Ocean Science Discussions, 2, S117–S126, 2005
www.ocean-science.net/osd/2/S117/
European Geosciences Union

Ocean Science Discussions

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

S. M. Griffies et al.

Received and published: 2 August 2005

- Section 2.2.1 Tripolar

  Based on the reviewer's comments, we have added the following paragraph to the revised manuscript in hopes of better clarifying our decision to choose a non-spherical grid for the Arctic Ocean.

  > There is a relatively mature literature detailing methods for removing the spherical coordinate singularity from the Arctic Ocean. Papers by [Deleersnijder et al.(1993)], [Coward et al.(1994)], [Eby and Holloway(1994)], [Smith et al.(1995)], [Murray(1996)], [Madec and Imbard(1996)], [Bentsen et al.(1999)], [Murray and Reason(2002)], [Marsland et al.(2003)], and [Roberts et al.(2005)] provide various options, present simulation comparisons, and detail various coordinate choices. Our conclusion

from this literature is that generalized orthogonal grids are of use for our global modelling efforts.

The reviewer suggests that further motivation for going non-spherical could be garnered by presenting direct comparisons between simulations with the tripolar ocean and older simulations from GFDL using spherical coordinate models. There are two reasons that we did not present these comparisons.

1. Given the mature literature noted above, the scientific case has been clearly made for the utility of non-spherical grids for Arctic simulations. Further analysis along these lines in the present manuscript would add little to the literature. The present discussion in the manuscript, along with the above added paragraph citing the literature, represents a brief review of these other studies, and this material is presented to explain our motivation for choosing this particular non-spherical grid.
2. Direct comparisons between previous GFDL climate model simulations and the new simulations are very difficult to present in a clean and succinct manner, given the completely different modelling frameworks (e.g., different component models) developed over the past 10 years at GFDL. Any such discussion in the present manuscript would require much background material which would dilute the focus of the present manuscript.

We agree with the reviewer that it is useful to mention the differences in model time step available in OM3 with a spherical grid versus the time step used with the tripolar grid. We have therefore added a discussion of this point near the end of the section. Here, we note that time step is increased by roughly an order of magnitude with the tripolar grid, as compared to an unfiltered spherical grid model.

- Section 2.2.2 and 2.2.3 Grid resolution

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We agree with the reviewer in his comments regarding the compromises that one must make when designing an ocean model grid, both vertical and horizontal. It is a very difficult task to do without some level of intuition and "executive" level decisions. It is certainly frustrating when the limitations of computer power confront the scientific needs of a model design. Nonetheless, we all do the best that is possible given resources.

As detailed in our manuscript, we have indeed been driven in our grid resolution choice by experience garnered from the ENSO modelling community. That community has advised that ocean models should employ grid resolutions sufficient to admit tropical waves, and they have pointed to the importance of refining the vertical grid sufficiently to maintain a tight thermocline [Latif et al.(1998)], [Meehl et al.(2001)], and [Schneider et al.(2003)]. Furthermore, nearly all previous generation climate models performed poorly in the tropics, largely due to inadequate horizontal and vertical resolution.

The present generation of computers at GFDL have sufficient power to run ocean climate models at the resolution previously restricted to our ENSO forecast models. We therefore decided to focus resolution in the tropics in hopes of enhancing the ENSO simulations in the model. We furthermore wished to combine modelling efforts at GFDL so that the climate change model will also be suitable for ENSO predictions.

As detailed in the study of [Wittenberg et al.(2005)], we believe that the present model achieves these goals. The discussions in that study should serve to answer queries from the reviewer regarding the integrity of the model's equatorial currents and ENSO variability.

As the reviewer points out, there are compromises required when focusing resolution in the tropics, and these compromises may affect the integrity of middle and high latitude simulations. We again must answer queries of the reviewer by pointing him to the thorough discussions given in the papers from

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[Gnanadesikan et al.(2005)] and [Russell et al.(2005)]. Here, analyses are presented of water masses, meridional overturning, mode waters, etc.

