Response to Reviewer 2 of article entitled “Surging of a Hudson Strait Scale Ice Stream: Subglacial hydrology matters but the process details don’t”

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We would like to thank Reviewer 2 for their detailed consideration of the manuscript and we feel that many of their suggestions will ameliorate the final draft.

Reviewer 2 raises concern with the treatment of the efficient drainage system.

While dynamically modelling flux through an efficient system might be ideal, the CFL criterion would impose prohibitively long run times for our context. For example, given a lower bound water velocity of 1 m/s in the channel system as measured by Chandler et al. (2013) and our (coarse) resolution of 50 km, a time step of 0.00158 model years is required. Because our down gradient routing, drainage solver is not restricted by CFL, the time step depends only on the inefficient system for our setup. This typically lies in the range of 0.5 to 0.25 model years (lower bound time step of 0.0625 yr). Directly modelling the efficient system would increase BraHms runtime anywhere from 150 to >300 fold rendering simulation of millenial scale variability infeasible. Dynamical changes in flow through the efficient system occur on diurnal to seasonal time scales. Given the disparity in time scales (we are examining centennial to millenial time scales) it seems unlikely that dynamical changes in the efficient system (requiring a dynamic model) would be a significant control on the longer scale variability (though it is difficult to rule such a coupling out completely in a non-linear system, and we will make this caveat explicit in the revised text). We are, however, keen to see any evidence or mechanistic description of how such a coupling could significantly occur across such a wide difference in timescales.

Arguably the most important aspect of subglacial hydrology is the mass conserving lateral transport, an aspect which we show does not matter for (*e.g.*) surge frequency and strength. We will add a shortened version of the above to the justification of the efficient drainage model design in the revised manuscript.

Regarding the length and density of the paper and the polish of the discussion, Reviewer 2 has made many helpful suggestions (as had reviewer 1) (i.e. in “Minor Issues”) which will address this point and improve the discussion which we intend to incorporate. We intend to incorporate all of the suggestions made by Reviewer 2 except where specifically stated otherwise herein. Below we address a subset of the points for which a bit more discussion is useful.

- 220: We did not attempt this and took the comparison with GlaDs output for SHMIP scenario A3 as sufficient verification in this vein.

- 250: We will visit this for the revised manuscript. The same domain is used for both the spatial and temporal convergence test, which we will make explicit in the revision.

- 344: The assumption here is more so that the hydraulic potential is controlled by the slope in ice surface elevation (Johnson and Fastook, 2002). Given a lower viscosity for water versus ice and the same potential gradient, the velocities for water should be faster than ice. There could be exceptions to this but they are the minority.

- 428: By "sieving" we simply mean discarding all runs that lie outside the given metric ranges (akin to history matching, explained in Tarasov and Goldstein (2023)). For example, in the case of the geometry sieve these metrics are the maximum ice thickness and north-south extent and those runs with ice which is too thick or thin or extents too large or small were excluded from the sieved set. This definition will be made clear in the revision We did not consider a formal Bayesian calibration but this would be interesting. However, such a calibration is outside the scope of this paper and the numbers of runs making it through the sieves are sufficient for our statistical analysis without re-running the ensembles with updated priors.

- 436: This non-parametric sensitivity method is novel to this study. We did consider other methods, for example those of Borgonovo (2007) and Pianosi and Wagener (2015), however for those methods to converge even larger ensembles would be needed. This sensitivity method is *relatively* cheap. We will make this clear in the revision

- 450: The sieve is a random sampling of the dummy variable as it was not used in the actual model. As such it would be unexpected for the sieved distribution to be identical to the unsieved. The KDFs drop off at the maximum and minimum of the range (instead of immediately going to zero) due to the bandwidth method used. We used the gaussian_kde method of the scipy.stats package (Virtanen et al., 2020) with the default Scott method for bandwidth determination. We suspect this explanation would not add to the revised manuscript.

- 459: The inflection point indicates the point of diminshing returns in parameter sensitivity with decreasing parameter rank (by sensitivity metric). The third degree polynomial fit between parameter rank and sensitivity metric is used to estimate this inflection in sensitivity. This will be explained in the manuscript.

- 510: Because the surge duration is sensitive to the number of events occuring within a fixed time period, the duration signal is different at different frequencies. It is therefore necessary to assess differences in surge duration between model setups across frequencies. This difference in duration is also a caveat to our central conclusion of the unimportance of process details for large ice stream surging. We will expand this in the revised manuscript.

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