

We are very grateful to both reviewers. They were both very supportive of the paper's messages and identified important areas where the original text could be clarified. We have responded to both reviews below (Reviewer's comments in bold, ours in plain type):

**Anonymous Referee #1**

**Received and published: 4 October 2012**

**This study is an up-to-date description of numerical experiments that aim to estimate the impact of scenario and model diversity uncertainty in global mean temperature. The novelty here is the emphasis on emission driven simulations. I found the manuscript interesting, important for its new contributions to the field and well written, although several sections lack clarity (these are pointed below). I recommend the article to be accepted with minor revisions.**

**page 1057 line 11: typo "development"**

Done. Thanks.

**page 1058 : please clarify second sentence.**

It was not clear. This sentence has been changed for clarity.

**page 1060 line 3: typo "aggressive"**

Done

**page 1060: For an even superficial understanding of this section more information should be given to the reader. The last paragraph of section 2.1.1 is difficult to follow.**

This is useful feedback. We have expanded the discussion to make this clear. Specifically, we have broken this paragraph into two. The first discusses the previous 4 experiments and how parameter combinations from these 4 experiments were combined to generate 68 simulations for this study. The second paragraph discusses the criteria used to spinup the simulations to a stable pre-industrial state and the criteria used to remove a subset of the simulations at this stage based on comparison with observed metrics.

**page 1061: typos line 15 "straightforward" and line 26 "we've"**

Done.

**page 1062 line 16: units for the radiative forcing.**

Included.

**page 1062 line 20: "the the"**

removed

**page 1063: last paragraph of section 2.2 unclear.**

This is been split in two and the discussion expanded to make this clearer.

**page 1064 line 14: missing word.**

added

**page 1065 paragraph starting in line 14: unclear.**

This paragraph has been completely rewritten to clarify this. It also more clearly makes the point that uncertainties in how we represent past forcings does have an impact, albeit a small one, on near term projection uncertainty.

**page 1065 line 25: typo "become"**

changed

**page 1065 line 26: "The inclusion of carbon..." I would remove this second announcement of information to come. It does not add much here.**  
removed.

**page 1066 line 1: unclear against what it is being compared.**  
clarified

**page 1066 last paragraph of section 3.1: I am unconvinced that the information about uncertainty increasing with RCP is of any real value. I may be wrong in my understanding but at least this should be clarified. Since each simulation is depicted as a difference with respect to a baseline, it seems obvious that for a very low RCP –let us say RCP0.0– the spread should be roughly that of the natural variability. This would be a minimum, and all models should agree in the reaction: a stationary evolution by definition. The farther you deviate from RCP0.0 the farther discrepancies between models should have an effect. How can be expected, as implied from the last sentence in the text, that reducing emissions to zero may not reduce uncertainty?**

This is useful feedback as it highlighted how unclear the original text was in this paragraph. The Reviewer is correct that we would expect the uncertainty to scale in some way with the magnitude of future emissions. We wanted to make the point that despite the increased uncertainty evident within the emission driven paradigm, that future temperature uncertainty is still expected to be smaller for smaller emissions. The other point was that the representation of uncertainties adopted here and in Hawkins and Sutton neglects non-linear interactions between model responses and scenarios, that are considered in Yip et al, 2011. We can see how the original text was really not clear. We've clarified this text in the revised version to bring out these two points.

**page 1067 line 13: grammatical issue makes sentence unclear.**  
Thanks. Sentence now restructured.

**page 1068 line 8: eliminate "which"**  
done.

**page 1068 line 27: typo "section sections"**  
fixed.

**page 1070 line 1-8: The reasons for differences include the amount of simulations. A larger amount of simulations will tend to span further (the variance will be barely effected but the range quite a bit). This concentration on extremes generate situations like the one described in line 15.**

Acknowledged. We've amended the first sentence to read *"The reasons for these differences at both the high and low end, relate to a larger ensemble size used to sample this range and .."*

**page 1070 line 11: typo "use" to "us"**  
changed.

**page 1071 line 22: Clarify what is meant not to "explore the corners". The covariates?**  
This has now been clarified. By corners we mean looking at responses for high atmospheric feedbacks combined with high carbon cycle feedbacks for example (or high atmospheric feedbacks with low carbon cycle feedbacks).

**page 1073 line 11: typo "previously thought"**

corrected.

**page 1073 line 12: see Skinner L (2012) A Long View on Climate Sensitivity. Science 337:917–919.**

Thanks. We've now added this reference.

**page 1073 line 14: typo**

**page 1073 line 14: please point out where this is coming from.**

This sentence has now been clarified to more clearly link it to the following discussion.

