

Interactive comment on “Modelling Ross Ice Shelf melting effect on the Southern Ocean in quasi-equilibrium” by Xiyang Liu

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First, I deeply apologize for taking significantly longer than requested to review this manuscript. I hope my thorough review helps to mitigate the inconvenience of waiting so long for my review.

General Comments:

This paper describes two global ocean-sea ice experiments run to quasi-equilibrium over 500 years, one with basal melting below the Ross Ice Shelf (RIS) and one without basal melting (but still with an ocean cavity below RIS). Most of the presented results examine differences between ocean and sea ice properties between the two experiments (results are typically averaged over the last 100 years of each experi-

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ment). Non-negligible differences in the horizontal and vertical distributions of temperature and salinity are found between the two experiments, leading to appreciable differences in both the meridional overturning circulation and meridional heat transport in the ocean. Differences in the surface and barotropic flow are also demonstrated, along with related changes in sea surface temperature, sea ice thickness and sea ice concentration.

The purpose of the study seems to be to show that freshwater fluxes from ice shelves have a significant impact on the Southern Ocean compared to a simulation without any freshwater fluxes. It seems unclear (and is not discussed in the manuscript) what the relevance of this study is to other modeling work or what this study might tell us about the melt-induced dynamics in the real world.

It has been known for some time in the Earth System Modeling community that some form of freshwater input into the deep ocean is required for adequate representation of deep ocean properties and of the meridional overturning circulation. Therefore all global Earth System Model (ESM) include some mechanism for freshwater input (typically surface “runoff” around Antarctica and Greenland), together with a mechanism for inducing overturning in polar regions (typically salinity restoring at the surface). No models I am aware of without sub-ice-shelf melting would leave out these mechanisms. Therefore, if the aim of this study is to show that current ESMs should be including the effects of ice-shelf melting in order to avoid inaccuracies in Southern Ocean properties, the “control” experiment (EN in the manuscript) should probably have been closer to a configuration used in ESMs: “runoff” at the surface to at least partially account for freshwater fluxes and no ice-shelf cavities.

If the purpose of the study is to show what features of the climate system are affected by the presence or absence of melting below RIS, there is another significant pitfall in this work. Very little effort is made to validate either the EI (with melting) or the EN (without melting) experiment against observations or previous modeling (except for the basal melt rate below RIS). This strikes me as highly problematic because the differ-

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ences between the simulations is unlikely to tell us something about the real world if the base state (either EI or EN) that is being perturbed can be shown to be representative of the real world. Given the *very* coarse horizontal resolution (150 km) and rather coarse vertical resolution (not stated but seemingly around 50 m), it seems unlikely that the model will be able to capture the complex chain of processes by which water masses are transformed on the Ross continental shelf, within the Ross cavity, and off the continental slope where they mix into the deep ocean. These processes have been shown to require horizontal resolutions at least 30x higher than this simulation (see specific comments), allowing interactions between small-scale topographic features and narrow oceanic currents. Without these transformations being captured adequately or the model state having been validated against a broader set of observations, conclusions in this work about how basal melting affects the Southern Ocean are likely to only apply to this particular model configuration, and not to be representative of the real world.

The manuscript presents much of the results results with little deeper analysis, discussion or explanation (the exception is a more careful analysis changes in sea surface temperature and sea ice properties resulting from flow anomalies near RIS). Except for the dynamics at the ocean surface, little attempt is made to explain how water masses are transformed to reach various ocean depths. Basal melting is found to *decrease* the global overturning circulation, seemingly due to increased stabilization of the water column, in contradiction to know physical processes of Antarctic Bottom Water formation (known to occur in the Ross Sea region) that are thought to be an important driver of global ocean circulation. No discussion is included of potential shortcomings of the model at capturing or resolving ocean processes that would be relevant to these transformations.

I can only recommend this manuscript for publication after major revisions to address these shortcomings.

This paper would benefit from significant editing by a native English speaker. I have

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attempted to point out typos and grammatical errors where I have seen them (I include about 3 pages of such corrections). Additionally, the figures all need significant formatting work before they are ready for publication, including labeling axes and increasing font sizes to make the labels more readable.

