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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

M. Prange

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I am very grateful to Steven Griffies and the anonymous referees for their very constructive comments. Taking all their helpful suggestions into account, the manuscript has been revised thoroughly (the revised manuscript has been resubmitted). The major bones of contention were that there was no clear motivation why the reader should care about the model, the manuscript was written too technical, and referencing was not thorough enough. To eliminate these weaknesses of the manuscript, the Discussion section has been enlarged in order to "put more science" into the paper and to make clear why this model could be an interesting alternative to, e.g., CCSM3/T31 for many applications. Referencing has strongly been improved by including 45 new references.

Detailed response to the Editor:

Editor: "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."

–> The introduction has completely been revised. It starts now with a comprehensive overview of the role of the Atlantic meridional overturning circulation (AMOC) and its variability in shaping the climate of the earth. Several paleoclimatic examples are given where variations in the AMOC triggered large-scale or even global climate shifts. I tried to make clear that the AMOC is a key metric that cannot be compromised in a model that is designed for paleoclimatic purposes. Many references have been included to underpin my argument.

Editor: "It is important to more completely compare what is done in the present manuscript with the analogous effort from Yeager etal (2006)."

–> Yeager et al. (2006) use CCSM3 rather than CCSM2. The model upgrade involves changes in every model component – changes which were not documented in detail by Yeager et al (2006). It is therefore extremely difficult, if not impossible, to compare Yeager et al's efforts with mine. For clarification a paragraph is included in the introduction: "It is basically unclear why CCSM3/T31 simulates a robust AMOC, and why CCSM2/T31 does not. The application of a different model grid in the ocean component might play a role (cf. Yeager et al., 2006), but any change in the other model components of CCSM (atmosphere, land or sea ice) could contribute to the improvement of the AMOC simulation as well (albeit more indirectly)." In fact, CCSM2/T31x3a is another model than CCSM3/T31. The control climates of the two models show very different behaviours in many regions. This is the new aspect in the revised manuscript (see below). This is summarized in the revised abstract: "Examples are shown in which CCSM2/T31x3a has a better simulation skill than CCSM3/T31 (e.g. simulation of North Atlantic hydrography, West African monsoon). On the other hand, CCSM3/T31

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produces a stronger and more realistic sea surface temperature variability in the tropical Pacific than CCSM2/T31x3a. It is argued that no one model is best for all climatic variables. Depending on the phenomenon under investigation and its geographical location, CCSM2/T31x3a may be a reasonable alternative to CCSM3/T31."

Editor: "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."

–> The 20% cost saving argument has now been removed from the abstract and the conclusions (although I kept a little remark at the end of the Discussion section). Instead, a new section has been included in the discussion (Section 5.3). Here, examples are shown where CCSM2/T31x3a has a better simulation skill than CCSM3/T31 (North Atlantic hydrography, West African monsoon). It is argued that no one model is best for all climatic variables. Depending on the phenomenon under investigation and its geographical location, CCSM2/T31x3a may be superior to CCSM3/T31 or vice versa. The conclusions have been modified accordingly.

Editor: "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).

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."

–> The model description in Chapter 2 has been revised. The fundamentals of the model are documented now in a thoroughly referenced manner.

Editor: "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?"

–> The new Section 5.3 shows examples in which the model has a better simulation skill than CCSM3/T31. I tried to convince the reader that CCSM2/T31x3a can be a reasonable alternative to CCSM3/T31, depending on the phenomenon under investigation. In fact, the simulation of the West African monsoon is extremely well in CCSM2/T31x3a. The summer migration of the tropical rainbelt onto the West African continent is well captured by the model. Note that a recent analysis of 18 state-of-the-art CGCMs uncovered that eight of these models were not capable to reproduce the summer migration of the tropical rain belt onto the African continent (Cook and Vizy, 2006, J. Climate).

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

–> The changes which have been applied to the default version of CCSM2 are described in detail in Chapter 2. The motivation for these changes is clearly pointed out in the manuscript: Boosting the AMOC.