**page 1074 line 23: Going from global to regional not only may increase the impact of scenarios driven by regional characteristics, but also increase the interannual variability noise, and perhaps model error too.**

This is undoubtedly true. These issues (increased role of internal variability and model uncertainty on regional scales) have been discussed within the concentration paradigm in Hawkins and Sutton (where the non-scenario components do indeed take a larger role). The note made in this line regarding increased role for scenario uncertainty earlier has not previously been made, however. Not wanting to swamp this in a greatly expanded discussion, we've left this as a throw away line in the text.

**page 1076 line 11, 28; page 1077 line 31: strange reference numbers.**

I believe these are links left in by the copernicus formatting software (which will provide hyperlinks to the references in the text in the final version). I will highlight this in our response to the editor.

**page 1081: Fig 1a is not very friendly, and less so is Fig 2. At least this last should be shown much larger.**

I've contacted the editorial staff and I have been told that in principle it should be possible to present larger versions of these figures in the final document.

**Caption of Fig 1 describes the figure on the right twice.**

Now corrected

**page 1082 caption: typo "also included is"**

Thanks. Corrected.

## **Anonymous Referee #2**

**Received and published: 20 October 2012**

**In this study, the authors examined scenario and modeling uncertainty on global mean temperature change derived from emission driven perturbed parameter ensemble of a global climate model, and compare the results with those from the CMIP5 models. This is a timing subject, and model results are carefully examined and useful insights have also been gained through this analysis. I would recommend its publication after my following comments are addressed:**

**Section 2.1.1: I totally agreed with Reiviewer #1 that this part needs more clarification. For example, it is not clear to me how you come to a 68 member ensemble. The authors need to list how parameters are perturbed for each constituent ensemble. Also, what is the exact criteria for rejecting those 11 members? Even if the authors refer Lambert et al. (2012) for details, the basic details needed to be provided here for readers to understand this manuscript without having to reading Lambert et al. (2012). What is more, Lambert et al. (2012) is a submitted manuscript.**

We have taken on board these comments. The existing description of the experiments has now been expanded to 2 paragraphs. The first more fully describes the previous experiments and discusses

how these configurations were used to generate a 68 member ensemble. The second paragraph now notes details of the spinup criteria for these simulations (criteria used to establish when they could be deemed stable) and the criteria subsequently used to remove the subsection of 11 members which were deemed sufficiently unrealistic. The Lambert paper is now accepted at Climate Dynamics, and I will discuss with the editor whether this can be made available to the reviewers. The revised text should provide a fuller description to enable the reader to follow the paper without necessarily referring to Lambert.

**Section 2.1.2, the first paragraph: This paragraph needs more clarification too. It is not clear to me why the comparison with C4MIP is not straightforward to do (so you mean that C4MIP use CO2 concentrations while this paper uses CO2 emission).**

This is useful feedback. We have now clarified the differences between C4MIP and ESE in the text (difference forcing scenarios – different emission pathways and different implementation of other non-CO2 forcings).

**Also, it is not clear to me how Booth et al. (2012a) addressed this issue using a simple model.** This has now been clarified with an expanded description.

**Section 2.2: In terms of RCP8.5 and 2.6, so you mean that only CMIP5 RCP8.5 experiments are emission-driven, while CMIP5 RCP2.6 is concentration-driven?**

The CMIP5 protocol only requested emission driven simulations for the RCP8.5. Concentration driven experiments were requested for all 4 RCP scenarios (but these experiments are not discussed in this paper). Driving data for the equivalent emission driven scenarios is available for all of them. We've clarified the text to make this clearer.

**Page 1063, lines 14-21, RCP2.6: Since there is no official equivalent emission pathway for RCP2.6, how did you derive CO2 emission for RCP2.6 in the ESE?**

The emission data is available from the same sources as the RCP8.5 (based on the simple model tools used to develop the emission-concentration relationship for the RCPs). This is hopefully now clarified in the text, in response to the previous comment.

**Section 2.2, last paragraph: This paragraph is not clear to me. First, I do not understand why the historical boundary conditions are different between RCP and SRES. I would think the model starts to diverge when you apply different future pathways, but how these affect the historical boundary conditions.**

Again, this is useful feedback. We've expanded this discussion to make this clearer. Specifically it now includes a fuller discussion of factors which influence differences between SRES and RCP historical datasets.

**Second, you mentioned this is due to current uncertainties in the nature of historical change. I am not sure why this is related to your configurations. I am also not sure why 1945 is chosen, but not a more recent time, such as 2000.**

The previous paragraph has been split in two. The second paragraph now motivates the 1945 split (parallel historical simulations) – it is done to avoid any climate adjustment by the simulations, due to different historical implementations, from contaminating the analysis (which is meant to illustrate historical-future changes).