Specific Comments:

p. 1 l. 6: In the field, BMR is typically used as an abbreviation of “basal melt rate”. The incorporation of the Ross Ice Shelf into this abbreviation is confusing. I would suggest replacing “basal melting of Ross Ice Shelf (BMR)” with “basal melting below the Ross Ice Shelf (Ross BM)” and elsewhere replace “BMR” with “Ross BM” to avoid confusion. If you can come up with an alternative shorthand that will not be confused with “basal melt rate”, that would be fine, too.

p. 1 l. 12: I would suggest replacing “substantially” and “not so significant” with something more quantitative if possible.

p. 1 l. 14: “local circulation anomalies”: In general, the abstract seems to treat the case of no basal melting as the control case and the case with basal melting as the modified experiment. I can understand this choice, since ocean models typically do not include ice-shelf cavities, though it seems strange from a physical standpoint to treat the less physical experiment as the control case. Here, the use of the word “anomalies” seems particularly strange to me, since it seems to imply “something that deviates from what is standard, normal, or expected”, whereas I would say the control case is the one more likely to deviate from the physical world. Perhaps another phrase such as “differences in local circulation) would be clearer

p. 1 l. 14: “with the help of ocean bathymetry”: This phrase seems rather vague to me. Maybe a better wording would be something like “The decreased density due to the effect of Ross BM, together with interactions with ocean bathymetry, creates local differences in circulation in the . . .”

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p. 1 l. 22-24: The audience for The Cryosphere is aware of what ice sheets, ice shelves, icebergs, etc. are so I don't think this level of introduction is necessary.

p. 1 l. 26: "beneath the currently stable Ross Ice Shelf": The phrase "currently stable" is both grammatically problematic and confusing, because it implies a past or future instability in RIS that is not addressed here, nor is there any widely accepted likelihood of RIS instability in the community. I would remove this phrase.

p. 1 l. 26: "can be larger than 2500% of the overall. . .": It is not clear that this fact or this reference is relevant to the rest of the paper, as you are not resolving melt channels in your simulations.

p. 2 l. 3: "Neglecting the sub-ice freshwater...for the Southern Ocean." While it is not stated here, the implication seems to be that common practice in ocean modeling of the Southern Ocean is to neglect sub-ice-shelf freshwater fluxes entirely, whereas this is not usually the case. Global (and I believe also regional Antarctic) ocean models without ice-shelf cavities still include an approximation of the total Antarctic freshwater input (melting + calving) but they almost universally do so by distributing the freshwater at the ocean surface and typically evenly around the continent. In my view, sub-ice-shelf freshwater fluxes aren't really "neglected" so much as they are estimated and distributed inaccurately. Here is one publication that discusses the differences in ocean model behavior depending on how freshwater fluxes are distributed: Mathiot, P., Jenkins, A., Harris, C., and Madec, G.: Explicit representation and parametrised impacts of under ice shelf seas in the zâLÛ coordinate ocean model NEMO 3.6, *Geosci. Model Dev.*, 10, 2849-2874, <https://doi.org/10.5194/gmd-10-2849-2017>, 2017.

p. 2 l. 3-4: "These are pronounced in the Weddell...broad continental shelves". It is not clear to me that the Weddell and Ross Seas are the regions of Antarctica that would be most affected by neglecting freshwater fluxes. The large size of RIS and Filchner-Ronne Ice Shelf (FRIS) together with their relatively cold ice-shelf cavities do make them particularly important for AABW formation but other regions of Antarctica

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with warmer cavities have been shown to produce significant amounts of freshwater that impact Antarctic climate both locally and regionally in significant ways, see e.g.:

Nakayama, Y., R. Timmermann, C. B. Rodehacke, M. Schröder, and H. H. Hellmer (2014), Modeling the spreading of glacial meltwater from the Amundsen and Bellinghousen Seas, *Geophys. Res. Lett.*, 41, 7942–7949, doi:10.1002/2014GL061600.

The Getz, Thwaites and Pine Island Ice Shelves, for example, each produce significantly more freshwater than RIS and nearly as much as FRIS, despite their significantly smaller areas:

Rignot, E., Jacobs, S., Mouginot, J., Scheuchl, B. Ice-shelf melting around Antarctica. *Science*. 2013 Jul 19;341(6143):266-70. doi: 10.1126/science.1235798.

p. 2 l. 8-9: It would be good to supply a more complete list of estimates of basal melting. Here are a few more important ones:

Moholdt, G., L. Padman, and H. A. Fricker (2014), Basal mass budget of Ross and Filchner-Ronne ice shelves, Antarctica, derived from Lagrangian analysis of ICESat altimetry, *J. Geophys. Res. Earth Surf.*, 119, 2361–2380, doi:10.1002/2014JF003171.

M. Depoorter, J. Bamber, J. Griggs, J. Lenaerts, S. Ligtenberg, M. van den Broeke, G. Moholdt. Calving fluxes and basal melt rates of Antarctic ice-shelves. *Nature*, 502 (7469) (2013), pp. 89-92

p. 2 l. 10: Other sources (Rignot et al 2013, Depoorter et al. 2013) estimate a significantly larger mean melt rate on the order of 0.8-0.9 m/a. Beckmann and Goosse, 2003 is not really an appropriate citation for the 0.5 m/a number, they are merely citing the Jacobs et al. 1996 estimate, converted from mSv to m/a. Given the significant improvements in satellite observations since 1996, I do not feel that number is particularly trustworthy.

p. 2 l. 11: “occurs at the base of the ice shelf edge”: This is sometimes true, particularly for warm ice-shelf cavities. But the freshwater plume in cold cavities typically reaches

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neutral buoyancy at depths significantly below the ice-shelf edge:

Jacobs, S S, H H Helmer, C S M Doake, A Jenkins, and R M Frolich. “Melting of Ice Shelves and the Mass Balance of Antarctica.” *Journal of Glaciology* 38, no. 130 (1992): 375–87. doi:10.3198/1992JoG38-130-375-387.