Editor: "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."

–> The model description in Chapter 2 has been revised. Many references have been

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added. Text and/or references have been included to explain the specialized terms.

Editor: "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?"

–> The introduction has been completely revised. It starts now with a comprehensive overview of the role of the Atlantic meridional overturning circulation (AMOC) and its variability in shaping the climate of the planet (see above). An additional section (Section 5.2) has been included in which potential "side-effects" of the AMOC tuning are analysed. It turns out that the effects of enhanced mixing on ENSO and the ACC are negligible in this study.

Editor: "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."

–> Some additional information have been included in Section 4.1: "In this stable climatic mode, the northern high-latitude freshwater flux correction totals 0.107 Sv (averaged over the last 100 years of the integration period); 69% (i.e. 0.074 Sv) of this amount is due to river runoff, while 31% (i.e. 0.034 Sv) is due to precipitation over the ocean. For comparison: Actual climatological river discharge into the Arctic Ocean is about 0.1 Sv (e.g. Prange and Gerdes, 2006)."

Editor: "Speculate on why it was sufficient to only adjust the water and not the heat."

–> In principle, heat flux adjustments could have led to a further improvement of the simulated climate. However, heat flux adjustments are much more problematic than

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freshwater flux adjustments due to the non-linear dependence of heat fluxes on SST and sea ice. A corresponding remark was added in the Conclusions section: "It is also worth noting that CCSM2/T31x3a does not rely on wind-stress correction to produce ENSO variability, unlike other coarse-resolution coupled climate models (e.g. Mikolajewicz et al., 2007). Heat flux adjustment is not implemented either. The absence of air-sea heat flux adjustment avoids distortion in the non-linear dependence of heat fluxes on SST and sea ice."

Editor: "Figure 4: the caption describes what is done in red and blue areas. Mention should be made that nothing is done in green area."

–> I added a remark to the caption of Figure 5 (formerly Figure 4): "All other parts of the world ocean (yellow area) are not affected by the flux adjustment."

Editor: "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."

–> This figure was actually not designed to focus on the biases relative to observations (the model biases are presented and discussed in detail later in the manuscript). The figure was rather designed to show to which extent the simulation has reached equilibrium after 400 years of integration. Nevertheless, one can easily calculate the bias of a quantity at a given time t relative to observations by quantity(t) – quantity(t=0).

Editor: "For all of the overturning plots, the effects of Gent-McWilliams should be in-

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cluded (Figures 7 and 9)."

–> Unfortunately, eddy-induced velocities are not part of the CCSM output files. It is therefore not possible to add this flow to the Eulerian portion with hindsight. However, I added a cautionary note to the caption of Figure 10: "Since only the Eulerian portion of the circulation is shown, a spurious wind-driven overturning cell (so-called Deacon cell) appears in the ACC region (Southern Ocean). As documented by Danabasoglu et al. (1994), there is a substantial cancellation between the Eulerian and bolus velocity terms in the Lagrangian tracer velocity for the ACC region when using the Gent-McWilliams isopycnal scheme as in CCSM2/T31x3a (not shown here)."

Editor: "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."

–> The advantage of showing absolute values (instead of differences) is that dispersion patterns of certain water masses can be recognized more easily. Therefore, I would like to keep these figures in the manuscript. Nevertheless, I agree with the editor that difference plots would also be helpful for the reader. Therefore, such difference plots for surface/subsurface temperature and salinity biases have now been added to the manuscript (new Figures 28 and 29) in the Discussion section. In the abyssal ocean, the biases are so dramatic that they can easily be identified in Figures 13 and 14 without plotting differences.

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

–> Unfortunately, there are not many high-quality observational sea-ice thickness data. Where available, I included some numbers (taken from Strass and Fahrbach, 1998; Harms et al., 2001; Rothrock et al., 2003) into the ice-thickness figures.

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