**Page 1065, lines 2-13: Although the authors pointed out that the approach for examining the role different sources of climate projection uncertainty play is from Hawkins and Sutton (2009) in the caption of Figure 1, it is better to briefly describe how this is performed (also, it is better to mention this in the main text, but not in the caption of Figure 1). Also, the last part of the caption for the second panel in Figure 2 is repetitive.**

We have now added a brief description, within the paragraph in the main manuscript, of what the data is. This now states that the figure shows the relative fraction, for each decade, of the total

variance (across the spread of simulations) explained by variance arising from scenario, model and internal variability, and cites Hawkins and Sutton for detailed description of the methodology. The second comment relates to the last part of Figure 2, that describes the various box and whiskers plots. We found it hard to identify how this could be consolidated while maintaining the information content, and as such we have maintained the original text in this part.

**Page 1065, lines 15-16: I am not sure why this “, as might be inferred at first glance, imply that the emission scenario uncertainty is playing a LARGER role on the timescale”, since Figure 1b shows a SMALL scenario component of the total variance in the next 30 years.**

This is useful feedback. The reviewer is correct. There is only a small scenario component of the total variance in the next 30 years. The point the text was trying to make was that the suggestion that there is any appreciable contribution from scenario uncertainty in this period is new, given that scenario contribution in this period is conventionally thought to be negligible. This did not come across clearly in the original text. We have worked hard to clarify this – linking this to differences in representation of historical driving data (SRES and RCP). The point we are making here is that differences in current historical datasets does have a small but appreciable impact on near future projections.

**Section 3.1, last paragraph: I also agreed with Reviewer#1 that this discussion of the increasing spread in temperature with increasing RCP is of any real value. For a given spread in climate sensitivity, the temperature spread will surely increase with increasing RCP.**

We fully agree with both reviewers. The mechanism is trivial (larger deviations from current CO<sub>2</sub> levels lead to larger spread in responses). The point we want to make is that despite the apparent larger spread in future projection spread, adopting a lower emission trajectory is still effective in reducing future projection uncertainty (for precisely the reason the reviewers identify). This whole paragraph has been reworked so this is clearer.

**Page 1067, Lowe et al. (2009), lines 10-14: So what is the physical explanation behind Lowe et al. (2009) finding, and is the physical mechanism identified in Lowe et al. (2009) included in the ESE used here?**

**[TO DO]**

**Page 1067, lines 22-25: “a small number of models” any references on this?**

The text now cites Figure 2 (where the high response of these small number of models can be seen).

**Page 1067, line 9: a brief description of SRES A2?**

The A2 SRES scenario is now introduced earlier in the revised “CMIP5 emission driven simulations” Section – with a reference to Nakicenovics 2000. We have also highlighted in the discussion highlighted by the reviewer here that C4MIP simulations were carried out under the A2 scenario.

**Page 1067, lines 14-15: not sure why you need to include the information of A2 to inform the A1B range.**

This discussion is not about business as usual projections (e.g. A2 or A1B). Instead it discusses coupled climate carbon cycle responses to future aggressive cuts in emissions (such as shown by the ESE responses to RCP2.6 or the HadCM3LC response discussed in Lowe et al, 2009). There is a suggestion that simple climate model tools calibrated against business as usual coupled climate carbon cycle models, such as those illustrated in Meinshausen 2011, show a different response under aggressive mitigation to the coupled climate carbon cycle models presented here.

**Page 1056, line 19: legecy -> legacy**

Thanks. Now corrected.

**Page 1056, lines 19-21: “concentration driven” appeared twice in that one sentence**  
Changed.

**Page 1056, lines 21-24: The sentence of “Our ensemble . . .” is not clear, and needs clarification**

This sentence has be split in two and reworded to clarify.

**Page 1058, lines 3-5: this sentence is not clear to me**

We have clarified the previous sentence, expanded this one and added an additional sentence contrasting this with computationally faster climate model tools, to address this.

**Page 1062, line 20: “the the” → “the”**

Done.

**Page 1063, line 20: “timeseries” → “time series”**

Done

**Page 1068, line 27: “Section Sections” → “Sections”**

Done

**Page 1069, line 25: “6.13 relative to #”?**

Done

**Page 1070, line 11: “use” → “us”**

Done

**Page 1071, line 19: “that that” →”that”**

done

**Page 1071, line 3: though → through**

done

**Page 1068, line 2: “sumulated” → “simulated”**

done