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Interactive comment on "Modeling intensive ocean–cryosphere interactions in Lützow-Holm Bay, East Antarctica" *by* Kazuya Kusahara et al.

Anonymous Referee #2

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In the manuscript "Modeling intensive ocean-cryosphere interactions in Luetzow-Holm Bay, East Antarctica", Kusahara et al. present results from a detailed, high-resolution model study of the sea ice-ice shelf-ocean system of Luetzow Holm Bay and the Shirase Glacier Tongue. The study examines the effect of fast ice on the flow of warm CDW towards the adjacent ice shelves through two sensitivity studies and the seasonal and interannual variability of the flow structure in Luetzow-Holm-Bay and at the shelf break.

I agree with the editor that there is sufficient novel material in the study with respect to Hirano et al. (2020) to recommend publication. Some of the figures are very similar to those in Hirano et al. (2020) and its supplementary material, but I will leave it to the editor to decide if they are sufficiently different.

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Discussion paper

I recommend publication of this study, but after major revisions. As I state below, I have my doubts whether using the ice shelf model unchanged to simulate the fast ice cover is appropriate. I would also like to see a more balanced, quantitative and detailed discussion of the drivers of the changes seen between the sensitivity runs and the interannual variability.

General comments

The authors base their model evaluation and analysis on two quasi-steady state runs with and without fast ice. For analysing the changes in the simulated circulation pattern and water mass distribution this is absolutely fine. However, the ocean observations they use to evaluate the model were taken in a period when there should still have been a transient response from the fast ice outbreak, if I understand the timing correctly (fast ice outbreak Mar-Apr 2016, ocean observations Jan 2017, ApRES Feb 2018-Jan 2019). While Fig. 4b indicates that the ApRES observations represent the NoFI case, I would like to see evidence how fast the modelled water column transitions to the NOFI case – something like a time series of ocean heat content or Hovmøller diagram of the temperature profile at a point in the bay would work. I do realise that the noFI case matches the CTD observations much better, but I would like to see that one winter is enough to change the water column to that state.

Your model treats the fast ice as ice shelf. The issue is that the computation of heat and freshwater fluxes in your ice shelf model uses exchange coefficients for heat and freshwater that imply an ocean surface speed of 15 cm/s, rather using the actual surface velocity as in the sea ice model. In the original Hellmer and Olbers (1989) model and follow ups this was adopted to simulate the effect of tides. Even though it is unclear which run this comes from and it is a long-term mean, your Fig. 12 makes me wonder if surface velocities are consistently (or ever) that large under the fast ice. Please check instantaneous surface speeds at a few points under the fast ice to confirm the actual magnitude of the surface speeds under the fast ice. My suspicion is that the freshwater input under fast ice is strongly overestimated due to the issue in the model formulation that I have outlined above. You are clearly aware that the freshwater input in the FI case is large judging your discussion in Section 4, where you try to justify the magnitude of that freshwater input. However, in my opinion this model issue overestimates the difference between the FI and NOFI sensitivity runs.

There needs to be an earlier and stronger focus on the effect of surface buoyancy forcing and sea ice formation in the study. The change you make between the FI and NOFI sensitivity runs strongly affects the surface buoyancy forcing, but you largely ignore this effect up until late in the manuscript in favour of the changes in Ekman downwelling due to the changing winds. Section 4 needs to be moved to an earlier position in the paper. Section 5 really needs some time series of the forcing i.e. wind stress, Ekman pumping, and surface heat fluxes. Please add these as suitable time series. Considering how much emphasis you place on the Ekman pumping mechanism, a more quantitative discussion of this mechanism by showing Ekman pumping velocities or estimating their magnitude at the sea ice edge would strengthen your argument.

I spent a lot of time looking at Fig. 12, because I think the seasonal current reversal at the shelf break and inflow shift is fascinating and a great demonstration how the strength of the easterlies determines the flow direction at the shelf break. The stratification and tilt of the isopycnals towards the shelf below 100m doesn't change very much between January and July in your Fig 13. So in principle, all that happens is that changing winds shift that baroclinic shear profile back and forth between westward surface and eastward undercurrent in summer (Amundsen) and strong westward surface flow and weaker westward flow at the shelf break in winter (Weddell). What I am missing in the text is how this shifts the inflow between the two troughs from flowing predominantly through the western trough and outflow in the eastern trough in summer to a weaker inflow in the west and some inflow in the east in winter.

