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Ocean Science Discussions

Interactive comment on "Formulation of an ocean model for global climate simulations" *by* S. M. Griffies et al.

Anonymous Referee #3

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General comments:

In my view, this is a very useful paper. It is rare to have the choices going into construction of an ocean climate model and the rationale presented in such detail. Further, the authors are to be commended on their frank discussion of some at hoc choices which had to be made to ready the GFDL CM2.0 and 2.1 in reasonable time. This paper is all the more relevant as GFDL CM2.0 and 2.1 are showing to be among the strongest overall performers versus the observations in the rather large field of models contributed to the IPCC AR4.

Specific comments (in page order):

Page 176, lines 15-20: Complex topography in strong flow regions in such ocean models is often associated with numerical noise and, sometimes, numerical instability, and so may require some artificial smoothing. Were such problems a consideration in the topography modifications here?

Page 177, lines 19-23: As acknowledged by the authors, the lack of understanding of the reasons for the ineffectiveness of the sigma diffusion scheme is not entirely satisfying. Any update in understanding? If not, I feel that the discussion on lines 5-24 could be made more succinct.

Page 179, lines 20-25: I'd be interested to see a little more on just how well the new tracer advection scheme "preserves shapes in three dimensions", in particular in comparison to Quicker. There has been comment that Quicker tends to be rather diffusive (though much less so than first order upwind).

Page 183, lines 17-22: I would like to see some more detail of the double diffusion and diffusive convection parameterisation used in the model, or at least a reference to where it is described in detail. The authors had this detail in an earlier draft, but seem to have removed it from the submitted manuscript.

Page 195, lines 6-10: In my opinion, there is no strong physical reason why the neutral diffusivity (AI) should in general be the same as the skew-diffusivity (Agm), as they relate to somewhat different processes. Skew-diffusion is intimately tied to the development of baroclinic eddies, whereas tracer diffusion along neutral surfaces involves passive subgrid-scale advection and mixing on the neutral surface, simply requiring some kind of eddying field to be present.

Page 195, lines 22-25: "Tapering also occurs Ě proximity to the ocean surface" - does this mean that the authors also include a prescribed z-dependent tapering in the upper ocean?

Page 196, lines 19-25: I'm comfortable with the authors' rationale for choosing an Smax of 1/500, however the results in Fig. 13 seems to show a more realistic profile of mixed layer depth in the Southern Ocean with the larger Smax of 1/100.

Page 198, lines 21-25: What effect does the reduction of neutral physics to horizontal diffusion near the boundary have on the solution. Does it cause significant unphysical dianeutral diffusion?

Technical corrections:

Page 182, line 26 and page 183, line 1: Replace "equatorial" with "equatorward"

"jettisoned" (page 188, line 3) and "guts" (page 167, line 16) - slang is best avoided in my opinion.

Page 197, equation (10) and page 198, line 1: Perhaps del-z should be written as del-H to indicate more clearly that reference is to a horizontal density gradient.

Interactive comment on Ocean Science Discussions, 2, 165, 2005.

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Interactive Comment