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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 Referee #1:

Referee: "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."

–> Indeed, the major goal was to maintain a robust AMOC. Potential "side-effects" of the AMOC tuning have been largely ignored in the first version of the manuscript. In the revised version, an additional section (Section 5.2) has been included to discuss the effects of enhanced mixing on ENSO and the ACC. The analysis reveals that the mixing parameters applied in this study have no significant impact on ENSO and the ACC.

Referee: "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."

–> The work by Yeager et al. (2006) is cited in the manuscript wherever possible. However, the model upgrade to CCSM3 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)."

Referee: "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

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is a thing of the past where it belongs. Incidentally, no flux adjustments were used in Yeager et al. (2006)."

–> This very important particular advantage of CCSM3/T31 (i.e. no flux-adjustment) is clearly stressed in the manuscript. However, this does not necessarily mean that CCSM2/T31x3a should be consigned to history. One of the main conclusions in the revised version of the manuscript is that there are several applications where CCSM2/T31x3a performs better than CCSM3/T31, depending on the phenomenon under investigation and its geographical location. The new Section 5.3 shows two examples (North Atlantic hydrography, West African monsoon) in which CCSM2/T31x3a has a better simulation skill than CCSM3/T31. However, it is also argued in the conclusions that no one model is best for all climatic variables.

Referee: "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."

–> The "selling point argument" has now been removed from the Abstract and the Conclusions section (although I kept a little remark at the end of the discussion section). The new "selling point" is that, depending on the phenomenon under investigation and its geographical location, CCSM2/T31x3a may perform better than CCSM3/T31 (or vice versa).

Moreover, I would like to add that – contrary to what the referee claims – the additions in the newer atmosphere model cannot simply be shut off by any user, because they are hardwired in the model code, well distributed over a bunch of subroutines and not documented at all.

Referee: "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."

–> This is a long and old debate and – as it is clearly pointed out in the paper – special care has to be taken when such an asynchronous integration technique is applied. In this study, the integration scheme is very similar to the one used by Danabasoglu (2004) who found that acceleration-induced errors in deep-ocean potential temperature and salinity are of order 0.1 K and 0.1 psu, respectively. These numbers are tolerable in a global, coupled climate simulation (where biases by all other model flaws are typically an order of magnitude larger). Previous modelling studies have demonstrated the ability of deep-ocean acceleration to reach an equilibrium climatic solution (e.g., Danabasoglu et al., 1996; Wang, 2001; Danabasoglu, 2004; Huber and Nof, 2006). It is important to realize that the 100-year synchronous extension (which, by the way, has been further extended now to 200 years without observing any drift in the solution) supports the stability of the climatic equilibrium. At the end of Section 4.1 it is written: "It is important to note that the time series of oceanic volume transports provide a hint on the stability and reliability of the accelerated equilibrium solution (cf. Peltier and Solheim, 2004). Large-scale volume transports (like the meridional overturning circulation or the ACC) quickly respond to changes in the forcing, generally adjusting within a few decades (e.g. Gerdes and Koeberle, 1995; Danabasoglu et al., 1996). If the accelerated integration led to a 'false equilibrium', a rapid reorganisation of the oceanic volume transports would be expected after switching from accelerated to synchronous integration at year 300 (which is obviously not the case)."

Referee: "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

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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."

–> Indeed, potential "side-effects" of the AMOC tuning have been largely ignored in the first version of the manuscript. In the revised version, an additional section (Section 5.2) has been included to discuss the effects of enhanced mixing on ENSO and the ACC (by comparing the CCSM2/T31x3a output with model output from the default CCSM2/T31 version with lower mixing). The analysis reveals that the mixing parameters applied in this study have no significant effect on ENSO and the ACC.

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