

Review of Nakayama et al. "Impact of West Antarctic Ice Shelf melting on the Southern Ocean Hydrography"

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:

Main points:

- Freshwater from the Amundsen and Bellingshausen (AB) Seas is shown to reach the Ross continental shelf in XXX years, the deeper Ross Sea within XXX years, the region near the Amery Ice Shelf after XXX years and the Weddell Sea in XXX years.
- For the most significant amounts of melting, on the order of 10 times currently observed melt rates in the AB region, freshwater reaches the Weddell Sea much more quickly (~10 years into the simulation) and the amount of freshwater reaching the Weddell continental shelf is enough to reduce the salinity there by a non-negligible amount.
- In simulations with AB melt rates comparable to or less than present-day, meltwater may reach the Weddell Sea after ~30 years but its impact on salinity are difficult to distinguish from temporal (and perhaps ensemble) variability
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I get the impression in several places in the text that the experiments were designed (and perhaps the manuscript was originally written) with LMELT as the intended control experiment, and that perhaps a later decision was made that CTRL should be the control because its melt rates are most comparable to observations. Differences are repeatedly taken with respect to LMELT, rather than CTRL and the coefficients used in LMELT are stated to be the ones commonly used in other FESOM simulations. I would urge you to consider explicitly renaming LMELT to CTRL and CTRL to something else because this would seem more consistent with the manuscript as written. Several specific comments point out in more detail where this inconsistency arises.

The manuscript seems short for TC, especially the discussion section (see below). It sometimes reads as if it were intended for a journal that requires a shorter page count. This may explain why several tables that are referenced repeatedly in the text are included in the supplement rather than the main manuscript. I am not aware of a limit on tables or figures that require putting so many tables in the supplement. In particular, values from Table S4 are repeated (in multiple places) in the text, which would presumably not be necessary if that table were in the main text.

Speaking of which, there is a lot of redundancy both within the text vs. in tables and between the results, discussion, and conclusion sections. I have pointed out where I find this redundancy in the specific comments. This redundancy comes at the expense of what could

have been a broader discussion of the results that synthesizes the findings in a somewhat more qualitative fashion and talks about their broader implications based on observed and projected changes in AB melting, impacts of freshening on both the Ross continental shelf, deeper Ross Sea and elsewhere, etc.

Colormaps are not very intuitive and are not friendly to readers with color blindness. The manuscript preparation guidelines include the following: “For maps and charts, please keep colour blindness in mind and avoid the parallel usage of green and red. For a list of colour scales that are illegible to a significant number of readers, please visit ColorBrewer 2.0.” In addition to concerns about color blindness, the colormaps used in this manuscript suffer from alternate banding of bright and dark colors that make it difficult for a reader to intuitively tell higher from lower values of the field. (In the terminology of color theory, they are not perceptually uniform). I would recommend that you consider using perceptually uniform colormaps such as those from cmocean (<https://matplotlib.org/cmocean/>) or Scientific Colour Maps (<http://www.fabiocramer.ch/colourmaps.php>). The colormap in Fig. 2a, b is the only one in the paper that seems reasonably perceptually uniform. I believe these colormaps are available in a format that can be imported into ParaView, the tool that I’m pretty sure you are using for this visualization.

I submitted my review well after Reviewer #1’s review became available and I feel the need to reiterate a point that she or he made. I fully agree that the paper does not sufficiently discuss the implications of changing heat- and salt-transfer coefficients to vary melt rates. Previous work, cited in this manuscript, have adjusted these coefficients and explored the sensitivity of AS and BS melting to these parameters. But these previous simulations did not, in my understanding, use adjustment of these parameters to change melt rates as a proxy for physical changes in the ocean state (e.g. ocean warming or thermocline shoaling). The implications of using parameter tuning to force melting needs some more discussion. One part of this discussion could presumably be that this approach makes it possible to explore changes to the ocean state (reduced salinity in this case) without complicating the simulation with other changes in state (e.g. changes in surface forcing) that would also impact the ocean state.

