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Geoscientific
Model Development
Discussions

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.

Anonymous Referee #1

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Review of 'An efficient method to generate a perturbed parameter ensemble of a fully coupled AOGCM without flux-adjustment' by Irvine et al. for consideration in Geoscientific Model Development.

Perturbed physics ensemble (PPE) is a widely used to tool to assess and understand uncertainty in climate models that arrise 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.

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.

2) Most of the argumentation around the high-sensitivity model is speculative. Just because there is a correlation between climate sensitivity tropopause-level water vapor in 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.

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.

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.

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).

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.

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.

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.

Minor comments

Pressure should given in SI units of hPa, not millibars

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.

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

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 cloud-top entrainment, which is a different process.

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

Page 864 line 13, HadCM3 I suppose.

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

References:

Gregory et al. 2004: A new method for diagnosing radiative forcing and climate sensitivity, Geophys. Res. Lett., 31, L03205, doi:10.1029/2003GL018747.

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Shiogama, et al. 2012: Perturbed physics ensemble using the MIROC5 coupled atmosphere–ocean GCM without ïňĆux corrections: experimental design and results. Clim Dyn (2012) 39:3041–3056 DOI 10.1007/s00382-012-1441-x.

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Vellinga and Wu 2008: Relations between northward ocean and atmosphere energy transports in a coupled climate model. J. Clim. 21, 561-575 ÂăÂăDOI: 10.1175/2007JCLI1754.1

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