Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-297-RC1, 2019

Geoscientific Model Development Discussions

Interactive comment on "MOMSO 1.0 - a near-global, coupled biogeochemical ocean-circulation model configuration with realistic eddy kinetic energy in the Southern Ocean" *by* Heiner Dietze et al.

Anonymous Referee #1

Received and published: 6 February 2019

This paper documents the development of an interesting model configuration designed to allow for an eddying simulation of the Southern Ocean that also includes biogeochemistry. The model represents a series of compromises that mean whilst none of the components are ideal, the whole produces a potentially useful model simulation. The meat of the paper is validation of the physical circulation. This section requires some work, as there is considerable detail missing and a lack of quantification. The closing section of the paper looks at the biogeochemistry in the model's reference run and a short experiment with increased winds. This shows the capability of the model

produce useful biogeochemical results, despite the physical circulation not being fully equilibrated. However, there is also a lack of detail here, which presents the full power of the modelling approach from being illustrated.

Major Comments

page 2, lines 4-9 : This is not an accurate representation of current thinking on the overturning circulation of the Southern Ocean. There is no debate as to whether the bolus overturning due to eddy fluxes of thickness opposes the so-called Deacon cell. It is extremely well established that it does so, to the degree that the Southern Ocean literature does not discuss the Deacon cell and instead talks about Eulerian overturning and bolus overturning. There is an on-going debate regarding the degree to which the bolus overturning cancels out the Eulerian, and that to which their residual responds to forcing changes, particularly with respect to wind stress changes, and model differences. However, that this cancellations takes place is widely accepted. See, e.g., Marshall & Radko (2003), Viebahn & Eden (2010), and Abernathey et al. (2011), etc.

page 2, line 24 onwards : There is at least one example of a fully-equilibrated, both thermodynamically and biogeochemically, model study that the authors could cite here; Munday et al. (2014). Whilst the model in Munday et al. is too coarse to be truly eddy resolving, it does have substantial internal variability and large-scale vortices, which leads to changes in sensitivity to wind stress of the physical circulation consistent with higher resolution models.

Section 2.1 : Also of importance is that, since z* is essentially an extension of the nonlinear free surface method of Campin et al. (2004) to all model levels, it also gives very accurate conservation of tracers.

page 5, lines 4-5 : The Smagorinsky scheme has a single coefficient, usually taken to be in the range 2-4, that allows some control over the viscosity. Please add the coefficient value you chose here. In addition, the issues arising near steep topography suggests that this coefficient was too small. Is there a reason why the authors chose to introduce additional viscosity instead of just increasing the viscosity value? Additionally, some of the issues could result from the use of purely Laplacian viscosity. Given the grid scale variance at the resolution of MOMSO, it is probably appropriate to start applying some biharmonic viscosity/diffusion to the model.

page 5, lines 5-7 : The PPM scheme is a good choice for the advection of temperature/salinity. The choice of advection scheme for the biogeochemistry is also very important (see Levy et al. 2001). Having different schemes for these two choices could have repercussions. Have the authors investigated this?

Section 2.5 : Initialising from a previous stratification/tracer distribution can be a very good way to shorten the spinup of high resolution models. Is there a reason why the temperature and salinity from the same run that produced the biogeochemical tracers wasn't also used?

Section 3.1 : A general criticism of this section is that it is largely qualitative, even when quantitative comparisons could be carried out, e.g. the spatial pattern and values of EKE/SSH variance are available from the altimetry the authors use. A properly quantitative comparison would allow more detail to be drawn out. In addition, the authors highlight the surface velocities and EKE as having "remarkable" comparison to the altimetry. This is hardly remarkable; the surface velocities and EKE look largely as one would expect for a model of $\sim$1/5o grid spacing in the Southern Ocean (see, e.g. Delworth et al., 2012, or Barnier et al., 2006, for examples of how similar resolution ocean models, or ocean components of coupled models, look).

In addition, the deficiencies in particular comparisons are thoroughly glossed over. Something that stood out to me is that the flow is too zonal and there is too much EKE south of Australia. The East Australia Current is also poorly represented and the Agulhas retroflection has too small a region of high EKE. I'd expect the mean flow and eddies south of Australia to be related to the high temperatures south of this region shown in Figure 9.

Most of the verification of the model is carried out on the surface quantities. Whilst this is appropriate given the length of the model run, which means that below the mixed layer there might not have been a great deal of adjustment, it is still useful to consider it briefly as it would place the later biogeochemical validation in context. At the very least, they should look at some temperature/salinity/density/biogeochemical transects across the Southern Ocean and the pycnocline depth. This will likely tie into the noted low value of the ACC transport.

There is no discussion of the mixed layer depth. This is fairly well observed in recent years thanks to Argo floats and would be a key parameter for the exchange of carbon with the atmosphere and for the subduction of biogeochemical tracers from the surface.

