Ocean Sci. Discuss., 3, S527–S529, 2006
www.ocean-sci-discuss.net/3/S527/2006/

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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Received and published: 29 September 2006

The main concern of referee #1 is that there exists "an alternative" to CCSM2/T31x3a (namely CCSM3/T31) that "relies on no flux adjustments". I would like to comment on this argumentation:

1) Today, there exists a large number of different climate models, so there is always "an alternative" to a specific model. This variety of models is of paramount importance for the reliability of climate predictions. Only a wide variety of models can yield something like a general consensus regarding climate changes in the past and in the future.

2) The main outcome of all model intercomparisons (CMIP, ENSIP, PMIP, etc.) has been: No one model is best for all climatic variables. This holds also true when comparing CCSM2/T31x3a with CCSM3/T31. There are plenty of examples where

CCSM2/T31x3a performs better than CCSM3/T31 (e.g., water mass characteristics of the Indonesian throughflow, precipitation in the eastern tropical Pacific, upper-ocean salinities in the northern North Atlantic, ...). Moreover, CCSM2/T31x3a's maximum in North Atlantic (north of 55N) meridional overturning of 8 Sv is not reached by CCSM3/T31. Even though a model intercomparison is far beyond the scope of the paper, some examples could be included in a revised version. Please note that I do NOT claim that CCSM2/T31x3a is the better model. CCSM3/T31 includes several improvements in the physics of the atmosphere model (as is clearly stated in the paper) and does not rely on flux adjustments (this is also clearly stated in the paper). Nevertheless, depending on the phenomenon under investigation and its geographical location, there are several applications conceivable where CCSM2/T31x3a might perform better than CCSM3/T31. As is stated in the manuscript, the individual researcher has to decide whether the adjustments applied to CCSM2/T31x3a are acceptable for her/his SPECIFIC application.

3) Last but not least, CCSM2/T31x3a has already been applied in paleoclimatic studies (and more studies will follow). It is therefore necessary to document the model and its performance in detail as an integral part of the scientific process (note that a CCSM2/T31 control run has never been published before!). It is also important to note that the original version of the model (with collapsed Atlantic meridional overturning) has been utilized in a bunch of published papers (Yoshimori et al., 2005, 2006; Raible et al., 2005, 2006). Therefore, this OS paper could also be considered as an important reminder that the results of these studies should be interpreted with caution.

In sum, a 20% reduction in computational costs should not be considered as THE "selling point". Both the abstract and the conclusions should be modified for clarification.

Remarks to the other points made by referee #1:

The referee points out that the higher runtime of CCSM3 compared to CCSM2 is due to physical improvements. This is correct and clearly stated in the introduction of the

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paper. However, contrary to what the referee claims, these additions cannot simply be "shut off" to reduce the computational expense, because they are hardwired in the atmospheric model code, well distributed over a bunch of subroutines and not documented at all. Removing these additions from the model code would require several weeks (if not months) of digging around in the source code - time which is usually not at a (paleo-)climate researcher's disposal.

The referee criticises the employment of deep-ocean acceleration. This is a long 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). Note 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.

As to the modified mixing parameters, the referee's comment is very constructive. Note however: Given the large uncertainty of vertical and horizontal mixing in the real ocean, the parameters applied to CCSM2/T31x3a are still in a "realistic range". Nevertheless, in a revised version of the paper, it would indeed be instructive to analyze the effect of the higher thickness diffusivity on the ACC transport by comparing the results with the original parameter setup. A significant effect of the modified vertical diffusivity on ENSO variability, however, has not been found.

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