William Ingram (Reviewer's comment ID #: 114-28)]	Text has been rewritten.
8-100	A	8:5	8:5	Comma needed after "example" [William Ingram (Reviewer's comment ID #: 114-29)]	Text has been rewritten.
8-101	A	8:6	8:6	"may be" in what is only a suggestion! If the evidence really is that weak, omit sentence. If not, change "may be" to "is" [William Ingram (Reviewer's comment ID #: 114-30)]	Taken into account. Text has been rewritten.
8-102	A	8:7	8:22	The idea remains unclear. For instance, on the basis of the present discussion it is hard to understand what is meant by "observational constraints". [Govt. of Finland (Reviewer's comment ID #: 2009-65)]	Taken into account. Text has been rewritten.
8-103	A	8:24	8:26	This sentence forgets that some climate change is not radiatively driven (e.g. land use change causing changes in roughness & water-holding capacity), but changing "radiative" to "climate change" is all that is needed to correct it.	Taken into account. Text modified.

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				[William Ingram (Reviewer's comment ID #: 114-31)]	
8-104	A	8:26	8:26	"reSponse" [William Ingram (Reviewer's comment ID #: 114-32)]	Accepted
8-105	A	8:26	8:26	Hyphen needed after "perturbed" [William Ingram (Reviewer's comment ID #: 114-33)]	Accepted
8-106	A	8:33	8:33	"exercised" -> "tested" or "evaluated"? [William Ingram (Reviewer's comment ID #: 114-34)]	Accepted.
8-107	A	8:33	8:41	It also worth noting that no paleoclimate reconstructions can provide us with a 3-dimensional view of the behaviour of the oceans and atmosphere and that presents a serious limitation since the surface information provides an inadequate constraint. [Julia Slingo (Reviewer's comment ID #: 243-8)]	Accepted. Text modified.
8-108	A	8:46	8:46	than is possible for climate. -> ...than is possible for climate simulations. [Govt. of Finland (Reviewer's comment ID #: 2009-66)]	Rejected. Believe text is clear as it stands.
8-109	A	8:50	6:50	"may be less" -> "are not" [William Ingram (Reviewer's comment ID #: 114-35)]	Rejected (e.g. ensebmble generation method may well have some influence)
8-110	A	8:52	8:53	but there are only a few preliminary studies and the inferences one can draw from the whole approach are not clear. [Govt. of United States of America (Reviewer's comment ID #: 2023-508)]	Noted. This is made clear in the main text on this topic in 8.4.11.
8-111	A	8:57	8:58	I am not sure what increased "speed" of the hydrological cycle means and what observational confirmation is being referred to here. [Isaac Held (Reviewer's comment ID #: 105-34)]	Taken into account. Text will be made more precise.and checked for consistency with Ch 3
8-112	A	8:57	8:57	"warming of the troposphere, especially in the polar regions" simply not true - the surface & bl warming is amplified at the poles, but the upper-tropospheric warming is much reduced [William Ingram (Reviewer's comment ID #: 114-36)]	Accepted. Text changed.
8-113	A	8:57	9:1	"increase in the speed of the hydrologic cycle" is the reverse of the truth: it is very robustly simulated that precipitation rates increase less with warming than atmospheric water contents, i.e. the residence time of moisture in the atmosphere increases. [William Ingram (Reviewer's comment ID #: 114-37)]	Taken into account. Text will be made more precise
8-114	A	9:3	9:2	Insert after "observed "but not quantitatively" [VINCENT GRAY (Reviewer's comment ID #: 88-893)]	Rejected. No justification given for suggestion.
8-115	A	9:3	9:4	Misleading - each model's projection has changed a great deal as it has evolved. What has not changed is the consensus - the things they agreed on 30 years ago they still agree on, but they still disagree about many of the things they disagreed on then - in particular, the "detail". [William Ingram (Reviewer's comment ID #: 114-38)]	Accepted. Text changed.

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8-116	A	9:4	9:4	Insert after "consistent" "but not in quantitative terms" [VINCENT GRAY (Reviewer's comment ID #: 88-894)]	Rejected. No justification given for suggestion.
8-117	A	9:6	9:11	Editorial point: Why not merge this short paragraph with the next section. [Martin Manning (Reviewer's comment ID #: 155-44)]	Noted. Has been subsumed in overall restructring of text.
8-118	A	9:16	9:16	What are "MIPS"? [Govt. of Finland (Reviewer's comment ID #: 2009-67)]	Noted, but believe this is clear from text.
8-119	A	9:16	9:17	"components" & "configurations" obscure to the innocent reader [William Ingram (Reviewer's comment ID #: 114-39)]	Taken into account. Hopefully clearer by repositioning in restructured text.
8-120	A	9:19	9:19	Is this unnamed (other than by pcmdi) set of simulations the same as the 'AR4 models'? A clearer exposition of this WCRP(?) project and the experiments is needed here (or somewhere), and this should be linked to discussions in Chapters 9 and 10. [Govt. of Australia (Reviewer's comment ID #: 2001-342)]	Accepted. Text will be clarified.
8-121	A	9:23	9:23	I suggest that "noise" be replaced with "internal variability". "Noise" has a perjorative connotation. [Francis Zwiers (Reviewer's comment ID #: 305-38)]	Accepted.
8-122	A	9:28	9:28	"Th" -> "The" [William Ingram (Reviewer's comment ID #: 114-40)]	Accepted
8-123	A	9:28	9:36	I wonder if this paragraph might be better suited to Ch 1, or perhaps should just be deleted. This is a bit off topic for Ch 8 because it doesn't bear directly on model assessment. We can only speculate as to whether or not community wide organized model intercomparison may, or may not, have affected progress on model improvement by consuming time that might have been used in other ways. [Francis Zwiers (Reviewer's comment ID #: 305-39)]	Accepted. Text deleted.
8-124	A	9:32	9:36	This is a misrepresentation of the function and value of MIPs, especially AMIP. Up till that time, the credibility of models was very low outside (some of) the research community. The discipline of AMIP provided essentially accreditation for models. There was and is real research in the associated projects. [Michael Manton (Reviewer's comment ID #: 157-38)]	Text has been deleted.
8-125	A	9:36	9:36	Add at end "It should be remembered that inercomparisons may merely standardise common errors" [VINCENT GRAY (Reviewer's comment ID #: 88-895)]	Text has been deleted.
8-126	A	9:38		Section # 8.1.3 The current text gives no sense of what it means "to optimise model simulation" (p9, line 53). It is worth saying in this section that there will be systematic biases in the model's simulation of the real world i.e. that the models are not perfect even after tuning because of approximations or the fact that not all processes will be captured. [David Sexton (Reviewer's comment ID #: 233-1)]	Noted. However space precludes a fuller discussion here. The point is made implicitly by the section as a whole.

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8-127	A	9:40	9:46	A discussion of the modelisation of the radiative power from the absorption spectrum of GHG molecules and radiative transfer is missing. [Govt. of France (Reviewer's comment ID #: 2010-55)]	Rejected. This paragraph is not intended to be a complete description of the contents of climate models (see section 8.2).
8-128	A	9:50	9:50	"entraining … schemes" -> "some convection schemes on entraining plume models" to flow easier & to make it plain not all convection schemes have such a basis [William Ingram (Reviewer's comment ID #: 114-41)]	Taken into account. 'some' added.
8-129	A	9:52	9:52	"chosen" unclear - "remaining within" or "limited to"? [William Ingram (Reviewer's comment ID #: 114-42)]	Rejected. Believe text is clear.
8-130	A	9:52	9:52	"prior distribution" - technical term of unclear meaning here: I assume meaning that a possible range is decided on before tuning starts, & tuning outside that range not allowed. Suggest "range pre-defined on physical grounds". [William Ingram (Reviewer's comment ID #: 114-43)]	Rejected. Believe text is clear.
8-131	A	9:53	9:53	I suggest you delete "or to improve global heat balance" (this is also a variable). [Francis Zwiers (Reviewer's comment ID #: 305-40)]	Rejected. The global heat balance is often tuned to be near to zero in a control run, rather than against observations.
8-132	A	9:56	9:57	It may be worth noting that this should not be done too naively: in some cases an "unphysical" value of an "observationally-constrained" quantity compensates for unavoidable distortions elsewhere (e.g. finite resolution) to give a more physically-based simulation. [William Ingram (Reviewer's comment ID #: 114-44)]	Rejected. Agree with the point but too much detail for available space.
8-133	A	10:7	10:10	There is, of course, a limit. If carried to extremes (and assuming resources were available), tuning would "overfit" the available observational data, with the result that confidence in projections outside the observational period would be reduced (e.g., like fitting an nth degree polynomial to n+1 data points). [Francis Zwiers (Reviewer's comment ID #: 305-41)]	Rejected. Agree with the point but too much detail for available space.
8-134	A	10:26		Section 8.2 The possible impacts of relative atmosphere/ocean resolution should be mentioned here as well as the impacts of alterig their resolution independently. [Gill Martin (Reviewer's comment ID #: 167-1)]	Noted.
8-135	A	10:29	10:29	The number of AR4 models in Table 8.2.1 should be 23. So the words " twenty-two AR4 models" should be " twenty-three AR4 models". [Govt. of China (Reviewer's comment ID #: 2006-59)]	Noted.
8-136	A	10:30	10:33	The phrases "dynamical cores" & "parametrizations of physical processes" have been slightly expanded & I suppose are intended to be useful to outsiders, but aren't - what is "unphysical" about advection? The phrases "resolved flow" and "other processes" are	Taken into account.

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				clear IMO. [William Ingram (Reviewer's comment ID #: 114-45)]	
8-137	A	10:32	10:33	While space constraints may prevent a completely comprehensive discussion of the topic of changes to the AR4 models since the TAR, table 8.2.1 needs further explanation to be of relevance to policy readers, and if it is possible, the most significant changes to the models should be explained. [Govt. of Australia (Reviewer's comment ID #: 2001-343)]	Taken into account.
8-138	A	10:43	10:43	I don't know what is meant by this - surely not just the triviality that a multi-model ensemble changes less as one model is updated than the model itself? [William Ingram (Reviewer's comment ID #: 114-46)]	Taken into account.
8-139	A	10:44	10:44	Table 8.2.1 does not contain "details of the formulation" but only fairly general useful information about the AOGCMs. [Govt. of Finland (Reviewer's comment ID #: 2009-68)]	Taken into account.
8-140	A	10:45		Shouldn't this section have a cross reference to the work on model intercomparison of radiative forcing that is covered in Chapter 10, Sectin 10.2. I suggest that the place to do that is here. [Martin Manning (Reviewer's comment ID #: 155-45)]	Noted
8-141	A	10:48		numeric changes were characterized in above paragraphs as improvements; this paragraph seems somewhat equivocal as to whether they really are improvements. [David Rind (Reviewer's comment ID #: 214-64)]	Rejected
8-142	A	10:48		In the whole paragraph, numeric changes were characterized in previous paragraphs (line 30) as improvements; this paragraph seems somewhat equivocal as to whether they really are improvements. [Govt. of United States of America (Reviewer's comment ID #: 2023-509)]	Rejected
8-143	A	10:52	10:53	One of AR4 models, FGOALS-g1.0, uses Eulerian finite-difference scheme with properties of mass-conservation and shape-preserving (Yu, 1994; Liu et al., 2002). So, the sentence between line 52 and line 53 should be revised as " In AR4, various models use spectral, semi-Lagrangian, Eulerian finite-difference (Yu, 1994; Liu et al., 2002), and Eulerian finite-volume advection schemes." [Govt. of China (Reviewer's comment ID #: 2006-60)]	Taken into account, but no need to refer the reference.It is generally knwon.
8-144	A	10:53	10:53	This sentence presupposes that one type of scheme must be best in a general sense, but I don't know of any evidence for this - why might not different types be best for different purposes? [William Ingram (Reviewer's comment ID #: 114-47)]	Taken into account.
8-145	A	10:56	11:2	The authors should consider the comment that "grid-point methods are commonly considered to the most appropriate". The reason given for this is the high cost of	Accepted. See the text.

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				transformation between grid space and wave space, more discussion is needed on how transformation between grid space and wave space may improve outputs, would be helpful. [Govt. of Australia (Reviewer's comment ID #: 2001-344)]	
8-146	A	10:57	11:2	The sentence that spans these lines must at least be qualified. For all resolutions practical for global climate models now and for the coming few years, the overheads of the transforms in spectral models need not be "very expensive". What is meant by "high-resolution"? ECMWF routinely runs a T799 atmospheric model in which the net cost of the Legendre and Fourier transforms is under 10% of the total cost of the model. At T2079 resolution the overhead is onlly 19%. If a grid-point rather than a spectral method is used, there will be some overhead connected with the solver for implicit time schemes unless a very inefficient explicit time stepping is used. Why is there no reference to the scientific literature to support the remarks made in this paragraph? [Adrian Simmons (Reviewer's comment ID #: 242-115)]	Accepted. See the text.
8-147	A	11:1	11:3	Might mention that there are exceptions in both climate and NWP. [Francis Zwiers (Reviewer's comment ID #: 305-42)]	Taken into account.
8-148	A	11:2	11:2	Stop missing after "computers". [Govt. of Finland (Reviewer's comment ID #: 2009-69)]	Taken into account.
8-149	A	11:2	11:2	Full stop omitted [William Ingram (Reviewer's comment ID #: 114-48)]	Taken into account.
8-150	A	11:2	11:3	The comments on spectral methods don't appear to be supported by the experience at ECMWF where the model has been run at very high resolutions (T1000 and above). [Julia Slingo (Reviewer's comment ID #: 243-10)]	Accepted See the text.
8-151	A	11:7	11:8	The AGCM used in FGOALS-g1.0 is a grid-point model that uses a new grid system called ' weighted equal-area grid', on which a finite difference scheme with exact quadratic conservation (i.e. effective energy conservation) and linear conservation (i.e. mass conservation) is constructed for the design of the dynamical core of the AGCM (Wang et al., 2004). The model needs no filter and smoothing near the poles anymore. This new gird system and the new finite-difference scheme should be mentioned in this paragraph. For this reason, the words " and new numerical algorithms" should be inserted into the front of the word 'have' at line 7, and the last word " These" at the end of line 7 should be replaced by " These systems", and the words " a weighted equal-area grid (Wang et al., 2004) and " should be inserted into the back of the first word " include" at line 8. [Govt. of China (Reviewer's comment ID #: 2006-61)]	Taken into account. Text revised.
8-152	A	11:10	11:10	Due to the revision in the above row, the last sentence at this line is replaced by 'Only the weighted equal-area grid is used in AR4 models.' Except for developing new grid	Rejected. The sentence is deleted.

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				systems, designing good numerical schemes is another efficient way to overcome the mentioned problems of grid-point models near the poles in the paragraph. Therefore, the new finite-difference scheme used in the AGCM of FGOALS-g1.0 should also be mentioned here. For this reason, a sentence is suggested to be added to the end of this paragraph, which is ' These new algorithms contain a finite-difference scheme with exact quadratic and linear conservations for solving primitive equations for baroclinic atmosphere (Wang et al., 2004), which is used in AR4 models to ensure computational stability near the poles. [Govt. of China (Reviewer's comment ID #: 2006-62)]	
8-153	A	11:12		Section 8.2.1.2. More information is required here as to the benefits of these increases in atmospheric model resolution, and whether there are any detrimental effects. The cost of climate models has increased substantially and it is important to state whether this additional cost is justified by improvements in the simulations. It would be helpful to separate the impact of increasing horizontal resolution (which generally tends to lead to improved simulations) from vertical resolution (which interacts with the parametrisations and thus could go either way). [Gill Martin (Reviewer's comment ID #: 167-2)]	Taken into account. See the text.
8-154	A	11:15	11:15	Add "in both atmosphere and ocean" to end of parenthesis for clarity [William Ingram (Reviewer's comment ID #: 114-49)]	Rejected. No need.
8-155	A	11:15		I do not see any definition of the spectral notation like T85, T42 which most logically belongs in this chapter rather than any other. Is it possible to add a short footnote explaining what these mnemonics mean. [Martin Manning (Reviewer's comment ID #: 155-46)]	Accepted.
8-156	A	11:17	11:17	T959? Should probably include a reference (Oouchi et al, 2006) to help readers find the right place in Ch 10. [Francis Zwiers (Reviewer's comment ID #: 305-43)]	Accepted.
8-157	A	11:18		"Briefly define ""time-slice mode"", or else cross-reference" [Govt. of Canada (Reviewer's comment ID #: 2004-154)]	Accepted.
8-158	A	11:21	11:23	Add a sentence at the end: " Although higher resolution is need it to be able to reproduce the mesocale features in complex terrain regions (Salvador, R., J. Calbó, and M. M. Millán, 1999: Horizontal grid selection and its influence on mesoscale model simulations. J. Appl. Meteor., 38, 1311-1329) . If it is not appropiated at the end of the paragraph may be worth still to introduce the sentence somewere else. [Govt. of Spain (Reviewer's comment ID #: 2019-11)]	Rejected. The sentence itself was deleted.
8-159	A	11:28	11:28	either not resolved or not fully resolved -> are not resolved adequately by the model grid [Govt. of Finland (Reviewer's comment ID #: 2009-70)]	Accepted.

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8-160	A	11:36	11:36	"regulating" implies a stabilizing control - the opposite of what most GCMs show! "altering"? [William Ingram (Reviewer's comment ID #: 114-50)]	Rejected. I don7t think so.
8-161	A	11:39	11:39	"the" too strong -> "some" or "the relevant" or "the necessary"? [William Ingram (Reviewer's comment ID #: 114-51)]	Rejected. I dont think so.
8-162	A	11:41	11:41	Doesn't "microphysical" need explaining? [William Ingram (Reviewer's comment ID #: 114-52)]	Accepted.
8-163	A	11:41	11:43	These 2 sentences are badly repetitive: omit 2nd, which contains little new information (or combine) [William Ingram (Reviewer's comment ID #: 114-53)]	Accepted.
8-164	A	11:42	11:43	Why is a reference to one model relevant here? [Michael Manton (Reviewer's comment ID #: 157-39)]	Accepted. The sentence is deleted.
8-165	A	11:54	11:56	Is this a correct place for the sentence discussing parameterization of radiative processes? [Govt. of Finland (Reviewer's comment ID #: 2009-71)]	Rejected.
8-166	A	11:57	11:57	Here and elsewhere, replace OAGCM with AOGCM? There should probably be a standard nomencalture throughout WG1. [Francis Zwiers (Reviewer's comment ID #: 305-44)]	Accepted.
8-167	A	12:14	12:14	What does "fully interactive" mean? This term is relative - it will mean something different in the future when the relevant processes are represented more completely than at present. [Francis Zwiers (Reviewer's comment ID #: 305-45)]	Taken into account. See the text.
8-168	A	12:14	12:16	The distinction that is being made should be made more clearly - what is the difference between having a "full interactive" parameterization (does this mean a size-distributed aerosol code) and other treatments being alluded to. [Francis Zwiers (Reviewer's comment ID #: 305-46)]	Rejected. Due to space limitation,we cannot give aful explanation
8-169	A	12:15	12:15	"HADGEM1" is usually (& previously in this chapter) "HadGEM1" [William Ingram (Reviewer's comment ID #: 114-54)]	Accepted.
8-170	A	12:26	12:26	A definition of thermobaracity would be helpful. [Govt. of Australia (Reviewer's comment ID #: 2001-345)]	Accepted. Text added.
8-171	A	12:26	12:26	I certainly don't know what "thermobaricity" means - explain [William Ingram (Reviewer's comment ID #: 114-55)]	Accepted. Text added.
8-172	A	12:27	12:27	Stop missing after "distorted". [Govt. of Finland (Reviewer's comment ID #: 2009-72)]	Accepted. "Period" added.
8-173	A	12:31	12:31	"unphysical" normally means in violation of physics, & so is misleading used here to refer to the best possible representation of a physical process in the context of certain models and their limitations. I suggest changing it to '... of the "virtual salt flux" which "rigid	Taken into account. "Unphysical" deleted. Text added to make meaning clearer.

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				lid" models have to use.' [William Ingram (Reviewer's comment ID #: 114-56)]	
8-174	A	12:45	12:47	A bit confusing, as MIROC3.2(hires) has been used for some scenario experiments, not only for idealized experiments (like 1% CO2), just as stated in the next sentense. How about changing it to "but since the TAR it has been used in some idealized and scenario-based climate experiments as discussed below." [Seita Emori (Reviewer's comment ID #: 62-2)]	Accepted. Text added.
8-175	A	12:45	12:46	"Eddy-permitting resolution" needs a very brief explanation - ah, it gets it later, in line 51: move this back [William Ingram (Reviewer's comment ID #: 114-57)]	Accepted. Text added.
8-176	A	12:47	12:48	Related to #2 and #3, how about moving this sentense ("A limited set of ...") after P.13 L.2 to better clarify that HadCEM is not in AR4 but MIROC3.2(hires) actually is. [Seita Emori (Reviewer's comment ID #: 62-4)]	Taken into account. Text modified.
8-177	A	12:56	12:56	What is a tracer? If it is just salt, why not call it so? [Govt. of Australia (Reviewer's comment ID #: 2001-346)]	Taken into account. Sentence deleted.
8-178	A	12:56	13:2	Similar to #2, not correct as MIROC3.2(hires) has actually been used in AR4 projections. How about changing it to "One of these models, HadCEM (Roberts et al., 2004), is not used in AR4 projections due to the computational cost, ... " [Seita Emori (Reviewer's comment ID #: 62-3)]	Accepted. Text reworded.
8-179	A	13:4		This sentence is not vey useful without being told the original resolution. "from XX deg by XX deg" could be added before "to 0.33deg by 0.33deg". [Adrian Simmons (Reviewer's comment ID #: 242-116)]	Accepted. Text added.
8-180	A	13:9	13:14	Is a distinction being made between the MOC and THC? [Francis Zwiers (Reviewer's comment ID #: 305-47)]	Taken into account. Changed THC to MOC.
8-181	A	13:17	13:17	"treatment for its pathway" - what does this mean? "treatment of its flow"? "resolution of its route"? [William Ingram (Reviewer's comment ID #: 114-58)]	Taken into account. Text modified.
8-182	A	13:20		Is "marginal seas" a well defined term? Personally I do not know what it really means. [Martin Manning (Reviewer's comment ID #: 155-47)]	Accepted. Text modified.
8-183	A	13:29	13:29	"El Nino" -> "ENSO" [William Ingram (Reviewer's comment ID #: 114-59)]	Accepted. Text modified.
8-184	A	13:39	13:44	"A widely held view is that enhanced deep mixing as described here has little influence on the North Atlantic meridional overturning and associated heat transport, whereas the Antarctic Bottom Water circulation (which ventilates the abyssal ocean but transports little heat) is strongly dependent on such mixing. This is demonstrated, e.g., by Saenko and Merryfield (J. Physical Oceanography 2005) using an OGCM whose deep mixing	Taken into account. The reference to Saenko and Merryfield is added. The main issue is one of the time scale and is too detailed to fully discuss here.

