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Interactive comment on "An efficient method to generate a perturbed parameter ensemble of a fully coupled AOGCM without flux-adjustment" *by* P. J. Irvine et al.

P. J. Irvine et al.

p.j.irvine@gmail.com

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"Perturbed physics ensemble (PPE) is a widely used to tool to assess and understand uncertainty in climate models that arise due to the somewhat arbitrary choices of uncertain parameter setting in the phase of tuning climate models. A particular problem in PPE-based studies is that by randomly perturbing model parameters, one frequently ends up with models that would not be deemed acceptable representations of the Earth. This is particularly problematic for coupled ocean-atmosphere models, where no prescribed sea surface temperatures or flux-corrections help the models stay close to reality.

In this study an effective, simple and straightforward methodology to construct a PPE of coupled ocean-atmosphere models is presented. The study builds on the Gregory et al. (2004) method to estimate equilibrium temperature based on regression between TOA radiation imbalance and global mean surface temperature. The method in many ways resemble how typical standard coupled models are actually tuned (Mauritsen et al. 2012), and in terms of its simplicity stands in stark contrast to the current emphasis on model emulators (e.g. Rougier and Sexton 2007, Shiogama et al. 2012). Unfortunately, the presentation is long, highly repetitive and the bulk of the text serves relatively little new findings. Make no mistake, I think the method is brilliant and it certainly deserves attention, but there must be substantial revisions made to the text and figures in order to be acceptable for publication. Below are some suggestions, which by no means are exhaustive of the potential improvements that could be made to the manuscript."

- We thank reviewer #1 for their efforts to point out the shortcomings in the presentation of this study. We have made major revisions to sharpen the text, avoid repetition and remove unnecessary figures and discussion.

"Major comments

1) The text is dominated by announcements of what is to come, and repetitions of things that have been found. For example, sections 3.6 and 4 could be dropped (6 pages) without significant loss of substance. I would strongly recommend to shorten and focus the text on the main idea, and that could easily be done with considerably less text and fewer figures."

- As the reviewer suggests we have cut section 3.6 from the paper and removed a lot of repetition from the results and discussion sections. We retain section 4, the discussion section, but it has changed substantially from the original and discusses a number of major issues that are not addressed in the updated results section.

"2) Most of the argumentation around the high-sensitivity model is speculative. Just because there is a correlation between climate sensitivity tropopause-level water vaporin the ensemble, it does not automatically mean it is the cause. I would suggest to either carry out a feedback analysis to show this, or to tone down and shorten this part."

- The paper now has a lot less material on the correlation between climate sensitivity and upper atmosphere humidity; however we have not removed this from the study. We have changed the emphasis of our analysis to focus on the non-linear nature of the member with high climate sensitivity and high upper atmosphere humidity. We have now emphasized the work of previous authors on the links between high altitude humidity and climate sensitivity in the text but we use more cautious language in our analysis and discussion of this correlation. We have added some discussion of the implications of such non-linear climate responses on other estimates of climate sensitivity made with PPEs of HadCM3.

"3) In a number of places the PPE is compared with the CMIP3 ensemble for 'plausibility'. If the goal is to have the PPE represent the CMIP3 ensemble I would try to use different words, such as 'representability of the multi-model ensemble'. If a model is plausible, I would think of it as representable of the real Earth."

- This is a valuable point and was also raised by reviewer #2 and so we have changed the basis of our evaluation of the PPE to the ERA-40 reanalysis for the period 1961 – 1990. CMIP3 is now used only to give context to the anomalies between the PPE and the ERA-40 data. Figures 4 and 5 and supplementary table 1 now include ERA-40 data.

"4) I had a hard time understanding why the authors spend so much time up front on rejection/selection criteria. I would think that given the simple choice of accepting anything within +/- 1 K from the observed, carry through the ensemble runs and analysis, then given the results evaluate the representability of the models. I would think that having as wide an ensemble as possible/practical is useful for statistical studies, not by artificially limiting the ensemble to yield desired results."

- We have reduced the up-front discussion of rejection/selection criteria (section 1.2 in

the original) and have updated the evaluation of the PPE by comparing it to the ERA-40 dataset. We judge that all members produce plausible pre-industrial climates but indicate a number of shortcomings of some members.

"5) A couple of times the authors claim to have the first PPE of coupled models that are not flux-corrected. This is simply not true (e.g. Vellinga and Wu 2008, Shiogama et al. 2012)."

- We acknowledge this error and have changed the text accordingly.

