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Geoscientific Model Development Discussions

Interactive comment on "Intercomparison of Antarctic ice shelf, ocean, and sea ice interactions simulated by two models" *by* Kaitlin A. Naughten et al.

Anonymous Referee #1

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Reviewer comment: Kaitlin Naughten : Intercomparison of Antarctic ice shelf, ocean, and sea ice interactions simulated by two models. This paper is primarily about the ice-shelf melt rates from two different ocean models. The difference in sea ice and ocean states is also described but the interactions are merely hinted at rather than proven. The message is that with the only reference point between the models as the imposed surface forcing (from ERA-I) it is difficult to unravel the differences in behaviour of ice shelf basal melt. The models have not been spun-up and so the ice shelf cavity water masses may not be in equilibrium. It is thus difficult to assess how significant are the results. By allocating a large chunk of the paper to model assessment, it becomes unwieldy and overly long. (although in formatted text probably not much so than that

of Mathiot et al. also in GMD) The model assessment should be almost perfunctory to show that there are differences in the mean state and drifts. This allows the reader to focus on the ice shelf comparison, however, this section needs to be tidied up and given a specific focus to draw strongly form Figure 8 (expanded on below). I would not suggest any more model simulations, just a simplification of how this data is presented.

Specific comments

2:2-3 Please quote the more comprehensive observational study of the mass balance;

Shepherd, A. et al. A reconciled estimate of ice-sheet mass balance. Science 338, 1183–1189, 2012.

2:3 Golledge is a strange reference to use, a better summary of the processes is provided by

Joughin & Alley, Stability of the West Antarctic ice sheet in a warming world, Nature Geoscience, 4, 506–513 doi:10.1038/ngeo1194, 2011

2:14 It is true that there are not many such measurements, but tribute should be made to those that are done (at great expense) e.g. Nicholls et al., 2006; McPhail et al 2009; Venables et al., 2014

2:20 Add references to cavity models in GCMs;

Losch, M.: Modeling ice shelf cavities in a z coordinate ocean general circulation model, J. Geophys. Res.-Oceans, 113, C08043, https://doi.org/10.1029/2007JC004368, 2008.

Mathiot, P., Jenkins, A., Harris, C., and Madec, G.: Explicit representation and parametrised impacts of under ice shelf seas in the zâĹŮ coordinate ocean model NEMO 3.6, Geosci. Model Dev., 10, 2849-2874, https://doi.org/10.5194/gmd-10-2849-2017, 2017.

9:8 Actually FESOM uses a different version of EVP than Hunke and so I suggest you instead reference Bouillon et al. (2013), as Danilov does.

9:12 Add a brief section on surface exchange scheme. Since these are forced models (ERA-I) then the difference in the bulk formula for surface exchange, particularly momentum, can contribute to differences in sea ice drift/formation, ACC transport and Ekman pumping/drift. A comparison of wind stress (on the ocean, not from ERA-I directly) from the two models would illustrate this.

9:27-30 The Intermediate waters which are pumped up on to the shelves will not be spun-up and since the initialization of these waters is based on almost no observations, the fidelity of the experiments is it doubt. However, this does not invalidate the model inter-comparison. I suggest a figure here which shows the far field anomaly of mean ocean temperature 300-1000m depth, compared with that of the initial conditions, for both models (see figure 14 of Mathiot et al., 2017).

10: 15-30 Consider including a brief note on the downside of salinity restoring – bulk salinity drift and impacts on ACC.

11:23 A lead in is required for this section describe the diagnostics that are going to be used and why. For example, the ACC "The Antarctic Circumpolar Current (ACC) is the most important current in the Southern Ocean, and the only current that flows completely around the globe. It is evaluated in models and observations at the Drake Passage, where by convention; all flow through the Passage is the ACC. The strength of the ACC is associated with the circumpolar winds (and the Southern Annular Mode - SAM) and the north-south oceanic salinity gradient. Various observations suggest the time-averaged Drake Passage transport is in between 134+-27 Sv (Cunningham et al., 2003) and 173+-11 Sv (Donohue et al., 2016)" – likewise for the MLD (continental shelf break mixing, heat exchange and sea ice formation in coastal polynyas) and sea ice properties.

12:1. Forced ocean simulations have significant drifts in the ACC because the bulk formulae do not constrain the surface fresh water balance, even with salinity restoration,

and hence the density structure changes. A stronger note of caution about validating these models with no spin-up is required.

13:17 – 15:34 There really is very little point in discussing the water mass properties and deep ocean when the model has not spun-up (a process requiring 300+yrs)! These do not add much to the discussion and should be removed to restrict the inter-comparison to near surface characteristics.

16:11 Turner (2013) is refereeing to coupled models where the issue is more likely associated with the coupled forcing and mixed –layer depth, not due to the sea ice model itself. It is thus misleading to attribute this to the sea ice model, when it is rather the response of the sea ice to the climate model forcing. In the case of a standalone ocean model, where the surface forcing should act to restore the sea ice to that in ERA-I, such a lack of summer sea ice serious deficit in the models or bulk formula.