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				parameterization is arguably more realistic than any in the references cited here. " [Govt. of Canada (Reviewer's comment ID #: 2004-155)]	
8-185	A	13:39	13:39	Reference needed at end of sentence. [William Ingram (Reviewer's comment ID #: 114-60)]	Accepted. Reference to section 8.3.2 added.
8-186	A	14:0	15:	Should be deleted. Refers to model capabilities outside the IPCC framework and is misleading by implying that these aspects feed into IPCC results. [Govt. of United States of America (Reviewer's comment ID #: 2023-510)]	Taken into account. The links between this material – that is why its relevent to the IPCC assessment is made more clear.
8-187	A	14:4	14:20	The sensitivity of the terrestrial biosphere to regional rainfall patterns and shifts in modes of variability should be emphasised. Some of the large positive feedback simulated by HadCM3 can be attributed to die-back of the Amazonian rainforest due to lack of rainfall. Errors in regional precipitation may represent a serious limitation to addressing earth system feedbacks. [Julia Slingo (Reviewer's comment ID #: 243-11)]	Reject – this material is regional in nature. There is insufficient space to effectively address all the regional issues in model evaluation of global climate models
8-188	A	14:5	14:5	"cutting edge" - please, this is supposed to be a scientific document! [William Ingram (Reviewer's comment ID #: 114-61)]	Reject. This is appropriate language, briefly communicating the point
8-189	A	14:5		I don't see how this follows. While bucket models may be worse, that doesn't indicate current land surface models are adequate. The response of vegetation and soil moisture to increasing temperature - the sensitivity of ET to warming - is still quite uncertain in models, and differs greatly between GCM land surface schemes and those used in Impact Models (which has in the past contributed to big differences in projections of future water availability changes between IPCC WGI and WGII) [and note the discussion starting on line 19 which points out that problems remain]. [David Rind (Reviewer's comment ID #: 214-65)]	Accept. Text revised – this was not the inference we wanted and therefore clearly we need to adjust the text to avoid this conclusion being reached by a reader.
8-190	A	14:6	14:6	"dynamics" doesn't mean what it has so far in this chapter - explain or replace [William Ingram (Reviewer's comment ID #: 114-62)]	Reject – this language is common in this specific field
8-191	A	14:15	14:15	"of" -> "for" [William Ingram (Reviewer's comment ID #: 114-63)]	Accept
8-192	A	14:22	14:26	About half of the TAR models did not treat the soil water freezing process. This neglect of phase change process results in different response of summer soil moisture in the northern high latitudes (Yamaguchi et al., 2005). Majority (please check most or not) of the AR4 models use multiple soil layers and include this soil water freezing process, thus reproducing permafrost distribution better than the TAR models. Yamaguchi, K., A. Noda and A. Kitoh, 2005: The changes of permafrost induced by greenhouse warming: A numerical study applying multiple layer ground model. J. Meteor.	Reject – most of the TAR models did include freezing processes. We are not aware of literature that evaluates models based on their incorporation of freezing. Permafrost is a different thing to soil freezing and is poorly included in models. This is noted in the text (see

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				Soc. Japan, 83, 799-815. [Akio Kitoh (Reviewer's comment ID #: 130-1)]	also comment 8-194)
8-193	A	14:22	15:8	The other major limitation of land surface models lies in the representation of soil hydrology, for eample in soil hydraulic properties. For example, Osborne et al. (Osborne, T. M., D. M. Lawrence, J. M. Slingo, A. J. Challinor and T. R. Wheeler, 2004: Influence of vegetation on the local climate and hydrology in the Tropics: Sensitivity to soil parameters. Climate Dynamics, 23, 45-61) showed that the climate sensitivity to changes in soil parameters can be as large as that associated with large vegetation changes. Also soil parameters affect the partitioning between fast and slow run-off and therefore have a critical role to play in estimating the impacts of climate change on water availability and its seasonality. [Julia Slingo (Reviewer's comment ID #: 243-12)]	Reject. This is a single model study. It is hard to infer general behaviour from one study.
8-194	A	14:25	14:27	I think this needs to describe a bit better what exactly is new. The inclusion of soil freezing and thawing, per se, is certainly not new, is it? Even very old bucket type models allow soil moisture to freeze and thaw. Also, very simply types of snow/vegetation interactions have been long standing features in models (e.g., modification of surface albebo with increasing snow depth to accound for more of the vegetation being covered by snow). What is more recent is canopy interception of snow, and the modelling of associated processes. [Francis Zwiers (Reviewer's comment ID #: 305-48)]	Accept – text clarified
8-195	A	14:26	14:28	This seems to have got slightly (or more?) garbled [William Ingram (Reviewer's comment ID #: 114-64)]	Accept – text rewritten
8-196	A	14:37	14:38	Ambiguous - is total runoff & total evapotranspiration, or evapotranspiration & total runoff, or the total of runoff & evapotranspiration, meant? [William Ingram (Reviewer's comment ID #: 114-65)]	Accept – text rewritten
8-197	A	14:46	14:47	It is not at all clear HOW further improvements depend on 'stable isotopes' etc. [Govt. of Finland (Reviewer's comment ID #: 2009-73)]	Accept – text clarified
8-198	A	14:56	14:57	"inclusion … in" or "addition … to" - & "carbon fluxes", not just "carbon", is meant [William Ingram (Reviewer's comment ID #: 114-66)]	Reject – it is not clear what the reviewer means here
8-199	A	15:4	15:4	"variable" - how? In time or space or both? As prescribed, or interactively? [William Ingram (Reviewer's comment ID #: 114-67)]	Accept – text clarified
8-200	A	15:5	15:8	I think these couple of sentences could be deleted. It's not clear what the basis is for the statement or the recommendation. [Francis Zwiers (Reviewer's comment ID #: 305-49)]	Reject – this is our evaluation. The basis is the preceeding text
8-201	A	15:5		While bucket models may be worse, that doesn't indicate current land surface models are	See 8-189

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				adequate. The response of vegetation and soil moisture to increasing temperature - the sensitivity of ET to warming - is still quite uncertain in models, and differs greatly between GCM land surface schemes and those used in Impact Models (which has in the past contributed to big differences in projections of future water availability changes between IPCC WGI and WGII) [and note the discussion starting on line 19 which points out that problems remain]. [Govt. of United States of America (Reviewer's comment ID #: 2023-511)]	
8-202	A	15:12	15:12	Comma needed after "TAR" [William Ingram (Reviewer's comment ID #: 114-68)]	Accept – text modified
8-203	A	15:23	15:24	Omit ". That … strength" - it adds no useful information [William Ingram (Reviewer's comment ID #: 114-69)]	Reject – text is corect
8-204	A	15:24	15:24	Define "coupling strength". [Francis Zwiers (Reviewer's comment ID #: 305-50)]	Reject – this is an understandable comment but due to space limits the best we can do is include the citation
8-205	A	15:31	15:31	"a high occurrence of" -> "frequent" [William Ingram (Reviewer's comment ID #: 114-70)]	Accept – text modifed
8-206	A	15:42	15:45	Given the societal importance of soil moisture, it may be worthwhile to suggest why there has been such a lack of progress and focus. Is it simply a matter of waiting for the carbon cycle to be better done, or is there a fundamental problem with the representation of soil moisture? [Michael Manton (Reviewer's comment ID #: 157-41)]	Accept – although very hard to be clear and specific some comments have been added to the text where these can be supported by the literature
8-207	A	16:1	16:1	"Glaciers" -> "Alpine glaciers" or add text making it explicit that the word is being used here to exclude ice-sheets, which the general reader would expect it to include. [William Ingram (Reviewer's comment ID #: 114-71)]	Rejected. Chapter 4 with the necessary details is referenced in this paragraph. See also the Glossary. However, text modified by inclusion also ice caps.
8-208	A	16:1	16:1	A cross-link to Ch 4 would be useful. [Francis Zwiers (Reviewer's comment ID #: 305-51)]	Rejected. Reference to Ch 4 is given at the end of the paragraph.
8-209	A	16:6	16:6	"area-covered fraction" -> "fractional cover" [William Ingram (Reviewer's comment ID #: 114-72)]	Accepted
8-210	A	16:47	16:47	"includes" is definitely not correct - "is affected by" or "reacts to"? [William Ingram (Reviewer's comment ID #: 114-73)]	Rejected. The whole paragraph is removed.
8-211	A	16:51	16:52	Indeed, there is no evidence they have any such importance, is there? [William Ingram (Reviewer's comment ID #: 114-74)]	Noted. The paragraph is removed.
8-212	A	16:54		Section 8.2.5. The discussion on chemistry modelling ends rather abruptly with the statement that "atmospheric chemistry model components are not included in AR4 models". This requires some explanation and ellaboration and a mention of where this	Accepted.

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				area of work will go in the future. [Gill Martin (Reviewer's comment ID #: 167-3)]	
8-213	A	16:57	16:57	It's not clear what is being said here (the word "through" is confusing). Either this means that modelled aerosol distributions now compare better with observations than at the time of the TAR, or it means that detailed analysis of observations has lead to understanding that has permitted model improvements. [Francis Zwiers (Reviewer's comment ID #: 305-52)]	Taken into account. See the text.
8-214	A	17:1	17:9	A link to the discussion of aerossols in chapter 1 would be appropriate. [Michael Manton (Reviewer's comment ID #: 157-42)]	Noted.
8-215	A	17:3	17:3	"been also" -> "also been" [William Ingram (Reviewer's comment ID #: 114-75)]	Noted.
8-216	A	17:7	17:9	See comment concerning page 12, lines 14-16. Are the models being called out here the same as the ones that were called out earlier? [Francis Zwiers (Reviewer's comment ID #: 305-53)]	Taken into account.
8-217	A	17:11	17:18	Perhaps this could be a bit more comprehensive, by talking about which models have tropospheric chemistry, stratospheric chemistry, and/or both. [Francis Zwiers (Reviewer's comment ID #: 305-54)]	Accepted. The sentence was deleted.
8-218	A	17:20		another example of a discussion of models that are not used for AR4. [David Rind (Reviewer's comment ID #: 214-66)]	Accepted.
8-219	A	17:20		Another example of a discussion of models that are not used for AR4. This refers to the whole section 8.2.5. [Govt. of United States of America (Reviewer's comment ID #: 2023-512)]	Accepted.
8-220	A	17:21	17:21	"to" -> "of" [William Ingram (Reviewer's comment ID #: 114-76)]	Accepted.
8-221	A	17:24	17:41	Is the assessment (rather than review) that couplers are on the right track? Are there alternative strategies, or could we all use the one type? [Michael Manton (Reviewer's comment ID #: 157-43)]	Taken into account. Text added.
8-222	A	17:36	17:41	The example introduced here (concerning the MIROC model) seems awfully specific. Also, are there any models that couple every time step? I think a couple of additional sentences of background to describe a bit more fully the typical coupling approaches would be helpful. [Francis Zwiers (Reviewer's comment ID #: 305-55)]	Taken into account. Text added.
8-223	A	17:40	17:40	In the MIROC model, the coupling interval is 3 hours instead of 1 hour. [Seita Emori (Reviewer's comment ID #: 62-5)]	Accepted. Text modified.
8-224	A	17:41	17:41	An important point here is that high frequency coupling will have little impact unless the vertical resolution of the mixed layer is sufficient to capture processes operating on those	Accepted. Text modified. Reference added.

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				timescales. All current coupled models have a 10 metre top layer which is woefully inadequate. Bernie et al. (Bernie, D., S. J. Woolnough, J. M. Slingo and E. Guilyardi, 2005: Modelling diurnal and intraseasonal variability of the ocean mixed layer. J. Clim., 15, 1190-1202.) showed that a resolution of 1 metre is required in the surface layer to capture this high frequency coupling. Subsequent research in which this vertical resolution was implemented in ocean-only and coupled mode showed that a proper representation of this high frequency coupling can significantly alter the mean state and variability of the tropical Pacific (Daniel Bernie, PhD Thesis, U. Reading). [Julia Slingo (Reviewer's comment ID #: 243-13)]	
8-225	A	18:7	18:7	Omit one "the" [William Ingram (Reviewer's comment ID #: 114-77)]	Accepted. "The" deleted.
8-226	A	18:9	18:10	The effects of climate drift on natural variability cannot be overemphasised. The climate system is highly non-linear and errors in the mean state, particularly in the equatorial Pacific have a substantial impact on MJO activity (Inness P. M., J. M. Slingo, E. Guilyardi and J. Cole 2003: Simulation of the MJO in a coupled GCM. II: The role of the basic state. J. Clim., 16, 365-382), ENSO and its global teleconnections (Turner, A. G., P. M. Inness and J. M. Slingo, 2005: The Role of the Basic State in Monsoon Prediction. Q. J. R. Meteorol. Soc., 131, 781-804). [Julia Slingo (Reviewer's comment ID #: 243-14)]	Taken into account. Climate drift is important for many aspects of the simulation and response. One reference added.
8-227	A	18:9	18:10	This needs a reference. I think we believe this - but has it been documented? [Francis Zwiers (Reviewer's comment ID #: 305-56)]	Taken into account. References added.
8-228	A	18:14	18:15	The last sentence is not entirely consistent with the first sentence on page 17 line 51. In particular, what is the estimate of our level of understanding of initialisation? [Michael Manton (Reviewer's comment ID #: 157-44)]	Rejected. Text okay as is.
8-229	A	18:17		Section 8.3. Most of the subsections of this section do a good job of stating how the simulations have improved since the TAR, but there are exceptions e.g. 8.3.1.1, 8.3.1.2 [Gill Martin (Reviewer's comment ID #: 167-5)]	Taken into account. The literature does not provide much evidence of sytematic improvement of coupled AOGCMs. We do show that the atmospheric models have shown general 8.3.12.
8-230	A	18:22	18:22	"… may in fact be linear to first order,…" Is not everything linear to first order? Perhaps use "approximately linear in response to modest forcing..." [Govt. of United States of America (Reviewer's comment ID #: 2023-513)]	Accepted. Text modified.
8-231	A	18:22	18:23	There is a bit on the additivity of the responses to different forcings in Chapter 9. See last paragragh, 9.4.1.2. [Francis Zwiers (Reviewer's comment ID #: 305-57)]	Accepted. Text modified.
8-232	A	18:23	18:23	Omit 1st full stop [William Ingram (Reviewer's comment ID #: 114-78)]	Accepted. Corrected.

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8-233	A	18:24	18:24	"perfect model" simulations? Please clarify. [Govt. of Finland (Reviewer's comment ID #: 2009-74)]	Accepted. Text modified.
8-234	A	18:25	18:26	Regarding the relationship to "the accurate prediction of future climate" - I'm wondering if this is stated precisely enough. The immediate question that comes to mind is, how do we know the predictions are accurate, and therefore, how can we know that there is, or is not, a relationship? [Francis Zwiers (Reviewer's comment ID #: 305-58)]	Accepted. Text modified.
8-235	A	18:26	18:26	Omit "accurate" to get a sensible sentence which I guess is what was meant - what is written is nonsense as we don't know what is an accurate prediction of the future & so can't see how it might relate to skill at climatology [William Ingram (Reviewer's comment ID #: 114-79)]	Accepted. Text modified.
8-236	A	18:26	18:26	How do we know if a prediction of future climate is 'accurate'? This statement makes no sense. [Julia Slingo (Reviewer's comment ID #: 243-15)]	Accepted. Text modified.
8-237	A	18:29		"or dynamical" could be added after "physical". [Adrian Simmons (Reviewer's comment ID #: 242-117)]	Accepted. Text modified.
8-238	A	18:29		Nevertheless deficiencies in simulating the current climate could indicate [Govt. of United States of America (Reviewer's comment ID #: 2023-514)]	Accepted. Text modified.
8-239	A	18:33	18:33	These two specific references are not needed here. [Akio Kitoh (Reviewer's comment ID #: 130-2)]	Rejected. Support from the literature adds authority to the assertion, but to indicate that these are just two of several references that could be cited, we now preface the references with "e.g.".
8-240	A	18:35	18:36	"which ... change" - I should hope not: we know none are! [William Ingram (Reviewer's comment ID #: 114-80)]	Accepted. Text modified.
8-241	A	18:40	18:40	Swap "natural ecosystems" & "societies" for clarity & to better reflect policymakers' priorities? [William Ingram (Reviewer's comment ID #: 114-81)]	Taken into account. Text revised.
8-242	A	18:42		Comment about most of the following discussion being focused on CMIP models is not followed in the subsequent discussion (e.g., 8.3.1.3, the land surface discussion, etc.) [Govt. of United States of America (Reviewer's comment ID #: 2023-515)]	Accepted. Replaced "Much" with "Some" at the beginning of the paragraph.
8-243	A	18:43	18:43	Are these CMIP 20thC simulations the same as the 'pcmdi' (8-9 119) set? [Govt. of Australia (Reviewer's comment ID #: 2001-347)]	Accepted. To clarify, through out the report the simulations will be referred to as "the multi-model dataset at PCMDI".

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8-244	A	18:51	18:51	Misleading - the model mean will not in general reflect a systematic error, though of course all systematic errors will be seen in it. [William Ingram (Reviewer's comment ID #: 114-82)]	Rejected. It seems to us that if all systematic errors are seen in the "multi-model mean field" (as stated by Ingram), then we are correct in saying the we can "identify errors that are systematic across models."
8-245	A	18:51	18:51	Should we call them CMIP models throughout the AR4? In Ch 9, we refer to IPCC AR4 models, which doesn't recognize CMIP, but which does distinguish between these models, and earlier versions of CMIP. [Francis Zwiers (Reviewer's comment ID #: 305-59)]	Accepted. See 8-243 response.
8-246	A	19:1	19:2	"less prone to bias" - actually, more prone to bias if "bias" means "systematic error", as I would expect the innocent reader to interpret it [William Ingram (Reviewer's comment ID #: 114-83)]	Accepted. Text reworded.
8-247	A	19:6	19:6	The title "Atmospheric component" does not reflect the contents of the section. The quality of the simulation of surface temperature and other atmospheric variables depends on all components of the AOGCM rather than merely on the atmospheric part. Consider replacing "Atmospheric component" by "Atmospheric variables" etc. [Govt. of Finland (Reviewer's comment ID #: 2009-75)]	Accepted. Changed to "Atmosphere."
8-248	A	19:6		Section 8.3.1. Evaluation of clouds is missing from this section. It is mentioned in the cloud feedback section (8.6.2.3) but should be included here also. E.g. Martin et al. (2006; J Climate, April 1st issue) shows improvements in the vertical distribution and optical thickness distribution in HadGEM1 compared with HadCM3, using the ISCCP simulator. Although the reason for the improvement in low clouds is not clear, it is hypothesised to be a result of the combination of new (Lock et al, 2000) boundary layer scheme, semi-Lagrangian dynamical core and vertical grid staggering which improves the interaction of the dynamics with the inversion. [Gill Martin (Reviewer's comment ID #: 167-8)]	No change necessary. Some discussion appears in section 8.6, and in order to adhere to report length constraints, it is not possible discuss individual model improvements.
8-249	A	19:18	19:18	What is meant by "surface air temperature"? As far as I know (almost) all models assume continuity of temperature at the surface, so the temperature of the air at the surface is the surface temperature. [William Ingram (Reviewer's comment ID #: 114-84)]	No change necessary. The surface air temperature differs some from surface temperature in most models, which use various methods to estimate it at 2 or 3 meters above the surface.
8-250	A	19:18	19:20	The contents of the Figure are not what the text says [William Ingram (Reviewer's comment ID #: 114-85)]	Accepted. Text corrected.
8-251	A	19:34	19:34	This large correlation is, of course, aided by the large land-sea temperature contrast. [Francis Zwiers (Reviewer's comment ID #: 305-60)]	No change necessary. Actually it is the meridional gradient of the pattern that primarily leads to the high correlation

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					in the *annual mean* temperature, whereas in individual seasons the land-sea contrast is also important.
8-252	A	19:42	19:44	This sentence is plainly untrue as written: I guess the 1st "the" on line 42 is the mistake [William Ingram (Reviewer's comment ID #: 114-86)]	Accepted.
8-253	A	19:47	19:48	"off the east coasts of North America and Asia" - quite untrue: these have a stronger seasonal cycle than any other part of the ice-free ocean [William Ingram (Reviewer's comment ID #: 114-87)]	Accepted. Text revised.
8-254	A	19:49	19:49	quite accurate -> fairly accurate; "quite" is a too emphatic expression in this context. [Govt. of Finland (Reviewer's comment ID #: 2009-76)]	Accepted. Text revised.
8-255	A	19:54	20:7	If possible, it would be useful to separate this analysis into max and min temperature in order to see if the error is greater in one or the other (which would point to different processes being in error e.g. stable versus unstable boundary layer). [Gill Martin (Reviewer's comment ID #: 167-4)]	Rejected. Space restrictions preclude expanding this discussion.
8-256	A	19:55	20:1	" so ... on" clearer & more accurate as "and so will only be discussed here for" [William Ingram (Reviewer's comment ID #: 114-88)]	Taken into account. Text revised.
8-257	A	20:1	20:7	This discussion of the diurnal cycle and diurnal temperature range is missing a vital piece of information - that is that many climate models poorly simulate the PHASE of the diurnal cycle in cloudiness and rainfall. In many models it rains before noon rather than in the late afternoon (see Yang, G-Y. and J. M. Slingo, 2001: The diurnal cycle in the tropics. Mon. Weath. Rev., 129, 784-801 for an example of this in HadAM3). That means that the cloudiness builds up to early in the day and effectively cuts off the solar heating. This is considered to be a major error in the models and one that suggests major shortcomings in the representation of boundary layer and convective processes. WCRP have identified this as one of the top priorities for future model improvement. [Julia Slingo (Reviewer's comment ID #: 243-16)]	Accepted. Discussion has been slightly expanded.
8-258	A	20:4	20:7	Another possibility could be the land surface. Quite a few models have trouble simulating surface temperature variability in the transition seasons in temperate climates (i.e., when the annual cycle passes through 0C) due to heat budget constraints associated with the freezing and thawing of soil. Kharin et al (2005, listed in the references) see the effects of this in simulated surface temperature extremes. This effect would also reduce DTR in the transition seasons - and thus in the annual mean. [Francis Zwiers (Reviewer's comment ID #: 305-61)]	Accepted. This is now noted in the text.
8-259	A	20:9	20:15	I was puzzled by this paragraph. Yes, the downwelling IR flux is large but it is balanced to within 50-150 Wm-2 by the upwelling IR flux. In fact the downwelling IR flux does not vary anything like as much as the incident solar radiation does due to cloudiness variations.	Taken into account. The discussion was revised and shortened.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Julia Slingo (Reviewer's comment ID #: 243-17)]	
8-260	A	20:12	20:13	Untrue, & based on thinking about the wrong quantity - the *net* longwave flux is much smaller [William Ingram (Reviewer's comment ID #: 114-89)]	Taken into account. The discussion was revised and shortened.
8-261	A	20:21	20:22	Omit "in some cases," as the sentence already has "may" in it [William Ingram (Reviewer's comment ID #: 114-90)]	Accepted.
8-262	A	20:23	20:23	What is "moist entropy" supposed to be? Not a concept known to thermodynamics - nor is entropy conserved: that indeed is one of the most fundamental pieces of physics around. I assume "energy" is meant. [William Ingram (Reviewer's comment ID #: 114-91)]	Rejected. "moist entropy" is defined and used in theoretical studies, and is approximately conserved, for example, during moist convection.
8-263	A	20:36		Agreement between model insolation has in fact been shown to be suspect by Raschke et al. 2005 [Raschke, E., M. A. Giorgetta, S. Kinne, and M. Wild (2005), How accurate did GCMs compute the insolation at TOA for AMIP-2?, Geophys. Res. Lett., 32, L23707, doi:10.1029/2005GL024411] with latitudinal differences up to +-7 Wm-2. [Richard Allan (Reviewer's comment ID #: 3-77)]	Rejected. Coupled models no longer show such large differences (which in AMIP 2 were of relatively little consequence because SST's were prescribed).
8-264	A	20:39	20:39	"appears to be fairly uniformly bright". This may be confusing! In fact, the planetary albedo is much larger in high than in low latitudes. [Govt. of Finland (Reviewer's comment ID #: 2009-77)]	Accepted. Text reworded.
8-265	A	20:39	20:39	"uniformly bright" - no, it must be increasingly bright at higher latitudes to reflect as much energy when less is incident! Well, to me "bright" most naturally means "reflective" for things which have light only by reflecting it. Change to unambiguous text like "... on average, reflects about as much energy (100 Wm-2) at all ..."? [William Ingram (Reviewer's comment ID #: 114-92)]	Accepted. Text reworded.
8-266	A	21:2	21:2	Is the observed outgoing SW radiation well known? If not, the whole 13.4 w/m2 shouldn't be pinned on the models. [Francis Zwiers (Reviewer's comment ID #: 305-62)]	Taken into account. Observational uncertainty is not a major factor compared with errors in cloud fields.
8-267	A	21:6	21:6	Why? Because all the modellers are aiming at reality, with variety of errors, of course! [William Ingram (Reviewer's comment ID #: 114-93)]	Rejected. This simple explanation cannot presently be justified.
8-268	A	21:10	21:11	In fact, the radiation balance is positive in the low and negative in the high latitudes. [Govt. of Finland (Reviewer's comment ID #: 2009-78)]	Accepted. Text revised.
8-269	A	21:10	21:12	Maybe mention that that fluxes only compensate in the equilibrium, and mention that the system is NOT supposed to be equilibrium today due to anthropogenic effects if our understanding of the current trends is correct. [Reto Knutti (Reviewer's comment ID #: 133-3)]	Rejected. This is discussed elsewhere.
8-270	A	21:11	21:11	Add "to space" to clarify for the innocent reader where this radiation "from the surface and the atmosphere" is going?	Accepted. Text revised.

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				[William Ingram (Reviewer's comment ID #: 114-94)]	
8-271	A	21:19	21:19	Is the rms error in Fig. 8.3.3b indeed caused by seasonal cycle only? [Govt. of Finland (Reviewer's comment ID #: 2009-79)]	Taken into account. Both seasonal and longitudinal variations are important.
8-272	A	21:34		Section 8.3.1.2 The discussion of precipitation in the models paints rather a rosy picture, despite the fact that precipitation is generally regarded as one of the most difficult things to simulate. This section would benefit from some discussion of changes since the TAR, given that model resolution and convection schemes have changed, both of which will affect the precipitation distribution. Also, some measure of the spread of the multi-model ensemble would be useful. [Gill Martin (Reviewer's comment ID #: 167-6)]	Accepted. Discussion of newly available information about changes in model ability to simulate precipitation now included.
8-273	A	21:35	21:35	Omit 1st "the" [William Ingram (Reviewer's comment ID #: 114-95)]	Accepted.
8-274	A	21:36	21:36	Omit last "the" [William Ingram (Reviewer's comment ID #: 114-96)]	Accepted.
8-275	A	21:36		It is not quite correct (here and in the figure caption) to call the Xie and Arkin observation-based estimates of precipitation "observed", especially over the oceans. There is not especially good agreement between different such estimates of rainfall over the tropical oceans. See section 3.3.2.5. [Adrian Simmons (Reviewer's comment ID #: 242-119)]	Accepted. "observed" changed to "observationally-based estimates of"
8-276	A	21:37	21:38	To say that high precipitation amounts in low latitudes is more directly related to temperature than insolation is questionable, and is probably best omitted. It rains less in the subtropics than in middle laitudes, even though it is warmer in the subtropics. And at very low latitudes much of the precipitation is associated with deep convection, where the (insolation-driven) warmth of the lowermost atmosphere is a key factor. [Adrian Simmons (Reviewer's comment ID #: 242-118)]	Accepted. Text shortened and revised.
8-277	A	21:46	22:15	Figure 8.3.4 is not helpful unless it also includes the difference between the 'observed' and model mean. Overall the text suggests that there is more skill in precipitation than is the case and we should not hide the fact that regional rainfall patterns remain a serious issue. Furthermore the temporal characteristics are also poor e.g. phase of the diurnal cycle, frequency of dry days. [Julia Slingo (Reviewer's comment ID #: 243-18)]	Rejected (mostly). Space constraints preclude inclusion of this figure, but it appears in the supplemental material. Text here and elsewhere revised to put more emphasis on remaining problems.
8-278	A	21:47	21:48	The lower net surface radiative heating is more important than the lower temperatures in causing the evaporation to be so low [William Ingram (Reviewer's comment ID #: 114-97)]	Accepted. Text shortened and revised.
8-279	A	21:50	21:50	Explain "ITCZ"? [William Ingram (Reviewer's comment ID #: 114-98)]	Accepted. Text revised.

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8-280	A	21:50		I don't believe the Pacific ITCZ in general does cross into the S.H. [David Rind (Reviewer's comment ID #: 214-67)]	Accepted. Text corrected.
8-281	A	21:50		The Pacific ITCZ in general does cross into the S.H., so the 'explanation' should be modified. [Govt. of United States of America (Reviewer's comment ID #: 2023-516)]	Accepted. Text corrected.
8-282	A	22:0	22:0	There is a serious omission here concerning tropical weather systems and organised convection. If there is to be a section on extra-tropical storms then there should also be one on tropical systems. The recent THORPEX-WCRP Workshop identified serious problems in the simukation of organised convection and tropical weather systems by climate models as shown in Lin et al. 2006. This issue was also discussed in Slingo, J. M., P. M. Inness, R. B. Neale, S. J. Woolnough and G-Y. Yang, 2003: Scale interactions on diurnal to seasonal timescales and their relevance to model systematic errors. Annales Geophysicae, 46, 139-155. [Julia Slingo (Reviewer's comment ID #: 243-19)]	Taken into account. Additional information on tropical precipitation added.
8-283	A	22:1	22:15	There seems to be a discrepancy between Figure 8.3.4 and the text at line 10-12, in relation to the annual precipitation in the tropical Atlantic, this should be reviewed and either the text or figure changed for consistency. [Govt. of Australia (Reviewer's comment ID #: 2001-348)]	Accepted. Text reworded.
8-284	A	22:12	22:13	"Some … fields" - presumably in that the AGCMs do better with real-world SSTs - say so? [William Ingram (Reviewer's comment ID #: 114-99)]	Taken into account. Text reworded.
8-285	A	22:14	22:14	This must be partly due to scaling problems - models make it rain on grid boxes rather than rain gages, so even if there were no problems with the parameterization of precipitation producing processes or with the observations, we might still expect to see some APPARENT bias in the frequency of precipitation (to high) and in intensity (too low). [Francis Zwiers (Reviewer's comment ID #: 305-63)]	Rejected. In the referenced material this potential explanation was ruled out.
8-286	A	22:20	22:21	In fact, the best way to assess changing atmospheric transport of water is to measure streamflow. In humid regions (I.e., those where the action is), terrestrial water storage variations are small and so runoff (observable with high accuracy as streamflow) is nearly identical to atmospheric water vapor convergence, especially at annual and longer time scales. See Milly et al. (2005). Measuring vapor content of the column is a great thing to do, but it is only a crude index of transport. [P.C.D. Milly (Reviewer's comment ID #: 179-20)]	Taken into account. This discussion was rewritten.
8-287	A	22:21	22:32	Replace "vapor" by "vapour" on lines 21, 27, 31 and 32. [Govt. of Finland (Reviewer's comment ID #: 2009-80)]	Taken into account. "Vapor" will be spelled consistently throughout the report.