A small note: The Cryosphere no longer requires, at least to the best of my knowledge, that the figures and tables be placed at the end of the text during the review process. My request for future manuscripts would be that you include the figures in the text during review and move them to the end only at the point where typesetting occurs (if requested). I review manuscripts electronically and flipping back between the text and the figures and captions gets quite tedious, even more so when I also have to flip back and forth between the main text and the supplement.

Specific Comments:

I. 60: “the coefficients are chosen following previous studies”: The values for these coefficients are never explicitly stated.

I. 61: “while they are set to 3-times larger values for the CTRL case”: As mentioned above, it isn’t clear why you chose this to be the control. If this was chosen because melt rates match observations better than for your other simulations, it would be important to state this. Also, as Reviewer #1 points out, it would be somewhat troubling if these larger values of the coefficients are required to compensate for a cooler-than-observed ocean state in this region. If this is the case, it would be worthy of discussion if not, it would be worth discussing why the values used in previous simulations are not the appropriate ones in this case.

I. 62: “is a convenient way to force the ocean model”: As I mentioned in the general comments, I think this approach is okay for showing the sensitivity of melting to unknown parameters but shouldn’t be treated as an easy substitute for ocean warming, increased inflow of CDW, thermocline shoaling, etc. This needs some more discussion either here or in the discussion section.

I. 71: “(Mazloff et al., 2010; Renault et al., 2011)”: Could you quote the observed values (preferably with uncertainties) from these sources? Otherwise, it’s hard for the unacquainted TC reader to know how reasonable FESOM’s Drake Passage transport is.

I. 73-74, 76: “The bottom temperature on the continental shelf is mostly close to the freezing point except for regions with CDW intrusions onto the AS and BS continental shelves (Figs. 2 and S1)”: I guess Fig. S1 is included here because a reader could be expected to deduce from C in Fig. 2 and C - L in Fig. S1 what L would look like, but this seems a little too indirect to me. I would remove the reference to Fig. S1. Similarly for the reference to Fig. S1 on I. 76.

I. 76: “These features are present both in the observations and the model results”: The salinity gradient you talk about in AS and BS seems to me to be much more visible in the model results than the observations.

Also, it seems like this is a good place for a discussion of features are not being captured well by the model and what their implications might be.

I. 87: I think Reviewer #1 may have also pointed this out, but Fig. 3 shows bottom salinity, so it’s a bit confusing that the text refers to this figure when comparing salinity at 200 m depth.

I. 101: “Despite [being] underestimated by ~50% in magnitude..”: Are these really underestimated? You’re simulating a different process, changing coefficients in the melt parameterization that lead to an instantaneous jump in meltwater production, than the melt increase seen in observations, so would you expect quantitative agreement with observations? I would reword this to make clear that the process is different and the additional meltwater you see simply is about half of that seen over the last 50 years without stating anything about underestimating.

I. 108-109: “...introduced with heat and salt transfer coefficients being set to 2-times and

30-times larger values, respectively.” If you continue to use your current CTRL, these values should be compared with it instead of LMELT (i.e. $\frac{2}{3}$ and 10 times the CTRL values, respectively). However, you are clearly treating LMELT as the control experiment throughout this section.

I. 111: “We subtract the LMELT results from MMELT, CTRL, and HMELT”: Again, you are using LMELT as the control run.

I. 114-115: “For MMELT-LMELT, the salinity decrease is confined mostly to the AS, BS, and RS continental shelves with a freshening of 0.025 g kg⁻¹ and 0.0030 g kg⁻¹ for the RS continental shelf and RS bottom basin, respectively (Table S4).” This seems like a restatement of the table. As stated below, this table would make more sense if it were moved into the text and the text were modified to provide a broader explanation of the implications of these numbers rather than just restating them.