Regarding sea ice, the issue we raised only has to do with the amount of liquid in the top model grid cell. When running the climate model, we encountered many occasions when the model lost the top model grid cell under sea ice, if we allowed for the full weight of sea ice to be felt by the ocean model. These occasions tended to occur during spin-up phases of the simulations. After a few tests, we chose a conservative setting of 4m maximum pressure from the ice. In retrospect, we suspect that after a sufficiently long spin-up, we could have increased this weight without compromising coupled model stability. Unfortunatley, such was not attempted due to time and resource limitations.

- Section 2.2.4 Bottom Topography

  We agree with the recommendation to allow others to use the bottom topography. As stated in a footnote in the concluding section of the manuscript, the ocean and sea ice configurations of the GFDL climate model are supported by the distribution of MOM4. The bottom topography is part of this distribution. The reader may access this code and test case datasets by going to the following web site

  http://www.gfdl.noaa.gov/fms/

  As suggested by the reviewer, the small effects from the overflow scheme in our simulations may have something to do with the tracer advection scheme. However, we believe there are other facets of the parameterization that may prove more important. We comment further on these points in response to Reviewer 1.

- 2.2.5 Equation of State

  We detail the “limitations” of the older approach in the manuscript. One limitation is related to the pre-calculation of the equation of state coefficients at specified vertical depths. Partial bottom steps, however, make this pre-calculation impractical. Another limitation is related to the narrow range of salinity used by [Bryan and Cox(1972)] to define the polynomial coefficients. This basic approach was used in earlier versions of MOM.

Based on the comment from the reviewer, we have slightly revised the manuscript's discussion of the equation of state with the following change.

> Additionally, the cubic approximation is inaccurate for many regimes of ocean climate modelling, such as wide ranges in salinity associated with rivers and sea ice.

now reads

> Additionally, the cubic approximation typically employed a narrow salinity range, which is inappropriate for many regimes of ocean climate modelling, such as wide ranges in salinity associated with rivers and sea ice.

So in summary, our purpose in this discussion is to highlight the algorithm issues related to the cubic polynomial, and to point to what we believe is a preferred method. Namely, (1) to not use precalculated cubic polynomial coefficients, but instead (2) to use an accurate equation of state recommended by [McDougall et al.(2003)]. Note that [McDougall et al.(2003)] thoroughly document the errors made when using various forms of the equation of state.

- 2.2.6 Tracer Advection

  We have commented on the diffusive nature of the advection scheme in the responses to Reviewers 1 and 3. The additional point raised by Reviewer 2, regarding internal waves, is quite important and some comment is now included in the revisions, with the material quoted in the response to Reviewer 1.

We note that the example described by Reviewer 2, with waves moving density interfaces up and down, was detailed in [Griffies et al.(2000)]. In that paper, we had in mind a seasonal cycle at the equator. But the example still raises the same issues as the reviewer with his comments on gravity waves arising from the diurnal cycle. [Griffies et al.(2000)] showed that central differenced advection, *combined with vertical convection* as in a realistic model, can lead to *more* spurious tracer mixing than third order upwind schemes. This example then provides more motivation for us to not use the central difference schemes. We comment more on this point in the revised manuscript, again as quoted in response to Reviewer 1.

- 2.2.8 Background vertical diffusivities

  We agree with the reviewer–more thorough analysis is warranted. We rely on the manuscript from [Gnanadesikan et al.(2005)] to discuss water mass properties and their relation to observations.

- 2.3.1 Free surface and fresh water

  We agree that the change needed in the models is relatively minor. However, we are unaware of this topic being discussed in the climate modelling literature. Furthermore, the treatment of water forcing in ocean climate models is of particular importance given the focus of climate science on changes in the hydrologic cycle.

  Regarding the reviewer's request to name those models still using virtual tracer fluxes, we prefer to let other modellers document their own methods. Our purpose is instead to highlight this issue, which has not been emphasized in the literature, and to articulate scientific reasons to motivate others to convert their models to real water fluxes.

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

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