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8-288	A	22:35	22:35	Section 8.3.1.3 Extra-tropical storms. Change the title into "Sea level pressure and atmospheric circulation". Start this section with a discussion on the quality of mean sea level pressure fields as simulated by the 23 coupled models that were run for AR4. Pertinent information on this issue can be found in: Van Ulden and Van Oldenborgh (2006), section 2 in particular. This new section will provide a better match with the corresponding section in Chapter 10 (10-3.2.4). Reference: Van Ulden, A.P. and G.J. van Oldenborgh, 2006: Large-scale atmospheric circulation biases and changes in global climate model simulations and their importance for climate change in Central Europe. Atmos. Chem. Phys., 6, 863-881. Freely accesible at: www.atmos-chem-phys.net/6/863/2006 [Govt. of Netherlands (Reviewer's comment ID #: 2016-44)]	Taken into account. This section was modified extensively. Space limitations preclude discussion of sea level pressure..
8-289	A	22:35	23:8	A great contribution to this section would be given by adding reference to the influential paper by Lucarini, V., Calmanti, S., Dell'Aquila, A., Ruti, P.M., Speranza, A., 2006: Intercomparison of the northern hemisphere winter mid-latitude atmospheric variability of the IPCC models. Climate Dynamics in press (also in the PCMDI preprint server). In this paper an assessment of the degree of mutual consistency and realism of the representation of the northern hemisphere mid-latitude winter atmospheric variability is performed on the available XX century simulations of 19 GCMs included in the IPCC4AR (time frame 1962-2000). The investigation relies on the space-time Hayashi spectra of the 500hPa geopotential height fields and models are evalutaed witha metrics based on ad hoc integral measure of the atmospheric variability on different spectral sub-domains. The total wave variability is taken as a global scalar metrics describing the overall performance of each model, while the total variability pertaining to the eastward propagating baroclinic waves and to the planetary waves are taken as scalar metrics describing the performance of each model in describing the corresponding specific physical process. Large biases, in most cases larger than 20%, are found in all the considered metrics between the wave climatologies of most IPCC models and the reanalises. The span of the climatologies of the various models is in all cases over 50% of the climatology of the reanalises. In particular, the baroclinic waves are typically overestimated by the climate models, while the planetary waves are usually underestimated. This closely resembles the results of many diagnostic studies performed in the past on global weather forecasting models. The vertical resolution of the atmosphere and, somewhat unexpectedly, of the adopted ocean model seem to be critical in determining the agreement of the climate models with the reanalyses. This study proposes some criticalities and suggests some caveats in the ability of most of the presently available climate models in describing the statistical properties of the global scale atmospheric dynamics of the present climate, and, a fortiori, in the perspective of climate change.	Taken into account. Paragraph rewritten inserting new references where pertinent.

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				[Teresa Nanni (Reviewer's comment ID #: 186-7)]	
8-290	A	22:41		Provides one piece of information on extratropical cyclones [Govt. of United States of America (Reviewer's comment ID #: 2023-517)]	Taken into account. Text reworded.
8-291	A	22:42	22:42	Paciorek et al also compare and discuss various cyclone indices. Paciorek, Christopher J., Risbey, James S., Ventura, Valerie, Rosen, Richard D. Multiple Indices of Northern Hemisphere Cyclone Activity, Winters 1949-99 Journal of Climate 2002 15: 1573-1590 [Ruth McDonald (Reviewer's comment ID #: 173-35)]	Taken into account. Paragraph rewritten inserting new references where pertinent.
8-292	A	22:44	22:47	Trigo (Clim Dyn 26: 127-143 2006) compared the cyclones in NCEP/NCAR reanalysis data to those in the ERA 40 data and found some discrepancies in the number of storms in the two datasets. [Ruth McDonald (Reviewer's comment ID #: 173-36)]	Taken into account. Paragraph rewritten inserting new references where pertinent.
8-293	A	22:45	22:46	Rewording is recommended here. Observation counts are dominated by those from satellites in the northern as well as the southern hemisphere. It would be better to replace "where observations are dominated by satellites" by "where there are fewer ground- and aircraft-based observations" [Adrian Simmons (Reviewer's comment ID #: 242-120)]	Accepted.
8-294	A	22:47	22:47	Add "and Bromwich and Fogt (2004). Reference: Bromwich, D.H. and R.L. Fogt, 2004: Strong trends in the skill of the ERA-40 and NCEP-NCAR reanalyses in the high and midlatitudes of the southern hemisphere, J. Climate, 17, 4603-4619. [Govt. of Netherlands (Reviewer's comment ID #: 2016-45)]	Taken into account. Paragraph rewritten inserting new references where pertinent.
8-295	A	22:54	22:54	I think it would be safe to delete "tend to". [Francis Zwiers (Reviewer's comment ID #: 305-64)]	Accepted.
8-296	A	22:55	23:3	Mention should be made of storm track analysis in Ringer et al, (2006; J Clim, 1st April issue) since this shows improvement in the simulation of NH storm tracks when both the dynamical core and the horizontal resolution are changed. The improvement can be attributed to both of these aspects. [Gill Martin (Reviewer's comment ID #: 167-7)]	Taken into account. Paragraph rewritten inserting new references where pertinent.
8-297	A	22:55	23:3	The CSRIO mk2 and mk3 models were compared by Watterson (Tellus 2006) and there is an improvement in the cyclones numbers in the higher resolution mk3 model. [Ruth McDonald (Reviewer's comment ID #: 173-37)]	Taken into account. Paragraph rewritten inserting new references where pertinent.
8-298	A	23:1	23:1	ECHAM5/MPI-OM (not ECHAM5-OM) [Marco A. Giorgetta (Reviewer's comment ID #: 85-2)]	Taken into account. That text has been entirely eliminated for other reasons.
8-299	A	23:1	23:2	Delete the bit in parentheses. This example is not needed - resolution change has already been dealt with as a general item. [Francis Zwiers (Reviewer's comment ID #: 305-65)]	Accepted.

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8-300	A	23:6	23:8	Didn't you just say exactly the same thing in the previous sentence – or am I missing something? [Martin Manning (Reviewer's comment ID #: 155-48)]	Accepted. Previous sentence removed.
8-301	A	23:10	23:10	The title "Oceanic component evaluation" does not sound good here. The quality of the simulation of oceanic variables depends on all components of the AOGCM rather than merely on the oceanic part. Consider replacing "Oceanic component evaluation" by "Oceanic variables" etc. [Govt. of Finland (Reviewer's comment ID #: 2009-81)]	Accepted. Title changed.
8-302	A	23:12	23:12	How do you know what variables are important? I would argue we know very little about which aspects of present day climate matter for the transient response. For example I doubt whether ocean salinity is important for the projections of the next few decades. [Reto Knutti (Reviewer's comment ID #: 133-4)]	Reject. Oceanic heat uptake is known to be important in the transient response of climate models (See early Hansen et al. Papers). To the extent oceanic heat uptake is impacted by surface processes, then SSS is important.
8-303	A	23:14	23:14	"supplemental" -> usual "supplementary" [William Ingram (Reviewer's comment ID #: 114-100)]	Accepted. Used SM everywhere.
8-304	A	23:16	23:16	Cross links to the observational chapters, pointing to the assessment of data quality or adjustment proceedure (e.g., Appendix 3.B.3) would be useful. [Francis Zwiers (Reviewer's comment ID #: 305-66)]	Accepted. Reference added.
8-305	A	23:19	23:19	"issueS" [William Ingram (Reviewer's comment ID #: 114-101)]	Accepted. "S" added.
8-306	A	23:24		"See section 8.3.2" – But this IS section 8.3.2 ! [Martin Manning (Reviewer's comment ID #: 155-49)]	Accepted. Text modified.
8-307	A	23:25	23:26	Remove "Based on" & add "shows that" after "experience"? [William Ingram (Reviewer's comment ID #: 114-102)]	Accepted. Text modified.
8-308	A	23:26	23:27	"Suggest changing ""this is a coupled problem where the fidelity"" by ""the ocean and atmosphere are coupled, so that the fidelity""" [Govt. of Canada (Reviewer's comment ID #: 2004-156)]	Accepted. Text modified.
8-309	A	23:32		"Suggest inserting ""for heat flux"" after ""W m-2"" " [Govt. of Canada (Reviewer's comment ID #: 2004-157)]	Accepted. Text added.
8-310	A	23:36	23:36	off-line -> on-line? [Reto Knutti (Reviewer's comment ID #: 133-5)]	Taken into account. Text modified. SM added.
8-311	A	23:39	23:39	"0.6 PW" - but at what latitude? [William Ingram (Reviewer's comment ID #: 114-103)]	Accepted. Latitude added.

No.	Batch	Page:line From	Page:line To	Comment	Notes
8-312	A	23:40	23:40	"45'N" - completely untrue: "30'N" may be meant [William Ingram (Reviewer's comment ID #: 114-104)]	Taken into acount. Text modified.
8-313	A	23:45	23:45	In Figure 8.3.5, same legend (green dashed) is used for MIROC3.2(medres) and GFDL-CM2.1. It appears that MIROC3.2(medres) should be drawn by a cyan dashed line according to other similar figures. [Seita Emori (Reviewer's comment ID #: 62-6)]	Taken into account. Figures modified.
8-314	A	23:48	23:49	Shorter & more informative to add "and delay" after "damp" & remove "and shift its phase" [William Ingram (Reviewer's comment ID #: 114-105)]	Accepted. Text modified as suggested.
8-315	A	23:50	23:50	Again, supplemental -> usual "supplementary" [William Ingram (Reviewer's comment ID #: 114-106)]	Taken into account. Used SM.
8-316	A	23:52	23:52	"tropical convergence zones" obscure to the general reader - the phrase (not in the Glossary) has previously been used & explained for the ITCZ &c [William Ingram (Reviewer's comment ID #: 114-107)]	Accepted. The end of the sentence is rewritten.
8-317	A	23:52	23:52	"pathways" again unexplained & I suspect misleading to the innocent reader [William Ingram (Reviewer's comment ID #: 114-108)]	Accepted. The end of the sentence is rewritten
8-318	A	24:3	24:3	"observational estimates" - the ERA windstresses are not observational estimates: they are model output, from a model some aspects of which are constrained to be very close to observations. Maybe the authors consider them the best guess available, but this needs justifying, if only with a quick reference. [William Ingram (Reviewer's comment ID #: 114-109)]	Accepted. Use "model-based observational estimates from reanalysis".
8-319	A	24:5	24:5	"observations" - again, we have no observations of this quantity. The ERA windstresses are model output, from a model some aspects of which are constrained to be very close to observations. Maybe the authors consider them the best guess available, but this needs justifying, if only with a quick reference. [William Ingram (Reviewer's comment ID #: 114-110)]	Accepted. Changed to "reanalysis"
8-320	A	24:6	24:6	"observations" - again, we have no observations of this quantity. The ERA windstresses are model output, from a model some aspects of which are constrained to be very close to observations. Maybe the authors consider them the best guess available, but this needs justifying, if only with a quick reference. [William Ingram (Reviewer's comment ID #: 114-111)]	Accepted. Changed to "reanalysis"
8-321	A	24:10	14:11	"when climate changes" maybe slightly clearer as "under climate change" [William Ingram (Reviewer's comment ID #: 114-112)]	Accepted. Text changed as suggested.
8-322	A	24:19	24:19	"related" -> "due" clearer & fully justifiable [William Ingram (Reviewer's comment ID #: 114-113)]	Accepted. Text modified as suggested.
8-323	A	24:22	24:22	Omit "it should be noted that" - it adds nothing	Accepted. Text modified as suggested.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[William Ingram (Reviewer's comment ID #: 114-114)]	
8-324	A	24:23	24:23	"due to" -> "by" slightly clearer [William Ingram (Reviewer's comment ID #: 114-115)]	Accepted. Text modified as suggested.
8-325	A	24:29	24:30	I was surprised not to see limited resolution mentioned as a contributor - is it really known not to be relevant? [William Ingram (Reviewer's comment ID #: 114-116)]	Accepted. Text modified to make it clearer.
8-326	A	24:36	24:39	It is important to emphasise here the cold tongue problem in the equatorial Pacific. The absolute errors may appear small but they are highly significant at those SSTs and affect the E-W temperature greadient with implications for El Nino. This section is overselling the skill of the models. [Julia Slingo (Reviewer's comment ID #: 243-20)]	Accepted. Sentence added with caveats on quality of the simulation.
8-327	A	24:43	24:51	"Should mention that, for a given model, the amount by which deep ocean temperatures depart from observations is strongly dependent on how long the model has been run, since it takes thousands of years for the deep ocean to come into equilibrium with surface forcing." [Govt. of Canada (Reviewer's comment ID #: 2004-158)]	Accepted. Sentence added.
8-328	A	24:44	24:46	Clearer to compress - remove "The error ..." sentence & add ", about 2K," after "error" in previous sentence [William Ingram (Reviewer's comment ID #: 114-118)]	Accepted. Text modified as suggested.
8-329	A	24:45	24:45	Ambiguous - is "the region where most of the models form their NADW" meant, or "most of the models have maximum error in the same place, that of NADW formation"? [William Ingram (Reviewer's comment ID #: 114-117)]	Accepted. Text modified to make meaning clearer.
8-330	A	24:48	24:48	"with the exception of" more readable as "except for" [William Ingram (Reviewer's comment ID #: 114-119)]	Accepted. Text modified as suggested.
8-331	A	25:2	25:2	mean model -> model mean. [Govt. of Finland (Reviewer's comment ID #: 2009-82)]	Rejected. "Mean model" is label given to multi-model average.
8-332	A	25:3	25:4	Again, I would have expected limited resolution to contribute, at least in some models - is it really known not to be relevant? [William Ingram (Reviewer's comment ID #: 114-120)]	Rejected. Resolution limitations are noted earlier in text. Space limitations will not allow repeating it here.
8-333	A	25:7	25:52	The acronym "AAIW" is used in line 7 but not defined till line 52 - shift definition from line 52 to line 7 [William Ingram (Reviewer's comment ID #: 114-121)]	Accepted. Text modified as suggested.
8-334	A	25:7		"AAIW should be defined here, rather than on line 52" [Govt. of Canada (Reviewer's comment ID #: 2004-159)]	Accepted. Text modified.

No.	Batch	Page:line From	Page:line To	Comment	Notes
8-335	A	25:7		"Need to change ""as discussed above"" to ""as discussed below"" (cf. line 52)" [Govt. of Canada (Reviewer's comment ID #: 2004-160)]	Accepted. Text modified as suggested.
8-336	A	25:26	25:26	"Sv" used but has not been defined, is not a standard physical unit & is not in the Glossary [William Ingram (Reviewer's comment ID #: 114-122)]	Accepted. Units added.
8-337	A	25:37	25:37	It is debatable whether "Overall, the simulation of the MOC has improved since TAR." Looking at Figure 10.3.13 as compared to the equivalent figure in the TAR you would be hard pressed to convince someone! Re-word - "Some aspects of the simulation of the MOC have improved since TAR." Earlier the chapter says (page 24, lines 44-46) that the NADW has an error of about 2K - since NADW is a major component of the MOC and this is a large error it is difficult to then claim that the MOC simulation has improved! [Meric Srokosz (Reviewer's comment ID #: 250-11)]	Taken into account. Text modified as suggested.
8-338	A	25:41	25:41	"GIN" also has not been defined, is not a standard geographical term & is not in the Glossary [William Ingram (Reviewer's comment ID #: 114-123)]	Accepted. GIN defined.
8-339	A	25:51	25:51	"placed" can be omitted to simplify the sentence slightly [William Ingram (Reviewer's comment ID #: 114-124)]	Accepted. Word deleted.
8-340	A	25:53	26:2	Confusing - what, if any, is the connexion between these 2 watermasses both being too warm & salty? Clarify [William Ingram (Reviewer's comment ID #: 114-125)]	Accepted. Wording changed.
8-341	A	26:18	26:18	Two "which"s read badly - suggest "… ocean, maximizing near the surface, which may …" [William Ingram (Reviewer's comment ID #: 114-126)]	Accepted. Wording changed.
8-342	A	26:20	26:20	"and this" -> ", which" slightly more readable [William Ingram (Reviewer's comment ID #: 114-127)]	Accepted. Text modified as suggested.
8-343	A	26:28	26:28	"inevitable" -> "inevitably" [William Ingram (Reviewer's comment ID #: 114-139)]	Accepted. NOTE: this refers to 28:26, not 26:28.
8-344	A	26:32	26:35	This long complicated sentence with several parenthetical remarks is tough to read! [Francis Zwiers (Reviewer's comment ID #: 305-67)]	Accepted. Text modified.
8-345	A	26:41	26:41	"extent" ambiguous (& not in Glossary) - does it mean where the stuff is (which seems the most natural meaning to me) or how great an area it covers (which is the conventional meaning in this context)? Explain. [William Ingram (Reviewer's comment ID #: 114-128)]	Taken into accouint. Explanation is added with a reference to Ch 4, where the term is introduced.
8-346	A	26:42	26:42	The parenthesis is bizarre given the text immediately proceeds to say that most models have unrealistically large ice extents [William Ingram (Reviewer's comment ID #: 114-129)]	Rejected. We don't say that other models have unrealistic sea ice extents. Text modified however for other

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					reasons.
8-347	A	26:48	26:49	Looking at Figure 8.3.11, neither of these statements look true, unless fractional spread is meant, in which case it should be stated. [William Ingram (Reviewer's comment ID #: 114-130)]	Taken into account. Text modified.
8-348	A	26:51	26:52	This sentence could be removed, and replaced by adding a phrase such as "suggesting that projections of sea ice extent remain highly uncertain" at the end of the previous sentence. [Francis Zwiers (Reviewer's comment ID #: 305-68)]	Taken into account. Text modified.
8-349	A	26:54		why is this a concern particularly for models with low to moderate high latitude amplification? Is there some assumption that there should be large amplitude amplification? [David Rind (Reviewer's comment ID #: 214-68)]	Taken into account. Text modified.
8-350	A	26:57	6:57	Omit "atmospheric" & "oceanic" [William Ingram (Reviewer's comment ID #: 114-131)]	Accepted. Text modified.
8-351	A	27:0		In this 8.3.4 Land Surface Component section, there is no mention of the Roesch (2006) result that showed significant positive biases in tropical desert albedo in several of the models. The most appropriate location for reporting this is probably "8.3.4.3 Surface Fluxes". It appears that Roesch's positive albedo results may be at odds with Wild's "increased absorption", although Roesch's results are specific to certain regions and add up to net global biases. [Martin Lewitt (Reviewer's comment ID #: 146-3)]	Reject – cannot find the paper cited (and appears to be regional according to the reviewers comments)
8-352	A	27:1	27:4	probably should include vertical mixing in the ocean in this paragraph. [David Rind (Reviewer's comment ID #: 214-69)]	Accepted.. Text modified.
8-353	A	27:1		Probably should include vertical mixing in the ocean in this paragraph, which is a dominant influence in the S.H. [Govt. of United States of America (Reviewer's comment ID #: 2023-518)]	Accepted. Text modified.
8-354	A	27:6	27:6	Land-surface component -> Land-surface simulation. The quality of land-surface simulation is determined by the entire model, not merely on the land-surface parameterizations. [Govt. of Finland (Reviewer's comment ID #: 2009-83)]	Accept – text modified
8-355	A	27:6		Section 8.3.4. Decrease in permafrost is one of major problems in high latitudes. This section should include assessment of permafrost in current AOGCMs. [Akio Kitoh (Reviewer's comment ID #: 130-3)]	Reject – there is no literature to systematically assess this component.
8-356	A	27:8	27:9	The suggestion that a lack of suitable observations limits ability to model coupled climate systems may be accurate, however, policy readers need an explanation of why there are failings in observations and how these can be addressed by researchers. This comment applies more generally, the authors should make it clear throughout the chapter where	Reject – this is a comment specific to the land surface

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				there are failings in models due to observational weaknesses. [Govt. of Australia (Reviewer's comment ID #: 2001-349)]	
8-357	A	27:14	27:46	"There are several redundancies in these three paragraphs: (1) ""excessive snow...in spring"": line 17 vs 33; (2) ""interannual variability is too low during melt"", line 25 vs ""Year-to-year variations are often underestimated... in winter and spring"", line 35; (3) ""surface albedo over snow-covered forests is generally too high"", line 36-37 vs ""largest discrepancies in albedo are for forested areas under snowy conditions"", line 39. Suggest consolidating paragraphs 1 & 2, deleting last sentence in paragraph 2. " [Govt. of Canada (Reviewer's comment ID #: 2004-161)]	Accept – text modified
8-358	A	27:15	27:37	the first paragraph says that the AMIP II models overestimate ablation during spring; the second paragraph says the CMIP models suffer from a delayed spring snow melt. Is this a real difference between the models, and if so, is it known why it occurred? [David Rind (Reviewer's comment ID #: 214-70)]	No change necessary – we are not aware of any analysis of this issue reported in the literautre
8-359	A	27:15	:37	Some models get too much snow in spring, some get too much ablation in spring, some get good seasonal variation, some don't - it's quite confusing. Again the focus should be on the models used for AR4. [Govt. of United States of America (Reviewer's comment ID #: 2023-519)]	See 8-358
8-360	A	27:16	27:17	"Roesch (2006) and Roesch and Roeckner (2006) (cf. line 31) are listed separately in the references but appear to refer to the same paper" [Govt. of Canada (Reviewer's comment ID #: 2004-162)]	Accept – text corrected
8-361	A	27:17	27:19	Contradictory results are simply quoted together without comment! If text has been corrupted, correct it. If they do contradict, at least acknowledge this with e.g. a "however" - preferably, give a proper explanation [William Ingram (Reviewer's comment ID #: 114-132)]	Accept – text clarified
8-362	A	27:20	27:21	Does "peak monthly" refer to the climatological peak - i.e., are climatological values being compared? [Francis Zwiers (Reviewer's comment ID #: 305-69)]	Text clarified
8-363	A	27:28	27:28	A "v" is missing from variability. [Francis Zwiers (Reviewer's comment ID #: 305-70)]	Accept – text modified
8-364	A	27:33	27:33	The only point of saying this, surely, is that it will exaggerate the albedo feedback - so say so! [William Ingram (Reviewer's comment ID #: 114-133)]	Reject – it is substantially more complex than this
8-365	A	27:37	27:37	"these models" refers to which models? [Francis Zwiers (Reviewer's comment ID #: 305-71)]	Accept – text clarified

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8-366	A	27:45	27:45	"limitationS" [William Ingram (Reviewer's comment ID #: 114-134)]	Accept – text clarified
8-367	A	27:45	27:46	What does "surface forcing distribution" mean? [William Ingram (Reviewer's comment ID #: 114-135)]	Accept – text clarified
8-368	A	27:49	28:2	Another example of a discussion of model results not relevant to AR4. Was Stitch et al. (2003) used for AR4, and if so, how badly did it do? [Govt. of United States of America (Reviewer's comment ID #: 2023-520)]	Reject – our assessment is that these results are relevant to AR4.
8-369	A	28:1	28:1	Omit "it is noteworthy that" [William Ingram (Reviewer's comment ID #: 114-136)]	Accept – text modified
8-370	A	28:2		any indication of how (badly) the Stute et al. model would have done? [David Rind (Reviewer's comment ID #: 214-71)]	No change necessary – we are not aware of any analysis of this issue reported in the literautre
8-371	A	28:4	28:14	This is essentially simple "detection & attribution" stuff: it should therefore cross-refer to Chapter 9 & be consistency-checked by the authors of that chapter. [William Ingram (Reviewer's comment ID #: 114-138)]	Reject – unclear of the reviewer's point
8-372	A	28:4	28:14	I think there is a small overlap here with Ch 9. Milly et al, together with some other papers relating to stream flow and drought, are assessed in the last two paragraphs of 9.5.4.2.1. Perhaps this paragraph could be shortened, with a cross-link to Ch 9 included? [Francis Zwiers (Reviewer's comment ID #: 305-72)]	Accept – paragraph modified
8-373	A	28:9	28:9	Omit "at" [William Ingram (Reviewer's comment ID #: 114-137)]	Accept – text modified
8-374	A	28:10		Can the term "partially predictable" be clarified? I find it a bit obscure. [Martin Manning (Reviewer's comment ID #: 155-50)]	Accept – text modified
8-375	A	28:10		of course, we don't know what the solar radiation variations were… [David Rind (Reviewer's comment ID #: 214-72)]	Noted – no change necessary
8-376	A	28:10		Since both solar radiation and atmospheric composition (including aerosols) are somewhat uncertain, perhaps the better comparison is with climate changes over the 20th century. [Govt. of United States of America (Reviewer's comment ID #: 2023-521)]	See 8-375
8-377	A	28:16	28:26	This subsection seems not fit into the Land-Surface Component, and should be merged into 8.3.1.1.2. [Akio Kitoh (Reviewer's comment ID #: 130-4)]	Reject – this was the most appropriate locaton for this text
8-378	A	28:19	28:20	The reference should be Wild et al. 2006 and not Wild et al. 2005, and the associated reference should be added in the reference list (Wild, M., Long, C.N., and Ohmura, A., 2006: Evaluation of clear-sky solar fluxes in GCMs participating in AMIP and IPCC-AR4	Accept – corrected

