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Interactive comment

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 #2

Received and published: 29 May 2019

General comments: This paper presents a new ocean model configuration, which couples an ecosystem model to an active ocean-sea ice model, with a refined horizontal resolution in the Southern Ocean. The stated aim of this new configuration is to examine the Southern Ocean carbon budget and its sensitivity to wind changes. Although this new model configuration is promising, this paper fails to provide quantitative evidence of the added value of this configuration, and lacks adequate discussion of the applicability and limitations of the technical choices made or the resulting biases.

Most results included are side-by-side plots of surface metrics in the simulation and

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observations. The model evaluation needs to include quantitative metrics and/or plots of differences, and would benefit from comparisons with existing reanalysis products or state estimates. Biases are only briefly (if at all) mentioned and there is generally no attempt to identify (and provide evidence for) the possible causes of these biases. In addition, the manuscript mentions a 'twin' configuration with coarser resolution, but does not include any comparison with that existing configuration (which would significantly improve the manuscript). The objectives of the wind sensitivity experiment are not clearly stated in the introduction, and the limitations of this experiment need to be discussed any time its results are included. This manuscript also needs to include a more detailed discussion about the applicability of the results and the limits of this configuration.

Specific comments:

Abstract: The abstract makes the case that the model 1) combines a carbon inventory and eddy-permitting resolution in a way that outperforms existing models, 2) is suited to sensitivity experiments at decadal timescales, 3) can provide boundary condition to ice-sheet models. This manuscript would benefit from focusing the analysis and figures to demonstrate these 3 points, and from including a discussion section on the limitations of the model with regards to those 3 points.

Introduction: Overall, this section would benefit from a more focused approach, targeting specific dynamics or metrics that will be covered in the body of the paper.

Page 1, Line 20: There is no justification as to why 40S was chosen, or even simply how it relates to the above discussion about the location of the STF. Page 2, 29-31: Please consider how this fits within other work of Southern Ocean response to winds, such as (Spence et al, 2010). Page 3, 14: Where is this comparison between coarse and finer resolution model included? The manuscript would benefit from one. Page 3, 15-22: the discussion of the role of meltwater on ocean circulation or biogeochemistry is out of place here. Given that the current configuration does not include sub-ice-shelf cavities or parameterized meltwater fluxes, it will not be able to account for meltwater feedbacks. At best, this could be included in a discussion section to discuss options for further development. Page 4, line 15: Some justification on why topography needs to be filtered 3 times (even just a qualitative target) is needed here. Page 5, line 8-11: I have some concerns about the increase of viscosity over the Drake Passage. Some justification and possible root causes of this problem over Drake Passage need to be included.

Section 2.3 This section is too short and vague. If a change was made to the standard code and led to different results, please show the impact that this change has made (in an Appendix if necessary).

Section 2.4 Design: please specify “complex-enough” for what? Or the model fidelity in representing what? At least include the target variables and their scientific relevance. Why is the comparison with the coarse-resolution model not included in this paper? This manuscript needs to be self-sufficient.

Section 2.6 Please include the frequency (or time scale) of boundary restoring. Also, the boundary restoring is necessary because the domain does not include dense water formation regions (it is not simply lack of processes). It is unclear what the wind experiment is supposed to address. If it is supposed to mirror the historical trend, please address why the physics are set to a historical trend while the carbon is maintained at a pre-industrial level. The objective of this experiment needs to be set up here, to provide context for the interpretation of results and discussion of their applicability.

Section 3. Why does this section not include any comparison with reanalysis data or state estimates? These datasets are commonly used in the literature, and provide more comprehensive data (sub-surface, time-varying…), which would help make a more compelling case for the fidelity of this new model configuration. As it is, this section appears extremely light and unconvincing.

Section 3.1.1. This section lack qualitative evidence of the model’s ability to represent the ocean circulation metrics mentioned. It is necessary to define the meridional overturning as calculated here. Without a clear definition, the quantitative results are difficult to interpret or compare to other studies.

Section 3.1.2 This section lack qualitative evidence of the model's ability to produce 'realistic' levels of EKE. Given that this is a main point (included in the paper's title), putting plots side-by-side (Fig 8) is far from enough. Please include a plot showing the difference between the two datasets, and a quantification of mean EKE per region to provide some level of quantitative assessment. In addition, some discussion of the results is needed: what are some of the biases present? What may cause these biases? Finally, the conclusion that a 'realistic' level of EKE necessarily equates a good representation of eddy-driven processes is simplistic. Here, the only metric is surface EKE, whereas eddy-driven processes, (including eddy-driven upwelling of nutrient-rich waters) occur over a range of depths. There is a vast body of literature investigating eddy processes in the Southern Ocean and what resolution may be necessary/sufficient to represent them adequately. Some discussion of this literature, and of how this particular model configuration fits in the context of other modeling studies is necessary.

Section 3.1.3 Again, some quantitative comparison between the observation-based datasets and the modeled values is needed (e.g. plot of the difference), as well as a discussion of the biases (especially between the bottom temperature values, which has no discussion of biases at all). Combining Fig 10 and 11 would help the comparison.

Section 3.1.5 A more comprehensive assessment of sea ice would make a more compelling case (e.g. sea ice concentration, sea ice thickness, annual cycle of sea ice area).

Section 3.1.6 This section needs a more comprehensive assessment of the model's performance in representing observed patterns of biogeochemical properties. A comparison to the Biogeochemical Southern Ocean State Estimate (B-SOSE) (Verdy and Mazloff, 2017) would be a good step forward. In addition, the biogeochemical performance of this model configuration should be shown (not just said) to be comparable to the one from the existing coarse resolution model. Likewise, there is little discussion of the possible causes of the biases, and lack of evidence to support the possible causes mentioned.

Section 3.2 It is unclear what this sensitivity experiment is for, and why only the winds were changed. It lacks discussion of the mechanisms leading to the change in overturning circulation, or to the change of bottom water temperature. For the carbon results, it should be specified that the change in the experiment is showing only the impact of the physical adjustment to winds (given that the carbon concentration is maintained to pre-industrial levels).

Section 4: summary and conclusions This section makes qualitative statements about the model's fidelity, which have not been adequately supported by the body of the paper (similar to the abstract). As the model performance with respect to biogeochemistry is said to be similar to the coarser configuration, it is essential to demonstrate the benefits of this configuration with respect to eddy processes. Describing the configuration as a 'quantum leap' in modeling sounds over-reaching, given the lack of quantitative evidence included in the current manuscript to demonstrate the improved performance of this model configuration compared to existing configurations. This section needs to include an in-depth discussion of the relevance and applicability of this model configuration. For example, the time scales examined here are the decadal timescales, while modeling of ice-shelf melt or Antarctic Bottom Water are more relevant to longer time scales.

Technical Corrections:

Page 2, line 10: "effects and affects" Line 12 budged → budget Line13 net-air → net air Line 25 'impinges' → impacts/affects Page 3, line 19: fuel → lead to Page 4, line 6: biogeochemical Page 5, line 23: it's fidelity → its

References: A. Verdy and M. Mazloff, 2017: " A data assimilating model for es-

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timating Southern Ocean biogeochemistry." J. Geophys. Res. Oceans., 122, doi:10.1002/2016JC012650.

Spence, P. , J. C. Fyfe , A. Montenegro , and A. J. Weaver , 2010: Southern Ocean response to strengthening winds in an eddy-permitting global climate model. J. Climate, 23, 5332– 5343, https://doi.org/10.1175/2010JCLI3098.1.

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