

Interactive comment on “21st century ocean forcing of the Greenland Ice Sheet for modeling of sea level contribution” by Donald A. Slater et al.

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The paper investigates the effect of two different parametrisations for ice/ocean interaction, specifically at Greenland's glacier termini, in the context of future ocean/atmospheric conditions as predicted by a range of climate forecast models. This is part of a wider community effort to adequately couple ice sheet models with coupled (ocean/atmosphere) climate models. While both parametrisations consistently predict greatly increased mass loss from the Greenland Ice Sheet under a high greenhouse gas emission forcing regime, the spatial distribution of mass loss varies depending on which climate model is used.

The paper is mostly well written with a clear, direct message and nice figures. I like

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the ethos of finding a workable solution to a tough problem at hand and helping the wider community. However, as the authors acknowledge, many aspects of the physical environment are not considered. I felt that this paper really highlighted that major obstacles must yet be overcome before we can expect models to predict future mass loss accurately. I therefore think the results, while certainly valuable, should be interpreted qualitatively rather than quantitatively.

Major comments

1. Thermal forcing, TF, is very simplistic. The authors do well to flag up the shortcomings in section 4.3, but nonetheless there are major shortcomings. The empirical tuning might alleviate this to some extent, but I would still expect the omission of these processes to result in large uncertainties. I understand that sheer necessity offsets these issues to some extent, as the next generation of climate models require parametrisations such as the ones presented here. But I think that any quantitative conclusions about future sea level drawn from those models (which will undoubtedly be very high-impact results) should come with the footnote that ice-ocean parametrisation is still very basic. This is not a criticism of the authors: it's hard to see where major advances will come without much higher resolution AOGCMs.
2. Using annual mean temperature is inappropriate when melt is nonlinear in TF (equation 1). Mean melt is not equal to melt calculated from mean TF. The effect is likely small as the exponent is close to one, but it will result in a systematic error.
3. My understand is that, if $dL = \text{melting} + \text{calving}$, retreat represents both terms while submarine melting represents only the first term. This should be made more explicit earlier on. Some of the language makes it a bit unclear what the inputs and outputs are for each parametrisations, and can seem at odds with Equations 1 and 2. I comment below on the specific instances of this.
4. Using EN4 for bias correction makes sense in theory, but do you have a sense of how many direct observations actually influence the EN4 gridded product for the

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regions/times of interest? EN4 has had issues in the Labrador Sea, and the EN4 temperature profile (Fig3c) is not a good representation of typical SE Greenland stratification (there should be a subsurface temperature maximum). You could add a figure in the supplementary material showing the mean EN4 confidence weightings for each ocean sector. Could bias correction be done instead using the available CTD profiles from each sector?

Minor comments

Some of these are stylistic comments which the authors are entitled to disagree with.

P1L8: It's misleading to say that retreat is a function of submarine melting, do you mean subglacial runoff? I read this to mean that one parametrisation feeds into the other. This is related to major comment 3.

P1L9: You should give RCP2.6 and 8.5 formal definitions, if not here then in the introduction or methods.

P2L21: Can you be more quantitative about the number of ice shelves than "a handful"?

P2L25: Perhaps also worth mentioning here that since these regions are very poorly observed, especially in winter, large uncertainties remain with regards to Greenland fjord/shelf processes (i.e. while you correctly state that these processes are not captured in models, we still don't know exactly what we are trying to capture!).

P3L13: I would consider moving this first paragraph to the introduction. I see that it leads nicely into the second paragraph in 2.1, nonetheless when I finished reading the nice introduction it was frustrating to find myself reading what was essentially just more introduction.

P4L1: If submarine melt rate is denoted by \dot{m} and dL is linear in submarine melt, then should this not be made explicit in the expression for dL ? Otherwise, perhaps more careful language should be used. Again this ties into major comment 3.

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P4L14: Personally I don't like multiplication signs in formulae, and I think it would read better if you dropped them.

P4L16: Refer to section 2.3.1 instead of "further below".

P4L22: I'd change "even in future projections" to "particularly in future projections" since one would anticipate annual and summer means to converge as summer becomes longer.

P5L25: I really like this section on bias correction. Very clearly thought out and explained. It might be worth citing Menary et al. 2015 GRL, who explore CMIP5 temperature and salinity biases in the Labrador Sea west of Greenland, to underline your motivation.

P10L5: I understand that certain simplifications are necessary for these parametrisations to work in coarse climate models, but this paragraph completely ignores a lot of the research into fjord/shelf hydrodynamics. Much of these shortcomings are acknowledged later in section 4.3, but I think they should be made clear up front.

P10L22: If TF used in equation 1 differs from the TF used in equation 2 then perhaps they should be given different symbols or subscripts.

P10L31: See major point 2.

P12L19: I'd remove the word "however" as it isn't necessary.

P13L18: This appears to be a strong argument for using more than one RCP2.6 model in your experiment.

P15L23: This seems to imply the thermal forcing is an input for the submarine melt regime only, when in fact it is an input for both. These two sentences could be rewritten to make it absolutely clear what the input and output variables are for each regime.

P15L25: Change "...as they see fit" to "... as required" or similar, to avoid referring to a model as "they".

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P15L25: Sentence starting “Each implementation...”: This sentence is really jarring and frankly bizarre. If it wasn’t interesting you wouldn’t be writing a paper on it!

P16L31: Typo, “large uncertainty in...”

P18L3: Even without dense overflows, the properties of the water trapped behind the sill can (and will) be modified by downward mixing of buoyancy from the upper layers.

P18L14: The paragraph could also mention wind-driven heat delivery via the internal wave field, which has been found to deliver ocean heat to fjords in Greenland. Also, ideally the submarine melt parametrisation would capture (horizontal) ocean current speed adjacent to glacier termini, which we know is related to e.g. fjord width (i.e. Jackson et al. 2018) and impacts melting.

Fig3c: EN4 temperature profile looks suspect, what are the EN4 confidence weightings here? (major point 4)

Fig6a: Is this figure saying that in 2100, ocean water will have flooded beneath the interior if the ice sheet? If so, this is a major result which should be flagged up in the text.

Fig9: To me negative retreat implies advance, so I’d change either the axis label (to “frontal position”?) or sign. Figure 10 uses positive values to denote mass loss, it’d be better if they were consistent.

Fig10f: There’s a missing dot above the m labelling the y-axis.

Overall, an important step towards the goal of coupled air-sea-ice climate models (but there is still a way to go).

Kind Regards,

Neil Fraser

Please also note the supplement to this comment:

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<https://www.the-cryosphere-discuss.net/tc-2019-222/tc-2019-222-RC2-supplement.pdf>

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-222>, 2019.