I. 118, 120: “...WS with values of 0.045, 0.0048, 0.0078, and 0.0035 for the RS shelf...” and “...CTRL case amounting to 0.14, 0.0015, 0.035, and 0.016 for the RS shelf...”: These numbers need units and would be better left in the table rather than repeated in the text, as in my previous comment. Instead, the text should presumably discuss the implications of these numbers in a more qualitative way.

I. 132: “could be strongly affected”: Could you please elaborate on what you mean by “strongly affected”? What would the effects be? The discussion is currently rather thin but expanding on these effects would help to flesh it out.

I. 139-140 and the remaining paragraph: “We also note that magnitudes of freshening caused by glacial meltwater from ice shelves in the AS and BS **represent linear and nonlinear behaviors.**” I think this end phrase is unnecessarily vague. It would be much better, and would flesh out the discussion more, if you discussed what these linear and nonlinear behaviors are rather than simply calling them behaviors. You go on to state the quantitative amounts of melting under various conditions (again, numbers better left to a table) but you do not explain clearly what the linear behaviors are and differentiate them from the nonlinear ones, nor do you explore why some are linear while others are nonlinear. The discussion section is not an appropriate place for the long lists of numbers you have here. These should only be in tables, and should be referred to in the results section, whereas the discussion section should focus on a more qualitative synthesis of the results and their broader implications.

I. 159-160: “We also show that magnitudes of freshening caused by glacial meltwater from the AS and BS represent linear and nonlinear behaviors”: This statement isn’t very useful to the reader. Can you talk about what these linear and nonlinear behaviors are?

I. 164-165, 174-175: “upon request”, “The model code, processing tools, and raw model output are difficult to make publicly available, and the authors recommend contacting the corresponding author for those interested in accessing the data.”: I would *strongly* encourage you to work out the logistics of making the specific code (not just a repository but

a specific DOI on a site like Zenodo) available. Having code available only on request really hampers open science and model development. Not recording in the paper exactly which version of the code was used further hampers reproducibility. I realize this is a bit of extra work and sometimes requires getting permission from the developers but it is worth the effort to the broader community. I ask you to reconsider.

I realize that data sets are harder to make available but I would encourage you to see if a database like Pangaea, Open Science Framework or the Earth System Grid might be an appropriate place to host your data in a public forum. Again, the lack of data availability really sets back open science.

Figs. 1, 2c,d, 3, 4, and 5: As mentioned in the general comments, I would encourage the authors to look at alternative, perceptually uniform colormaps.

Fig. 2a, b: If you end up switching to using LMELT as the reference simulation as I have recommended, it may be more appropriate to plot that one here. Also, the obs. plots are really tiny and hard to compare. It would be helpful to have bias plots (CTRL - Obs) in addition.

Fig. 2c, d: It would likely make sense to move these panels to another figure to make more room for expanding the first two panels of this figure.

Fig. 3: As in the text, it seems like you are treating the LMELT as the control run here, since differences are C-L, not L-C.

Table S1: It seems like the ice shelf-ocean drag coefficient is missing. I would also suggest putting the control values of the heat- and salt-transfer coefficients here. Are there any other parameter values related to the ice shelf-ocean interface or boundary layer that you did not include? Is there a good reason this should be in the supplement rather than the main text? I don't have strong feelings either way, but as a reader I often don't download the supplement if I don't need to.

Table S2: This data seems too simple to be worth having a table, and the text repeated in each entry seems unnecessary.

Table S3: As Reviewer 1 pointed out, references 16 and 17 need to be replaced with the proper reference or shortcuts of some kind. Maybe these were the numbers of the references when this paper was submitted to another journal with another citation format?

Table S4: This table belongs in the main manuscript. This is also a case where LMELT, rather than CTRL, seems to be treated as the control run. The formatting of the leftmost column is hard to follow as the spacing between adjacent lines in the same entry and between table rows is the same (hopefully typesetting will fix this).