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				from a surface perspective, J. Geophys. Res.111, D01104, doi:10.1029/2005JD006118) [Martin Wild (Reviewer's comment ID #: 288-3)]	
8-379	A	28:28	28:49	Section 8.3.4.4 does not present a coherent argument or story about the assessment capability of land surface models to simulate carbon. This section lacks focus and needs to be reviewed. It should also be made clear that no coupled climate models currently simulate carbon. [Govt. of Australia (Reviewer's comment ID #: 2001-350)]	Accept – text rewritten
8-380	A	28:28	28:49	8.3.4.4 should cross link to Ch 7. In fact, perhaps Ch 8 should simply rely on the Ch. 7 assessment. I think the main points to make here might be that the capacity to represent c-cycle processes in AOGCMs is evolving, and that the inclusion of c-cycle processes has implications for the models (e.g., increased costs, constraints on acceptable climate biases, coupling issues, etc). [Francis Zwiers (Reviewer's comment ID #: 305-73)]	Accept – text rewritten – but Chapter 7 does not do the model evaluation side and this should remain in Chapter 8
8-381	A	28:32	28:33	So what's the difference between the exchange of carbon & carbon fluxes? [William Ingram (Reviewer's comment ID #: 114-140)]	Accept – text modified
8-382	A	28:33	28:33	One can't evaluate "against" a completely different quantity, only "using" it [William Ingram (Reviewer's comment ID #: 114-141)]	Reject – unclear what the reviewer means here
8-383	A	28:35	28:35	"were" -> "was" [William Ingram (Reviewer's comment ID #: 114-142)]	Accept – text modified
8-384	A	28:42		The opening sentence of the paragraph could be written more clearly, such as "There has been some evaluation of the carbon models coupled with climate models." [Adrian Simmons (Reviewer's comment ID #: 242-122)]	Accept – text modified
8-385	A	28:46	28:46	The point of this line is that the past gives us little or no useful information about the future, but this is not made explicit - it should be! [William Ingram (Reviewer's comment ID #: 114-143)]	Reject – this is clearly untrue else palaeoclimate work would be redudent
8-386	A	29:3	29:3	"which is" -> "and" [William Ingram (Reviewer's comment ID #: 114-144)]	Accepted. Text modified.
8-387	A	29:7	29:7	"important" - isn't "appropriate" really meant? [William Ingram (Reviewer's comment ID #: 114-145)]	Accepted. Word replaced.
8-388	A	29:15	29:17	This information should be included in the figure caption as well. [Govt. of Finland (Reviewer's comment ID #: 2009-84)]	Accepted, space constraints permitting.
8-389	A	29:23	29:23	"multi-model median result" reads as if the median of the result of applying the process to each model is shown, but the Figure caption makes it plain that the process is applied to the median field for each quantity. The text should be clearer - & anyway, why is the former not done: it sounds as if it is the way to get a good idea of the typical model improvement?	Rejected. The term "multi-model median result" is clearly defined in the text immediately following its introduction and also in the figure caption. There would be a problem in

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				[William Ingram (Reviewer's comment ID #: 114-146)]	choosing the alternative suggested because there are 3 statistics shown on the diagram and taking the median value of each model's statistics would not preserve the mathematical relationship among them.
8-390	A	29:32		Surely one can't expect much global skill for individual seasonal anomalies, so what does Fig 8.3.12 show? [Govt. of Australia (Reviewer's comment ID #: 2001-351)]	Taken into account. Text now makes it clear that the statistics are dominated by the climatology, not the anomalies.
8-391	A	29:36	29:36	"mulit" -> "multi" [William Ingram (Reviewer's comment ID #: 114-147)]	Accepted.
8-392	A	29:39		how model climatology has evolved [Govt. of United States of America (Reviewer's comment ID #: 2023-522)]	Taken into account. Sentence reworded for clarity.
8-393	A	29:45	29:45	Section 8.4 generally seems to lack cross links to Ch 3. It might be possible to save some space by drawing on descriptions of phenomena and observed changes found in Ch 3. Cross references to Section 9.5.3 would also be appropriate. [Francis Zwiers (Reviewer's comment ID #: 305-74)]	Accepted. Cross links to Chapters 3 and 9 will be added where appropriate.
8-394	A	29:45		This should really be AR4 models, but even more, these are not all coupled models (or even all models) being discussed, despite the subchapter title. [Govt. of United States of America (Reviewer's comment ID #: 2023-523)]	Rejected. The AR4 models are the focus. Some non-AR4 models are discussed where appropriate.
8-395	A	29:51	37:44	"There appears to be no obvious rationale for how these modes of variability are ordered (e.g. by region, ""importance"", time scale, etc.) Suggest: 8.4.1 NAM and SAM; 8.4.2 MJO; 8.4.3 ENSO; 8.4.4 Monsoon; 8.4.5 PDO; 8.4.6 PNA; 8.4.7 COWL; 8.4.8 Atlantic Multidecadal Variability; 8.4.9 QBO; 8.4.10 Atmospheric Regimes and Blocking (One could quibble with this ordering as well, but I prefer it.) " [Govt. of Canada (Reviewer's comment ID #: 2004-163)]	Rejected. The ordering is such that extratropical modes are discussed first (beginning with the annular modes) and then the tropical modes (beginning with ENSO).
8-396	A	29:51	38:34	An interesting section with some good material. I think the section would benefit from more references to other parts of the report, particularly the discussion of modes of variability in chapter 3 (3.6 and 3.7). [Nathan Gillett (Reviewer's comment ID #: 84-105)]	See comment 8-393.
8-397	A	29:53	30:2	Refer to 3.6.4 here. [Nathan Gillett (Reviewer's comment ID #: 84-107)]	Accepted.
8-398	A	30:4	30:4	"the model's " - implies not the real world's, which I don't think is meant. Is what is meant that each model's response resembles its own NAM? [William Ingram (Reviewer's comment ID #: 114-148)]	Rejected. What is meant is that each model's response resembles its own NAM – as the original text states.

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8-399	A	30:5	30:5	In "Analyses of individual coupled GCMs (e.g., Fyfe et al., 1999;...", add the following reference: Zhou T. X. Zhang R. Yu, Y. Yu S. Wang, 2000 The North Atlantic Oscillation Simulated by Version 2 and 4 of IAP/LASG GOALS Model. Advances in Atmospheric Sciences 17(4) 601-616 [Govt. of China (Reviewer's comment ID #: 2006-65)]	Reject. The existing references suffice.
8-400	A	30:12	30:12	"and thus" -> "so that" for readability [William Ingram (Reviewer's comment ID #: 114-149)]	Accepted.
8-401	A	30:19	30:19	"also" - is this not (part of) the cause? If the text is intended to imply so, it fails. [William Ingram (Reviewer's comment ID #: 114-150)]	Accepted.
8-402	A	30:21	30:21	"can also not" unEnglish - "also cannot" [William Ingram (Reviewer's comment ID #: 114-151)]	Accepted.
8-403	A	30:22		A self-serving reference admittedly, but my paper (N. P. Gillett, Northern Hemisphere circulation, Nature, 437, 496, 2005.) on changes in the Northern annular mode in the AR4 models might be relevant here. I found similar conclusions to Osborn (2004), but for more models, and taking natural forcings and ozone depletion also into account. [Nathan Gillett (Reviewer's comment ID #: 84-106)]	Accepted. Reference included.
8-404	A	30:29	30:29	The normal meaning of "veracity" is the reliability of people or texts, where honesty or gullibility are the issues, not models! Is "trustworthiness" meant? [William Ingram (Reviewer's comment ID #: 114-152)]	Accepted.
8-405	A	30:31	30:35	Refer to 3.6.5 here. [Nathan Gillett (Reviewer's comment ID #: 84-108)]	Accepted.
8-406	A	30:38	30:38	"including" - that's all they are! Omit that word [William Ingram (Reviewer's comment ID #: 114-153)]	Accepted.
8-407	A	30:39	30:39	"In ...0.95." sentence clearly contradicted by the Figure [William Ingram (Reviewer's comment ID #: 114-154)]	Accepted. Changed wording to "two models".
8-408	A	30:42	30:43	To indicate the robustness of coupled GCMs in capturing the SAM signature in surface temperature (such as the surface warm anomaly over the Antarctic Peninsula associated with the postive SAM phase), it would be good to cite several other models. The CCSM also has a realistic simulation: Otto-Bliesner, B.L., R. Tomas, E.C. Brady, C. Ammann, Z. Kothavala, and G. Clauzet, 2006: Climate sensitivity of moderate and low resolution versions of CCSM3 to preindustrial forcings. J. Climate, 19, 2567-2583. [Bette Otto-Bliesner (Reviewer's comment ID #: 193-3)]	Accepted. Reference included.
8-409	A	30:42		Carril et al. (2005) examined the SAM response in the AR4 models, and the influence on surface climate (GEOPHYSICAL RESEARCH LETTERS, VOL. 32, L16713, doi:10.1029/2005GL023581, 2005). [Nathan Gillett (Reviewer's comment ID #: 84-109)]	Rejected. Existing references suffice.

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8-410	A	30:49	30:57	There are references here to "the Reanalysis SAM". Is there a unique such thing? Is the SAM as depicted by the NCEP/NCAR reanalysis the same as depicted by the ERA-40 or JRA-25 reanalyses? If all the reanalyses agree, they may well be depicting the truth. If they disagree, reference should not be made to "the Reanalysis SAM". [Adrian Simmons (Reviewer's comment ID #: 242-123)]	Accepted. Replaced "Reanalysis SAM" with "NCEP Reanalysis SAM" in this paragraph. Direct comparisons, such as these, between the AR4 model SAMs and other Reanalysis SAMs appear not to be available.
8-411	A	30:55	30:57	I agree that there may be problems with the variability in the NCEP reanalysis. But since the variability between the models varies by a factor of 3, it is clear that not all the models can be right. [Nathan Gillett (Reviewer's comment ID #: 84-110)]	Noted.
8-412	A	30:55		"Suggest changing ""is problematic when compared to"" to ""does not compare well to"" " [Govt. of Canada (Reviewer's comment ID #: 2004-164)]	Accepted.
8-413	A	30:55		how about the ERA-40 SAM variance? [David Rind (Reviewer's comment ID #: 214-73)]	See comment 8-410.
8-414	A	30:55		NCEP reanalysis used in this comparison [could have used ERA40] [Govt. of United States of America (Reviewer's comment ID #: 2023-524)]	See comment 8-410.
8-415	A	30:57		"Suggest changing ""problems in sampling in the observed analysis"" to ""sampling differences from the observed analysis""" [Govt. of Canada (Reviewer's comment ID #: 2004-165)]	Accepted.
8-416	A	31:9	31:11	After "For example", add the following statement: "when forced by historical sea surface temperature, the interannual variation of the SAM can be partly reproduced by AGCMs (Zhou and Yu,2004)". For reference, see: Zhou T., and R. Yu, 2004, Sea-surface temperature induced variability of the Southern Annular Mode in an atmospheric general circulation model Geophysical Research Letters, 31,L24206,doi:10.1029/2004GL021473 [Govt. of China (Reviewer's comment ID #: 2006-66)]	Rejected. AR4 models are the focus.
8-417	A	31:11	31:12	"Suggest changing ""; these could easily implicate air-sea interactions in SAM dynamics"" to "", suggesting a potential for air-sea intereactions to influence SAM dynamics""" [Govt. of Canada (Reviewer's comment ID #: 2004-166)]	Accepted.
8-418	A	31:12	31:15	Watterson (2000, J Clim) and (2001, JGR) have explored this air-sea interaction (for the HLM/SAM) and warrant assessment. [Govt. of Australia (Reviewer's comment ID #: 2001-352)]	Accepted. The 2nd reference has been included.

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8-419	A	31:19	31:25	Refer to 3.6.3 and 9.5.2.1 here. [Nathan Gillett (Reviewer's comment ID #: 84-111)]	Accepted.
8-420	A	31:19	31:31	The PDO/IPO are not physical modes, but the echo & projection of ENSO onto decadal means, with resulting greater concentration at higher latitudes because the governing timescales are longer there. The text from line 36 expaining this should start 8.4.2, & the text currently at lines 19-31 should be condensed & made to reflect this more clearly. The sentence across lines 31 to 33 should be removed. The whole subsection should be condensed anyway: since we know the models have trouble with ENSO itself they can't be expected to be good at a smoothed version of it. [William Ingram (Reviewer's comment ID #: 114-156)]	Accepted. This section has shortened and a reference to Section 3.6.3 made where some of these issues are discussed. See also comment 8-67.
8-421	A	31:23	31:23	"hierarchy" This is definitely not a hierarchy! Just omit the word, I suggest [William Ingram (Reviewer's comment ID #: 114-155)]	Accepted. This text has been removed..
8-422	A	31:23		"Suggest changing ""heirarchy"" to ""ordering""" [Govt. of Canada (Reviewer's comment ID #: 2004-167)]	See comment 8-421.
8-423	A	31:27	31:27	PDO-like mode they examined -> PDO-like mode which (or that) they examined [Govt. of Finland (Reviewer's comment ID #: 2009-85)]	Accepted. This text have been removed.
8-424	A	31:39	31:41	The behaviour of the climate system in the extratropical areas is rather chaotic; how can this produce a predictable component? [Govt. of Finland (Reviewer's comment ID #: 2009-86)]	Noted.
8-425	A	31:51		Does the 'poor resolution of the coastal wave-guide' refer to the models, or the observations? This should be specified. [Franklin SCHWING (Reviewer's comment ID #: 230-16)]	Accepted. Added "in coupled models".
8-426	A	31:51		Does the 'poor resolution of the coastal wave-guide' refer to the models, or the observations? This should be specified. Use "...coastal wave guide in models." [Govt. of United States of America (Reviewer's comment ID #: 2023-525)]	See comment 8-425.
8-427	A	32:5	32:5	"wave-like" - totally misleading to the outsider & not much help to a specialist who doesn't know what sort of "wave" is meant. "Rossby-wave-like" will be informative to the specialist & at least indicate to the non-specialist that the meaning is not what he would expect [William Ingram (Reviewer's comment ID #: 114-157)]	Accepted. This text has been removed and a reference to Chapter 3 made instead.
8-428	A	32:9		The context of the sentence "Hence both external and internal processes may contribute to the formation of this pattern" makes it appear that this is something that has been learnt from GCM experiments. A reference might thus be given here to Simmons, Wallace and Branstator (1983, J. Atmos. Sci., 1363-1392, who presented the same conclusion, based on analysis of barotropic wave propagation and instability.	Rejected. The chapter focus is in GCMs.

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				[Adrian Simmons (Reviewer's comment ID #: 242-124)]	
8-429	A	32:21	32:21	Replace "produced ... models" with "the participating models' " after "of", to avoid the current "models of the atmospheric anomalies" [William Ingram (Reviewer's comment ID #: 114-158)]	Accepted.
8-430	A	32:35	32:37	This discussion of the DEMETER project seems out of place in an IPCC assessment. [Nathan Gillett (Reviewer's comment ID #: 84-112)]	Rejected. That these coupled models have skill at seasonal prediction gives some degree of confidence in the ability of coupled models in general.
8-431	A	32:50	32:51	"Suggest changing ""one of such coupled experiments indicates that the ENSO events appearing in the integration"" to ""one such coupled integration indicates that the modeled ENSO events""" [Govt. of Canada (Reviewer's comment ID #: 2004-168)]	Accepted.
8-432	A	32:51	32:56	Unclear whether the 4th-6th sentences are about "various institutions" or "one of such" - the language suggests the former but I guess the latter is meant: clarify [William Ingram (Reviewer's comment ID #: 114-159)]	Accepted. Text has been reworked.
8-433	A	33:3	33:4	This is a rather impenetrable sentence – can it be clarified please. [Martin Manning (Reviewer's comment ID #: 155-51)]	Accepted. This sentence has been removed.
8-434	A	33:16	33:16	"ambiguous" - in what way? I suspect something else is actually meant, e.g. "potentially misleading". Anyway, the problems described in this paragraph are typical pitfalls of looking for physical modes using statistical means, as should be briefly acknowledged. [William Ingram (Reviewer's comment ID #: 114-160)]	Accepted.
8-435	A	33:27	34:12	Add reference to the following work:Ruti, P.M., V. Lucarini, A. Dell'Aquila, S. Calmanti, and A. Speranza, 2006: Does the subtropical jet catalyze the mid-latitude atmospheric regimes?. Geophysical Research Letters 33(6): L06814 (also in the PCMDI preprint server). It is shown that winter planetary waves of the Northern Hemisphere obey a non-gaussian statistics and may present a multimodal probability density function, thus characterizing the low-frequency portion of the climate system. It is shown that the upper tropospheric jet strength is a critical parameter in determining whether the planetary waves indicator exhibits a uni- or bimodal behavior. The results are obtained by considering the data of the NCEP-NCAR and ECMWF reanalyses for the overlapping period. The results agree with the non-linear orographic theory, which explains the statistical non-normality of the low-frequency variability of the atmosphere and its possible bimodality, and sets a bridge for ENSO effects on mid-latitude climate. [Teresa Nanni (Reviewer's comment ID #: 186-5)]	Rejected. The existing references suffice.
8-436	A	33:27		Section 8.4.5 Mention should be made here of the analysis of European weather regimes in HadGAM/GEM1 versus HadAM3/CM3 included in Ringer et al. (2006; J Climate,	Accepted. A senetence on the Ringer et al. paper has been included.

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				April 1st issue) [Gill Martin (Reviewer's comment ID #: 167-9)]	
8-437	A	33:29	33:46	The last sentence of this paragraph reports that the 'statistical significance of the regimes has been discussed and remains an unresolved issue'. I dispute this - I think that for example the multi-modality of the PDF of the type reported by Corti et al. has been clearly demonstrated not to be statistically significant in the cited references. Because of this I would suggest giving less prevalence to the discussion of regimes. [Nathan Gillett (Reviewer's comment ID #: 84-113)]	Rejected. The significance of the Corti et. al. regimes was disputed in Hsu and Zwiers. The existence of regimes more generally remains unresolved.
8-438	A	33:37	33:37	"sectorial" -> "sectoral" [William Ingram (Reviewer's comment ID #: 114-161)]	Accepted.
8-439	A	33:44	33:44	"sectorial" -> "sectoral" [William Ingram (Reviewer's comment ID #: 114-162)]	Accepted.
8-440	A	33:46	33:46	All the cited references support one side of this controversial issue. However, because the issue is controversial and still unresolved, at least one reference which supports the other side should be quoted. For example:.: Molteni et al. 2006: [Molteni, Kuchraski and Corti, On the predictability of flow-regime properties on interannual to interdecadal timescales. In Predictability of Weather and Climate, Cambridge Press, Palmer and Hagedorn Eds. Cambridge 2006 DOI: 10.2277/0521848822] [Susanna Corti (Reviewer's comment ID #: 47-2)]	Accepted.
8-441	A	33:48	33:48	"sectorial" -> "sectoral" [William Ingram (Reviewer's comment ID #: 114-163)]	Accepted.
8-442	A	33:52	33:53	"less frequent" -> "rarer" [William Ingram (Reviewer's comment ID #: 114-164)]	Accepted.
8-443	A	34:16	34:36	This section needs to be reviewed to provide information concerning the relevance of multi-decadal variability on climate model evaluation. [Govt. of Australia (Reviewer's comment ID #: 2001-353)]	Rejected. As stated, AMV is an important aspect of the climate system, as well as being linked to other important aspects such hurricane frequency and Sahel rainfall.
8-444	A	34:17	34:17	"stable" - that's one thing variability isn't! "consistent"? "robust"? [William Ingram (Reviewer's comment ID #: 114-165)]	Accepted. Changed to robust.
8-445	A	34:17		"Suggest changing ""stable feature"" to ""persistent feature""" [Govt. of Canada (Reviewer's comment ID #: 2004-169)]	See comment 8-444.
8-446	A	34:27	34:27	"quite" can mean "totally" or "fairly": usually it's clear but not here! Replace with unambiguous word [William Ingram (Reviewer's comment ID #: 114-166)]	Accepted. Wording changed.

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8-447	A	34:34	34:36	This is not a model validation issue, and appears to be beyond the scope of this chapter. [Nathan Gillett (Reviewer's comment ID #: 84-114)]	Accepted. This sentence has been removed.
8-448	A	34:35	34:36	The statement that anthropogenic weakening of the THC may be masked by Atlantic multidecadal variability, is an important point and needs a formal uncertainty value attached to it. [Govt. of Australia (Reviewer's comment ID #: 2001-354)]	See comment 8-447.
8-449	A	34:35	34:35	It could mask it, true - but also combine to exacerbate it [William Ingram (Reviewer's comment ID #: 114-167)]	See comment 8-447.
8-450	A	34:51	34:54	Not all models have a double ITCZ. [Francis Zwiers (Reviewer's comment ID #: 305-75)]	Accepted. Wording changed to "in most models".
8-451	A	34:55	34:55	Omit 2nd "too" [William Ingram (Reviewer's comment ID #: 114-168)]	Accepted.
8-452	A	35:8	35:8	Add "and van Oldenborgh et al (2005)". [Govt. of Netherlands (Reviewer's comment ID #: 2016-46)]	Accepted. Reference included.
8-453	A	35:14	35:14	"characteristicS" [William Ingram (Reviewer's comment ID #: 114-169)]	Accepted.
8-454	A	35:19	35:23	I think "breakthrough" is a bit excessive. These are important advances, but neither represents a fundamental improvement in understanding of the origins of predictability. [Francis Zwiers (Reviewer's comment ID #: 305-76)]	Accepted. Replaced with "advances".
8-455	A	35:19	:35	This is not at all relevant to AR4 models or the rest of this section, and should be deleted. [Govt. of United States of America (Reviewer's comment ID #: 2023-526)]	Rejected. Successful ENSO prediction using AR4 related models increases our confidence in the AR4 models used for climate predictions of the future.
8-456	A	35:22	35:23	"Suggest changing ""(Palmer et al. 2004). Palmer et al. (2004, Figure 2), for example, demonstrates"" to ""e.g. Palmer et al. (2004), in which Figure 2 demonstrates""" [Govt. of Canada (Reviewer's comment ID #: 2004-170)]	Accepted.
8-457	A	35:22	35:22	"resolving" has a precise technical meaning, but obviously not here - I don't really know what is meant: clarify. [William Ingram (Reviewer's comment ID #: 114-170)]	Accepted. Changed to "for adequately dealing with"
8-458	A	35:26	35:28	The word "recent" seems at odds with the citation of Chen et al (1995). [Francis Zwiers (Reviewer's comment ID #: 305-77)]	Accepted. Replaced "recent research indicates" with "other".
8-459	A	35:26		"Suggest deleting ""recent research indicates that"" (shorter, plus Chen et al. 1995 doesn't seem ""recent"")" [Govt. of Canada (Reviewer's comment ID #: 2004-171)]	See comment 8-458.

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8-460	A	35:37	36:41	This section is missing some important points concerning the simulation of the MJO (see Slingo, J. M., P. M. Inness and K. R. Sperber, 2005: Modelling the MJO. Chapter in 'Intraseasonal variability of the atmosphere-ocean climate system'. Editors W. K-M. Lau and D. E. Waliser, Springer/Praxis Book Company, pp. 361-383 for areview of the MJO). Slingo et al. also discuss the boreal summer MJO which has a major role in monsoon active-break cycles. This section needs to discuss these since they may be a major factor in dtermining monsoon volatility under climate change. [Julia Slingo (Reviewer's comment ID #: 243-22)]	Noted. Space constraints prevent us from discussing this particular aspect however we do refer to Slingo et al. (2005) where apparently this is discussed. Take note that we have added a sentence on the role of cloud-radiative and convection-moisture interactions on MJO simulation.
8-461	A	35:48	35:48	It should also be noted that the MJO can significantly alter the evolution of El Nino and hence may limit its predictability (see Slingo, J. M., D. P. Rowell, K. R. Sperber and F. Nortley, 1999: On the predictability of the interannual behaviour of the Madden-Julian Oscillation and its relationship with El Nino. Q. J. R. Meteorol. Soc., 125, 583-609 and Lengaigne, M. E., E. Guilyardi, J-P. Boulanger, C. Menkes, P. M. Inness, P. Delecluse, J. Cole and J. M. Slingo, 2004: Triggering of El Nino by westerly wind events in a coupled general circulation model. Clim. Dyn., doi:10.1007/s00382-004-0457-2). [Julia Slingo (Reviewer's comment ID #: 243-21)]	See comment 8-460.
8-462	A	36:16	36:16	Is Waliser 1999 the best new reference for a potential effect of the ocean on the MJO? [Govt. of Australia (Reviewer's comment ID #: 2001-357)]	Rejected. This reference suffices.
8-463	A	36:21	36:28	Is there any indication of whether there is sensitivity to the particular coupling strategy used, or coupling frequency? [Francis Zwiers (Reviewer's comment ID #: 305-78)]	Noted. Even if such a sensitivities were documented in the literature, severe space constraints would prevent us from discussing them.
8-464	A	36:23		Watterson (2002, JGR) demonstrated a dramatic improvement in eastward propagation due to air-sea interaction. [Govt. of Australia (Reviewer's comment ID #: 2001-356)]	Rejected. The existing references suffice.
8-465	A	36:26	36:26	"seasonal" & "annual" used in same line with same meaning: stick to one! [William Ingram (Reviewer's comment ID #: 114-171)]	Accepted.
8-466	A	36:30	36:33	It would be useful here to include links back to other parts of 8.4 where the double ITCZ, etc., are discussed. [Francis Zwiers (Reviewer's comment ID #: 305-79)]	Accepted.
8-467	A	36:46	36:48	The QBO is not restricted to the lower stratosphere as explained on line 46. The well known effect of the QBO on tracer distributions in the global middle atmosphere, specifically on ozone, should be mentionned. Please use the following text: ... dominates the inter-annual variability of the zonal wind in the equatorial stratosphere. The QBO affects tracer distributions throughout the middle stratosphere, as seen for example in the global total ozone, and affects strength and stability of the wintertime polar vortex. QBO and QBO effects are reviewed in Baldwin et al. (2001). ...	Rejected. We are seriously space constrained.

