Review of Naughten et al. “Intercomparison of Antarctic ice shelf, ocean, and sea ice interactions simulated by two models”

Reviewer: Xylar Asay-Davis

I wish my name to be relayed to the authors, as I do not support the practice of anonymous review.

General comments:

This paper presents a comparison of two sea ice/ocean models with ice-shelf cavities, MetROMS and FESOM. Biases are analyzed in detail and across many regions where observations are available, including a discussion of directions for future model improvements that might reduce these biases. Where direct observations are not available, (e.g. for flow patterns under ice shelves), a comparison is made between models and biases are inferred from more indirect observations (e.g. the observed locations of inflow into and outflow from ice-shelf cavities). The paper is very well written, very clearly organized and makes for a compelling read. I feel it is nearly ready for publication, requiring only a few minor changes.

Perhaps the most significant change I would wish for the authors to consider is the addition of a figure of the rate of sea-ice production from each model, possibly compared with satellite-derived estimates (see my specific comment below). While the figure of mixed-layer depth serves as a proxy for this quantity, I feel like a figure showing sea-ice production would more directly get at the source of biases seen in both models that are inferred to come from too much or too little (or incorrectly located) sea-ice production in different regions.

As an aside, given that it is not (any longer) a requirement from GMD to put the figures and tables at the end of the manuscript, as a reviewer I would have liked to have the figures interspersed with the text for easier reviewing and reference. This is just something to keep in mind for any future submissions to a Copernicus journal.

Specific comments:

p. 2 l. 3 “The rate of retreat of much of the AIS will be governed by the ocean.” While I agree that the ocean will almost certainly play an important role in AIS retreat, there are a couple of reasons I would suggest toning down this statement. First, internal ice dynamics and topography may govern the rate of retreat at least as much if not more than the ocean, with the ocean acting more as a trigger mechanism or an intermittent forcing. Second, we should not discount the potentially important role that atmospheric forcing will likely play in this process (e.g. via surface melting and potential hydrofracture).

p. 3 l. 27-29 “how many features of fluid flow can be represented by a mesh of a certain spacing” and “the number of features resolved”. Flow features are not easily countable in the way this wording implies, and increasing resolution does not typically lead to a binary transition from unresolved to resolved. Instead, there is a messy transition from unresolved through partially resolved to fully resolved. I would suggest something like, “smallest flow features that are captured by a mesh of a certain spacing” and “the smallest resolved feature” for these.

p. 4 l. 4-5 “These differences... dominates” My experience with MISOMIP and my own pan-Antarctic modeling is that eddy-permitting and eddy-resolving simulations do *not* necessarily behave more similarly, and other modeling choices still play an important (if not dominant role) even when eddies are included. For example, even fairly subtle differences in how topography is represented can lead to changes in how eddies are shed or small-scale currents interact with the topography.

p. 4 l. 21-22 and Fig. 5 “Bottom nodes are allowed to deviate from the standard z-levels in order to match the given bathymetry” This is not obvious in Fig. 5. If this is a plotting artifact in Fig. 5, it would be best to fix that so the true nature of how FESOM represents bathymetry is shown in the figure. If not, it is unclear why there are full-cell jumps in the bathymetry in Fig. 5, given that FESOM should have some kind of equivalent of the partial-cell methods used in other z-level models.

p. 4 l. 24-25 “extremely high vertical resolution.” I would suggest avoiding subjective phrases like this that include an implicit value judgement. What might seem today like “extremely high” vertical resolution is likely to become closer to standard resolution in the not-too-distant future. We frequently run global ocean simulations with the Model for Prediction Across Scales Ocean (MPAS-O) including 1 meter vertical resolution in the upper mixed layer.

p. 4 l. 26 “with 30 barotropic timesteps for each baroclinic” This is largely an aside for your future work, not a request for a change in the paper. Did you experiment at all with fewer barotropic time steps per baroclinic? If vertical resolution is controlling the time step, it should follow that the barotropic time step would not be strongly affected and fewer subcycles might be possible.

p. 6 l. 10-11 “very minimal spurious sea ice formation” maybe remove “very” (since it sounds kind of subjective)

p. 6 l. 10-12 I’m kind of confused by this sentence. My understanding was that that the flux-limited advection scheme in Naughten et al. 2017 reduced the spurious sea-ice formation. Do you perhaps mean “comparable to” instead of “compared to”? Then, I would understand a bit better.

p. 7 l. 9-11 It is a little unclear what “no significant impacts on Weddell Sea convection” means in this context. I take it to mean that KPP appears to work just as well as Pacanowski-Philander, at least over the 5 years. If that is the case maybe a comment is warranted here to the effect that this deserves further investigation. I’m a little unclear on what you (or I as the reader) should take from this.

p. 8 l. 5-6: The values of u* need units (presumably m/s)

p. 8 eq. (4): 530, 10^-3 and 10^-8 need units, since they appear to all be dimensional.

p. 10 l. 6-8: “which are not interpolated in time...as they represent total fluxes…” This is a fine approach but there would be ways to interpolate these data in time while preserving the 12-hourly mean.

p. 10 l. 13: “additional surface freshwater flux representing iceberg melt” Could you give a description in a sentence or 2 of what the characteristics are of this climatology? It seems like it comes from an iceberg model, rather than from observations, which is perfectly reasonable but probably deserves a mention.

p. 11 l. 6-7 “...taken from the AVISO climatology...which is a single time record.” If I understand right, you use the annual mean rather than the monthly climatology? (Near as I can tell, both are available from AVISO.)