Specific points

l. 33-37: There is some duplication in these sentences that could be tightened.

l. 44-45: Amundsen Sea bottom topography isn't that certain either.

l. 49-54: I agree that Luetzow-Holm Bay is in a location equivalent to parts of the Amundsen Sea with respect to the Weddell and Ross Gyres being located south of their eastern flanks. However, in the Bellingshausen Sea the ACC flows along the shelf break and it is therefore distinctly different from the Amundsen. Also, both the Amundsen and Bellinghausen Seas have wide continental shelves that have their own circulation regimes, so to talk about the coast in that context is misleading. There are more appropriate publications to cite here: Ryan et al. (Deep Sea Research, 2016) investigated the eastern end of the Weddell, Dotto et al. (GRL, 2018) for Ross Gyre, and Armitage et al. (JGR 2018) identify the gyre circulation patterns over the entire Southern Ocean from satellite SSH observations.

l. 66-67: How do the 1990-92 observations compare to your FI case? How does the thickness of your simulated WW layer in the FI case compare to this?

l. 131: The reference for that new bathymetry is Hirano et al. (2020). The abstract states that it is "newly compiled" which would suggest to me that you are the first to present this bathymetry data set.

l. 132: 2.2 not 2.3. I really like the comparison of bathymetry products. Something that definitely needs more attention when modelling the circulation on Antarctic shelves.

l. 146-150: That hydrographic data set is old and Arctic-focussed. I am just curious why you chose it and not more recent data sets like WOA. However, since it is mainly used as initialization, I don't think it has a strong bearing on you model results. l. 146-150: What convection scheme do you use? Have you evaluated its suitability for the Southern Ocean?

l. 186: typo "trough" not "tough"

l. 186-187: Trough T4 is not labelled in Fig 2a, but in Fig. 6.

l. 190-191: But your new bathymetry is based on the JARE measurements...? Thus using that data set to evaluate your model bathymetry seems inappropriate. Section 2.3 and Fig.3: I like the figure and presentation of the seasonal cycle, but I think you should show a mean over the years that go into your FI and NOFI cases, i.e., 2008-2018. Possibly also 2005-18.

l. 207-209: Already stated in the introduction, superfluous here. Maybe re-state later?

l. 218: should be Fig. 4b, since this shows the drift in the control run, the interannual variability and the two sensitivity runs. Figure 5 has only one panel.

l. 222-223: Please re-write this sentence.

l. 226-228: Please re-order your figures according to the order in the text. Since Fig. 5 is not used before Fig. 6, it needs to be moved. In addition, this sentence duplicates the figure caption to Fig. 6. SGT and SkG should be explained in the Fig.6 caption.

l. 233-241 & Fig. 7: You comment on the outer stations, but not E1 and A3 which show the largest discrepancies in salinity. The observations have S > 34.4 up to 400 m, i.e. there is a 400 m layer of mCDW in the observations that your model section does not show. Since this is the NOFI case I think this is being mixed away over the 13 years since you removed the fast ice. This also feeds back to my question after the convection scheme used and whether it is appropriate and whether the quasi-steady state NOFI case mean should be used to evaluate the model against observations. There is also the possibility that in the real ocean there wasn't enough convection in the first winter after the fast ice outbreak to erode the mCDW in front of SGT.

l. 243-250 and Fig. 8: After my previous comment, please add some evaluation of the simulated salinities as well to check how your simulated density structure compares and what the implications for the circulation might be.

l. 251-258: This is a great identification of the seasonal shift in inflow location, but his gets lost later?

l. 290-291: "The observed estimate from the single location includes the entire SGT variability and regional variability.“ Not sure what you mean here. The next sentence states correctly that it is unclear how representative the ApRES location is. Please re-phrase or remove. N.B. Comparing the melt rate in your Fig. 6 with the location of the ApRES in Hirano et al. 2020, it appears that the instrument was located an area of the ice shelf with large gradients in melt rate. Thus it may be a tricky location for comparison. Have you done a point comparison to see if the model melt rate pattern shifts throughout the year?

l. 297-299: Your NOFI and FI sensitivity studies are designed to demonstrate that the seasonal cycle and upper ocean processes control the access of the warm water to the cavity. That is what Fig. 6-10 show: the model does have warm water on the shelf, but the surface processes control how much of it reaches the cavity. Remove or re-phrase this sentence.

l. 305-309 and Fig. 11: The seasonal cycle of the wind and of surface buoyancy forcing due to cooling and the addition of salt during sea ice formation coincide and using a model gives you the opportunity to pick these effects apart in more detail. The NOFI case shows strong evidence of sea ice formation causing first cooling and salinification of the surface and deep convection down to 350m around September. That seasonal cycle due to surface buoyancy forcing, partly due to a more strongly stratified upper ocean, is a lot weaker in the FI case (see general comments) and therefore you see the depression of the isopycnals due to Ekman downwelling more clearly.

Fig. 12 & 13: Unclear which run you show here.

Fig. 13 & 14: Please be consistent in defining flow directions as positive or negative. In Fig 13 eastward is negative, in Fig. 14 it is positive.

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