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				[Marco A. Giorgetta (Reviewer's comment ID #: 85-6)]	
8-468	A	36:48	36:51	The sentence "Recent ... 2002)" is misleading because it presents resolved wave forcing and non-orographic gravity wave drag as alternative explanations of the QBO. The second set of referenced experiments includes for example wave forcing from resolved and parameterized waves, though at different levels. This sentence can be replaced by: ... Theory and observations indicate that a broad spectrum of vertically propagating waves in the equatorial atmosphere must be considered to explain the QBO. Realistic simulation of the QBO in GCMs therefore depend on 3 important conditions: (1) sufficient vertical resolution in the stratosphere to allow the representation of equatorial waves at the horizontally resolved scales of a GCM, (2) a realistic excitation of resolved equatorial waves by simulated tropical weather, and (3) parameterization of the effects of unresolved gravity waves. [Marco A. Giorgetta (Reviewer's comment ID #: 85-7)]	Accepted. New wording inserted.
8-469	A	36:53	36:54	"a notorious issue" -> "notorious" [William Ingram (Reviewer's comment ID #: 114-172)]	See comment 8-470.
8-470	A	36:53	36:53	"Notorius" doesn't quite seem an appropriate adjective for an IPCC report. Perhaps replace "notorius issue for some time" with "long standing issue". [Francis Zwiers (Reviewer's comment ID #: 305-80)]	Accepted.
8-471	A	37:1	37:1	I don't understand the point regarding moist-convective adjustment that is being made here. [Francis Zwiers (Reviewer's comment ID #: 305-81)]	Accepted. The text in parentheses has been removed.
8-472	A	37:6	37:7	Giorgetta et al., 2002, 2006; Reference for Giorgetta et al. 2006: Giorgetta M. A., E. Manzini, E. Roeckner, M. Esch, and L. Bengtsson, 2006: Climatology and forcing of the quasi-biennial oscillation in the MAECHAM5 model, J. Climate, in press. Downloaded from: http://www.ametsoc.org/journal_abstracts/get_pta.cfm?sJcode=JCLI [Marco A. Giorgetta (Reviewer's comment ID #: 85-8)]	Accepted. Reference added.
8-473	A	37:7	37:7	McLandress et al (2002) actually shows the inutility of gravity wave parameterizations to simulate a realistic QBO without substantial resolved wave forcing. The stratospheric equatorial oscillation in McLandress (2002) shows two deficiencies that are typical for exagerated gravity wave forcing, as applied in McLandress (2002): (1) the period is much shorter than 2 years, and (2) westerlies are too strong and easterlies are too weak. This paper actually does not claim to simulate a realistic QBO or to obtain a QBO for the right reasons. I am wondering why this paper is referenced twice in this section. [Marco A. Giorgetta (Reviewer's comment ID #: 85-9)]	Accepted. The second reference has been removed.
8-474	A	37:10	37:12	The problem to understand how to parameterize gravity wave sources as a function of simulated weather should be mentioned. This could be included as follows: ... At this time we require better observational estimates of tropical convective variability and	Rejected. Serious space limitations prevent us from adding this.

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				emerging wave fields to constrain convective parameterizations and to develop parameterizations of convective sources of gravity waves used in non-orographic gravity wave parameterizations. [Marco A. Giorgetta (Reviewer's comment ID #: 85-10)]	
8-475	A	37:17	37:44	This monsoon section must be rewritten. There are huge gaps here, for example, discussion of monsoon intraseasonal variability, decadal variations in monsoon-ENSO telecoonections and the challenge that that represents for models. [Julia Slingo (Reviewer's comment ID #: 243-23)]	Accepted. The monsoon subsection has been rewritten and linked to Chapters 3, 9 and 11 which provide additional information.
8-476	A	37:17		Section 8.4.10 Despite its title, "Monsoon variability", this section discusses simulation of the monsoon climatology as well as interannual variability, and intraseasonal variability is scarcely mentioned. It is not clear from this section how well the models simulate the monsoon in general and how this has changed since the TAR. Perhaps a short section on the monsoon climatology should be included in section 8.3.1. [Gill Martin (Reviewer's comment ID #: 167-10)]	See comment 8-475.
8-477	A	37:19	37:24	The text states that GCMs failed to simulate the strong Indian monsoon of 1988 which was coincident with strong warming in the western equatorial Indian Ocean, and therefore argues that the GCMs cannot capture 'the linkage between the equatorial Indian Ocean and the Indian summer monsoon'. How do the authors know that the strong monsoon was cause by the warming in the equatorial Indian Ocean? Isn't it also possible that the strong monsoon was a result of internal atmospheric variability? [Nathan Gillett (Reviewer's comment ID #: 84-115)]	Accepted. This text is no longer present.
8-478	A	37:24	37:26	Why is this relevant? [Nathan Gillett (Reviewer's comment ID #: 84-116)]	Accepted. This text is no longer present.
8-479	A	37:26	37:26	"monsoon" - ambiguous: does it mean monsoons generally or the Indian summer monsoon or what? [William Ingram (Reviewer's comment ID #: 114-173)]	Accepted. This text is no longer present.
8-480	A	37:26	37:29	Delete these two sentences as this is based on single model (COLA). [Akio Kitoh (Reviewer's comment ID #: 130-5)]	Accepted. This text is no longer present.
8-481	A	37:31	37:34	This comparison of simulated and observed precip changes in the Sahel seems beyond the scope of this chapter. This material is discussed in more detail in 9.5.3.3.1 - this should be referenced here. [Nathan Gillett (Reviewer's comment ID #: 84-117)]	Accepted. This text is no longer present.
8-482	A	37:31	37:44	Much too detailed (particularly lines 37-42) [William Ingram (Reviewer's comment ID #: 114-174)]	Accepted. Lines removed.
8-483	A	37:31	37:44	there are large discrepancies in the observational data sets for these regions, so any analysis of this time should be heavily caveated.	See comment 8-470.

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				[David Rind (Reviewer's comment ID #: 214-74)]	
8-484	A	37:31	37:32	See also 9.5.4.3.1. Note that Hoerling et al (2005a - see Ch 9 references) put the GFDL result in the context of a larger group of models. [Francis Zwiers (Reviewer's comment ID #: 305-82)]	Accepted. Reference removed and link to Chapter 9 made.
8-485	A	37:31	:44	Should be noted that these comparisons are being made with observations of precipitation, which have large differences between different observational data sets. [Govt. of United States of America (Reviewer's comment ID #: 2023-527)]	See comment 8-470.
8-486	A	37:35	37:36	Delete this sentence as this is based on single model (CNRM). [Akio Kitoh (Reviewer's comment ID #: 130-6)]	Accepted. Lines removed.
8-487	A	37:36	37:43	Delete these four sentences as this is based on single model (ECHAM4/OPYC3). [Akio Kitoh (Reviewer's comment ID #: 130-7)]	Accepted. Lines removed.
8-488	A	37:42		SPCZ is not defined. [Nathan Gillett (Reviewer's comment ID #: 84-118)]	Accepted. SPCZ line removed.
8-489	A	37:46	37:46	Title should be changed to "Predictions using AR4 Models" for consistency with text. [Govt. of Australia (Reviewer's comment ID #: 2001-355)]	Taken into account. Title modified.
8-490	A	37:46	37:46	Totally unclear that "predictions" is supposed to mean NWP. Start heading with "Short-term" or "Weather". [William Ingram (Reviewer's comment ID #: 114-175)]	Taken into account. Title modified.
8-491	A	37:46	38:34	This section should be removed. It is not relevant to AR4 models (perhaps by the time of AR5 it will be relevant). [Govt. of United States of America (Reviewer's comment ID #: 2023-528)]	Reject. While only a few studies are available with the "AR4" models, they represent progress in a new area since the TAR. Text will however be reviewed for relevance and shortened where appropriate.
8-492	A	37:46		I suggeest inserting 'Deterministic' before 'Predictions using IPCC models'. This will clearly differentiate the section from predictions of climate change over the 21st century which are dealt with in chapter 10. [Nathan Gillett (Reviewer's comment ID #: 84-119)]	Taken into account. Title modified.
8-493	A	37:48	37:48	Add "(weather)" after "value" [William Ingram (Reviewer's comment ID #: 114-176)]	Rejected. Seasonal also included. Believe text is clear.
8-494	A	37:52	37:53	I disagree with the statement that climate model evaluation has traditionally been limited to month-mean outout. Certainly in our center it has been standard practic, for more than 20 years, to archive and analyse high frequency (typically 12 hourly) output. What is relatively recent is the exchange of large quantities of high frequency data for use in intercomparison projects (this has not previously been possible simply because of the logistics of data transfer).	Accepted. Text will be modified.

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				[Francis Zwiers (Reviewer's comment ID #: 305-83)]	
8-495	A	37:53	37:53	"Since the TAR" - this was known in AMIP I ! [William Ingram (Reviewer's comment ID #: 114-177)]	Rejected. Not clear what (if any) change is suggested.
8-496	A	37:55	37:55	Is this really the reason for the advance? Many climate models have their roots in NWP, and some share common architecture and infrastructure. I think this has come about because some groups that happen to have common architecture and infrastructure have demonstrated to others that this approach has benefits. [Francis Zwiers (Reviewer's comment ID #: 305-84)]	Rejected. While the reasons given may also have been an important contributor, we focus here on the physical/modelling factors rather than human factors.
8-497	A	38:5	38:5	Include reference to Martin et al., (2006; J Climate, April 1st issue) here as this paper showed how a systematic error in precipitation develops during an ensemble of 5-day "spin-up" runs [Gill Martin (Reviewer's comment ID #: 167-11)]	Accepted.
8-498	A	38:9	38:9	I think the previous paragraph has made the case for testing climate models in NWP mode, but it hasn't really pointed up results. Were improvements found, or found more quickly than would have been the case if the models were only evaluated in climate mode? [Francis Zwiers (Reviewer's comment ID #: 305-85)]	Accepted. Text will be revised to make this clearer.
8-499	A	38:11	38:34	I don't see the relevance of this section. Why is it there? The GloSea model is not used in IPCC and is different from HadCM3 and HadGEM1. I suggest removing this text. [Julia Slingo (Reviewer's comment ID #: 243-24)]	Taken into account. Only results that are expected to be transferrable to HadCM3 will be cited.
8-500	A	38:19		which 6 months? It is well known that given the initial conditions for April, ENSO state forecasts for December can be well done; but given the initial conditions for December, April conditions are very difficult to forecast. Is that true in these studies as well? If so, then this aspect is misleading. If the 'six month forecast' statement is meant in general, then these models are doing better than models specifically designed to forecast ENSO conditions, often with much finer resolution. This also raises the more general question: how far removed is GloSea from the models used for the IPCC assessment? If it is much different, e.g., much finer resolution, than this chapter has to be careful not to mislead readers into thinking that the results are relevant for this IPCC report. [David Rind (Reviewer's comment ID #: 214-75)]	Taken into account. Only results that are expected to be transferrable to HadCM3 will be cited. The question of the seasonal prediction barrier is too detailed to discuss in the space available.
8-501	A	38:19		Which 6 months? It is well known that given the initial conditions for April, ENSO state forecasts for December can be well done; but given the initial conditions for December, April conditions are very difficult to forecast. Is that true in these studies as well? If so, then this aspect is misleading. If the 'six month forecast' statement is meant in general, then these models are doing better than models specifically designed to forecast ENSO	Taken into account. Only results that are expected to be transferrable to HadCM3 will be cited. The question of the seasonal prediction barrier is too detailed to discuss in the

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				conditions, often with much finer resolution. This also raises the more general question: how far removed is GloSea from the models used for the IPCC assessment? If it is much different, e.g., much finer resolution, than this chapter has to be careful not to mislead readers into thinking that the results are relevant for this IPCC report. [Govt. of United States of America (Reviewer's comment ID #: 2023-529)]	space available.
8-502	A	38:35	38:35	I think this section could be a bit more comprehensive. Certainly there are other models being used for seasonal to interannual prediction (e.g., at ECMWF and the IRI), and the atmospheric components of quite a few models are being used in 2-tier seasonal forecasting systems (e.g., by the APEC Climate Centre - http://www.apcc21.net/index.php). A version of the UKMO model is also being used for decadal prediction. [Francis Zwiers (Reviewer's comment ID #: 305-86)]	Rejected. To maintain focus we concentrate on models that are very close to those used in AR4. The decadal prediction work at UKMO did not reach peer-reviewed publication in time for inclusion in the review process, and so has been omitted.
8-503	A	38:46	38:48	A confusing sentence. Extreme temperatures, for example, are NOT related to any kind of instability of the system. [Govt. of Finland (Reviewer's comment ID #: 2009-87)]	Agree, text modified
8-504	A	38:46	38:46	This describes a particular kind of extreme event. Other kinds of high impact events that occur on different space and time scales (e.g., drought) could also be considered to be extreme. The glossary definition uses the phrase "rare within its pdf (probability density function)" to describe extremes - which I think is appropriate and can be applied to all scales. [Francis Zwiers (Reviewer's comment ID #: 305-87)]	Agree, text modified
8-505	A	38:49	38:51	"This sentence is overly general and ignores the lesser ability of the models to simulate precipitation extremes as compared to temperature extremes." [Govt. of Canada (Reviewer's comment ID #: 2004-172)]	Agree, text modified
8-506	A	38:50	38:50	I would add "surprisingly" or "perhaps surprisingly" [William Ingram (Reviewer's comment ID #: 114-178)]	Agree, text modified
8-507	A	38:53	38:57	"Again, these summary statements focus on simulation of temperature extremes without mentioning less skillfull simulation of precipitation extremes " [Govt. of Canada (Reviewer's comment ID #: 2004-174)]	Agree, text modified (also see 8.5.2)
8-508	A	38:53	38:57	I think the wording here should be changed to make it clear what the authors of the chapter think. As written you seem to be finessing around providing your own assessment of the models. [Martin Manning (Reviewer's comment ID #: 155-52)]	Agree, text modified
8-509	A	38:53		"Suggest changing ""summarized"" to ""exemplified"" since quotes pertain only to simulation of temperature extremes"	noted

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				e [Govt. of Canada (Reviewer's comment ID #: 2004-173)]	
8-510	A	38:54	38:55	"It is difficult to understand why Kharin et al. (2005) is quoted here, whereas sections 8.5.1 and 8.5.2 omit any mention of results from this paper. (While it is true that Kharin et al. considered atmosphere-only GCMs, so did Kiktev et al., whose results are accorded an entire paragraph under 8.5.1.) " [Govt. of Canada (Reviewer's comment ID #: 2004-175)]	Agree, text modified
8-511	A	39:1	39:1	The place of a comma is wrong? "temperature has, been" should be "temperature, has been"? [Seita Emori (Reviewer's comment ID #: 62-10)]	agree
8-512	A	39:2	39:7	"The second half of this paragraph says much the same thing as the first. Suggest omitting the first sentence." [Govt. of Canada (Reviewer's comment ID #: 2004-176)]	Agree, text modified
8-513	A	39:4		"Suggest changing ""In this section, we assess the extreme events by examining"" to ""The remainder of this section assesses the model simulation of"" " [Govt. of Canada (Reviewer's comment ID #: 2004-177)]	noted, text modified
8-514	A	39:9	39:43	The selection of certain locations to focus on temperature extremes (eg. South Australia, Russia and south-eastern USA) needs to be explained. That is, why were these locations chosen as exemplars? [Govt. of Australia (Reviewer's comment ID #: 2001-358)]	noted, we are assessing published paper
8-515	A	39:9	40:32	"Subsections 8.5.1 and 8.5.2 read as a sequence of disconnected summaries of individual papers, rather than as a synthesis in the manner of most of the rest of this chapter." [Govt. of Canada (Reviewer's comment ID #: 2004-178)]	noted
8-516	A	39:16	39:16	"simulated by HadAM3" - with anthropogenic forcing? [William Ingram (Reviewer's comment ID #: 114-179)]	agree
8-517	A	39:28		but doesn't the run use 'ocean forcing', aka prescribed SSTs? Then whether the effect is caused by ENSO or not, the model is not producing the proper extreme response. [David Rind (Reviewer's comment ID #: 214-76)]	Agree, text modified
8-518	A	39:28		But doesn't the run use 'ocean forcing', aka prescribed SSTs? Then whether the effect is caused by ENSO or not, the model is not producing the proper extreme response. [Govt. of United States of America (Reviewer's comment ID #: 2023-530)]	Agree, text modified
8-519	A	39:40	39:40	favored -> favoured	noted

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				[Govt. of Finland (Reviewer's comment ID #: 2009-88)]	
8-520	A	39:44	39:44	I don't like to make (and also don't like to receive) comments suggesting that there has been oversight in not citing work that I am personally associated with - but I think there has been such an oversight in the case of this sub-section and the next. Kharin et al (2005) document the performance of AMIP-2 models (which are closely related to the atmospheric components used in the IPCC AR4 models) in simulating temperature and precipitation extremes. This paper is quoted in the introduction, but not assessed here. While not citable in the AR4, a follow-on paper (still in review) assesses the AR4 models and draws similar conclusions. [Francis Zwiers (Reviewer's comment ID #: 305-88)]	Agree, text modified
8-521	A	39:48	39:49	This sentence is saying no more than what ought to occur (i.e. would with a perfect model) if point observations are compared with grid-box means from a model. If that is all that's going on it's too trivial to mention - if that has been properly allowed for as I trust is the case, this should be mentioned explicitly to avoid the possibility of confusion. [William Ingram (Reviewer's comment ID #: 114-180)]	noted
8-522	A	40:4	40:5	Please give the specific model name of the AOGCM as in many other citations in this chapter. "an AOGCM with two different resolutions (hires and medres of MIROC3.2) and found …" [Seita Emori (Reviewer's comment ID #: 62-7)]	Agree, included
8-523	A	40:4	40:21	This paragraph is too long & detailed: compress drastically [William Ingram (Reviewer's comment ID #: 114-181)]	noted
8-524	A	40:4	40:4	Kimoto te al. (2005) missing in References section (page 8-73) [Masahide Kimoto (Reviewer's comment ID #: 127-2)]	Agree, included
8-525	A	40:5	40:6	Similar to #7, "a high-resolution AGCM (the atmospheric part of MIROC3.2(hires)) can …" [Seita Emori (Reviewer's comment ID #: 62-8)]	Agree, included
8-526	A	40:23	40:24	Refer the percentages to the entire globe or a certain area? [Govt. of Finland (Reviewer's comment ID #: 2009-89)]	Agree; it is global; text is modified
8-527	A	40:23	40:32	It looks like these two paragraphs should be combined, the Burke reference is quoted twice saying more or less the same thing. [Gareth S. Jones (Reviewer's comment ID #: 121-69)]	Agree, text modified
8-528	A	40:36	40:45	Oouchi et al (J Met Soc Japan 2006) used a 20km GCM to investigate future changes in tropical cyclones. This should be referenced in Chapter 8 to remain consistent with Chapter 10. [Ruth McDonald (Reviewer's comment ID #: 173-13)]	Agree, included

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8-529	A	40:36	41:2	The model simulation of interannual variability of tropical cyclone frequency has been assed by Camargo et al. (Tellus, 57A, 2005), Sugi et al. (J Met Soc Japan, 2002) and McDonald et al. (Climate Dynamics 2005). [Ruth McDonald (Reviewer's comment ID #: 173-16)]	Agree, included
8-530	A	40:47	40:51	Other studies include Camargo et al. (Tellus 57A 589-604 2005) and Yoshimura et al (2006). Camargo et al. assessed tropical cyclones in 3 low resolution AGCMs. Yoshimura et al assessed trpoical cyclones in T106 AGCM. [Ruth McDonald (Reviewer's comment ID #: 173-14)]	Agree, included
8-531	A	40:48	40:50	The intensity of the tropical cyclones in Bengtsson et al (2006) are only assessed against cyclones in ERA40 data and not observed cyclones. The cyclones in ERA40 may be less intense than observed tropical cyclones. [Ruth McDonald (Reviewer's comment ID #: 173-15)]	Agree, no change necessary
8-532	A	40:51	40:51	However, varying degrees of errors (in some cases substantial) in simulated tropical storm frequency have been noted in different models (e.g., GFDL Global Atmospheric Model Development Team (GAMDT) 2004; Camargo et al. 2005). [Ref: Camargo, S., A. G. Barnston, and S. E. Zebiak, 2005: A statistical assessment of troical cyclone activity in atmospheric general circulation models. Tellus, 57A, 589-604. [Thomas Knutson (Reviewer's comment ID #: 132-4)]	Agree, included
8-533	A	40:53	40:55	Comment: The performance of models in simulating track differences for El Nino years is not even analyzed in most studies I am aware of. You should supply some references to back up this statement. Some studies have shown some skill with simulating interannual variations in numbers of storms (e.g., Carmargo et al. 2005; Vitart et al. 1997). [Thomas Knutson (Reviewer's comment ID #: 132-5)]	Agree, deleted
8-534	A	40:53	40:55	Citation is necessary. [Masato Sugi (Reviewer's comment ID #: 259-1)]	Agree, deleted
8-535	A	40:53	41:2	What is meant by "almost all the papers" is not very clear. Do they refer to papers cited in the previous paragraphs or model experiments in general? The reviewer wonders whether the SST-dependence of TC tracks is so much a robust result or not. Also, the reviewer, a non-expert in typhoons, does not know of the evidence of the shift in western N. Pacific typhoon tracks. A reference is nedded. [Masahide Kimoto (Reviewer's comment ID #: 127-4)]	Agree, deleted
8-536	A	40:55	:57	Should remove this sentence - it's a policy-related (or at least WGII related) concept. [Govt. of United States of America (Reviewer's comment ID #: 2023-531)]	Agree, deleted
8-537	A	40:57	41:1	Needs references to the observational studies. [Seita Emori (Reviewer's comment ID #: 62-9)]	Agree, reference included
8-538	A	40:57	41:1	Please provide a reference for the statement "Observational studies have shown…"	Agree, reference included

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				[Thomas Knutson (Reviewer's comment ID #: 132-6)]	
8-539	A	41:6	41:8	The models tend to be poor at simulating the intensity of tropical cyclones. [Ruth McDonald (Reviewer's comment ID #: 173-17)]	Agree, text modified
8-540	A	41:9	41:9	It doesn't appear that wind-related extremes have really been assessed here. [Seita Emori (Reviewer's comment ID #: 62-11)]	Agree, "wind" deleted
8-541	A	41:10	41:13	This is a statement of an opinion, rather than an assessment based on literature, and thus should be avoided in the AR4 report. Certainly there is evidence that the simulation of extremes is sensitive to changes in small details in, for example, the representation of deep convection, but this is not the same as saying that the only hope is to get down to resolutions that allow explicit representation of convective systems. There is some basis for believing that, despite poor performance on extremes, models can provide at least useful qualitative information regarding future changes in precipitation extremes. To the extent possible, that is an aspect that should be discussed. [Francis Zwiers (Reviewer's comment ID #: 305-89)]	Noted, reference included to support assessment, text modified
8-542	A	41:20		after "forcing" I would add ", taking into account only the "fast" feedback processes (involving water vapour, seasonal snow and ice, clouds, and lapse rate changes)" [Danny Harvey (Reviewer's comment ID #: 101-44)]	Rejected. The definition of climate sensitivity does not depend on the timescale of feedback processes.
8-543	A	41:25		why the word "largely"? Climate sensitivtiy is determined solely by internal feedback processes (recalling that the increase of IR emission as given by the Stefan-Boltzman law is also a feedback process, although some don't count it as such). [Danny Harvey (Reviewer's comment ID #: 101-45)]	Rejected. The transient climate response (which is one particular measure of climate sensitivity) also depends on the ocean heat uptake. Equilibrium climate sensitivity also depends on strength of CO2 forcing as it is defined with respect to CO2 doubling.
8-544	A	41:28		Chapter 6 itself does not assess climate sensitivity based on information from the LGM, but merely refers the reader (on pg 16, line 18) to Chapter 9 (except that the wrong section of Ch 9 is given; it should be 9.6.3.2). Thus, you are just sending the reader on a goose-chase by refering him/her to Chapter 6. Instead, delete "(see Chapter 6)" and instead just leave the reference to Chapter 9. [Danny Harvey (Reviewer's comment ID #: 101-51)]	Accepted. We now refer to section 9.6.
8-545	A	41:29		refer to the specific section of Ch 9 (9.6.3) or, more specifically, 9.6.3.1 for the last millennium and 9.6.3.2 for the LGM. [Danny Harvey (Reviewer's comment ID #: 101-52)]	Accepted. We now refer to section 9.6.
8-546	A	41:30		you refer to further discussion of climate sensitivity in Box 10.2 of Chapter 10, but in fact, climate sensitivity (and pdfs of climate sensitivity) is discussed in Sections 10.5.2, 10.5.4.2, and 10.5.4.5, as well as here in Section 8.6. I would give some serious thought to	Accepted. A synthesis of climate sensitivity estimates from observations and from models is given in Box 10.2.

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				consolidating all of these discussions into one single discussion, preferably in Chapter 8. It is difficult with material on sensitivity pdfs discussed in 4 different places, on top of there being other relevant material in Chapter 9 (9.6.3). I have raised this idea in my comments to Chapter 10 as well. [Danny Harvey (Reviewer's comment ID #: 101-53)]	We have extended the discussion on the role of climate feedbacks in climate sensitivity in this box (see chapter 10).
8-547	A	41:41	41:42	Is it true that feedbacks between soil moisture and precipitation, etc., do not have any influence on the global top-of-atmosphere radiation balance? [Govt. of Finland (Reviewer's comment ID #: 2009-90)]	Accepted. We have removed the end of the sentence about the impact of these feedbacks on the radiation budget.
8-548	A	41:53	41:56	I object to the appropriation of a general term, transient climate response, to mean one specific example of a transient climate response. This is almost as bad as the UNFCCC defining "climate change" as "climate change due to human activities"! [Danny Harvey (Reviewer's comment ID #: 101-46)]	Rejected. We define this term (TCR) as it is defined in the glossary.
8-549	A	41:55		Should be "centred on the time of CO2 doubling" both for clarity and for consistency with the Glossary. [Martin Manning (Reviewer's comment ID #: 155-53)]	Accepted. Text modified to be consistent with the glossary.
8-550	A	42:1		the term "effective climate sensitivity" should be properly defined. It is the climate sensitivity that would occur in equilibrium if the strengths of the individual feedback processes observed at some point during the transient were to persist, unchanged, to the new equilibrium, [Danny Harvey (Reviewer's comment ID #: 101-47)]	Accepted. Text modified.
8-551	A	42:4		Insert "weakly (+-20% for well-mixed greenhouse gases and non-absorbing aerosols)" after "depends" [Danny Harvey (Reviewer's comment ID #: 101-48)]	Rejected. The impact may be larger in the case of ozone forcing (e.g. Stuber et al., 2001).
8-552	A	42:12	42:12	"gives no indication" - certainly not true. "only considers"? "is based solely on"? [William Ingram (Reviewer's comment ID #: 114-182)]	Accepted. Text modified.
8-553	A	42:18	42:18	I suggest inserting "regional" before "variability" as global-mean variability will still be present. [Keith Williams (Reviewer's comment ID #: 290-4)]	Accepted. Text modified.
8-554	A	42:27	42:28	This sentence is dangerously misleading, appearing to say that model development is centred on matching observations. It is not, a similarly important drive (more important in my experience) is the drive to improve the physical basis. [William Ingram (Reviewer's comment ID #: 114-183)]	Accepted. Text modified.
8-555	A	42:32		: this statement is fairly disingenuous. While climate sensitivity is not necessarily a factor in putting changes into a model, once the model is run, climate sensitivity does become an issue. Witness the scramble at NCAR to increase its climate sensitivity from the very low values that prevailed at the time of the TAR and subsequently.	Accepted. Sentence removed.