For the purposes of the point you are making, it would be sufficient to say, “Since the injection of this freshwater occurs at depth rather than at the ocean surface. . .”

p. 2 l. 16-17: “can provide no direction information about sub-ice shelf circulation”: This is not entirely true, as sub-ice-shelf observations include velocity measurements that can be used to infer at least some basic information about the sub-ice-shelf circulation. Temperature and salinity measurements can also be used to infer, through the fraction of Ice Shelf Water, the degree of interaction with the ice-shelf base, which also can provide information about the broad sub-ice-shelf circulation. I would suggest toning this down to say that it is difficult to infer the sub-ice-shelf circulation from borehole observations.

p. 2. l. 15-29: The citations in this paragraph seems out of date and incomplete. These reviews provide many citations that could help to fill in the gaps:

Dinniman, Michael, Xylar Asay-Davis, Benjamin Galton-Fenzi, Paul Holland, Adrian Jenkins, and Ralph Timmermann. “Modeling Ice Shelf/Ocean Interaction in Antarctica: A Review.” *Oceanography* 29, no. 4 (December 2016): 144–53. <https://doi.org/10.5670/oceanog.2016.106>.

Asay-Davis, Xylar S., Nicolas C. Jourdain, and Yoshihiro Nakayama. “Developments in Simulating and Parameterizing Interactions Between the Southern Ocean and the Antarctic Ice Sheet.” *Current Climate Change Reports*, October 24, 2017, 1–14. <https://doi.org/10.1007/s40641-017-0071-0>.

I would suggest a complete rewrite of the paragraph with a more complete list of the numerical methods, domains, time periods covered, etc.

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In particular, there are several studies that have used the MITgcm with ice-shelf cavities in regional configurations to study Antarctica and the Southern Ocean. Since these use the same model as this study, it would seem like they might get particular emphasis here.

p. 2 l. 19: “dynamic”: this could use further clarification. I think you mean dynamic ice-shelf geometry? How is this different from Walter and Holland (2007)?

p. 2. l. 21: “fixed cavity and thermodynamics”: The cavity geometry is fixed but the thermodynamics is not – melt rates evolve with changing ocean conditions.

p. 2 l. 21: “parameterization”: Again, more details on what this means would be helpful.

p. 2 l. 22-23: What would the other options be besides the list given? Global? Indeed, there are several studies with global models (Losch, 2008; Helmer et al. 2012; Timmermann et al. 2012, etc.)

p. 2 l. 23: “two-dimensional” needs more clarification – one horizontal dimension and one vertical.

p. 2. l. 28-29: “At present, this kind of research has rarely been reported.” I think it is fair to say that this has not been done before.

p. 2 l. 30-p. 3 l. 6: Again, I think this paragraph is missing some important work. Many modeling efforts not mentioned here include the Ross Sea in larger regional or global models that are big enough to look at the effect of RIS on the Southern Ocean. Two examples are:

Timmermann, Ralph, and Hartmut H. Hellmer. “Southern Ocean Warming and Increased Ice Shelf Basal Melting in the Twenty-First and Twenty-Second Centuries Based on Coupled Ice-Ocean Finite-Element Modelling.” *Ocean Dynamics* 63, no. 9–10 (October 2013): 1011–1026. <https://doi.org/10.1007/s10236-013-0642-0>.

Dinniman, Michael S., John M. Klinck, Eileen E. Hofmann, and Walker O. Smith. “Ef-

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fects of Projected Changes in Wind, Atmospheric Temperature, and Freshwater Inflow on the Ross Sea.” *Journal of Climate*, December 1, 2017. <https://doi.org/10.1175/JCLI-D-17-0351.1>.

You are correct that these models were not able to run for long enough times to look at quasi-equilibrium effects.

p. 3 l. 12: “will be an interesting topic”: I don’t think this belongs here, as it is a very subjective statement. I would remove this whole sentence.

p. 3 l. 17-19: Both the topography data and the forcing data are not the most up-to-date versions, see references below. Both Bedmap2 (Fretwell et al. 2013) and RTOPO2 (Schaffer et al. 2016) have updated topography, though I am not sure whether these changes affect RIS specifically. There is a CORE-NYF.v2 data set (http://data1.gfdl.noaa.gov/nomads/forms/core/COREv2/CNYF_v2.html), which is a climatology from the interannual forcing described in Large and Yeager (2009). It would be worth explaining why these earlier versions were used instead of the more up-to-date versions.