"6) Models do not necessarily conserve energy, which is however implicitly assumed by the methodology. Some models generate energy, i.e. have artificial sources of heat, while most models leak energy. In the former case one will underestimate equilibrium temperature, and vice-versa. There is an easy way to deal with model energy leakage presented by Mauritsen et al. (2012), their equation 1. Judging from the material HadCM3 seems to possibly have a small but negligible artificial energy source, however, to be generally applicable to other models I would suggest extending the methodology."

- This was a very valuable insight from the reviewer and we investigated this effect in our PPE. We found that the standard version of HadCM3 is indeed close to, but it is not in perfect radiative equilibrium, showing a radiative imbalance of only -0.13 Wm-2. This can be compared to the much larger radiative imbalances found in other CMIP3 and CMIP5 models of up to 4 Wm-2, as shown by Mauritsen et al. (2012). We believe the only change to our methodology that is needed is to replace the absolute radiative imbalance with the anomaly of radiative imbalance, to allow it to be used in other models. This allows the cancellation of the energy source or sink term, assuming that the energy source or sink is unaffected by the parameter perturbations. We have made changes to this effect in section 2.3 of the methodology.

- We further note that the approach that Mauritsen et al. (2012) use to estimate the persistent radiative imbalance of the CMIP3 and CMIP5 models (figure 4 in their paper) cannot be applied to models that are not in equilibrium so we cannot test whether the PPE members have the same energy source term as the standard model. However, additional analysis suggested that those PPE members which remained close to the standard model's conditions over the course of the pre-industrial experiment had a similar persistent radiative imbalance providing some confidence that the energy source term has remained unchanged despite the parameter perturbations. The supplementary PDF shows the evolution of TOA radiative imbalance (Wm-2) and SAT over the last 100 years of the pre-industrial control for all PPE members.

"7) As the 1%/year simulations are used very little, I would suggest skipping them altogether in order to save space. On a side note, I would have been much more interested in historical runs, as these offer a means to compare the coupled model PPE with reality."

- We have removed the 1% per year simulations from the study as suggested but did not add additional historical runs due to resource limitations and a lack of easily accessible boundary conditions.

"8) Figures are poor, some close to unreadable, please go over all figures, labels and captions. Legends could further be useful to explain the many symbols and colors. For example: - Figure 1 has funny blue horizontal lines, and the x-axis of panel b) seems to be shifted. - Most figures are too small in print. In the worst case I had to use the computer to zoom in on individual panels of - - Figure 12 in order to read the labels. - A number of figures are redundant or are used very little. - Figure 12 has disordered panels. - Figure 13 could be replaced by a Gregory-plot (e.g. as in Stevens et al. 2013, Figure 18). This would also help emphasize the near-runaway warming of one model. - Figure 14 does not show all models."

- We agree with the reviewer's assessment on the number of figures and have cut many from the study, reducing the number from 15 to 9, and have either removed the relevant text from the results section or summarized the findings in the text without reference to

a figure. All figures are now limited to 4 panels which will increase their individual size and clarity. A number of changes were made to clarify or tidy up the figures including removing the blue lines from figure 1.

- We thank the reviewer for the suggestion to include Gregory-style plots as this helped to highlight the non-linear nature of the climate response of the warmest member and illustrate some issues of estimating climate sensitivity using the Gregory method. Figures 8 and 9 include these Gregory-style plots.

"Minor comments Pressure should given in SI units of hPa, not millibars"

- hPa is now used throughout the text and figures.

"TOA radiation imbalance is frequently named 'Forcing'. It is preferable if the latter term is reserved to externally imposed changes in the boundary conditions, such as CO2 or solar irradiance."

- This correction has been made in the text and figures.

"Page 846, comparing a 66 percent interval with a 90 percent likelihood interval is misleading."

- This section has been removed from the text.

"Page 848, I suppose that by entrainment rate, the authors mean the lateral entrainment rate from the environment into convective clouds. There is also something called cloudtop entrainment, which is a different process. [see Mauritsen 2012]"

- We have clarified that we are referring to the lateral entrainment rate into convective clouds.

"Page 849 line 7, 'sea-ice minimum albedo' could be a better choice."

- We have made this change.

"Page 864 line 13, HadCM3 I suppose."

- This section has been removed from the text.

"Page 870, most of what is stated under ‘Future work’ is not really that."

- We have cut this section from the study.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/6/C978/2013/gmdd-6-C978-2013-supplement.pdf

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Interactive comment on Geosci. Model Dev. Discuss., 6, 841, 2013.