p. 11 l. 9 “in y” I would probably change this to “in latitude” even though I understand that “y” is not exactly the same as latitude over the whole domain. At the northern boundary, they are presumably the same and I think that would be clearer to the reader.

p. 14 l. 3-4 “In both models RSBW temperatures more or less agree with observations...”. This seems a little generous to me. MetROMS is clearly missing some colder temperatures, while FESOM reaches temperatures that are significantly colder than observed in WOCE. I guess that is what the “more or less” is meant to cover.

p. 14 l. 16 LSSW isn’t really analyzed here. Is this because Fig. 4 doesn’t really have anything to say about this water mass?

p. 15 l. 24: “In FESOM, AAIW is slightly better preserved at low resolution than at high resolution.” Do you care to speculate on why this is?

p. 20 l. 11: “extremely high resolution.” Again, I would recommend a less subjective wording.

p. 21 l. 4 and Fig. 11a: I’m not sure what can be one but I found the figure to be too small to clearly see the features that are being described. When I zoomed in to 400% in the pdf viewer, the figure was kind of pixelated, meaning this didn’t really help.

p. 22 l. 15: “steeper ice draft” I would also suggest that better resolved currents may be the reason.

p. 25 l. 2: “and this process requires resolutions of 1 km or less” This is stated in a couple of places. My experience is that model properties improve significantly even at eddy-permitting resolution (~2-4 km) even when the largest eddies are not fully resolved. My point is that eddy transport is not typically absent in eddy-permitting simulations, it is just diminished from what it would be at higher resolution.

p. 27 l. 27-28: “First, the location and rate of sea ice formation impacts the properties of shelf water masses flowing into the ice shelf cavities.” You did a very thorough job of plotting a lot of fields from the 2 models. If there is one field I wished you’d included, it is the rate of sea-ice formation, perhaps comparing with a satellite-derived data set such as Tamura et al. (2016). I will not insist that you add such a plot but I would appreciate it if you would consider it at least.

p. 29 l. 10-12: “Therefore, in the future it may be worthwhile to experiment with different topographic smoothing methods, which may uncover options to minimise the trade-off between numerical stability and geometric accuracy.” I would suggest adding to this that it might be worth investigating other numerical methods for computing the horizontal pressure-gradient force (HPGF) in FESOM, since this is an area of active research (Engwirda, Kelley, and Marshall 2017). I would have more confidence that improvements in the HPGF would lead to less topographic smoothing than that a better smoothing algorithm can solve the problem on its own.

p. 29 l. 23-24: “None of our simulations resolve eddies on the Antarctic continental shelf, which would require resolution of approximately 1 km (St-Laurent et al., 2013).” See my previous comment about 1 km resolution. I think the higher resolution FESOM simulation is probably at least eddy permitting. This is suggested at least by some of the features shown in the zoomed-in velocity plots. If this is the case, it might be worth mentioning. If not, it might be worth mentioning that none of the simulations is even eddy permitting.

p. 30 l. 18-19: “Furthermore, alternative parameterisations of ice shelf basal melt are being explored by the community (Jenkins, 2011), which may provide valuable intercomparisons with the three-equation parameterisation in the future.” I would suggest including Jenkins (2016) here. I think this work is more likely to lead to an alternative to the three equations than the plume-model approach as in Jenkins (2011).

p. 30 l. 28-29: “Sea ice in both MetROMS and FESOM mostly agrees with observations...” Can you be more specific about which fields were compared with observations? For example, I don’t think the rates and locations of sea-ice production are likely to agree with satellite-derived estimates (see my suggestion for a figure above), as you discuss in the context of mixed-layer depth in the ocean.

p. 30 l. 31: “In the interior Southern Ocean and the ACC, FESOM has an advantage due to its vertical coordinate system.” I think it would be worth restating that FESOM’s vertical coordinate is z-level (and perhaps also that MetROMS’ is terrain-following) in this region.

p. 31 l. 1: “...more reliable atmospheric forcing...” I think this needs some clarification. What does this mean to you? I realize this has been covered in the discussion but it’s worth summarizing in at least a little more detail here.

Typographic and grammatical corrections:

p. 1 l. 15-16: In my experience, it is customary to refer to a model's "terrain-following coordinate" or "z-coordinate" (both singular). Plural would be appropriate if we were referring to coordinates in multiple dimensions (e.g. spherical coordinates).

p. 4 l. 26-29 (and possibly elsewhere) I would suggest using only "time step" and not "timestep". You use both in this paragraph.

p. 8 l. 1, 4, 12, 15: There is an incorrect new paragraph on each of these lines causing an indentation.

p. 21 l. 26: "which has ~~the~~ one of the deepest ice shelf drafts in Antarctica" an extra "the"

References

Engwirda, Darren, Maxwell Kelley, and John Marshall. 2017. "High-Order Accurate Finite-Volume Formulations for the Pressure Gradient Force in Layered Ocean Models." *Ocean Modelling* 116 (August):1–15.

Jenkins, Adrian. 2016. "A Simple Model of the Ice Shelf–Ocean Boundary Layer and Current." *Journal of Physical Oceanography* 46 (6):1785–1803.

Tamura, Takeshi, Kay I. Ohshima, Alexander D. Fraser, and Guy D. Williams. 2016. "Sea Ice Production Variability in Antarctic Coastal Polynyas: ANTARCTIC SEA ICE PRODUCTION VARIABILITY." *Journal of Geophysical Research, C: Oceans* 121 (5):2967–79.