Fretwell, P, H D Pritchard, D G Vaughan, J L Bamber, N E Barrand, R Bell, C Bianchi, et al. “Bedmap2: Improved Ice Bed, Surface and Thickness Datasets for Antarctica.” *The Cryosphere* 7, no. 1 (2013): 375–93. <https://doi.org/10.5194/tc-7-375-2013>.

Schaffer, Janin, Ralph Timmermann, Jan Erik Arndt, Steen Savstrup Kristensen, Christoph Mayer, Mathieu Morlighem, and Daniel Steinhage. “A Global, High-Resolution Data Set of Ice Sheet Topography, Cavity Geometry, and Ocean Bathymetry.” *Earth System Science Data* 8, no. 2 (October 2016): 543–57. <https://doi.org/10.5194/essd-8-543-2016>.

Large, W. G., and S. G. Yeager. “The Global Climatology of an Interannually Varying Air–sea Flux Data Set.” *Climate Dynamics* 33, no. 2–3 (August 2009): 341–64. <https://doi.org/10.1007/s00382-008-0441-3>.

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p. 3 l. 17-19: How is “runoff” handled in each experiment (EI and EN)? I believe CORE specifies a runoff field that inputs freshwater into the Antarctic region equally around the continent and at the ocean surface at a level that is supposed to roughly match the surface accumulation over the continent (therefore accounting for the combined effect of runoff, sub-ice-shelf melting and calving, assuming AIS is in equilibrium). Was this runoff field included in your simulations?

p. 3 l. 24-26: I would suggest making this sentence a footnote.

p. 3. l. 27-28: Please explain the abbreviations “EI” and “EN”.

p. 3 l. 29-30: More detail should be given about what the vertical resolution actually is. What is the resolution at the surface? At 1000 m depth? The coarsest resolution (at depth)? I suspect that, even with finer resolution in the upper 1000 m, 30 layers is inadequate to resolve the sub-ice-shelf plume in detail. Finer resolution would likely lead to a significantly different answer, see:

Losch, M. “Modeling Ice Shelf Cavities in a z Coordinate Ocean General Circulation Model.” *Journal of Geophysical Research* 113, no. C8 (August 2008): 1–15. <https://doi.org/10.1029/2007JC004368>.

Schodlok, Michael P., Dimitris Menemenlis, Eric Rignot, and Michael Studinger. “Sensitivity of the Ice-Shelf/Ocean System to the Sub-Ice-Shelf Cavity Shape Measured by NASA IceBridge in Pine Island Glacier, West Antarctica.” *Annals of Glaciology* 53, no. 60 (2012): 156–162. <https://doi.org/10.3189/2012AoG60A073>.

Schodlok, M. P., D. Menemenlis, and E. J. Rignot. “Ice Shelf Basal Melt Rates around Antarctica from Simulations and Observations.” *Journal of Geophysical Research: Oceans* 121, no. 2 (February 2016): 1085–1109. <https://doi.org/10.1002/2015JC011117>.

p. 4 l. 2: “the horizontal resolution is about 150 km”. This is one of my biggest concerns about this work. I realize that long time integrations are expensive but this coarse

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resolution (coarser even than CMIP5 and CMIP6 models of the region) seems *far* too coarse to capture the relevant dynamics for the Antarctic region, most importantly the pathways for transporting freshwater from the RIS to the Southern Ocean. See the following paper for a discussion of the pathways and the resolution (~ 5 km) required to capture them:

Dinniman, Michael S., John M. Klinck, Eileen E. Hofmann, and Walker O. Smith. “Effects of Projected Changes in Wind, Atmospheric Temperature, and Freshwater Inflow on the Ross Sea.” *Journal of Climate*, December 1, 2017. <https://doi.org/10.1175/JCLI-D-17-0351.1>.

See this paper for a discussion of the inadequacy of CMIP5 models at capturing Antarctic continental shelf processes:

Little, Christopher M., and Nathan M. Urban. “CMIP5 Temperature Biases and 21st Century Warming around the Antarctic Coast.” *Annals of Glaciology* 57, no. 73 (September 2016): 69–78. <https://doi.org/10.1017/aog.2016.25>.

p. 4 Table 1: Please reformat values in scientific notation rather than “e” notation used in programming languages (e.g. 1.0×10^{-4} if you are using LaTeX). Here and elsewhere, “m/s” should be “ $m\ s^{-1}$ ” and similarly “m/a” should be “ $m\ a^{-1}$ ”, etc.

p. 4 Table 1: Could you explain the choice to use ISOMIP thermodynamics? Neglecting the velocity dependence of the heat- and salt-transfer coefficients has been shown to reduce the accuracy of melt fields, see discussions in:

Dansereau V, Heimbach P, Losch M. Simulation of subice shelf melt rates in a general circulation model: velocity-dependent transfer and the role of friction. *Journal of Geophysical Research: Oceans*. 2014;119(3):1765–90. <https://doi.org/10.1002/2013JC008846>.

