Review of: Past and future response of Greenland's tidewater glaciers to submarine melting

General comments

This paper presents a new parameterization for Greenland marine-terminating glacier retreat based on subglacial runoff and ocean thermal forcing. The authors use data from 1960-present to develop and test the parameterization, and model projections to 2100 to examine the parameterization projections to 2100. Overall, the paper is well written and easy to follow. Data and methods are explained in appropriate detail with useful and clear figures. Apart from the comments below the paper is thorough in discussing the assumptions, limits, opportunities, and other research related to this contribution. The paper will be a valuable reference for ice sheet modelers and the cryosphere community, particularly given that it outlines the recommended parameterization method for ISMIP6. It also provides an excellent discussion of the methods and opportunities for improvement, and the paper will be an important launching point for further research. Progress on incorporating ice sheet-ocean interaction into ice sheet models is critical, and the step provided in this paper is welcome.

I do have some concerns about discussion of projections made in the paper and think that adjustments are needed in the discussion and messaging to ensure that the authors are not overstepping the limits of the science. Throughout the paper the authors emphasize that there is a statistically significant relationship between terminus change and submarine melt (here $Q^{0.4}$TF), but Figure 7b also highlights how tenuous this relationship is, and the authors results themselves also demonstrate this (e.g., how much of terminus change is explained by submarine melt, etc.). It is clear that the authors are well aware of this, as they emphasize multiple times that the parameterization is best for groups of glaciers (or even an ice-sheet-side grouping), and that there are other components of the system, like topography, that may play a similar or larger role in determining retreat. Despite these statements, however, the authors still move forward on including projections for individual glaciers as well as ice-sheet-wide projections regarding retreat. These items are misleading and likely to cause confusion and misuse by readers who are not as familiar with the details of marine-terminating systems or are coming to the paper specifically for this projected information but with little attention to the paper details. By including these details (inappropriately I suggest), the authors are providing projections that can too easily be taken out of context or cited without inclusion of the nuances and caveats that appear in this manuscript. My feeling is that because there is a statistically significant relationship, the authors have in several instances pushed their statements beyond what is actually justified by the data (and especially considering that the parameterization does not account for other major known influences, like topography). There are two areas that are particularly problematic:

1) The title. The main point of this paper is a new parameterization. The paper is not providing new and improved details on past glacier response to submarine melting nor future projections with a confidence level that warrants the current title. A more appropriate title would be something along the lines of: 'New parameterization for incorporating Greenland tidewater glacier response to submarine melt in ice sheet

models' or 'Parameterizing Greenland tidewater glacier response to submarine melting for improved ice sheet projections'

2) Figure 10 and related discussion of projections. First, labeling on Figure 10 is misleading. 'Helheim-like' (and similar) has little functional meaning – like Helheim in terms of climate, bed topography, shape, ice flux, etc.? While the caption points out that these are projections that are more appropriately commenting on regional glacier behavior, the figure and elements of the discussion (e.g., 16/15 specifically states 'retreat for Helheim…') do not reflect this. Second, given the limits to the technique, I find little justification for discussing projections of individual glaciers – or even of ice-sheet-wide glaciers - in any context. Using this parameterization alone to project future glacier behavior (e.g, 17/15) is problematic.

Also, the broader issue of overstating the conclusions in ways that do not reflect the full limits of the parameterization does rear its head in a few other places, and I have tried to capture those in the specific comments below. Once these elements of the discussion align appropriately with the actual skill (and known limits) of the parameterization, this paper will be an excellent addition to The Cryosphere.

One other general comment – I would like to see some discussion regarding the assumption that surface topography is unchanged. This assumption is made for developing the parameterization from past data, and in projecting future changes. Substantial peripheral thinning across Greenlandis well documented, however, and increasing future runoff suggests that this trend will continue or accelerate. Ice thickness is an important component for retreat, and I am concerned that without considering it in future projections, the authors are omitting an important system change, but it is not as well acknowledged or discussed as some other omitted factors.

Specific comments (by page/line number)

4/29. Specify if you are using the mean, weighted mean, etc. over the 5-year bins.

8/18. Label Kangerdlugssuaq in one of the figures.

8/24+. It is a bit confusing here to start by saying runoff and TF are estimated using MIROC5, but then explain that runoff is actually from MAR forced by MIROC5. Better to simplify and streamline here by going straight for – runoff from MAR forced by MIROC5 and TF from MIROC5.

11/14. Changing 'but' to 'except' makes this sentence easier to understand.

15/17. Consider including plots related to the RCP2.6 scenario in the supplementary material. These can be valuable for communicating the substantial differences in retreat under an RCP2.6 v. RCP8.5 scenario.

13/30. In essence, this parameterization assumes that the *setting* of a region of glaciers – and by this I mean all of the other elements influencing behavior that are outside of submarine melt – will remain the same in the future as it has been in the past. This is what provides regional or ice sheet scale confidence for using the parameterization in projections. I think it would be appropriate for the authors to more clearly acknowledge this. Given the importance of

topography for short-term AND long-term retreat behavior, it is important to raise this issue as intrinsically tied to the assumptions made regarding future terminus change.

17/11. Given the parameterization, 'similar-sized glaciers' is more correctly 'similar-sized glacier basins'.

18/11. While bed topography does control short-term variability, it is also unquestionably a major factor in determining long-term change when it comes to patterns of retreat. For example, the multi-centurial behavior a glacier that extends into a long overdeepening (e.g., Jakobshavn) will be dramatically different than a glacier that may retreat onto shallow topography or even become land-terminating.

18/32. Thinning may also occur directly at the terminus from SMB changes.

19/3-5. This sentence is overly confident. Perhaps something like: 'The principal advantages of the parameterization are its simplicity and context provided via empirical validation, thus the critical interaction of the ice sheet with the ocean can be represented in a manner which is informed by observations and scales to large region and ice-sheet-wide applications.'

21/10. Delete 'behind'

21/12 & 17, 22/3. Consider removing 'huge' in all cases.

22/20. Change 'and' to 'of'

22/31. It is very odd to say " 'observations' " – the reader is left to wonder about these quote unquote 'observations'. Instead, give the more detailed information on what is used for the comparison.

23/25-32. Rather than just say at the end of the paragraph that it 'is also more consistent with the focus on populations of glaciers rather than individual glaciers in this study', it would be appropriate to state clearly mid-section that the parameterization is not appropriate for use in projecting individual glacier behavior.

24/15+. The concern and fix included here re: using flux-weighted means should be included in the main text.

Figure 1. Nowhere in the paper do you discuss the implications for using different models for past forcing v. future forcing. You note that the time series are aligned through bias adjustment, but I'd like to know more about the effect of using one set of data for creating the parameterization and another set for projecting. Given that the parameterization will likely be used in a wide set of models using a variety of climate forcing data sources, does use of a single set for establishing the parameterization create any limits in confidence?

Figure 3. Include in parentheses the data source for the bathymetry. Also change 'TF' to '(TF)' in first line of caption.

Figure 6. There is no longer an (a), (b), and (c) panel. Caption needs correction.

Figure S4. Make this into two rows of three and increase the size so that it's easier to see.

Figure S5. Suggest removing panel (c).