Section 3.2 : Unfortunately, this section is completely deficient. There are many ways to calculate overturning (see discussion above about Eulerian vs. residual vs. bolus overturning) and there is no information given here as to what variety of overturning is presented in Figure 23. It should be some measure of the residual circulation, although given the increase in the WIND simulation it looks like it might be the Eulerian overturning. A better comparison would be to calculate the residual overturning in density coordinates and compare the residual overturning, as well as the Eulerian and bolus overturning. This would give a much better sense of how the overturning is changing between the two experiments and how much the remaining drift in REF is affecting their results. If the subsurface stratification is changing, then it will make interpretation more difficult, since the there could be significant diapycnal transformations in the streamfunction. This is another reason to look at the subsurface stratification earlier in the paper.

Minor Comments

page 1, line 20 : The choice of 40oS is not well justified, why not 35oS or 45oS? There are physical aspects of the circulation that could be cut off by this choice, for example the region in which the Agulhas current interacts with the ACC is very close to 40oS.

page 6, line 5-8 : Do the climatological estimates include the seasonal cycle?

Section 3.1.4 : A missing quantitative comparison here is between the flux due to the surface restoring of salinity and the other surface fluxes that contribute to salinity changes.

Figures such as 14 & 15, where the reader is invited to compare the REF and WIND experiments variation with the observed quantity would work much better as a single figure. Preferably as a single panel, with the observations as a third line.

Section 3.1.6 : Based on Figures 19 & 20, the average concentration of both iron and phosphate appears low. Is this because the initial conditions for these fields are biased low? And if so, could the issues raised in this section be improved by simply adding more phosphate & iron?

page 12, line 26 : In what sense to the compiler optimisations break reproducibility? Will the same model year, run on the same machine, be different if the model was rerun? Or will it only change when run on a different machine with different compilers? The first is rather alarming, the second would be very common.

page 13, line 12 : Quantum leap is an overstatement, given the length of the physical model spinup, the still low resolution of the physical model and the documented deficiencies of both physical and biogeochemical system.

Technical Issues

page 2, lines 13-14 & 27 : Work in progress for whom? The authors or the community in general, it could be either!

page 2, line 14 : Put comma after progress and delete comma from after extent.

page 2, line 18 : “As for now we know”?

page 3, line 11 “by chance”, isn’t it more by design?

page 3, line 13 : “allows to test” -> “allows us to test”.

page 4, line 9, page 7, line 10 : Orphan lines like this pop up quite a bit throughout the paper.

page 4, line 19 : 42.429.759 -> 42 429 759

page 5, line 15 : I think the authors mean vicious cycle! (Even though viscous seems appropriate for numerical models)

page 5, line 24 : Galbreith -> Galbraith

page 8, line 5 : Spacial is an accepted alternative to the more common spatial spelling. However, the paper uses both spellings at different places.

page 10, lines 21-24 : Is the reference to “module” meaning a specific part of the numerical code? or an inaccuracy in the overall representation of part of the system?

page 12, line 16 : There’s something wrong with the brackets here.

Figure 23 : noetig? in the caption.

References

Abernathey, R., J. Marshall, and D. Ferreira, 2011: The dependence of Southern Ocean meridional overturning on wind stress. J. Phys. Oceanogr., 41, 2261–2278.

Barnier, B., G. Madec, T. Penduff, J. M. Molines, A. M. Treguier, J. Le Sommer, A. Beckmann, A. Biastoch, C. Boning, J. Dengg, C. Derval, E. Durand, S. Gulev, E. Remy, C. Talandier, S. Theetten, M. Maltrud, J. McClean, and B. de Cuevas, 2006: Impact of partial steps and momentum advection schemes in a global ocean circulation model at eddy-permitting resolution. Ocean Dyn., 56, 543–567.

Campin, J. M., A. Adcroft, C. Hill, and J. Marshall, 2004: Conservation properties in a free-surface model. Ocean Modell., 6, 221–244.

Delworth, T. L., A. Rosati, W. Anderson, A. J. Adcroft, V. Balaji, R. Benson, K. Dixon,

S. M. Griffies, H.-C. Lee, R. C. Pacanowski, G. A. Vecchi, A. T. Wittenberg, F. Zeng, and R. Zhang, 2012: Simulated climate and climate change in the GFDL CM2.5 high-resolution coupled climate model. J. Clim., 25, 2755–2781.

Levy, M., A. Estublier, and G. Madec, 2001: Choice of advection scheme for biogeochemical models. Geophys. Res. Lett., 28, 3725–3728.

Marshall, J. and T. Radko, 2003: Residual-mean solutions for the Antarctic Circumpolar Current and its associated overturning circulation. J. Phys. Oceanogr., 33, 2341–2354.

Munday, D. R., H. L. Johnson, and D. P. Marshall, 2014: Impacts and effects of mesoscale ocean eddies on ocean carbon storage and atmospheric pCO2. Global Biogeochem. Cycles, 28, 877–896, doi:10.1002/2014GB004836.

Viebahn, J. and C. Eden, 2010: Towards the impact of eddies on the response of the Southern Ocean to climate change. Ocean Modell., 34, 150–165.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-297, 2019.