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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.

Douglas Benn (Referee)

dib2@st-andrews.ac.uk

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This paper represents a significant advance in our ability to analyse the basal drainage systems of tidewater glaciers. It is undoubtedly suitable for publication in The Cryosphere, and needs very little revision. It is very well written, has a clear and logical structure, and presents key results clearly and effectively.

At present, the paper is very tightly focused on the results, and there is some risk that it could be viewed simply as an interesting case study. As pointed out by the authors in the Introduction, the hydrology of tidewater glaciers has important consequences for rates of ice flow & the impact of meltwater plumes, and it would be worth linking

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back more strongly to these themes in the Discussion. I therefore recommend that the authors make a few additions to the text to highlight the wider significance of the results and increase the impact of the work.

A key area in this regard is the prospect of including basal hydrology in prognostic models of tidewater glaciers. At present, prognostic models typically employ simple parameterizations of frontal ablation and basal friction. The hydrological model presented in this paper opens up the exciting possibility of modelling both frontal ablation and ice dynamics from first principles, making models much more adaptable to changing environmental and glaciological conditions. The present paper is, of course, still some steps away from this, but the results can already be used to flag up some important issues. In particular, the authors can shed light on the importance of seasonal and diurnal fluctuations, and comment on appropriate temporal averaging periods in simulations.

The importance of the contrast between winter and summer conditions, and of interannual variability, is made clear in the Winter Baseline and SummerAverage12 & SummerAverage17 runs. However, the wider significance of the Daily runs could be usefully explored in more detail. The authors limit presentation of the Daily runs to describing the 'maximum' and 'end' states. These results are of course interesting, but the differences between these states and the SummerAverage runs are hardly surprising. It would be of potentially greater interest to see how the seasonally integrated results of the Daily runs compare with the SummerAverage runs. In other words, do the SummerAverage simulations yield good approximations of the integrated Daily outputs? Do they predict the same mean plume locations and overall frontal melt totals? Or does system non-linearity mean that the SummerAverage and integrated Daily results yield different answers? A comparison between the SummerAverage and integrated Daily results could shed important light on the appropriate timescale for modelling frontal ablation and dynamic processes. Do we need to model these systems with daily resolution? Or are seasonal (or annual) averages sufficient? How can models help us

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bridge between these timescales?

Another important issue is that of model validation. The model results are described clearly and in detail, and are of obvious interest in themselves. But the real power of the results lies in what they might tell us about what is going on beneath actual tidewater glaciers. 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.

Specific comments:

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."

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.

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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.

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?

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.

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?

p. 19, 4: The deep fjord water is not 'subtropical'. Use 'warm Atlantic Water' instead.

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