Figs. S1 and S2: Once again, by taking CTRL - LMELT rather than the other way around, it seems like LMELT is the control run.

Typographical and grammatical corrections:

I. 13: “based on satellite-based” sounds a bit redundant so I’d suggest changing “based on” to something like “as shown by”

I. 19: “Their” probably refers to ice shelves in AS and BS, but this is not entirely clear from the context, so I would make this explicit.

I. 19: The comma after “observations” should be removed.

I. 21: “evidences” should be something like “lines of evidence”

I. 31: “Kusahara and Hasumi (2014); Dinniman et al. (2016); Kusahara et al. (2017)”: I don’t think explicit (“citet” in LaTeX) citations should be combined in this way. I would reword the sentence so these become parenthetical citations, e.g. “Using circum-Antarctic or global domains, several studies (Kusahara and Hasumi 2014; Dinniman et al. 2016; Kusahara et al. 2017) also showed...” If you want to keep them more as they are, I think you need to put “and” before “Kusahara et al.”.

I. 34: “are developed” should be “have been developed”

I. 38: “(FESOM) (Timmermann...” should be “(FESOM; Timmermann...”

I. 61: “3-times” should be “three times”

I. 76: “both in” should be “in both”

I. 80-81: “(Depoorter et al., 2013; Rignot et al., 2013) (Table S3)” might be more cleanly formatted as “(Table S3; Depoorter et al., 2013; Rignot et al., 2013)” or by rephrasing so that the satellite estimates are mentioned earlier (with references) than the CTRL results.

I. 83: “LMELT case” should either be “the LMELT case” or just “LMELT”. I would suggest rephrasing this whole sentence: “...may represent better the melt rates ~~at that the time~~ in the middle of the last century”

I. 84: “largely” should be something like “significantly”

I. 85: “flown” should be “flowed”

I. 89-90: The citations would be better formatted as “(Fig. 2; Nakayama et al., 2014; Dinniman et al., 2016)”

I. 97: “RS dense shelf water observed for about 50 years shows” should be something like “Fifty years of observations of RS dense shelf water show...”

I. 99: “(RSBW) (Purkey and Johnson, 2013)” should be “(RSBW; Purkey and Johnson, 2013)”, although this is confusing since the citation isn’t about RSBW but rather its warming and freshening so it might be best to reword the sentence so the citations and the abbreviation can be separated.

I. 100: “RSBW shows warming and freshening of ~ 0.1 ° C and ~ 0.01 g kg⁻¹, respectively” might be better as “RSBW experiences a ~ 0.1 ° C warming and a ~ 0.01 g kg⁻¹ freshening”. (Sorry for the formatting.)

I. 101: “Despite underestimated by $\sim 50\%$ in magnitude”: something is missing here and this should maybe be “Despite being underestimated by $\sim 50\%$ in magnitude”

I. 107: “focusing on both small (200-m depth) and large (bottom) depths”: I find this wording confusing and I think it would work just as well as “focusing on both 200 meters depth and the sea floor” (or “ocean bottom” if you prefer).

I. 108: “introduced with heat and salt transfer coefficients ~~being~~ set to...”

I. 132: no comma is needed after “affected”

I. 134-135: “...the idea presented by (e.g. Beckmann and Timmermann, 2001)”: This should be “...the idea presented by e.g. Beckmann and Timmermann (2001)”. You might want “...by, e.g., Beckmann...” but I don’t think the commas are required.

I. 137-138: “However, considering the magnitude of the salinity decrease in the CTRL experiment, circum-Antarctic freshening could ~~possibly~~ be ~~possibly—undergoing~~ ~~be~~ ~~underway~~.”

I. 141: “enhances” should be “is enhanced”

I. 154: “existing” would be better as something like “recent”

I. 155: “We further show...propagates further downstream”: It’s a little jarring to have “further” twice in this sentence so I’d suggest starting with something like “In addition, we show”

I. 166: “All authors commented ~~on~~ the manuscript.”