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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

Anonymous Referee #1

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As the title clearly states, the low resolution (T31x3) version of the NCAR CCSM2 is revisited with the main purpose of maintaining a robust Atlantic Meridional Overturning Circulation (MOC) which does collapse in the original model. The second purpose of the paper is to document the resulting (new) coupled solutions for the present-day climate. To achieve the primary goal of the study, the author takes a rather narrow view, i.e. maintaining MOC, and starts to modify some aspects of the ocean model physics with no or little regard for other issues such as the ACC transport, ENSO variability, effects of flux adjustments in the Pacific basin, etc. Although the manuscript refers to Yeager et al. (2006) for the CCSM3 version of the T31x3 resolution model, it notably neglects to mention that Yeager et al. (2006) has the same goal.

I have a major problem with the flux adjustment used in the present work. The author states in section 5 that this flux adjustment is not a step backwards compared to .... CSM1. The issue is that CSM1 was the model of almost a decade ago. The community has moved to CCSM2 and CCSM3 since then, and the flux adjustment is a thing of the past where it belongs. Incidentally, no flux adjustments were used in Yeager et al. (2006).

A selling point argument is made in favor of this modified CCSM2 model based on a 20% reduction in the computational cost compared to the CCSM3 version. Several additions / modifications to the atmospheric component in CCSM3 are listed as the potential reasons for a slower CCSM3 version. What is neglected here is that these additions actually represent physical improvements. If one does not want to use them, you can just shut them off and reduce the computational expense.

Application of depth acceleration techniques in fully coupled integrations where surface fluxes are highly time dependent is extremely dangerous and should be avoided. Such usage violates some of the underlying assumptions behind such techniques. Also, since the ocean model is non-conservative with depth acceleration, it is unclear how much of the ocean drift is due to this non-conservation vs. the surface fluxes and the top of the atmosphere imbalance.

Among others, the author makes two parameter changes, again with the sole purpose of improving the model's MOC behavior. The first is an increase of the isopycnal and thickness diffusivities. It is well-known that increasing thickness diffusivity leads to reduced ACC transports. So this is probably the primary reason for the reduction of this transport in the present study to well below the observational estimates. The second is the change in the vertical diffusivities. This change can substantially affect ENSO variability and the deep ocean behavior. Again, the latter is largely masked due to the depth acceleration used.

Based on the above concerns and the fact that there is already an alternative that relies on no flux adjustments, I recommend that the paper should be rejected.

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Interactive Comment

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