Asay-Davis, Xylar S., Nicolas C. Jourdain, and Yoshihiro Nakayama. “Developments in Simulating and Parameterizing Interactions Between the Southern Ocean and the

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Antarctic Ice Sheet.” Current Climate Change Reports, October 24, 2017, 1–14.
<https://doi.org/10.1007/s40641-017-0071-0>.

p. 4 Table 1: Could you explain why the Jenkins et al. (2001) form was not used? They show that this can lead to a drift away from the expected linear relationship between T and S over long timescales, which seems problematic given that this study is focused precisely on long timescales.

p. 4 l. 11-12: Could you please explain the choice to remove the ice shelf cavity in the 4 grid boxes rather than thicken the cavity? What criterion was used to decide whether the cavity is too thin and should be set to zero? How does the cavity thickness in the model compare with that of the original RTOPO-1 data set, averaged over each grid cell? Was the cavity thickness increased in some cells to match some required threshold (e.g. the column is more than x cells thick)? If so, was the ice draft moved up or was the bathymetry moved down, or both? What is the area of the modeled cavity compare to the area in RTOPO-1 and what would you expect the effect of this difference to be (I would expect the modeled cavity is much smaller and that this would lead to a reduced freshwater flux but a similar melt rate to observations). In summary, more explanation of the method is needed.

p. 4 l. 12: I believe “depth” actually refers to “water-column thickness”. Is that correct? If so, please make this substitution.

p. 4 Fig. 1: “indicate grids where cavities are resolved”. Since 4 grid boxes have water-column thicknesses of zero, I would argue those grid boxes don’t resolve the cavity and should probably be removed from the figure or shaded differently.

p. 4 Fig. 1: I am deeply concerned that RIS, the main focus of the study, is captured by only 15 grid cells and with seemingly 50 m vertical resolution and seemingly without partial bottom cells (though neither of these are discussed in the text). The introduction suggests that it is important to capture the sub-ice-shelf flow in models because it cannot be observed directly, but such coarse resolution seems entirely inadequate to

do that job.

p. 5 l. 9-13: It would be helpful to have a figure, panel of a figure or table to compare these various melt rates. It would be useful to be more quantitative than “larger” and “smaller”. It would also be important to separate results derived from modeling from those derived from satellite measurements. It is encouraging that the melt rate lies within the range of observational and previous model estimates. What about freshwater fluxes (given that the area of RIS in the model is probably significantly different from observations)? How do these compare with other studies?

p. 5 l. 13: What is meant by “net melt rate”?

p. 5 l. 15: What is meant by “model system evolution stage”? Does this refer to the numerical methods used to discretize the equations of motion?

p. 6 l. 7-8: “salinity bias and temperature bias”: I don’t think “bias” is the correct word here, as this would assume that the control case (without melting) are the observations, which they most certainly are not. I would also suggest avoiding the word anomaly unless you make clearer why you have chosen the EN experiment to be the “control” (implying you expect it to be the “normal” case in some sense). I think the most correct term, free from value judgments, would simply be “difference”. So the sentence should probably read something like “The relationship between salinity and temperature differences in RIS cavity water between the two experiments. . .”

p. 6 l. 7-11: This linear relationship between T and S resulting from melting is well known and is called the Gade line:

Gade, HG. “Melting of Ice in Sea Water: A Primitive Model with Application to the Antarctic Ice Shelf and Icebergs.” *Journal of Physical Oceanography* 9 (1979): 189–198.

It would be important to show if your line has the expected slope for a Gade line. Otherwise, it could indicate something is amiss with the sub-ice-shelf boundary conditions.

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p. 6 l. 10: “ppt” should probably be “PSU”, which is slightly different. I do not believe MITgcm uses ppt to measure salinity.

p. 6 l. 11: “there seems to be no significant influence on the inflow and outflow in the cavity”: The only way I can make sense of this phrase is if “influence on” is changed to “difference between”. Melting clearly has an influence on both the inflow and the outflow so it is clearly not correct to say there is no influence. I would suggest that this finding deserves more discussion. The only way to make sense of this is that, in quasi-equilibrium, a significant amount of outflowing freshwater recirculates into the cavity. This is a somewhat surprising finding and I think possibly a significant difference between these simulations and those at higher resolution (e.g. Nakayama et al. 2014, Dinniman et al. 2017):

Nakayama, Y., R. Timmermann, C. B. Rodehacke, M. Schröder, and H. H. Hellmer. “Modeling the Spreading of Glacial Meltwater from the Amundsen and Bellingshausen Seas.” *Geophysical Research Letters* 41, no. 22 (November 2014): 7942–49. <https://doi.org/10.1002/2014GL061600>.