16:16-19 Delete. It is not necessary to reiterate that the sea ice concentration is a time average (as it is for the inferred observations).

16:21-22 This must happen because ERAI surface fluxes are attempting to restore sea ice to observations, so providing the mechanism them makes the variability self explanatory.

16:23-26 The difference between high and low resolution is hardly significant in Fig 6b and not worthy of mention unless the bathymetry is causing the low ice extent in the first place. That ACC water is entering the Weddell Sea gyre from this direction would be unusual, and as a consequence be worth discussing as a model bias.

16:27-34 Comparing sea ice concentrations is not valid unless there is a strong point to make. The observations are uncertain to 0.07 anyway and the concentration is consequently capped. The point about strong winter heat loss causing a sensible heat polynya and deep convection is supported by:

Goosse, H. and Fichefet, T.: Open-ocean convection and polynya formation in a largescale ice-ocean model, Tellus A, 53, 94–11, doi:10.1034/j.1600-0870.2001.01061.x, 2001.

Timmermann, R. and Beckmann, A.: Parameterization of vertical mixing in the Weddell Sea, Ocean Model., 6, 83–100, doi:10.1016/S1463-5003(02)00061-6, 2004

Consequently, the authors could strengthen this argument, but it is really a sideline which should be avoided and allow a stronger continuity to the paper.

17:7-11 Compared with coupled models the ice extents are pretty good, but they are still outside the decadal variability in the observations. Consequently the difference must be related to SST. This could be due to the path of the ACC (bathymetry or bulk formulae) or even due to the salinity restoration method being applied differently in the two models.

17: 15 Ice thickness is indeed a good means to inter-compare processes between models. I would be far more concerned with the thick ice in FESOM (and MetROMS) sitting directly in front of the Filchner_Ronne ice shelf. This should be a region of thin ice in JJA due to the winter latent heat polynya, which will not form in the model due to the thick ice. Consequently the Weddell sea ice formation and drift would be abnormal as is evidenced by the ice extent and thickness. On the other hand the Ross Ice-shelf polynya looks fine.

17:29 This launch into the cavity melt is too abrupt. It would be better to start with how this is assess observationally and the uncertainties (the Deporter and Rignot estimates are easily inside the error bounds and no comparison is required).

17:29-18:34 The intriguing aspect of figure 8 is that both models deviate from Rignot in the same direction. We would expect differences associated with the individual model resolution (vertical and horizontal), bulk formulae of momentum at the continental shelf break, basal melt parameterisations (and bathymetry). A description of possible sources of discrepancies is required before launching into the individual shelf

regions. For both models to deviate from Rignot in the same way either says that there are regional specific biases (e.g. lack of tides in models), ERAI is regionally biased from an unknown reality, or that Rignot is methodology is flawed. The latter could be tested by determining if the entire pattern changes with the Depoorter results. A comparison with Table 3 of Mathiot et al. (2017) might also be instructive.

19-25 The descriptions of ice shelves by region This type of analysis lends itself to repetition and long-winded descriptions. Instead I suggest that the analysis focus on specific ice shelves that show particular characteristics in Figure 8. For example Pine Island shows all the models closely clustered. The analysis should then show that the actual behavior of the models is quite different. Another might be the Ross where the melt rates are quite different and there is resolution sensitivity. Another might be, say, West, which is a poorly resolved small (shallow draft) ice shelf.

Keep the figures presented internally consistent, the style used for Figure 11 is good with a cross-section of water properties (including of-shelf) to reveal the bathymetry issues, perhaps showing the vertical overturning stream function and horizontal barotropic stream function. The small vertically integrated velocity vectors are not easy to interpret and if this approach is used a schematic representation of circulation may be better.

27:20-30:19 Discussion It is certainly true that all models, even ERAI show a cloud bias associated with cyclones, see:

Bodas-Salcedo, A., T. Andrews, A. V. Karmalkar, and M. A. Ringer (2016), Cloud liquid water path and radiative feedbacks over the Southern Ocean, Geophys. Res. Lett., 43, 10,938–10,946, doi:10.1002/2016GL070770.

Williams, K. D., and Coauthors, 2013: The Transpose-AMIP II experiment and its application to the understanding of Southern Ocean cloud biases in climate models. J. Climate, 26, 3258–3274, doi:https://doi.org/10.1175/JCLI-D-12-00429.1.

Include reference to (Graham et al., 2016) when discussing resolution.

Graham, JA, Dinniman, MS and Klinck, JM : Impact of model resolution for on-shelf heat transport along the West Antarctic Peninsula, J. Geophys. Res., 121, 7880-7897, doi: 10.1002/2016JC011875, 2016.

Refer to Mathiot et al., 2017 to compare with a pure z-level model.

Some discussion of the variability is required. Is the melt rate dominated by a few years of periodic high melt, or is the interannual variability small (probably varies according to distance from the shelf break and strength of the ocean baratropic circulation on shelf

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