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				[David Rind (Reviewer's comment ID #: 214-77)]	
8-556	A	42:32		This statement should be removed - model sensitivity is an issue in model development, perhaps not in the initial implementation of subroutines but certainly in the assessment of the model before it is released. [Govt. of United States of America (Reviewer's comment ID #: 2023-532)]	Accepted. Sentence removed.
8-557	A	42:34	42:35	I wonder if all the details given here are correct. The equilibrium climate sensisitivity for CGCM3.1 (both versions) given in Table 8.8.1 is 3.4K, while that listed in the TAR for CGCM1 is 3.5K, so the sensitivity of the CCCma model has been ever so slightly reduced, rather than increased. [Francis Zwiers (Reviewer's comment ID #: 305-90)]	Accepted. Text has been updated based on the final estimates of climate sensitivity from GCMs.
8-558	A	42:35	42:35	"coupled to a slab ocean" seems to qualify "Hadley Centre model" only - I don't expect that is intended? [William Ingram (Reviewer's comment ID #: 114-184)]	Noted. It is actually intended, and the distinction between AOGCMs and GCMs coupled to a slab ocean is now done for more models.
8-559	A	42:45	42:48	The example given is not one of the same change in different models: the bl schemes introduced were supposedly the same (though independently coded, & I believe never checked against each other), but the ones removed were totally different. [William Ingram (Reviewer's comment ID #: 114-185)]	Noted. The Reviewer's remark is consistent with the text (interactions between parameterizations make the change model-dependent)
8-560	A	42:52	42:55	As climate sensitivity is defined as the RATIO of temperature response to radiative forcing, differences in the radiative forcing (if they have been properly diagnosed and used in the computation of climate sensitivity) cannot explain differences in the climate sensitivity among different models. [Danny Harvey (Reviewer's comment ID #: 101-49)]	Rejected. Equilibrium climate sensitivity is often defined as the temperature change associated with a doubling of the CO2 concentration.
8-561	A	43:2	43:6	The methods should be explained. Moreover, is the abbreviation CRF defined? [Govt. of Finland (Reviewer's comment ID #: 2009-91)]	1st comment rejected (about methods) owing to length limitations. As mentionned in the text, these methods are explained for instance in Bony et al. (2006). 2nd comment accepted (CRF now defined).
8-562	A	43:3		A review of feedbacks is also provided by Stephens et al. (2005); note this paper (in the reference list) does not only deal with cloud feedback as the title suggests and provides an alternative perspective to that of Bony et al. (2006) [Richard Allan (Reviewer's comment ID #: 3-78)]	Accepted. Citation added.
8-563	A	43:14		why should a 'substantial spread' indicate a 'closer consensus'? [David Rind (Reviewer's comment ID #: 214-78)]	Accepted. Text clarified.

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8-564	A	43:20	43:20	Vapor -> vapour; explain here WHY these feedbacks are anticorrelated. [Govt. of Finland (Reviewer's comment ID #: 2009-92)]	Accepted. Explanation added.
8-565	A	43:31		"Suggest changing ""global feedback"" to ""global surface albedo feedback""" [Govt. of Canada (Reviewer's comment ID #: 2004-179)]	Accepted. Text modified.
8-566	A	43:38	43:38	in approach with -> with approach in [Govt. of Finland (Reviewer's comment ID #: 2009-93)]	Rejected. Would not improve the text.
8-567	A	43:39	43:39	we will -> we shall [Govt. of Finland (Reviewer's comment ID #: 2009-94)]	Rejected. Both formulations (will or shall) are correct.
8-568	A	43:42		Section 8.6.3.1 Despite the importance of water vapour feedback, I feel that this section is too long and too detailed in comparison with many other sections. [Gill Martin (Reviewer's comment ID #: 167-12)]	Rejected. Length of text commensurate with importance of feedback and extent of developments since the TAR.
8-569	A	43:44		Replacing "and" with "while" may improve the readability somewhat. Here is an alternative suggestion: "Absorption of longwave radiation increases approximately with the logarithm of water-vapour concentration. Since the Clausius-Clapeyron equation dictates a near-exponential increase in moisture holding capacity with temperature and atmospheric and surface temperatures are closely coupled (see Chapter 3, Section 3.4.1), these constraints predict a strongly positive water vapour feedback if RH is close to unchanged." [Richard Allan (Reviewer's comment ID #: 3-79)]	First suggestion accepted, text modified.
8-570	A	43:45	43:45	"atmospheric" plainly wrong: "tropospheric" meant [William Ingram (Reviewer's comment ID #: 114-186)]	Accepted, text modified.
8-571	A	43:47	43:48	To help the reader, this claim needs some justification. [Govt. of Finland (Reviewer's comment ID #: 2009-95)]	Rejected. The anti-correlation between lapse rate and water vapour feedbacks is discussed in 8.6.2.3, and additionally in Box 8.1. Space limitation precludes discussion here also.
8-572	A	44:16	44:16	"confidence" is a state of mind. "reliability" seems to be meant [William Ingram (Reviewer's comment ID #: 114-187)]	Accepted, text modified.
8-573	A	44:26	44:30	What is a CGCM? In earlier parts of this chapter it seems to mean a particular name for a model from the Canadian Centre for Climate Modelling and Analysis (CCCma) e.g. CGCM2. But I think here it is a generic term for a type of model (Coupled Global Climate Models). Is this defined anywhere? [Gareth S. Jones (Reviewer's comment ID #: 121-102)]	Accepted. Text modified to "AOGCM" throughout section in line with usage throughout chapter.
8-574	A	45:5	45:5	Insert "water vapor/lapse rate" after "Evaluation of" in title of sub-section. [Keith Williams (Reviewer's comment ID #: 290-5)]	Accepted, text modified.

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8-575	A	45:16		differences also exist among the satellite reconstructions of UTH in the dry regions, making comparisons somewhat tricky. [David Rind (Reviewer's comment ID #: 214-79)]	Accepted, comment added on satellite data uncertainty.
8-576	A	45:16		Show a range of results [Govt. of United States of America (Reviewer's comment ID #: 2023-533)]	Rejected. Current word usage deliberate as assessment is of model 'skill'.
8-577	A	45:24		I think it is better to change "RH" to "humidity" since this Held and Soden (2000) show that the errors in vapour pressure and relative humidity do not strongly influence the sensitivity of longwave radiation to the change in vapour pressure. [Richard Allan (Reviewer's comment ID #: 3-80)]	Accepted, text modified.
8-578	A	45:28		2xsimulated in quick succession: suggest removing second "simulated" [Richard Allan (Reviewer's comment ID #: 3-81)]	Accepted, text modified.
8-579	A	45:46	45:46	"in part" - "largely" would give a more accurate impression [William Ingram (Reviewer's comment ID #: 114-188)]	Accepted, text modified.
8-580	A	46:2	46:21	There are a number of significant caveats placed upon the Soden et al. (2002) study, namely using Pinatubo to test water vapour feedback. While these are correctly noted, there is no reason why the trends in humidity or interannual variability should provide any more reliable information on the precise strength of the water vapour feedback, yet these studies are not heavily caveated. I suggest, to improve the clarity of this paragraph, a slight re-ordering: ["...A second approach uses the cooling following the eruption of Mt Pinatubo. Using estimated aerosol forcing, Soden et al. (2002) found a model simulated response of HIRS 6.7μm radiance consistent with satellite observations. They also found that the model could only reproduce the observed global temperature response but only if the water vapour feedback was active. Using radiation calculations based on humidity observations, Forster and Collins (2004) found consistency in inferred water vapour feedback strength with an ensemble of coupled model integrations (Figure 8.6.2), although the latitude-height pattern of the observed humidity response did not closely match any single realization. They deduced a water vapour feedback of 0.9–2.5 W m–2 K–1, a range which covers that of models under GHG forcing (see Figure 8.6.1). An important caveat on these studies is that climate perturbation from Pinatubo is small, not sitting clearly above natural variability (Forster and Collins, 2004). Caution is also required when comparing with feedbacks from increased GHGs, because radiative forcing from volcanic aerosol is differently distributed and occurs over shorter timescales, which can induce different changes in circulation and bias the relative land/ocean response (although a recent CGCM study has found similar global longwave clear sky feedbacks between the two forcings; Yokohata et al., 2005). Nevertheless, comparing observed and modelled water vapour response to Mt Pinatubo constitutes one way to test model ability	Accepted, text modified.

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				to simulate humidity changes induced by an external global scale forcing."] [Richard Allan (Reviewer's comment ID #: 3-82)]	
8-581	A	46:15	46:16	line break messed up [William Ingram (Reviewer's comment ID #: 114-189)]	Accepted, text modified.
8-582	A	46:20	46:21	I am not sure that the statement that "climate perturbation from Pinatubo is small...." is actually supported by the evidence. The impact on the SW and LW budgets is very clearly seen. The monthly mean temperature variations were clearly above the internal variability. So the statement is incorrect. [Gareth S. Jones (Reviewer's comment ID #: 121-70)]	Rejected. Forster and Collins (2004) find from both observations and AOGCM experiments that the large range in their estimates of the water vapour feedback are the result of an inability to separate the forced response from natural climate variatiability.
8-583	A	46:40	46:40	"broadscale" -> more usual "large-scale" [William Ingram (Reviewer's comment ID #: 114-190)]	Accepted, text modified.
8-584	A	46:45	46:45	Please add text to the chapter explaining what this represents in terms of the magnitude of the combined water vapor/lapse rate feedback (i.e., the assessed evidence suggests a positive feedback representing about a 50% amplification of the response to global warming, as shown in figure 8.6.1). [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-6)]	Accepted. This is now discussed in section 8.6.2.3.
8-585	A	46:47		Box 8.1. What is this for? This box seems to repeat discussion in the previous sections and I cannot find a reference to it. That said, the box summarises the preceding sections so could perhaps be used instead? [Gill Martin (Reviewer's comment ID #: 167-13)]	Rejected. Box 8.1 serves to synthesise the upper tropospheric humidity components of 8.6 as well as 2.3 and 3.4, and in addition to explain some of the basic feedback processes related to upper tropospheric humidity and temperature changes.
8-586	A	47:54	48:5	It should also be noted that the cooling effect of clouds is primarily felt at the surface during the daytime, while the greenhouse effect of cloud generally heats the atmosphere. [Richard Allan (Reviewer's comment ID #: 3-83)]	Rejected due to space restrictions (this addition would not be fundamental for the following discussion).
8-587	A	48:3	48:3	"may" reads as if one or the other will happen: "might" therefore better [William Ingram (Reviewer's comment ID #: 114-191)]	Accepted. Text modified.
8-588	A	48:23	48:23	"to estimate" doesn't fit grammatically. Possibly some text has been lost, otherwise "estimating" is needed [William Ingram (Reviewer's comment ID #: 114-192)]	Accepted. Text modified as following: "..requires an understanding of...and an estimate of..."
8-589	A	48:30	48:46	A suggested addition to the discussion of cloud altitude feedbacks: "Cess et al. (2001) [The influence of the 1998 El Nin˜o upon cloud radiative forcing over the Pacific warm pool. J. Climate, 14, 2129–2137] suggested a strong influence of ENSO on cloud altitude	Rejected. We do not review all the cloud feedback studies published, but assess the main progress that has been

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				and hence the balance between longwave heating and shortwave cooling. It is likely that this is partly a regional effect relating to changes in the vertical motion fields (Allan et al. 2002 [Influence of Dynamics on the Changes in Tropical Cloud Radiative Forcing during the 1998 El Nin˜o J. Climate, 15, 1979-1986]) that may also be linked with decadal fluctuations in cloud properties (Wielicki et al. 2002 [Evidence for large decadal variability in the tropical mean radiative energy budget. Science, 295, 841–844.]) and is unlikely to be related to cloud feedback." [Richard Allan (Reviewer's comment ID #: 3-84)]	done since the TAR in understanding climate change cloud feedbacks. Therefore we do not discuss processes that are unlikely to be involved in climate change cloud feedbacks (e.g. the dynamically-driven change in clouds associated with El-Nino).
8-590	A	48:38	48:38	First "the" sounds as if this is a definite fact: change to "a" [William Ingram (Reviewer's comment ID #: 114-193)]	Accepted. Text modified.
8-591	A	48:41	48:41	"IRIS" -> "iris" (it's not an acronym) [William Ingram (Reviewer's comment ID #: 114-194)]	Accepted. Text modified.
8-592	A	48:52	48:52	"into" -> "in" [William Ingram (Reviewer's comment ID #: 114-195)]	Accepted. Text modified.
8-593	A	48:56	48:56	Omit " 's " [William Ingram (Reviewer's comment ID #: 114-196)]	Accepted. Text modified.
8-594	A	48:57	48:57	Omit " 's " [William Ingram (Reviewer's comment ID #: 114-197)]	Accepted. Text modified.
8-595	A	49:9	49:9	Not quite accurate - add "or no" after "low-level" [William Ingram (Reviewer's comment ID #: 114-198)]	Accepted. Text modified.
8-596	A	49:9		statement is inaccurate; low level clouds exist immediately before a warm front, not only in regions of descent. [David Rind (Reviewer's comment ID #: 214-80)]	Rejected. Text says "with *prevailing* thick, high-top frontal clouds in regions of synoptic ascent and low-level or no clouds in regions of synoptic descent"; it does not suggest that low-level clouds exist only in regions of descent, nor that high-top clouds are the only clouds found in regions of synoptic ascent.
8-597	A	49:9		Statement is inaccurate; low level clouds exist immediately before a warm front, not only in regions of descent. [Govt. of United States of America (Reviewer's comment ID #: 2023-534)]	Rejected. Text says "with *prevailing* thick, high-top frontal clouds in regions of synoptic ascent and low-level or no clouds in regions of synoptic descent"; it does not suggest that low-level clouds exist only in regions of descent, nor that high-top clouds are the only clouds found in regions of synoptic ascent. .

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8-598	A	49:29	49:40	paragraph is confusing - if CRF approach shows half of the models having a positive and half a negative feedback, and PRP shows them all positive, is what is meant by the two approaches are 'well-correlated' is that their relative ranking (which is most positive, which is less positive, etc.) remained the same? Perhaps this could be said more clearly. The last phrase 'similar range of magnitude' is also quite confusing, given the positive versus negative differences in the two approaches. [David Rind (Reviewer's comment ID #: 214-81)]	Accepted. Text clarified. We now write: "..are well correlated (i.e. their relative ranking is similar), and their exhibit a similar spread among GCMs."
8-599	A	49:29	:49	Paragraph is confusing. If CRF approach shows half of the models having a positive and half a negative feedback, and PRP shows them all positive, is what is meant by the two approaches are 'well-correlated' is that their relative ranking (which is most positive, which is less positive, etc.) remained the same? Perhaps this could be said more clearly. The last phrase 'similar range of magnitude' is also quite confusing, given the positive versus negative differences in the two approaches. [Govt. of United States of America (Reviewer's comment ID #: 2023-535)]	Accepted. Text clarified. We now write: "..are well correlated (i.e. their relative ranking is similar), and their exhibit a similar spread among GCMs."
8-600	A	49:53	49:53	Misleading in that in some models the clouds *don't* cover the large areas they should! Add "that should be" before "covered"? [William Ingram (Reviewer's comment ID #: 114-199)]	Rejected. Despite the fact that models often underestimate the low-level cloud cover, the regions covered by low-level clouds cover large areas of the globe in all the models.
8-601	A	50:13	50:13	"has thus become more constraining" doesn't read well - "has thus become more powerful"? "thus constrains the models more"? [William Ingram (Reviewer's comment ID #: 114-200)]	Accepted. Text modified according to your suggestion.
8-602	A	50:25		At the end of the paragraph, add "Unfortunately, large uncertainties exist in the relative amounts of clouds in different layers as well as their optical properties due to inherent difficulties determining the cloud layers using any passive satellite observations especially for overlapped clouds (Chang and Li 2005a). The latest global cloud statistics obtained from MODIS (Chang and Li 200b) showed much less mid-level clouds and more low-level clouds than those obtained from the ISCCP (Rossow and Schiffer 1999) due to different treatments of overlapped clouds. In comparison with the new MODIS cloud product, problems suffered by GCMs seem to be less serious in generating mid-level clouds than low-level clouds." [Zhanqing Li (Reviewer's comment ID #: 147-12)]	Accepted. Text was saying "(note however that uncertainties remain in the observational determination of the relative amounts of the different cloud types)." We have modified the text by removing brackets and by citing Chang and Li (2005b) at the end of this statement.
8-603	A	50:26	50:27	The sentence would flow better with "... inability to simulate the right strength ..." [William Ingram (Reviewer's comment ID #: 114-201)]	Accepted. Text modified according to your suggestion.
8-604	A	50:30	50:33	This sentence is of course true, but the stronger statement that even the more plausible possibility of right fractional change in cloud optical depth would also give too little effect seems worth making to me.	Rejected. A fractional change in optical depth is still a change in magnitude and is covered by the statement already in

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				[William Ingram (Reviewer's comment ID #: 114-202)]	the text. It is not obvious to us that a right fractional change in cloud optical depth is more plausible.
8-605	A	50:37	50:38	"mixed-phase cloud water distribution" -> "distribution of each phase of cloud water" [William Ingram (Reviewer's comment ID #: 114-203)]	Accepted. Text modified according to your suggestion.
8-606	A	50:41	50:41	"clouds" -> "cloud" [William Ingram (Reviewer's comment ID #: 114-204)]	Accepted. Text modified.
8-607	A	50:43	50:43	"conditionS" [William Ingram (Reviewer's comment ID #: 114-205)]	Accepted. Text modified.
8-608	A	50:49	50:50	"exhibit … observations" -> are most different and least realistic" [William Ingram (Reviewer's comment ID #: 114-206)]	Accepted. Text modified according to your suggestion.
8-609	A	50:55	50:55	"clouds' " -> "cloud" [William Ingram (Reviewer's comment ID #: 114-207)]	Accepted. Text modified.
8-610	A	51:2	51:2	Insert "current" before "models" - we know, for instance, that HadCM2 had a very different high cloud feedback linked to it having extensive very thin cirrus, which other GCMs do not but which is observed - if some future GCMs are more realistic in this regard they might also show a similar feedback [William Ingram (Reviewer's comment ID #: 114-208)]	Accepted. Text modified.
8-611	A	51:7	51:7	"introduce by" -> "due to"? [William Ingram (Reviewer's comment ID #: 114-209)]	Accepted.
8-612	A	51:26	51:31	These 2 sentences say nothing that is not obvious: unless quantitative quotation is thought worthwhile, omit. [William Ingram (Reviewer's comment ID #: 114-210)]	Accepted. The two sentences removed.
8-613	A	51:26		I may be missing something but I did not see the connection between the argument being made in the text and what is shown in the Figure. Why does a correlation between modelled spring time and seasonal marginal change in albedo, which I would imagine could be due to any number of model factors, mean that addressing the seasonal cycle biases not guarantee a (more?) realistic result. I suspect some steps in your thinking are missing from the text. [Martin Manning (Reviewer's comment ID #: 155-54)]	Rejected. For details see Hall and Qu (2006).
8-614	A	51:29	51:29	increase in solar radiation -> increase in absorbed solar radiation [Govt. of Finland (Reviewer's comment ID #: 2009-96)]	Rejected. The text has been removed
8-615	A	51:46	51:46	"by" -> "in" [William Ingram (Reviewer's comment ID #: 114-211)]	Accepted
8-616	A	51:52	51:52	What is UML? I guess a typo for OML, meaning slab - if so, say that & be consistent with rest of chapter: if not, explain	Taken into account. Text modified.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[William Ingram (Reviewer's comment ID #: 114-212)]	
8-617	A	51:55	51:55	"numerous" -> "other" [William Ingram (Reviewer's comment ID #: 114-213)]	Accepted
8-618	A	52:7	52:7	"temperature"! So temperature changes can affect warming! Is that really what's meant, or was "lapse rate"? [William Ingram (Reviewer's comment ID #: 114-214)]	Taken into account. Text modified.
8-619	A	52:13	52:46	The report would benefit if this section could discuss the likely relative magnitudes of water vapor/lapse rate, clouds, and cryosphere feedbacks, at least broadly. The report notes the enhanced confidence in the water vapor/lapse rate feedback and suggests it is a large and positive term but stops short of comparing it to the other terms - water vapor is probably the largest feedback, correct? although clouds may be comparable models do not suggest clouds are larger? then finally please state the best assessment of the size of the cryosphere feedbacks, which are significant but likely smaller, right? A few sentences summarizes this would be very helpful. [Susan Solomon (co-chair WG1) (Reviewer's comment ID #: 246-7)]	Accepted. The relative magnitude of the different feedbacks is now discussed in section 8.6.2.3.
8-620	A	52:18	52:20	The point about the metrics being insensitive to the methodology doesn't seem very clear. It is hard to imagine how you might make a metric insensitive to the particular measure of the difference between models and obs that is used, but still make it sensitive to differences between models in those differences between models and obs. [Francis Zwiers (Reviewer's comment ID #: 305-91)]	Accepted. Sentence removed.
8-621	A	52:29	:46	Climate metrics should also include the simulation skill of the AR4 models for the 20th century (it could be right for the wrong reasons, but nevertheless, it is a test). [Govt. of United States of America (Reviewer's comment ID #: 2023-536)]	Noted. The simulation of the AR4 models for the 20th century is part of "a wide variety of climate statistics, including simulations of the mean climate and variability".
8-622	A	52:36	52:36	Should "upper relative humidity" be "upper troposphere relative humidity"? [Francis Zwiers (Reviewer's comment ID #: 305-92)]	Accepted. Text modified.
8-623	A	52:41	52:41	Should there be additional candidates that relate to coupled atmosphere-ocean and/or atmosphere-land processes? [Francis Zwiers (Reviewer's comment ID #: 305-93)]	Rejected. The list of processes that is listed in this paragraph is obviously not exhaustive. We restrict the list to those processes whose role in climate sensitivity has been highlighted in 8.6 (note also that coupled atmosphere-ocean processes are involved in cloud feedbacks and atmophere-land processes in snow-albedo feedbacks).
8-624	A	52:53	52:54	"The … threshold" too strong as a general statement: many aspects are smooth	Rejected. Text added to make defintion