p. 7 Fig 3: “ppt” should be “PSU”. Typically, we use degrees C instead of K in cryospheric research but that makes no difference for this particular plot.

p. 7 l. 6-20: Most of this paragraph seems simply to describe Fig. 4 without providing any physical insight into why these differences occur. To a limited degree, it is helpful to have you point out the most salient features of each panel but it would be far more useful to get some understanding of why changes in salinity occur where they do (and similarly for temperature). Why are they so different?

p. 7 l. 13-15: It is not at all obvious to me how you are backing up the assertion that freshwater flux is more significant than heat flux. The way I would expect to see that is in the influence of each on density changes, which in turn affect large-scale overturning and mixing into the deeper ocean. But Fig. 4 provides no information about the effects on density. Given that T and S have completely different units, there seems to be no

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basis for comparing the relative importance of these differences on their own. The fact that temperature differences are more scattered does not seem in any obvious way to support the conclusion that heat fluxes are less influential on these differences.

p. 7 l. 19: I think it would make sense to include the figure indicated by “Figure not shown”, as I think the changes in the ACC would be an important finding.

p. 7 l. 19-20: The discussion of Fig. 5 is so short that it is not at all clear what the figure is justified. I did not get any physical insight into the spatial pattern of freshening at the seafloor from the figure or the discussion here.

p. 8 Fig 4: The axis need descriptive labels including units. Tick mark labels should be larger. Caption should include the color of the curves (since figure will always be in color). There is no obvious reason that the x axes of the 3 panels are different, and this makes comparing the panels more difficult. The x axis is for both salinity and temperature differences? The depth axis should be inverted so that the deep ocean is down. It is also standard to have these depths be negative, indicating that they are elevations below sea level. What is the northern boundary of each of these regions? What longitudes separate them?

p. 9 Fig 5: This figure does not seem at all useful to me. The color contours are set such that all we can tell is the sign of the salinity difference (and that it is greater than -0.05 PSU) over the vast majority of the sea floor. A nonlinear color bar or one with many more contour values would be needed to make this figure at all useful.

p. 9 l. 5 “surface ocean”: A careful point has been made in the manuscript that the freshwater flux is not at the ocean surface, so this should probably be “upper ocean”.

p. 9 l. 5-p. 10 l. 2: This paragraph again refers to “anomalies”, whereas I would encourage you to use “differences”. Other than this small issue, I think this paragraph has some of the best analysis in the paper.

p. 9 l. 9-10: It’s not clear to me what the difference between the warm advection

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anomaly and the sarm SST anomaly is. It seems obvious that the one would cause the other but maybe I'm missing something.

p. 9 l. 10-11: "The cold water from BMR is advected by the ACC westward": A couple of things here, the ACC flows eastward (which seems to be the direction most of the cold difference is being advected) not westward. There is also the Antarctic Coastal Current (ACoC) that does flow westward on the continental shelf so maybe that's what is advecting a bit of the colder melt water to the west toward that SIT dipole?

p. 9 l. 15: I don't understand the cause of the increased SST near the sea-ice edge. Could you explain further why downwelling is associated with increased SST?

p. 9 l. 16: It seems worth exploring in more detail *how* the results from the two studies are different, not just to point out that they are different and that they are simulating different conditions (transient vs. quasi-steady; cavity geometry vs. no cavities for the "control").

p. 10 Fig 6: labels (tick marks, lat/lon, color bars) are all far too small. Please make them bigger and crisper. Please add more lines for lat and lon if possible so the reader can more easily find the lat/lon coordinates identified in the text.

p. 11 l. 9-10: It seems entirely backwards to me that including basal melting would decrease the MOC. If the model were correctly producing more AABW from ice-shelf melting and subsequent climate and topographic interactions, there should be an increase in downwelling just off the Ross continental shelf break and an associated increase in southward transport in the upper ocean (by conservation of mass). This should lead to an increased MOC strength. This is my understand of the main contribution of Antarctic climate dynamics to the global ocean circulation. To me, the decreased MOC in your simulations with melt fluxes suggest that something is wrong in the simulations and AABW is not being produced. This would not be surprising at coarse resolution, since ESMs have a very hard time producing AABW for the right reasons at CMIP-type resolutions.

