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The Cryosphere
Discussions

Interactive comment on "Coupled modelling of subglacial hydrology and calving-front melting at Store Glacier, West Greenland" *by* Samuel J. Cook et al.

Anonymous Referee #2

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This is a potentially interesting paper describing a modelling study of sub-glacial water flow beneath Store Glacier and its impact on plume activity at the calving front. The subject matter lies clearly within the scope of the Cryosphere, and the study is novel in its combination of sub-glacial hydrology and buoyant plume theory. The links have been discussed extensively in the literature, but have not previously been quantitatively linked in the way that they have in this study.

Despite its clear merits, I think that some further work is required on the manuscript before it can be accepted for publication. The main weakness is that the scientific motivation for the study is never clearly presented or addressed. This issue is apparent even in the title, which focuses on the technical achievement of running the model rather than the findings of the experiments. Throughout, the presentation focuses on detailed numerical outputs (often quoted to 3 significant figures), but nowhere is there an assessment of how reliable those numbers are, either through a detailed comparison with observation or a study of model sensitivity to parameter choice. So the reader (this one at least) is left puzzled as to what the key messages are. The qualitative results that surface melting drives a more active sub-glacial hydrological system, that there is a delay as the increased supply of water works its way through the system to emerge at the grounding line, and that increased discharge there drives higher melting along the calving front, are all well known. If my interest is in the details of Store Glacier during the two summers studied, I could potentially use the quantitative results, but then I really need that missing assessment of how well-constrained the numbers are. If my interest is in the broader subject of sub-glacial drainage and its role in stimulating melting along the calving front, how do these numerical results help me? The authors really need to think through what they want readers to take from this study. If it is nothing more than the demonstration of a working model, perhaps the paper should be recast as a more thorough description and critical assessment of that model. Such a paper might be more appropriate for a journal such as Geoscientific Model Development.

More detailed comments:

Page 1, line 16: Here and in a number of other places the authors comment on the "common assumption" that sub-glacial discharge at a tidewater glacier terminus falls to zero in winter. I was not aware of that being a common assumption. Indeed plume models tend to require a non-zero outflow as an initial condition, so all the studies I know of that are based on plume theory have a background flow in winter by default. Perhaps the authors could provide specific citations to the studies that assume zero outflow?

Page 6, line 5: I'm not sure I follow these equations. Are they specific to the calving front? In that case shouldn't the density in (3) be the seawater density?

Table 1: How were these parameter values chosen? How sensitive are your results to those choices?

Figure 2: I think this figure would be more informative if one panel showed temperature in winter and summer (on the same scale), while the other showed salinity in winter and summer.

Page 12, lines 1-4: I don't agree with this statement. If the bed topography controlled the hydraulic potential gradient, the water would pool in the deepest part of the bed.

Results: The text in this section could be reduced significantly. Much of it presents information that is readily available in Table 3. The reader should simply be told why those numbers are of interest. A detailed quantification of their relative sizes is unnecessary.

Figure 4: You never say whether the "Daily" results are from the end of the summer, or the time of peak meltwater input, or perhaps the time of peak meltwater discharge.

Page 18-19: This is one of the few places where any comparison with observation is made. The results do not compare very well: the grounding line flux is at the low end, but the calving front melt rate is an order of magnitude smaller. However, the observational numbers appear to be poorly constrained. When were the observations made? Were there no analogous measurements made in summer? A little more discussion of is called for.

Page 20, lines 16-19: Another very cursory comparison with observation. Is the nature of the drainage system the only result that can be used to validate the model? It would help to plot the location of the observations in Figure 5.

**Page 22, lines 5-6: Aren't these also results that could be compared with observation? Are sediment-laden plumes seen in satellite imagery obtained in the summer months? How do the times and locations of their appearance compare with your model

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results?

Page 22, lines 25-27: The sub-linear relationships discussed are between sub-glacial discharge and melting. I am not aware of any study that has related surface melting (except in the average sense) directly to sub-glacial melting for the simple reason that there is a delay before surface melt emerges at the grounding line. Your Figure 10 does not take this into account, so I'm not sure what the point of showing it is. If you related discharge to melt, I assume you would see the same sort of relationship as others, since it would be a product of your plume model. More useful would be if you could show some sort of correlation between surface and submarine melt with some simple processing (maybe smoothing and a lag) applied to the surface melt signal. That would be a step towards a simple parameterisation of the overall impact of the sub-glacial hydrological network.

Figure 12: You show the differences between the results obtained with a coarse and a fine model grid, but has the solution converged on the finer mesh, or would further refinement give different results again?

Page 25-26: There is a suggestion here that the agreement between modelled and observed plume locations (see ** above) is poor. It would be more honest to actually show this comparison, especially if the discrepancy can be explained. But if explanation is the "unrealistic" grounding line, why not use a more realistic one?

Page 26, lines 13-14: There is a mention of parameter uncertainty in the plume models, but no mention here or anywhere else about parameter uncertainty in the sub-glacial hydrology model.

Page 26, lines 22-24: This claim is really not supported by the paper. There is very little comparison with observation, and the comparisons that are made show significant discrepancies. That is a major issue with the paper.

Page 27, lines 3-8: Are these potentially testable results? If you have measurements

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of water properties in the fjord, can you diagnose the relative inputs of sub-glacial meltwater versus that produced by melting of the calving front?

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