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[William Ingram (Reviewer's comment ID #: 114-215)]	clearer.
8-625	A	53:4	53:8	Is this paragraph really needed? It doesn't really provide the reader with an assessment of threshold detection methods, and I don't think it helps him/her understand the material that follows. Moreover, I suspect that the methods listed here (and one could add quite a few others as well) were not developed specifically for the problem of detecting abrubt change of the kind that is considered here (i.e., that which results from a change in forcing, and is disportionately large as compared to responses to forcing under other circumstances). [Francis Zwiers (Reviewer's comment ID #: 305-94)]	Rejected. This paragraph gives the reader some information on statistical methods used to find abrupt changes. The use of these methods in some sense determines what is "abrupt".
8-626	A	53:18	53:18	For consistency with above paragraph and later section, the section heading should read "Forced, Abrupt Climate Change". [Govt. of Australia (Reviewer's comment ID #: 2001-359)]	Accepted. Title changed.
8-627	A	53:20	53:52	Maybe I've missed it but I think the text only says that the MOC weakens but doesn't say anywhere WHY, i.e. mainly reduced density through surface warming and freshening in the North Atlantic, with some other more complicated and less understood processes. [Reto Knutti (Reviewer's comment ID #: 133-7)]	Accepted. Text added.
8-628	A	53:30	53:33	I wouldn't say the drivers of the MOC are 'unclear', in fact you list the two important drivers, it's only unclear how much they both contribute. The current sentence implies that we don't know anything about it, which I think is incorrect. I would also mention that the MOC is to some degree a self-sustaining process with the salt advection to the North Atlantic. [Reto Knutti (Reviewer's comment ID #: 133-6)]	Accepted. Text modified.
8-629	A	53:34		"Some modelling studies..." I have noted already for the FOD that the choice of references here is inappropriate: the results of Tziperman 97 have been shown in the peer-reviewed literature (Rahmstorf & Ganopolski, J. Clim. 1999,) to be an artefact of an unphysical experimental design; this paper should either not be cited, or together with a caveat pointing to the rebuttal paper. It does not even clearly investigate a threshold. Rind et al 2001 also does not demonstrate thresholds. On the other hand, we now have a systematic model intercomparison study for thresholds with 11 participating models, which can be cited instead (and is referenced later in the chapter anyway). I propose to replace this sentence with: "A systematic model intercomparison study (Rahmstorf et al. 2005) found that all 11 participating models of intermediate complexity have a threshold where the MOC shuts down. Due to the high computational cost, such a search for thresholds has not yet been performed with full coupled GCMs, but some of the participating models included ocean GCMs." [Stefan Rahmstorf (Reviewer's comment ID #: 206-32)]	Accepted. Text used as suggested.
8-630	A	53:41	53:41	"This" -> "Such"?	Accepted. Text modified as suggested.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[William Ingram (Reviewer's comment ID #: 114-216)]	
8-631	A	53:44	53:44	"recovers" -> "returns" [William Ingram (Reviewer's comment ID #: 114-217)]	Accepted. Text modified as suggested.
8-632	A	53:47	53:48	Clearer to drop "it" & then replace "the circulation" with "it"? [William Ingram (Reviewer's comment ID #: 114-218)]	Accepted. Text modified as suggested.
8-633	A	53:54	54:3	Although it may be true that an idealized, imposed shutdown of the MOC can cause strong local cooling, but not an ice age (which is the main point of this paragraph), I would add the following at the end of the parpagraph, so that the reader has a good perspective on the latest model results: "However, in a recent intercomparison involving 11 coupled atmosphere-ocean models (Gregory et al., 2005), the MOC decreases by only 10-50% during a 140-year period (as CO2 quadruples), and in no model is there a cooling anywhere (as the global-scale heating due to increasing CO2 overwhelms the local cooling effect due to reduced MOC)" [Danny Harvey (Reviewer's comment ID #: 101-50)]	Accepted. Text modified as suggested.
8-634	A	53:54	54:3	As one of the two authors (not Keigwin, & please note spelling) mentioned in reference to a website article on abrupt climate change, let me say that we NEVER said that any global warming-induced abrupt collapse of the MOC could lead to an ice age. This is simply not true and must be corrected. If one reads our reference in the present IPCC draft, we DO refer to possible climate change akin to the Little Ice Age, but this is an order of magnitude different and should not be confused with an ice age. The entire paragraph should be stricken unless a bonafide scientific reference to an MOC-collapse leading to an ice age can be found. [Terrence Joyce (Reviewer's comment ID #: 122-6)]	Taken into account. Sentence deleted.
8-635	A	53:54		It belongs to the procedures of the IPCC that it bases its assessment on peer reviewed and published scientific/technical literature. The given reference in connection with the statement "the change of state of the MOC could cool the Northern Hemisphere as GHG increase and potentially cause a future ice age" (Joyce and Keigwin) do not belong to this category of literature. Please add a peer reviewed and published reference that actually validate this statement or delete it completely. [Govt. of Germany (Reviewer's comment ID #: 2011-54)]	Taken into account. Sentence deleted.
8-636	A	53:54		Review Editor, please check this! I made this comment before, but the authors apparently insist on promoting false statements, which as a reviewer I find highly frustrating and difficult to understand the reasons for. No researcher to my knowledge has ever speculated that "the change of state of the MOC could cool the Northern Hemisphere as GHG increase and potentially cause a future ice age", and even the reference given simply does not do it. I repeat my FOD comment here: "This discussion does not belong in this chapter, and also seems to be a knee-jerk reaction to a Hollywood film - why discuss a	Taken into account. Sentence deleted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				Hollywood desaster movie scenario? As far as I know, no scientist has ever suggested that greenhouse warming could cause an ice age - as witnessed by the fact that the reference given (Joyce and Keigwin) is only to a web page, and this page does not even say that an ice age could be caused. (It does speak about a "little ice age", refer to Chapter 6 if there is any confusion here between LIA and a real ice age.)" You've got to either find a reference that actually says what you want to rebut here, namely that researchers have speculated about a new ice age being triggered this way (and I doubt you'll find one), or you've got to drop this paragraph. What's the point? It is disturbing that the chapter authors did not even bother to correct the spelling of Lloyd Keigwin's name in response to my comment - did you read it at all? [Stefan Rahmstorf (Reviewer's comment ID #: 206-7)]	
8-637	A	54:11	54:11	"lead" -> "led" [William Ingram (Reviewer's comment ID #: 114-219)]	Accepted. Text modified as suggested.
8-638	A	54:15		The dependence of thresholds on location has not been systematically investigated by Rind et al. 2001, but it has been by Rahmstorf 1996. Sorry this is pro domo, but I think the latter is clearly the more appropriate reference here. Rahmstorf, S. (1996), On the freshwater forcing and transport of the Atlantic thermohaline circulation, Clim. Dyn., 12, 799-811. [Stefan Rahmstorf (Reviewer's comment ID #: 206-33)]	Accepted. Reference added.
8-639	A	54:17	54:17	"models" -> "models' " [William Ingram (Reviewer's comment ID #: 114-220)]	Accepted. Text modified as suggested.
8-640	A	54:21	54:22	Add the following reference after "Gregory et al. 2005": Zhou T., R. Yu, X. Liu,Y. Guo et al., 2005, Weak response of the Atlantic thermohaline circulation to an increase of atmospheric carbon dioxide in IAP/LASG Climate System Model, Chinese Science Bulletin, 50(6), 592-598 [Govt. of China (Reviewer's comment ID #: 2006-67)]	Rejected. Many references for water hosings not included: Dixon et al., etc.
8-641	A	54:22		Add one sentence: "Meltwater runoff from a melting of the Greenland ice sheet is a potentially major source of freshening not yet included in these models (see 8.7.2.2)." [Stefan Rahmstorf (Reviewer's comment ID #: 206-34)]	Accepted. Text modified as suggested.
8-642	A	54:23	54:23	Replace "are important in many models" with "are also important in many models"? [Francis Zwiers (Reviewer's comment ID #: 305-95)]	Accepted. "also" added to text.
8-643	A	54:38	54:39	Perhaps cross-link with Ch. 4 here - they point out that new observations show ice-streams and glaciers can accelerate quickly (which might change the balance between runoff and calving). [Francis Zwiers (Reviewer's comment ID #: 305-96)]	Accepted. Chapter 4 reference added.
8-644	A	54:43	54:43	For brevity, omit "the reader is encouraged"	Accepted. Text deleted.

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				[William Ingram (Reviewer's comment ID #: 114-221)]	
8-645	A	54:57	54:57	Omit "leading to a" & "of" [William Ingram (Reviewer's comment ID #: 114-222)]	Accepted. Text deleted.
8-646	A	54:57		note there is also the potential for NADW changes to instigate changes in the deep water formation around Antarctica, with potential impacts on Antarctica - see Rind et al., 2001 (reference already listed). [David Rind (Reviewer's comment ID #: 214-82)]	Taken into account. Sentence added refeencing Rind et al. 2001.
8-647	A	54:57		Note that there is also the potential for NADW changes to instigate changes in the deep water formation around Antarctica, with potential impacts on Antarctica - see Rind et al., 2001 (reference already listed). [Govt. of United States of America (Reviewer's comment ID #: 2023-537)]	Rind et al. (2001) found that changes in the NADW formation rate could instigate changes in the deepwater formation around Antarctica.
8-648	A	55:1	55:1	Capitalize initials of "bottom water" [William Ingram (Reviewer's comment ID #: 114-223)]	Accepted. Text modified as suggested.
8-649	A	55:7	55:7	Add "possible" before "climate" [William Ingram (Reviewer's comment ID #: 114-224)]	Accepted. Text modified as suggested.
8-650	A	55:12	55:14	These two sentences do not hang together very well. The 3 year timescale is mentioned twice, once would probably do! [Gareth S. Jones (Reviewer's comment ID #: 121-71)]	Accepted. Text modified as suggested
8-651	A	55:18	55:20	It is stated in section 8.6 that the climate sensitivity can vary with different types of forcing. This caveat should be added here. It would also help to have "conceptually" after "seems". [Keith Williams (Reviewer's comment ID #: 290-6)]	Accepted. Text modified as suggested
8-652	A	55:18	55:22	Might be useful to cross-link to Ch 9 here (e.g., discussion at the end of 9.6.2.2 on whether the response to volcanic forcing can be used to constrain the climate sensitivity). [Francis Zwiers (Reviewer's comment ID #: 305-97)]	Accepted. Text modified as suggested
8-653	A	55:25	55:25	"in the oceans" -> "on the sea bed" [William Ingram (Reviewer's comment ID #: 114-225)]	Accepted. Text modified as suggested
8-654	A	55:25	55:26	"in situ water pressure and temperature fields" -> "high pressures and low temperatures" [William Ingram (Reviewer's comment ID #: 114-226)]	Accepted. Text modified as suggested
8-655	A	55:25	55:26	Cross link to 4.7.2.4 (which talks in part about subsea permafrost and methane gas hydrates) [Francis Zwiers (Reviewer's comment ID #: 305-98)]	Accepted. Text modified as suggested
8-656	A	55:28	55:28	melti -> melting [Govt. of Finland (Reviewer's comment ID #: 2009-97)]	Accepted. Text modified as suggested
8-657	A	55:28	55:28	... permafrost melting and ... [Marco A. Giorgetta (Reviewer's comment ID #: 85-5)]	Accepted. Text modified as suggested

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8-658	A	55:28	55:28	"melti" -> "melting" [William Ingram (Reviewer's comment ID #: 114-227)]	Accepted. Text modified as suggested
8-659	A	55:35	55:35	Also cite Ch 4. [Francis Zwiers (Reviewer's comment ID #: 305-99)]	Accepted. Text modified as suggested
8-660	A	55:41	55:42	Typo at the end of line 41, beginning of line 42. [Francis Zwiers (Reviewer's comment ID #: 305-100)]	Accepted. Text modified as suggested
8-661	A	55:42	55:42	Remove 'ne is can'. [Govt. of Finland (Reviewer's comment ID #: 2009-98)]	Accepted. Text modified as suggested
8-662	A	55:42	55:42	Omit "ne is can" [William Ingram (Reviewer's comment ID #: 114-228)]	Accepted. Text modified as suggested
8-663	A	55:43	55:43	Remove duplicate 'can'. [Govt. of Finland (Reviewer's comment ID #: 2009-99)]	Accepted. Text modified as suggested
8-664	A	55:43	55:43	Omit "can" [William Ingram (Reviewer's comment ID #: 114-229)]	Accepted. Text modified as suggested
8-665	A	55:51	55:52	I don't think the reason why the model's climate sensitivity matters will be obvious to the innocent reader: cross-reference or very brief explanation [William Ingram (Reviewer's comment ID #: 114-230)]	Accepted. Text added.
8-666	A	55:54	56:2	Should make it clearer that these are only model results (& I think fairly preliminary, in that they have not yet been confirmed by a range of models?) [William Ingram (Reviewer's comment ID #: 114-231)]	Accepted. Text added.
8-667	A	55:55	55:55	In what kind of model? [Francis Zwiers (Reviewer's comment ID #: 305-101)]	Taken into account. Text added.
8-668	A	56:4	56:4	Why 'preliminary' in this sentence? I agree there are only few studies, but preliminary implies somehow inferior, incomplete, or uncertain. [Reto Knutti (Reviewer's comment ID #: 133-8)]	Rejected. The model results are incomplete in the sense that they are ocean-only, land changes are not included.
8-669	A	56:4	56:7	the paleo-perspective is useful here - abrupt climate changes in the paleorecord are in general associated with only small changes in atmospheric CO2. [David Rind (Reviewer's comment ID #: 214-83)]	Noted.
8-670	A	56:4	:7	The paleo-perspective is useful here - abrupt climate changes in the paleorecord are in general associated with only small changes in atmospheric CO2. [Govt. of United States of America (Reviewer's comment ID #: 2023-538)]	Noted.
8-671	A	56:9	56:10	This sentence is hard to read (so hard that I don't really understand what is being said). I think "similar" needs changing or adding to. [Keith Williams (Reviewer's comment ID #: 290-7)]	Taken into acount. Text deleted.
8-672	A	56:26	56:27	Should "period" be "periods" or has "a" been left out after "during"?	Accepted. Text modified as suggested

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[William Ingram (Reviewer's comment ID #: 114-232)]	
8-673	A	56:26	56:26	An article is missing before "relatively". [Francis Zwiers (Reviewer's comment ID #: 305-102)]	Accepted. Text modified as suggested
8-674	A	56:29	56:29	Omit 2nd "is" [William Ingram (Reviewer's comment ID #: 114-233)]	Accepted. Text modified as suggested
8-675	A	56:29	56:33	I don't think there is an issue for detection and attribution. The deterministic response to external forcing appears to provide a good explaination for historical global scale changes during the 20th century, and during the last millennium at least (Chapter 9). However, if there were a real possibility of unforced abrubt climate change in the future, then that would make the projections less certain, and thus would make it more difficult to formulate mitigation and adaptation policy. So the real question, I think, is whether anthropogenic forcing is driving the earth system closer to a base state where unforced abrubt change becomes more likely. Is there any evidence to suggest that this might be happening? [Francis Zwiers (Reviewer's comment ID #: 305-103)]	Rejected. We disagree with the reviewer. A large abrupt event in the future may look like a forced response and therefore be a problem for detection/attribution studies.
8-676	A	56:33	56:33	Omit 2nd "the" [William Ingram (Reviewer's comment ID #: 114-234)]	Accepted. Word deleted.
8-677	A	56:40	56:40	Omit "of which" [William Ingram (Reviewer's comment ID #: 114-235)]	Accepted.
8-678	A	56:47	56:49	The number can be very large if the model is computationally cheap enough & the different cases can be generated (semi-)automatically, as climateprediction.net has shown (though certainly those are important restrictions) [William Ingram (Reviewer's comment ID #: 114-236)]	Accepted. Text modified.
8-679	A	56:53	56:55	Points 1 & 2 oddly arranged - the relationship between emissions & concentrations is raised to a point in its own right for gases but the harder task for aerosols is absorbed into the end of point 2. Treat them more consistently! [William Ingram (Reviewer's comment ID #: 114-237)]	Rejected. In simple climate models, the radiative forcing associated with aerosols is scaled on aerosol precursor emissions.
8-680	A	57:4	57:4	"particularity" is not a common word & I'm uncertain what is implied - "feature"? "property"? [William Ingram (Reviewer's comment ID #: 114-238)]	Accepted. Text modified.
8-681	A	57:16		General comment on EMICs: in response to question 8.1 (p.91), the first source of confidence listed for climate models is that they solve the fundamental equations for conservation of mass, momentum, and energy (as well as moisture). To the extent that EMICs violate this condition, they cannot be thought of as supplying a confident numerical conclusion regardless of how well they can reproduce results from GCMs - simulating the right result for the wrong reason does not improve a model's reliability.	Rejected. Some EMICs, like the atmospheric part of UVIC, are energy balance models which are derived from the constraint to fulfill the conservation of heat and moisture. Other EMICS, like LOVECLIM, are "simplified

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				[David Rind (Reviewer's comment ID #: 214-84)]	GCMs", and they conserve heat, mass and momentum as good as GCMs. For some other EMICs, like CLIMBER-2, the governing equations are derived from first principles. Note that not all GCMs solve the fundamental equations. Indeed, most GCMs are based on filtered equations, and all GCMs use numerical approximations of these equations. At the end, both EMICs and GCMs conserve heat, mass and momentum as good as the implemented numerical schemes permit. The Authors consider that the EMIC limitations are clearly mentioned in the text and do not need to be further underlined.
8-682	A	57:16		General comment on EMICs: in response to question 8.1 (p.91), the first source of confidence listed for climate models is that they solve the fundamental equations for conservation of mass, momentum, and energy (as well as moisture). To the extent that EMICs violate this condition, they cannot be thought of as supplying a confident numerical conclusion regardless of how well they can reproduce results from GCMs - simulating the right result for the wrong reason does not improve a model's reliability. [Govt. of United States of America (Reviewer's comment ID #: 2023-539)]	See answer to comment 8-681.
8-683	A	57:21	57:21	"designed" - is "intended" or "suitable" or (my guess - but if so a bit too much is being packed into one word) both meant? "though the design of some is not suitable"? [William Ingram (Reviewer's comment ID #: 114-239)]	Accepted. "Designed" has been replaced by "suitable".
8-684	A	57:25	57:25	invaluable -> valuable? [Govt. of Finland (Reviewer's comment ID #: 2009-100)]	Rejected. "Invaluable" means "extremely valuable".
8-685	A	57:48	57:48	Omit "in the vertical direction", or at the very least, "direction" [William Ingram (Reviewer's comment ID #: 114-240)]	Accepted.
8-686	A	57:57	58:1	So one "scenario" is simply the 1st half of the other scenario? I suspect not: I suspect both scenarios also include a stabilization this text forgets to tell us about [William Ingram (Reviewer's comment ID #: 114-241)]	Accepted. Text clarified.
8-687	A	58:17	58:17	The tuned sensitivities are from fitting to a coupled run, while does not necessarily have the sam e sensitivity as the slab run normally used to determine sensitivity. I believe this is more important than some of the points mentioned and should be added.	Rejected. This information is given at page 58, lines 11-17 of the SOD.

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				[Reto Knutti (Reviewer's comment ID #: 133-9)]	
8-688	A	58:22	58:22	If "integration" needs explaining, don't use it - just say "the number of components..." [William Ingram (Reviewer's comment ID #: 114-242)]	Accepted. Text modified.
8-689	A	58:22	58:22	Omit "Earth's" & "being" to simplify this complex phrase [William Ingram (Reviewer's comment ID #: 114-243)]	Accepted.
8-690	A	58:34	58:34	What does "integrity" mean? "variation"? [William Ingram (Reviewer's comment ID #: 114-244)]	Accepted. Text clarified.
8-691	A	58:40	58:40	Present day is what period? [Francis Zwiers (Reviewer's comment ID #: 305-104)]	Accepted. The term "present-day climate" was used as a substitute for "pre-industrial climate in equilibrium with an atmospheric CO_2 concentration of 280 ppmv". The text has been modified to explain more precisely what has been done.
8-692	A	58:48	58:48	"favourably" normally means "better", not, as here, "a bit worse overall, but not by far considering how much simpler they are". I can't think of a word that is just what's needed ("satisfactorily" or "surprisingly well" don't seem really right) so suggest adding ", given their comparative simplicity" to the end of the sentence [William Ingram (Reviewer's comment ID #: 114-245)]	Accepted. Text modified
8-693	A	59:23	59:23	A synthesis section is vital for this chapter, to allow policy readers to get an accurate picture of the scope of current climate modelling work, the capacity of that modelling to project future climate change and advances in the modelling that have occurred since the TAR. [Govt. of Australia (Reviewer's comment ID #: 2001-360)]	Rejected. This function is served by the Executive Summary.
8-694	A	63:20		References to be added to Chapter 8: Chang, F.-L., ad Z, Li, 2005a: A new method for detection of cirrus overlapping water clouds and determination of their optical properties, J. Atmos. Sci., 62, 3993–4009. Chang, F.-L., and Z. Li, 2005b, A near-global climatology of single-layer and overlapped clouds and their optical properties retrieved from Terra/MODIS data using a new algorithm, J. Climate, 18, 4752-4771. Rossow, W. B., and R. A. Schiffer, 1999: Advances in understanding clouds from ISCCP, Bull. Amer. Meteor. Soc., 80, 2261-2287. [Zhanqing Li (Reviewer's comment ID #: 147-13)]	References will be added if cited in revised text.
8-695	A	73:18	73:20	Jungclaus, J.H. , M. Botzet, H. Haak, N. Keenlyside, J.-J. Luo, M. Latif, J. Marotzke, U. Mikolajewicz, and E. Roeckner, 2006: Ocean circulation and tropical variability in the coupled model ECHAM5/MPI-OM, J. Climate, in press.	Text modified

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				[Marco A. Giorgetta (Reviewer's comment ID #: 85-3)]	
8-696	A	73:21	73:21	K-1 developers --> K-1 model developers; also in Table 8.2.1 (page 8-95) [Masahide Kimoto (Reviewer's comment ID #: 127-5)]	Text modified
8-697	A	73:47	73:47	Error in reference "Kiktev, D., D.N.H. Sexton, L. Alexander, and C.K. Folland, 2003:", should be D.M.H. Sexton. [John Caesar (Reviewer's comment ID #: 36-7)]	Text modified
8-698	A	73:51	73:51	A cited reference is missing: Kimoto, M., N. Yasutomi, C. Yokoyama and S. Emori, 2005: Projected changes in precipitation characteristics near Japan under the global warming, SOLA, 1, 85-88, doi: 10.2151/sola. 2005-023. [Masahide Kimoto (Reviewer's comment ID #: 127-3)]	Text modified
8-699	A	76:1	76:1	The reference paper ' Liu, H., B. Wang, F. Xue and R. Yu, 2002: The sensitivity of precipitation simulation to difference schemes of water vapor equation in atmospheric general circulation model. Climatic and Environmental Research, 7(1), 121-134 (in Chinese).' should be added at the beginning of this page. [Govt. of China (Reviewer's comment ID #: 2006-63)]	Reference will be added if cited in revised text.
8-700	A	82:1	82:2	Roesch, A., and E. Roeckner, 2006: Assessment of snow cover and surface albedo in ECHAM4 and ECHAM5, J. Climate, in press. [Marco A. Giorgetta (Reviewer's comment ID #: 85-4)]	Accept – text modified
8-701	A	83:49	83:49	"fildelity" -> "fidelity" [William Ingram (Reviewer's comment ID #: 114-246)]	Text modified
8-702	A	87:6	87:8	Tsushima, Y., S. Emori, T. Ogura, M. Kimoto, M. J. Webb, K. D. Williams, M. A. Ringer, B. J. Soden, B. Li, and N. Andronova: Importance of the mixed-phase cloud distribution in the control climate for assessing the response of clouds to carbon dioxide increase: a multi-model study. Clim. Dyn., in press. (The authors and title has been updated) [Seita Emori (Reviewer's comment ID #: 62-30)]	Text modified
8-703	A	89:32	89:33	changing (submitted) into Special Report on Climate Change, No.4, 1-15 [Zong-Ci Zhao (Reviewer's comment ID #: 302-1)]	Text modified
8-704	A	89:52	89:52	The reference paper ' Yu, R., 1994: A two-step shape-preserving advection scheme. Advances in Atmospheric Sciences, 11(4), 479-490.' should be inserted before line 52. [Govt. of China (Reviewer's comment ID #: 2006-64)]	Reference will be added if cited in revised text.
8-705	A	90:3	90:5	Yukimoto and Noda, 2003 should be replaced by Yukimoto et al., 2006: Yukimoto, S., A. Noda, A. Kitoh, M. Hosaka, H. Yoshimura, T. Uchiyama, K. Shibata, O. Arakawa, and S. Kusunoki, 2006: Present-day climate and climate sensitivity in the	Reference will be added if cited in revised text. (Table 8.2)

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				Meteorological Research Institute Coupled GCM version 2.3 (MRI-CGCM2.3). J. Meteor. Soc. Japan, 84, 333-363. [Akira Noda (Reviewer's comment ID #: 192-2)]	
8-706	A	91:0		Comment on Question 8.1: I think it would be a good idea to point out that one of the largest uncertainties in the predictions of models of future changes in the climate is the uncertainiy of in future humans activities and how humans respond to the problems addressed in this assesment. [Wilmer Anderson (Reviewer's comment ID #: 5-60)]	Rejected. The question strictly covers reliability of climate models for projections, and this issue extends beyond that scope to the human responses to climate change.
8-707	A	91:0		Comment on Question 8.1: I think it would be a good idea to point out that while long term trends can not be reversed quickly, this is not a reason to delay action. [Wilmer Anderson (Reviewer's comment ID #: 5-61)]	Rejected. The question strictly covers reliability of climate models for projections, and this issue extends beyond that scope to the human responses to climate change.
8-708	A	91:1	92:19	Comment on Question 8.1 This question is rather long because the characterization of the software given in Question 8.1 is so severely and significantly incomplete. A brief description of a more nearly complete characterization of the software is given first. Specific issues related to the characterization given in Question 8.1 are then given. The basic question is formulated following those discussions. It is frequently stated that the basis of the large AOLGCMs are, "the equations for conservation of mass, momentum, and energy." In Question 8.1 the statement is given as, " One source of confidence in models comes from the fact that model fundamentals are based on established physical laws, such as conservation of mass, energy and momentum, along with a wealth of observations." However, this statement is not entirely correct and it is severely incomplete on several significant levels. A more nearly complete characterization of the software is given in the following short summary. Characterization of the Software Software for real-world complex phenomena and processes is generally comprised of the following models and methods components: 1. Fundamental basic model equations from continuum mechanics such as the Navier-Stokes for mass, momentum and energy conservation, heat conduction, radiative energy transport, chemical-reaction laws, the Boltzmann equation, and many others. The fundamental equations include also the constitutive equations for the behavior and properties of the associated materials; equation of state, thermo-physical and transport properties and basic material properties. Generally the basic equations refer to the behavior and properties of the material of interest. 2. Engineering models and empirical correlations of experimental data needed to close the basic model equations; turbulent fluid flow, heat transfer and friction factor correlations,	Rejected. This comment would require a response to question 8.1 discussing in full detail all aspects of model specification from equation formulation and discretization through to specification of all physical parametrisations. This is clearly beyond the scope of the question, and would furthermore clearly be inappropritate for the target audience, even if scope or space permitted. The aim of the FAQ's is not to serve as a textbook on climate modelling in this way. Furthermore, uncertainties in models, e.g. due to use of parametrisations of unresolved physical processes, are already covered in a manner appropriate for the audience and to a degree of detail permitted by space restrictions.

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				mass exchange coefficients, for examples. Generally the engineering models and empirical correlations refer to specific states of the materials of interest, not the materials themselves, and are thus usually of much less than a fundamental nature. Many times these are basically interpolation methods for experimental data. 3. Special purpose models for phenomena and processes that are too complex or insufficiently understood to model from basic principles, or would require excessive computing resources if modeled from basic principles. 4. Models for phenomena and processes occurring in complex engineering equipment, if a physical system of interest includes hardware. In the case of the large general AOLGCMs, the equipment and processes involved in conversion of materials in one form and composition into other forms and compositions. 5. Analytical and numerical solution methods for all the equations that comprise the models. 6. Auxiliary functional methods for installation, code input and output, analyses of calculated results, and other user-aids. 7. Non-functional aspects of the software include its ease of, or fitness for, understandability, maintainability, extensibility and portability. The resulting equations that are used to model the physical phenomena and processes always form a large system of coupled, non-linear partial and/or ordinary differential equations (PDEs and ODEs) plus a very large number of algebraic equations. All of the above are generally incorporated into computer software for use and application to the analyses for which the models and methods were designed to be applied. For real-world models of inherently complex physical phenomena and processes the software itself will generally be complex and somewhat difficult to accurately apply and the calculated results somewhat difficult to understand. Users of such software must usually receive training in applications of the software. Documentation of all the above characteristics, in sufficient detail to allow independent replication of the software and its applications, is generally a very important aspect of development and use of production-grade software. Almost all complex physical phenomena are non-linear with a multitude of temporal and spatial scales, interactions and feedbacks. Universally, numerical solution methods via finite-difference, finite-element, spectral, and other discrete-approximation approaches, are about the only alternative for solving the system of equations. When applied to the continuous PDEs and ODEs and the algebraic equations of the model these approximations give systems of coupled, nonlinear algebraic equations which are enormous in size; millions of degrees of freedom. Based on the characterization of the software as listed above, the following paragraphs illustrate that the statement " … the fact that model fundamentals are based on established	

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				physical laws, such as conservation of mass, energy and momentum, ..." is not entirely correct and is severely incomplete on several significant levels. [Dan Hughes (Reviewer's comment ID #: 111-1)]	
8-709	A	91:1	92:19	omment on Question 8.1 Continued I. Numerical Solution Methods It is a true fact that numerical solution methods are the dominant aspect of almost all modeling and calculation of inherently complex physical phenomena and processes in inherently complex geometries. The spatial and temporal scales of the application area of AOLGCMs are enormous, maybe unsurpassed in all of modeling and calculations. The tremendous spatial scale of the atmosphere and oceans has so far proven to be a very limiting aspect relative to computing requirements, especially when coupled with the large temporal scale of interest; centuries of time, for example. All important physical processes occur at spatial scales which are less than the discrete spatial resolution employed in all calculations. (This aspect is mentioned in Lines 40 through 55 of Question 8.1.) Additionally, the range of temporal scales of the phenomena and processes encountered in applications range from those associated with chemical reactions to time spans on the order of a century. Not all of these scales are accurately resolved. Unlike a "pure" problem, such as solution of the incompressible Navier-Stokes equations to resolve directly turbulent motions for which the basic equations are solved, the correlations and parameterizations and finite-difference aspects of the AOLGCMs are the overriding concerns. Spatial discontinuities in all fluid-state properties (density, velocity, temperature, pressure, etc.) introduce the potential for instabilities, as do discontinuities in the discrete representation of the geometry of the solution domain. Additionally, physical instabilities are known to be captured by the equations in AOLGCMs, and the behavior of the numerical solution methods when these are resolved becomes vitally important. The algebraic approximations to the original continuous equations are only approximately solved. Grid independence has never been demonstrated, for example. The lack on demonstrated grid independence is proof that the algebraic equations have been only approximately solved. Evidence of independent Verification of (1) the coding and (2) the actual achieved accuracy of the numerical solution methods also have never been demonstrated. Finally, while the fundamental equations are usually written in conservation form, not all numerical solution methods exactly conserve the physical quantities. Actually, a test of numerical methods might be that conserved quantities in the continuous partial differential equations are in fact conserved in actual calculations. II. Incomplete Basic Equations As noted in Item 3 above, some fundamentals of some phenomena and processes are either not know, or are too complex for mathematical description from first principles, or	Rejected. This comment would require a response to question 8.1 discussing in full detail all aspects of model specification from equation formulation and discretization through to specification of all physical parametrisations. This is clearly beyond the scope of the question, and would furthermore clearly be inappropritate for the target audience, even if scope or space permitted. The aim of the FAQ's is not to serve as a textbook on climate modelling in this way. Furthermore, uncertainties in models, e.g. due to use of parametrisations of unresolved physical processes, are already covered in a manner appropriate for the audience and to a degree of detail permitted by space restrictions.