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p. 11 l. 13: The formation and spreading of AABW should be the cause (not the effect) here. Changes in AABW formation should be driving the changes in the MOC.

p. 12 l. 4-6: I suggest you look further into these difference as part of this paper. It is precisely this kind of comparison with previous work that I feel is missing from this paper. Without more of this kind of validation work, it remains hard to trust the conclusions about the effects of melt fluxes on the ocean-sea ice system.

p. 12 Fig. 9: I find it very hard to tell what is going on with the difference contours. The color plot is quite clear in most regions but hard to discern near the Antarctic the contours are hard to get the sign of, let alone the magnitude in the Antarctic. Maybe the figure should give more space to the region from -90 to -60 (i.e. a nonlinear x axis).

p. 12 l. 14: “contributes to northward heat transport anomaly”: I find this confusing, since at least in the real world there should be a consistent southward transport of heat. In your simulations, you seem to see a mix of northward and southward transport do the “anomaly” is contributing to a reduction in southward heat transport at some latitudes and enhanced northward transport in others. Maybe “contributes to a reduction in southward heat transport”?

Also, this needs some discussion. Consistent with my concern about the MOC above, it seems like you should be seeing steady southward heat transport in both cases and that southward heat transport should be enhanced by AABW formation, whereas you are seeing a consistent global reduction (with varying behavior in each ocean basin).

The discussion of the individual basins is clearer in terms of describing enhanced or reduced transport.

p. 13 Fig 10: It would be helpful to compare the global MHT in 10a with observations, such as: Trenberth, Kevin E., and Julie M. Caron. “Estimates of Meridional Atmosphere and Ocean Heat Transports.” *Journal of Climate* 14, no. 16 (August 1, 2001): 3433–43. [https://doi.org/10.1175/1520-0442\(2001\)014<3433:EOMAAO>2.0.CO;2](https://doi.org/10.1175/1520-0442(2001)014<3433:EOMAAO>2.0.CO;2).

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Eyeballing the comparison, the global MHT isn't too bad north of 40S but it is odd that you are seeing significant *northward* transport of heat between 60S and 40S, which is not consistent with observations.

p. 13 Conclusion and discussion: Overall, I find that this is mostly just a summary of the results with insufficient interpretation of the findings, discussion of the implications of this work for other modeling efforts and/or the behavior of the “real world” and insufficient introspection about what the missing processes and other shortcomings of the work might be.

p. 13 l. 9: “profoundly”: This is a very subjective term and I'm not sure it is supported by the results. The differences between simulations with and without RIS melting are detectable to be sure but the changes generally seem to be subtle rather than profound.

p. 13 l. 9-p. 14 l. 2: My concerns about the “latent heat flux anomaly” and associated complexity of the temperature evolution remain the same as above. I do not think there has been sufficient analysis of the physical processes leading to the temperature evolution to conclude that they are even the result of the latent heat flux from ice-shelf melting. Instead, they are likely to result primarily from density changes, which are in turn primarily controlled by freshwater fluxes. Thus, I think the conclusion that the latent heat flux plays a secondary role is correct but I don't think anything presented in this manuscript has supported that conclusion directly.

p. 14 l. 3-9: The manuscript did not present the circulation from either EI or EN or make any attempts to compare these with observations, so it is difficult to know how much (if any) credence can be given to the difference in circulation between the two experiments. That being said, Again I find the discussion of the surface processes to be among the most useful analysis in the paper.

p. 14 l. 7: Again, the fact that basal melting stabilizes the water column and weakens overturning just seems to indicate that the processes we know to occur as part of AABW formation are missing from the model.

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p. 14 l. 10-13: The discussion of fixed ice-shelf area seems unrelated to the manuscript and its findings. There is nothing to suggest that having dynamic ice-sheet geometry in this configuration would enhance our understanding of the quasi-equilibrium state of the ice sheet-ocean-sea ice system because: 1) the resolution of the ocean model is very much insufficient to supply realistic melt patterns to drive ice-sheet evolution; 2) the steady-state melting, if consistent with present-day observed melting, would be unlikely to drive any significant ice-sheet evolution because melt rates under RIS are very small. 3) the context in which melt-driven ice sheet dynamics are interesting are precisely those that are *not* in quasi-equilibrium.

p. 14 l. 14-19: I appreciated this discussion of possible future directions for the research.

Typographical and Grammatical Corrections:

Title: The title would read better as “Modelling the effect of Ross Ice Shelf melting on the Southern Ocean in quasi-equilibrium”

p. 1 l. 6: “basal melting of Ross Ice Shelf” should be “basal melting of *the* Ross Ice Shelf”

p. 1 l. 19: remove “And, “. It is not necessary and is grammatically incorrect.

p. 1 l. 20: “accompanied accordingly”: This phrase doesn’t make sense. Perhaps you mean something like, “There is an accompanying northward anomaly in meridional heat transport at most latitudes of the global ocean”?

p. 1 l. 23: “Ices accumulated . . . are” should be “Ice accumulated . . . is”. Ice is only plural if there are multiple classes of ice or something along those lines, which doesn’t seem to be the case here.

p. 2 l. 11: 2 should be written out a “two”.

p. 2. l. 13: I suggest changing “regarding” to “of”.

