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

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To round off the discussion on this paper, we have listed the specific comments from both referees, as well as any general comments not covered by one or more specific comments, below, and provided our response to each one. Each block of text consists of an editorial comment followed immediately by our response to it.

Some useful comparisons between model output and independent data are made in the present text (e.g. comparing modelled winter frontal melt rates with the results of Chauché (p. 18); and comparison of the modelled basal water pressures and channel extent with the work of Doyle et al. and Young et al. (p. 20)). More should be made

of these comparisons, and I suggest expanding these sections to provide more detail on the success (or limitations) of the model. Current opportunities for additional model validation are somewhat limited, but some should be possible. Are the predicted locations of plumes consistent with observations? Comparison of model output with time series of ice velocity might powerfully validate the water pressure results, but this would require additional model runs (e.g. to overlap with the TerraSAR-X data for 2014-2015 reported in Young et al., 2019). I don't expect the authors to undertake extra work for the current paper, but the possibility of this strand of model validation should be mentioned, and certainly considered for the future. We will add text to our comparisons to observations to make them more meaningful, and will add the study site location for Doyle et al. (2018) and Young et al. (2019) to Figures 5d and 6d to assist the reader. We will also add text discussing the match between modelled and observed plume locations and give more detail on our plans for model validation.

Abstract, line 15. "In winter, we find channels over 1 m2 in area occurring up to 5 km inland, which shows that the common inference of zero winter freshwater flux is invalid" You could have non-zero flux without channels, so this statement does not follow logically. Change to something like: "We show that the common assumption of zero winter freshwater flux is invalid, and find channels over 1 m2 in area occurring up to 5 km inland." Wording will be changed as suggested.

p. 6, 15: regarding the assumption that "surface melt travels straight to the bed at the point of production", it is worth noting that this is reasonable on a heavily crevassed glacier. Words will be added to make this point.

p. 11, 14: it would be useful to cite a typical thickness of the sheet, and a threshold value when the sheet begins to transition to small channels. Words will be added to provide some information on typical sheet thicknesses. We will not, however, quote a value for when the transition to channels happens as this transition is not simply dependent on the sheet thickness and occurs at different values in different parts of the glacier.

Section 3.2: Did the modelled drainage system reach steady state by the end of the 3 month simulations? Additionally, more detail is needed in the caption to Fig. 4: Panels b & c: are these pictures of the end of the simulation? Panels d & e: what days of the simulation are shown? Are these for 'maximum' conditions? We will clarify that the simulations did not reach a steady state and will expand the caption for Figure 4 to state more clearly what the panels are showing..

Section 3.3 and Table 3: See comments above on presenting results from integrated Daily runs. How do the overall mean values of the Daily runs compare with the Average runs? Can simulations based on seasonal averages yield good approximations of seasonal average outputs (e.g. location of plumes and melt-undercutting totals), or does system non-linearity mean that daily simulations are necessary? Add data to Table 3 and present results in Section 3.3, plus appropriate discussion in Section 4.2 & 4.3. We will add a row to Table 3 detailing integrated plume melt across all simulations and include text in Sections 3.3 and 4.3 presenting and discussing this.

p. 18, 18: Here you compare the model output with the results of Chauché (2016). Since this source is an unpublished PhD thesis, you need to provide more context here. What methods were used by Chauché? What were the associated errors? Are the current results more or less reliable than those of Chauché? Text will be added to provide more context on Chauché (2016).

p. 19, 4: The deep fjord water is not 'subtropical'. Use 'warm Atlantic Water' instead. Wording will be changed as suggested.

The main weakness is that the scientific motivation for the study is never clearly presented or addressed. By extending our comparison to observations throughout the paper, we believe we will address this point by making the paper a more useful, constrained modelling study of Store Glacier. We also highlight the text in Section 1, which clearly positions the paper within the relevant theoretical context.

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? We will add some relevant references.

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? Yes, these equations are specific to the boundary condition at the calving front, be this in a lake or a fjord. In this case, therefore, the relevant density would be of seawater, but the equations do not require this – the density term is just the density of whatever water the calving front is in. We will add text to clarify this.

Table 1: How were these parameter values chosen? How sensitive are your results to those choices? The choosing of the parameter values is explained in the text preceding Table 1 (p.6, line 18ff). The values were taken from previously published work (Gagliardini and Werder, 2018) and sensitivity analysis of the GlaDS model was undertaken by Werder et al. (2013). We accept that this is not necessarily directly applicable to our model domain, but a full sensitivity analysis seems both unnecessary and outside the scope of this paper.

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. Will be changed as suggested.

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. We say that the hydraulic potential gradient is mainly controlled by the bed topography, with flow paths following the deeper parts of the bed. We agree that if we were stating that the bed topography were the sole control on the hydraulic potential gradient, we would be in error, but that is not the claim we are making.

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. We understand the point being made here and will consider the best way of addressing this going forwards.

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. See response to earlier referee comment on Section 3.2, above.

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. We will add further discussion of the context surrounding the winter observations and some text to Section 4.3 to deal with the measurements made in summer. These were not analogous, being derived from side-scan sonar rather than CTD and ADCP data fed through a model, but offer a useful additional constraint.

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. The location of the observations will be plotted in Figures 5d and 6d and the comparison to observations will be expanded.

**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 results? We will add some text comparing the modelled and observed locations of

plumes.

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. On further consideration, we agree that Figure 10 is not showing anything particularly valuable, so we will remove it and rewrite the accompanying paragraph to refer to Figures 7 and 8. It is sufficiently clear from Figures 7 and 8 that there is very little, if any, relationship between surface and plume melt, even if lags or smoothing were applied, so we have decided to not pursue this further.

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? No, the model has not converged on either mesh. In both cases, the end point of the run is shown and, in both cases, the channel network was still growing. We tried several different mesh resolutions in the initial work for this paper and a finer mesh resolution than the one eventually chosen both significantly increases model run time and generates numerical instabilities that crash the model, so was not pursued further, being impractical.

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? We will expand our discussion of the contrasts between observed and modelled plume locations in Sect. 4.3 to give more detail on the mismatch; given the relatively simple nature of this, an additional figure is unnecessary. We agree that using a more realistic grounding line would be ideal, but it is not a straightforward change to implement independently of the calving code we are currently working to integrate with the model, hence our decision to omit doing so for this study.

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. We will add a reference to uncertainty in the GlaDS parameters and discuss our planned solution of this by conducting a full validation exercise as part of future work.

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. When we have expanded the comparisons to observations made throughout the paper, we will consider whether this statement remains valid. The model is by no means perfect, but its current failings are expected based on its simplified state. We acknowledge the lack of a full validation exercise undertaken as part of this study, but re-emphasise that this is something we intend to undertake, and subsequently publish, with the fully coupled version of the model.

Page 27, lines 3-8: Are these potentially testable results? If you have measurements 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? No, the measurements available do not allow this to be done.

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