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				would require computer calculations that would make modeling of the phenomena out of reach. For climate models one of the most important to fall under this category is turbulent fluid flow and the associated mass, momentum and energy exchanges that occur at the boundaries of the fluid masses. Turbulent flow is the expected flow regime for atmospheric and oceanic flows, and for the vast majority of flows in climate science. The exchanges at the interfaces are typically modeled by use of algebraic correlations of empirical data. For air-ocean-land interactions these correlations are notoriously imprecise. Data are exceedingly difficult to obtain for the large spatial scales of interest and under the wide ranges of the complexity and state of the interfaces. Note also that the large codes do not attempt to model and calculate the mass conservation equation for CO2. Instead, the assumed concentrations of CO2 of interest are simply specified to be present in the atmosphere as an initial condition for the calculations. III. Approximations in Original Equations Even though fundamental basic equations of mass, momentum, and energy conservation are taken as the starting point for the modeling of a few of the physical phenomena and processes of importance, several assumptions and approximations are generally needed in order to make the problem tractable, even with the tremendous computing power available today. The scalar mass and energy equations are typically less effected than the vector momentum equations in this regard. The exact radiative transfer equations, for example, are not solved, but instead approximations are introduced to make the problem tractable. IV. Predictive Power Is Not in the Basic PDEs and ODEs For the class of models of interest here, and for models of inherently-complex, real-world problems in general, the predictive power is maintained in the modeling under Items 2, 3, and 4 listed above. The basic equations generally transport the mass and energy redistributions while the mass and energy content to be transported is generally determined by at the interfaces between the physical subsystems (the atmosphere, ocean, and lands) and other boundary conditions. The driving gradients at the interfaces are not resolved by the grid of discrete points used to represent the spatial scale. The effects of these driving gradients are represented by correlations of empirical data. The apparently all-encompassing parameterizations used in almost all AOLGCM models and codes fall under these items. (The importance of the parameterizations is mentioned in Lines 40 through 55 of Question 8.1.) SUMMARY The statement about the basis of the models as given in Question 8.1 is an incomplete representation of nearly all important aspects of the large AOLGCM models and computer codes. The statement should be modified so as to include a more nearly complete discussion of the correct characterization of the models and methods.	

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				The discussions that I have given here do not begin to be exhaustive in any way. The status of the Documentation, independent Verification and Validation, and software Quality Assurance of the models and methods and application calculations have not been touched upon. These aspects are as important as the foundations of the models and methods. Please indicate how all the issues discussed above will be addressed in the final version of the document. The issues that should be especially discussed include (1) the lack of use of some basic equation models such as turbulence, (2) the approximations that are made in order to modify and simplify some of the basic equations, (3) the extensive use of algebraic models and engineering correlations to represent some phenomena and processes in the place of basic equations, (4) the extensive use and reliance on parameterizations, and (5) the overriding and dominant issues associated with numerical solution methods; especially, the lack of grid independence in the numerical solutions. Finally, some discussions of the very significant aspects of Documentation, independent Verification and Validation, and software Quality Assurance of the models and methods and application calculations should also be addressed. [Dan Hughes (Reviewer's comment ID #: 111-2)]	
8-710	A	91:3	91:5	models cannot really produce a confident estimate of climate change even on hemispheric scales as long as climate sensitivity is not known to within a factor of two (or three). The scale in that case does not matter as much as the overall feedback (e.g., uncertainty from clouds). [David Rind (Reviewer's comment ID #: 214-85)]	Rejected: confidence in the ability of models to produce quantitative estimates of climate change is not contradicted by there being a range of such projections, this being conveyed by the use of the word "estimates" in the opening sentence. Uncertainty from aspects such as clouds is also discussed explicitly in the question. Scale is important in that the overall assessment is that confidence is greater for GCM projections at larger spatial scales (e.g. global) compared with smaller (e.g. local).
8-711	A	91:3	:5	Plausible quantitative estimates within a range (still a factor of 2 to 3 in climate sensitivity even on the global scale). [Govt. of United States of America (Reviewer's comment ID #: 2023-540)]	Rejected: The notion of there being a range of projections is conveyed by the use of the word "estimates" in the opening sentence. Further discussion of range of projections is also included later in discussion in the question.

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8-712	A	91:4	91:4	Insert after "above". "but there is no supporting evidence for this claim" [VINCENT GRAY (Reviewer's comment ID #: 88-896)]	Rejected: see chapter for supporting evidence.
8-713	A	91:5		Delete extra period. [David Wratt & David Fahey (Reviewer's comment ID #: 67-58)]	Accepted: text modified.
8-714	A	91:7	91:7	Add at end ".but there are no examples of successful future climate prediction" [VINCENT GRAY (Reviewer's comment ID #: 88-897)]	Rejected; refer Fig 1.1, Chapter 1 as example of such.
8-715	A	91:12	91:12	Add at end "but not from successful prediction" [VINCENT GRAY (Reviewer's comment ID #: 88-898)]	Rejected; refer Fig 1.1, Chapter 1 as example of such.
8-716	A	91:16		Suggest change for simplicity: '…land surface. Unprecedented…' [David Wratt & David Fahey (Reviewer's comment ID #: 67-59)]	Accepted: text modified.
8-717	A	91:18		Suggest omitting commas. [David Wratt & David Fahey (Reviewer's comment ID #: 67-60)]	Accepted: text modified.
8-718	A	91:21		Suggest for clarity to non-expert readership omitting 'or closely related variants' [David Wratt & David Fahey (Reviewer's comment ID #: 67-61)]	Rejected: retained for strict accuracy.
8-719	A	91:30	91:31	unfortunately, models cannot simulate the proper amount of ice age cooling because we really don't know what that is (we don't know how cool the tropics, or half the globe, really were).. In fact, no climate model has produced, on its own, an ice age climate, without specification of boundary conditions a priori. And the mid-Holcene warmth is due entirely to enhanced solar insolation over northern latitudes during summer - that is not really a test of models. [David Rind (Reviewer's comment ID #: 214-86)]	Accepted: text changed to align closely with Chapter 6 Executive Summary wording on model simulation of LGM cooling.
8-720	A	91:30	:31	Unfortunately, models cannot simulate the proper amount of ice age cooling because we really don't know what that is (we don't know how cool the tropics, or half the globe, really were).. In fact, no climate model has produced, on its own, an ice age climate, without specification of boundary conditions a priori. And the mid-Holcene warmth is due entirely to enhanced solar insolation over northern latitudes during summer - that is not really a test of models. [Govt. of United States of America (Reviewer's comment ID #: 2023-541)]	Accepted: text changed to align closely with Chapter 6 Executive Summary wording on model simulation of LGM cooling.
8-721	A	91:32	91:34	"although … climate." not very clear, & omits the fact that a model which does get the 20th-century changes wrong may still get the future right. How about "though the quantitative value of this is limited by the uncertainty in how much cooling from anthropogenic sulphate particles should be prescribed to force the models"? [William Ingram (Reviewer's comment ID #: 114-247)]	Taken into account: text modified following this and comment 8-722
8-722	A	91:32		Suggest describing better what Figure 1 shows as 'One example is the global temperature trend over the past century (shown in figure 1) which can be modeled with high skill when both anthropogenic and and natural forcings are included. The large uncertainties in	Accepted: text modified.

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				the magnitude....' [David Wratt & David Fahey (Reviewer's comment ID #: 67-63)]	
8-723	A	91:43	91:43	"some" -> "many" (It should be acknowledged somewhere in this chapter that some things are easier to simulate than others, & that ENSO, as an alternation between different quasi-equilibria, is by its very nature a very hard one.) [William Ingram (Reviewer's comment ID #: 114-248)]	Taken into account: word 'some' dropped.
8-724	A	91:43	91:43	Capitalize initial of "southern" [William Ingram (Reviewer's comment ID #: 114-249)]	Accepted: text modified.
8-725	A	91:46		Suggest improving structure as "limitations in scientific understanding of some physical processes, or in some cases the availability of observations.' [David Wratt & David Fahey (Reviewer's comment ID #: 67-62)]	Rejected: suggested changes do not improve text.
8-726	A	91:47	91:47	The point about observations might confuse some readers - people often ask me whether the models have to be continually fed by data. I think therefore that it would be a good idea to clarify what the data are for - e.g., to describe land surface properties, or to parameterize subgrid scale processes, or to develop process understanding,... [Francis Zwiers (Reviewer's comment ID #: 305-105)]	Taken into account in overall modifications to this part of text.
8-727	A	92:15	92:15	Insert after "scales" "but they have never actually done so" [VINCENT GRAY (Reviewer's comment ID #: 88-899)]	Rejected; refer Fig 1.1, Chapter 1.
8-728	A	92:16	92:16	Would "limitations" be a better word than "weaknesses"? [Melinda Marquis (Reviewer's comment ID #: 162-90)]	Accepted: text modified.
8-729	A	92:19	92:19	Add at end "even if it doesn't seem to happen" [VINCENT GRAY (Reviewer's comment ID #: 88-900)]	Rejected; refer Fig 1.1, Chapter 1, and Figure 1 of this question.
8-730	A	93:0	95:	The figure caption reads as if the top comes after the resolution, whereas the reverse is the case [William Ingram (Reviewer's comment ID #: 114-250)]	Accepted. Text revised.
8-731	A	93:0	95:	The "land" column keeps saying "layers" but not how many - this would be interesting additional information & take up negligible space [William Ingram (Reviewer's comment ID #: 114-251)]	Rejected. This information has not been forthcoming from the groups.
8-732	A	94:0	95:	Where there are 2 models from 1 centre, there are no }{ to indicate how the one "Sponsor(s), Country" applies to both [William Ingram (Reviewer's comment ID #: 114-252)]	Accepted. Table will be reformatted.
8-733	A	95:0	95:	The ocean resolution for UKMO-HadCM3 is wrong, surely? [William Ingram (Reviewer's comment ID #: 114-253)]	Taken into account. The values will be confirmed.
8-734	A	95:0		Table 8.2.1. Yukimoto et al., 2006 should be added to the reference for "Atmosphere" component of the "20: MRI-CGCM2.3.2" model. [Akira Noda (Reviewer's comment ID #: 192-3)]	Accepted.

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8-735	A	95:0		Table 8.2.1. Yukimoto et al., 2006 should be added to the reference for "Ocean" component of the "20: MRI-CGCM2.3.2" model. [Akira Noda (Reviewer's comment ID #: 192-4)]	Accepted.
8-736	A	95:0		Table 8.2.1. Yukimoto and Noda, 2003 should be replaced by Yukimoto et al., 2006 as the reference for the "Coupling" of the "20: MRI-CGCM2.3.2" model. [Akira Noda (Reviewer's comment ID #: 192-5)]	Accepted.
8-737	A	96:0	96:	The F2x claimed for the UKMO models are not the values I'm used to [William Ingram (Reviewer's comment ID #: 114-256)]	Accepted. Table modified.
8-738	A	96:0	97:	Superscript a is used for 2 different purposes in this Table - very confusing: replace one [William Ingram (Reviewer's comment ID #: 114-255)]	Accepted. Table modified.
8-739	A	96:0		Figure 6.13: Is model AJS (mentioned in table 6.2 included anywhere in this? [Gareth S. Jones (Reviewer's comment ID #: 121-66)]	Comment appears misplaced. Will pass to Ch 6.
8-740	A	96:0		Figure 6.13: -Is GSZ2003 included in the solar/volcanic/all other forcings plot? Or does it overlap with another line? [Gareth S. Jones (Reviewer's comment ID #: 121-67)]	Comment appears misplaced. Will pass to Ch 6
8-741	A	96:0		Table 8.8.1 This table is refered to from section 8.8.2 page 8-57 line 51 and purports to contain the parameter values used by the simple climate model (MAGICC). There were 4 sets of simple model parameters on the table originally submitted to ch8 but only 3 of them appear in Table 8.8.1. The missing parameter is the effective climate sensitivity which is the most important input parameter. The TAR Table 9.1 carried both the equilibrium (mixed layer) climate sensitivity and the effective climate sensitivity (suplied by me) so there is a precedent for doing this, both terms are clearly defined in the TAR text. The climate feedback parameter (column 3, supplied and calculated by Jonathan) is not a substitue because the required number cannot be derrived even if the method to do it were transparant. The F2x values (column 2) are not compatible with the climate feedback parameter (column 3) and ocean heat uptake efficiency (column 5). [Sarah Raper (Reviewer's comment ID #: 208-4)]	Accepted. Table modified.
8-742	A	96:0		Table 8.8.1 The 4 MAGICC input parameters need to be clearly identified and grouped together if possible (presently columns 2, 7 and 8), as should the 2 columns produced by Jonathan (presently columns 3 and 5). [Sarah Raper (Reviewer's comment ID #: 208-5)]	Accepted. Table modified.
8-743	A	96:1	96:1	"simulate" confusing in a context where we're used to it meaning GCMs - perfectly correct, of course, but I suggest expanding slightly - "simulate AOGCM results in simple models"? [William Ingram (Reviewer's comment ID #: 114-254)]	Accepted. Caption modified.

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8-744	A	96:11	96:11	"1pctto2x" needs explanation [William Ingram (Reviewer's comment ID #: 114-257)]	Accepted. Caption modified.
8-745	A	97:2	97:3	line break messed up [William Ingram (Reviewer's comment ID #: 114-258)]	Accepted.
8-746	A	97:2	97:6	GSZ is a dashed line in the figure and it does not contain A (in Table 6.2) but this piece of text only refers to dotted lines not having A. Some mention of the GSZ/dashed line should be here [Gareth S. Jones (Reviewer's comment ID #: 121-68)]	Comment appears misplaced. Will pass to Ch 6.
8-747	A	97:5	97:5	With or without stratospheric adjustment? [William Ingram (Reviewer's comment ID #: 114-259)]	Accepted. We now mention that the radiative forcing values are adjusted ones.
8-748	A	98:0	98:	Heading "INLAND ICE" should be "LAND ICE" - most of it does reach a coast [William Ingram (Reviewer's comment ID #: 114-260)]	Accepted. "Inland ice" has been replaced by "ice sheets".
8-749	A	98:0	99:	Replace the Ms of M-LT, M-LIT & M-LST with the actual number of levels, to give much more information with no more space needed [William Ingram (Reviewer's comment ID #: 114-261)]	Accepted. Table and caption modified.
8-750	A	99:20	99:20	"Inland" -> "land" - most of it does reach a coast [William Ingram (Reviewer's comment ID #: 114-262)]	See answer to comment 8-748.
8-751	A	102:1	102:1	What are the units of the quantity displayed? [Francis Zwiers (Reviewer's comment ID #: 305-106)]	Figure deleted
8-752	A	102:5	102:9	Give an exact definition for the concept 'coupling strength diagnostic'. [Govt. of Finland (Reviewer's comment ID #: 2009-101)]	Figure deleted
8-753	A	102:5	102:5	"the difference" doesn't mean much! Clarify, or replace by "a quantity" [William Ingram (Reviewer's comment ID #: 114-263)]	Figure deleted
8-754	A	102:8	102:9	The point of this sentence (that the insets don't cover any signal) is not immediately obvious: "No signal appears in the small land areas covered by the insets"? [William Ingram (Reviewer's comment ID #: 114-264)]	Figure deleted
8-755	A	103:1	103:3	Are you sure that the 'observed' SST/surface air temperature is correct? For example, below-zero annual-mean SSTs are reported for the ice-free Barents Sea. Moreover, the SST distributions in Figs. 8.3.1 and 8.3.8 are distinctly different. [Govt. of Finland (Reviewer's comment ID #: 2009-102)]	Accepted. Values will be checked.
8-756	A	103:8	103:9	What is meant by "surface air temperature"? As far as I know (almost) all models assume continuity of temperature at the surface, so the temperature of the air at the surface is the surface temperature. [William Ingram (Reviewer's comment ID #: 114-265)]	No change necessary. The surface air temperature differs some from surface temperature in most models, which use various methods to estimate it at 2 or 3 meters above the surface.

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8-757	A	103:10	103:10	"typical model error" No! It is the typical *size* of the model error [William Ingram (Reviewer's comment ID #: 114-266)]	Accepted.
8-758	A	105:6	105:12	figure mentioned - - mean model. The curves are lower than all of the models. [Zong-Ci Zhao (Reviewer's comment ID #: 302-2)]	Taken into account. This is explained in the caption.
8-759	A	105:7	105:7	"scattered and reflected" as if these were 2 different processes! Just "reflected" will do nicely [William Ingram (Reviewer's comment ID #: 114-267)]	Accepted.
8-760	A	106:0		Figure 8.3.4 Only one key bar needed [Gareth S. Jones (Reviewer's comment ID #: 121-72)]	Accepted.
8-761	A	106:1	106:10	Would mm/year be a more convenient unit? That unit is commonly used in climatological maps. [Govt. of Finland (Reviewer's comment ID #: 2009-103)]	Taken into account. Units will be made consistent with other chapters in so far as possible.
8-762	A	107:5	107:5	"implied" totally mysterious to the innocent reader! And why use "implied" for the models anyway, when you should have the actual transports - unless you need to keep it like-for-like because the "implication" is unreliable (i.e. "implied" & actual oceanic heat transports do not match), in which case this should certainly be explained [William Ingram (Reviewer's comment ID #: 114-268)]	Accepted. Text revised.
8-763	A	108:6	108:6	"observationally-based estimates" - the ERA windstresses are not directly based on observations: they are model output, from a model some aspects of which are constrained to be very close to observations. They may be the best guess available, but should not be referred to as "Obs". [William Ingram (Reviewer's comment ID #: 114-269)]	Accepted. Text changed to "observationally-constrained."
8-764	A	108:6	108:6	"for the period" -> "of" [William Ingram (Reviewer's comment ID #: 114-270)]	Accepted.
8-765	A	108:6		Several things wrong here. ERA-40 was a 45-year reanalysis, not a 40-year reanalysis. "European" should be replaced by "ECMWF". ERA-40 ran from September 1957 to August 2002. Was the sub-period 1960-2000 as quoted in the figure caption chosen for a particular reason - it does not in any case match the years 1980-1999 for which model results are shown. Why not show the years 1980-1999 from ERA-40? [Adrian Simmons (Reviewer's comment ID #: 242-121)]	Accepted.
8-766	A	110:1	110:10	To facilitate interpretation, use a different colour for continents and regions with sea-ice. [Govt. of Finland (Reviewer's comment ID #: 2009-104)]	Accepted. Continental outlines will be included.
8-767	A	110:7	110:9	I am a bit confused about why the observations are partially from an earlier period to the model. Won't there be a possible (likely as temperatures are rising) warm bias in the models because of the later period? Surely it is not too difficult to look at the same two periods.	Taken into account.

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				[Gareth S. Jones (Reviewer's comment ID #: 121-73)]	
8-768	A	112:1	112:8	State in the caption whether the figure is based on observational data or model output. Include a few arrows in the figure to facilitate interpretation. On l. 5, merdional -> meridional. [Govt. of Finland (Reviewer's comment ID #: 2009-105)]	Taken into account. Figure removed to meet length constraints.
8-769	A	112:1	112:1	Caption should state what data this diagram originates from (e.g., which models and forcing). [Francis Zwiers (Reviewer's comment ID #: 305-107)]	Taken into account. Figure removed to meet length constraints.
8-770	A	112:5	112:5	"merdional" -> "meridional" [William Ingram (Reviewer's comment ID #: 114-271)]	Taken into account. Figure removed to meet length constraints.
8-771	A	112:5	112:5	Sv have not been defined, are not a standard physical unit & are not in the Glossary [William Ingram (Reviewer's comment ID #: 114-272)]	Taken into account. Figure removed to meet length constraints.
8-772	A	112:5	112:7	The discription of the direction of flow is correct, but I suggest that a simpler explanation would be to say anti-clockwise for positive flows? [Gareth S. Jones (Reviewer's comment ID #: 121-74)]	Taken into account. Figure removed to meet length constraints.
8-773	A	113:0	113:	The "colour bar" is not correct - it indicates all the map should have at least the green of "less than 1 model" [William Ingram (Reviewer's comment ID #: 114-273)]	Accepted. Figure modified.
8-774	A	113:1	113:3	The colour scale should be broader and colours more discernible. Especially, areas with more than 50% and less than 50% of models simulating sea-ice should be clearly distinguished. [Govt. of Finland (Reviewer's comment ID #: 2009-106)]	Accepted. Figure modified.
8-775	A	114:0	114:	The colours are not explained - I assume blue is "closer to observed" & red "further from observed" [William Ingram (Reviewer's comment ID #: 114-274)]	Accepted. Text revised.
8-776	A	114:0	114:	The change in U200, PSL & Z500 are so small only the arrowhead appears: comment or, better, alter so the problem goes away (e.g. to "outline arrowheads", i.e. just 2 short lines coming back from the tip) [William Ingram (Reviewer's comment ID #: 114-275)]	Accepted. Text revised.
8-777	A	114:0	114:	U200 tangled up with OLR in both arrows & labels: clarify [William Ingram (Reviewer's comment ID #: 114-276)]	Accepted. Figure modified.
8-778	A	114:0		Please explain red vs. blue arrows. [Reto Knutti (Reviewer's comment ID #: 133-12)]	Accepted. Figure caption revised.
8-779	A	114:5	114:20	"no mention in caption or text as to meaning of blue vs red arrows (red = worse in newer models?)"	Accepted. Figure caption revised.

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				[Govt. of Canada (Reviewer's comment ID #: 2004-180)]	
8-780	A	114:7	11:7	Add comma before "500" [William Ingram (Reviewer's comment ID #: 114-277)]	Accepted.
8-781	A	115:0		There is a bit much in fig. 8.4.1. Maybe the vertical text min/max on the right of each panel could be removed to make it less overloaded. [Reto Knutti (Reviewer's comment ID #: 133-13)]	Rejected. The detail is warranted.
8-782	A	116:0	116:	I was very confused, till I realized, by the y scales being different - fix or warn! [William Ingram (Reviewer's comment ID #: 114-278)]	Accepted. Text will be added.
8-783	A	116:0		The legend in the upper panel of figure 8.4.2 refers (second entry) to "ERA-15 Reanalysis", whereas in the lower panel the legend refers to "ECMWF Reanalysis". The latter could be either ERA-15 or ERA-40, and should be changed accordingly. [Adrian Simmons (Reviewer's comment ID #: 242-125)]	Accepted. This will be clarified.
8-784	A	116:6	116:9	So is this from the 2002 or the 2006 paper? Or is a from one & b from the other? Clarify & remove any unnecessary reference. [William Ingram (Reviewer's comment ID #: 114-279)]	Accepted. Situation will be clarified.
8-785	A	118:1	118:14	Give an exact definition for the quantity 'feedback strength'. [Govt. of Finland (Reviewer's comment ID #: 2009-107)]	Y-axis label changed ("feedback strength" has been replaced by "feedback parameter", whose definition is in the glossary.
8-786	A	119:7	119:7	"denotes" -> "represents"? [William Ingram (Reviewer's comment ID #: 114-280)]	Noted. Owing to space restrictions, this figure has been removed from the Third-Order draft.
8-787	A	119:8	119:8	"normal" -> "Normal" or "Gaussian" [William Ingram (Reviewer's comment ID #: 114-281)]	Noted. Owing to space restrictions, this figure has been removed from the Third-Order draft.
8-788	A	120:0	120:	Inset caption - surely these are total, not partial, derivatives? [William Ingram (Reviewer's comment ID #: 114-282)]	Accepted. Caption modified.
8-789	A	120:9	120:11	I don't see the point of the parenthesis - to be expected, & not immediately relevant [William Ingram (Reviewer's comment ID #: 114-283)]	Accepted. Text in parenthesis removed.
8-790	A	120:10	120:10	Subscript the "2" if this text is kept [William Ingram (Reviewer's comment ID #: 114-284)]	Accepted (but text removed).
8-791	A	121:9	121:9	So when is "springtime" if not April & May, & why are they different? [William Ingram (Reviewer's comment ID #: 114-285)]	Rejected. Springtime could be defined slightly differently (e.g. as MAM months).
8-792	A	122:6	122:6	The "present day" represents what period? [Francis Zwiers (Reviewer's comment ID #: 305-108)]	Accepted. The term "present-day climate" was used as a substitute for

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					"pre-industrial climate in equilibrium with an atmospheric CO_2 concentration of 280 ppmv". The caption has been modified to better reflect the figure content.
8-793	A	123:5		If exactness is required, it is actually over the 1906-2005 period. [Daithi Stone (Reviewer's comment ID #: 256-53)]	Noted, however figure and caption are both changed.
8-794	A	123:8	123:9	In fact the naturally forced simulations are centred relative to the 1901-1997 mean of the corresponding all forced simulation. [Daithi Stone (Reviewer's comment ID #: 256-54)]	Noted, however figure and caption are both changed.