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p. 2 l. 17: here and elsewhere “sub-ice shelf” should be “sub-ice-shelf”

p. 2 l. 19: “representation” should be “representations”

p. 2 l. 21: “parameterization should be “parameterized”

p. 2 l. 27: “In study such as modeling Ice Shelf melting effect on the Ocean...” this whole sentence is needs some significant grammatical work. Here’s my best guess at what is intended: “In studies that include the effect of ice-shelf melting on the ocean in quasi-equilibrium, it is necessary to use a global model with thermodynamically active ice-shelf cavities and to perform integration over hundreds of years”

p. 3 l. 13-14: “assuming the RIS being in steady state” should be “assuming the RIS to be in steady state”

p. 3 l. 17: “should be “to get *the* RIS draft”

p. 3 l. 29 and 31: “1ooo” should be “1000” (zeros, not o’s).

p. 3 l. 29: “To resolve the RIS vertically better” should be “To better vertically resolve the RIS”

p. 4 l. 3: “to that in” should be “to those in”

p. 4 l. 8-9: “and the Antarctic situates on the..” should be “with Antarctica situated on the..”

p. 4 l. 9: “the bathymetry of ocean around the Antarctica and cavity geometry of RIS is” should be “the ocean bathymetry around Antarctica and the cavity geometry of RIS are”

p. 4 l. 10: “grids” should be “grid cells” or “grid boxes”. To me, the whole 64x64 face is a grid.

p. 4 l. 11: “of which 15 having cavities and being calculated basal melting” should be something like “of which 15 have nonzero cavity thickness and include basal melt

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calculations”

p. 4 Fig. 1: “(a)” and “(b)” should go before the phrases describing each panel rather than after.

p. 4 Fig. 1: “yellow shades in (b) indicate grids where cavities...” should probably be “grid boxes shaded light green indicate locations where cavities...”. (To my eyes, the shading is light green, not yellow.)

p. 5 l. 15: “modelling ice shelf” should be “modeled ice shelf”. “lateral boundary” should be “lateral boundaries”.

p. 5 Fig 2: The tick mark labels on the axes are too small to easily read. The melt-rate values are also somewhat small but perhaps large enough to read (but I see no reason to include so many empty cells around the 15 active cells. The 3 panels probably will need to be combined into a single figure for typesetting but I guess that’s up to you and the journal to work out.

p. 5 Fig 2: “annual mean areal average” should probably be something like “the annual and area mean”; “for the last 100 years’ mean” should be “for the mean over the last 100 years”; “areal mean averaged over the last 100 years” might be clearer as “averaged over the ice-shelf area and the last 100 years”.

p. 6 l. 6: “cold and fresh water are” should be “cold and fresh water *is*”

p. 6 l. 7: “become” should be “becomes”

p. 6 l. 7: “compared its counterpart” should be “compared *to* its counterpart”

p. 6 Fig 3: The axis labels should be more descriptive (not variable names) and should include units.

p. 7 l. 1: “Figure 3. Figure 3.” should just be “Figure 3”

p. 7 l. 6-20: There is no need to continually reference Fig. 4 here. It is clear that most

of this text refers to that figure.

p. 7 l. 7: “from ocean surface” should be “from *the* ocean surface”

p. 7 l. 9: “freshening effect” should be “*the* freshening effect”

p. 7 l. 16: why 1005 m instead of just 1000 m?

p. 7 l. 17-18: “This is due to that”: this phrase is kind of confusing. I would suggest something like “This is due to the relatively stronger...at that level, which constrains...”

p. 7 l. 19: “water in most area” should be “water in most of the area”

p. 9 l. 16 and p. 10 l. 1: “the work” should be “this work”

p. 10 l. 12: “BMR effect” should be “the BMR effect” (or maybe “the Ross BM effect”, see earlier comment).

p. 11 l. 1: No need to reference Fig. 8 again.

p. 11 l. 2: “Figures not shown” should just be “not shown”

p. 11 l. 3: “motion field” should be “flow field”

p. 11 l. 8: “by meridional transport” should be “by a meridional transport” or “by the meridional transport”

p. 11 l. 10 “here” should be lowercase or this should be made a separate sentence in parentheses (though The Cryosphere’s typographic editors discourage theses)

p. 11 l. 15 “the path” should be lowercase

p. 11 l. 15 “it’s” should be “it is”

p. 12 l. 2 “the calculation” should be lowercase

p. 12 Fig. 9: Axes need labels including units. All labels are too small to be readable.

p. 12 l. 14: “contributes to northward” should be “contributes to the northward”

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p. 13 Fig 10: the customary way of handling multiple y axes is to put one axis on the left of the figure and the other on the right. It is even more helpful if the axes are the same color as the curves they correspond with. As in all figures, the tick mark labels are far too small.

p. 14 l. 7: “stables” should be “stabilizes”

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-228>, 2017.

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