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Ocean Science Discussions

Interactive comment on "The low-resolution CCSM2 revisited: new adjustments and a present-day control run" *by* M. Prange

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Editor (Stephen Griffies) comments on draft 1 of "CCSM2 Revisited" by Prange.

This manuscript provides a thorough presentation of a retuned CCSM2 coupled climate model. These coarse resolution models are notoriously difficult to run stably for order 100s of years. The efforts presented here are but one of those in the community aimed at developing a realistic and relatively cost effective coupled model for use in paleoclimate simulations.

As tough as it is to develop models such as this, it is often tougher to get the details of the development documented in the peer-reviewed literature. I applaud Prange for his efforts to do just this. I believe journals should encourage such papers to be submitted

so to better appreciate the relevance of climate science resulting from the increasingly sophisticated model tools.

Even so, the present manuscript needs a thorough revision to address all of the comments made from the reviewers. I strongly recommend the author to spend some time revising the document to address the concerns raised by the reviewers.

In particular, at the start of the paper it is important to establish the scientific framework within which this particular model class is to live. What sorts of problems are to be tackled by the model? What are the key metrics that cannot be compromised in order to attack the scientific questions? Provide thorough references throughout to other related efforts in order to place the present work in a broader context. It is important to more completely compare what is done in the present manuscript with the analogous effort from Yeager etal (2006). The 20% cost savings provided by the "flux adjusted" method proposed in the present manuscript, versus an improved atmosphere used by Yeager etal (2006), is not motivation enough for this editor, nor I believe for many others. But as another model within a relatively small class of coarse coupled models, it may be worthy of a place in this suite of models. It is your job to argue such in this paper.

Manuscripts that focus on model development are fine for the peer-review, so long as they satisfy the following criteria (some of which are well satisfied by the present manuscript, but most need to be carefully kept in mind with the rewrite).

A/ The manuscript should pedogogically document in a clearly written and thoroughly referenced manner the fundamentals of the model being constructed. Jargon usage should be supported by references.

B/ Motivate why the reader should care about this model. What other research is being done with the model that concerns the scientific community? What is the state-of-the-art presently?

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C/ It should rationalize the decisions made during the development. Why were changes made? What scientific, numerical, mathematical, or computational motivation was used?

D/ If something is done solely "to get the model to run", then say so, and say so in a candid and clear manner, exposing possible undersides to the model.

Now for some detailed comments for the revisions.

1/ If something is important enough to mention, then it is critical to provide a full suite of primary references for the reader. Pointing the reader to the CCSM web site is insufficient for the discussion of model components and parameterizations at the start of Section 2. Additionally, there are lots of specialized terms used here, again made without references (e.g., "Bryan-Cox type", "sigma coordinates", "spectal dynamical core", etc). The nonspecialist will have no idea what is being said here. So please add a few sentences and many references for the interested reader to have a sense for what is being said.

2/ The MOC is the key metric of focus in this paper. The introduction must devote some energy to motivating this metric. In particular, why should an enhanced vertical diffusivity, which strengthens the MOC but also weakens ENSO (Meehl etal), be chosen for this model?

3/ The fresh water adjustment is indeed an unfortunate aspect of this model. But as the author notes, other so-called non-flux adjusted models (e.g., CCSM1) actually employed an implicit adjstument by removing river input to the Arctic. Nonetheless, it is important to provide a sense for the strength of the adjustment being used. The 0.107Sv noted on page 1302 should be compared to river input in the Arctic, ice melt, and precipitation in order to gauge its size relative to physical sources of water.

4/ Speculate on why it was sufficient to only adjust the water and not the heat.

5/ Figure 4: the caption describes what is done in red and blue areas. Mention should

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be made that nothing is done in green area.

6/ Figure 6: The sense for the difference plots is opposite what should be done. Namely, modelers are most interested in the biases of their simulations relative to observations. Hence, assuming the model started from an estimate of observations, the time series should show

differenceA = model simulation(t) - model simulation (t=0)

Instead, what is shown is

differenceB = model simulation(t) - model simulation (t=400years)

differenceB is of no interest.

7/ For all of the overturning plots, the effects of Gent-McWilliams should be included (Figures 7 and 9).

8/ Figures 11 and 12: One of the two columns should show the biases of the model relative to Levitus (i.e., model - Levitus). The reader should not be asked to perform this difference calculation by eye.

9/ figure 13: Reference should be made to some observational sea-ice thickness maps. Perhaps you should show such information on the maps.

Interactive comment on Ocean Sci. Discuss., 3, 1293, 2006.