Author response to comment on “Observed and modeled moulin heads in the Pâkitsoq region of Greenland suggest subglacial channel network effects

Response to Anonymous Referee #2

The authors’ replies are in bold blue

General comments

We thank Referee #2 for their comprehensive review and for their useful comments.

This is an interesting and well-written paper that tackles the problem of how to interpret moulin water pressure (head) records. The paper addresses an important topic because moulins are a less inconvenient way of observing subglacial hydrology, than methods such as boreholes.

The main focus of the paper is a suite of experiments with the MouSh model coupled to a subglacial channel model. The experiments attempt to match a moulin head record obtained in the well-studied Paakitsoq region of Greenland. The crux of the problem seems to be creating enough damping in the simulation, so that simulated diurnal head variations remain small enough. A match can be optimised by specifying a suitable subglacial base flow or a very large moulin shaft. Although the experiments themselves are interesting conceptually, there are two very significant weaknesses that limit how useful this study is in its present form.

First, to match the simulated moulin head with observations, the authors need to add subglacial water (base flow). Although this is of course quite reasonable when modelling a moulin that joins a wider scale drainage system, it’s not clear how much of the discrepancy between modelled/observed moulin head is due to (i) the subglacial base flow and subglacial channel model, (ii) uncertainties in the moulin model, and (iii) the rather poorly prescribed melt input. Taking these in turn:

In the case of base flow, it is confusing that it is prescribed as a flow rate (m3/s) even though that is on first impression an obvious choice. The moulin head is a proxy for subglacial water pressure (Pw), not flow, and even though these quantities are related, prescribing flow rather than Pw presumably requires some speculation of drainage system characteristics to calculate Pw in an extra step. In fact without any base flow or stream input at all, the moulin head could simply reflect changes in Pw driven entirely non-locally, provided the moulin remains hydrologically connected. This aspect needs to be clearer so we know what assumptions are needed and how are the associated parameters constrained. It is also not obvious how Qout is calculated at the bottom of the moulin, nor how the subglacial channel model is driven in the context of wider-scale drainage evolution through the season.

> **The moulin head is indeed a proxy for the water pressure in the subglacial channel, however that water pressure (or head) is related to water flow: the moulin head is a function of the water input in the moulin (m3/s), the water output in the subglacial channel (m3/s), and the cross-sectional area of the moulin (m2). The subglacial baseflow is added to the subglacial output and does not transit through the moulin. This baseflow influences the moulin head and the subglacial cross-sectional area by modifying Qout only. We will add this description in the Methods section to make it clear how we add baseflow to the model.**

A minor related point: the upper limit for baseflow of 5 m3/s in the experiments seems very low for a typical Greenland catchment or even a small part of one.

> **This may be, but our moulin and its surface catchment are quite small: the catchment is just 0.24km^2. For our specific simulations, 5 m^3/s is the equivalent of 20 additional moulins being fed the same surface melt upstream of our moulin, and directly connected to a subglacial channel which would have to flow directly into the subglacial channel formed by our moulin. Moulin density is often lower than 1/km^2 in Greenland (Banwell et al., 2016). In addition, the baseflow parameter is a raw parameter and it is not yet clear how this parameter is constrained. A very large water flow in the main subglacial channel might only transfer a smaller amount of water to a nearby moulin through its tributary channel. Our simulation results worked best with a 2m^3/s baseflow. We will add a discussion point in section 5.4 .**

For the moulin model we need to remember that MouSh is effectively unvalidated, even though it is quite a detailed model. Inevitably there are many unknowns here – for example, my understanding is that MouSh assumes an initial moulin that is a vertical cylinder, which I suspect is far from reality given that most moulins appear to form as hydrofractures in Greenland, which are initially planar. Anybody that has tried lowering sensors into a moulin can guess they are not vertical shafts! Limited exploration confirms that. Perhaps several moulins are connected englacially within an initially planar fracture zone, before even reaching a subglacial channel. Almost like a 'distributed' englacial drainage system that might also produce the dampened head record sought by the authors. Of course this is speculative, maybe moulins are vertical shafts after all, but in the experiment we should account for our lack of knowledge in this respect.

> **We recognize that the initial shape of the moulin in the MouSh is simplified as a cylinder and that more complex shapes are more likely to originate from hydrofracture, and that the upper part of the moulin does have plunge pools created by the surface stream which creates a challenge for lowering instruments. The possibility of "planar storage" is an interesting thought that is a possibility for Greenland moulins in general. Indeed, observations of other moulins near our field site showed some lines of moulins that could be connected with each other through a larger crevasse, and some did have visible crevasses associated with them. However, neither applied to the moulin we study here. We also observed small early-season moulins starting in small crevasses. As mentioned in our answer to Reviewer 1, "To substantially influence the**

water level fluctuation, the cross-sectional area of the englacial void would have to be about 80m^2, so for example a crevasse of 10m long and 8 cm wide, at a depth of 150-200m." However, at our site, surface observation shows no crevasses. In addition, Andrews et al. (2022) demonstrate that in most cases, initialization processes smaller than two weeks are overwritten by creep processes. While it seems unlikely, we cannot discard the potential for planar storage at our site, therefore, we will expand the discussion section to discuss the potential for englacial storage from moulins connected through fractures in section 5.2.2.

Melt input: this is the last important source of uncertainty, and it seems very much brushed under the carpet in Section 3.1.1. Even the very short (2-day) observed time series is not well represented by either the 'Modeled' or 'Idealized' time series (Fig 3), and the extended parts of the Q time series shown in Fig 3 do not follow the trend of the steam water level, which is discouraging. The vast majority of the melt input time series is extrapolated outside of this short (poorly matched) tuning window. I strongly disagree with their "good confidence in our derived runoff values R and thus our model forcing Qin,model".

We recognize that the direct stream measurements used to calibrate the routing model have a short duration and the modeled meltwater input might appear not perfectly constrained.

Though the length of the measured time series we use to calibrate our stream discharge is only two days, it still reduces the uncertainty from what we would obtain with the met station measurements alone. Calculated meltwater inputs can be orders of magnitude over or under-estimated with melt models (McGrath et al 2011, Smith et al. 2017). Melt models calculate an expected melt rate (m. w.e.). To convert this to runoff, the drainage basin, the ice density, and the fraction of water refreezing are minimally constrained parameters.

Because of the extremely limited supraglacial stream discharge and moulin head measurements on the Greenland Ice Sheet, this is the only study that can use direct stream measurements simultaneously with direct measurements in moulins. Please note that a key finding in this manuscript is that our relative diurnal range of meltwater input falls in the range of other measured meltwater stream fluxes in Greenland (the usual diurnal range ranges between 1-2x the mean discharge - Table 3). Figure 8a shows that in the absence of baseflow, the diurnal range would need to be less than a quarter of the mean discharge, which has not been observed in Greenland (Table 3). Therefore, uncertainties in the meltwater model calibration are negligible compared to those values.

We realize that our description of the meltwater input may have been misleading, and we will remove our statement of good confidence and expand 3.1 to include more details about the meltwater input model calibration, its limitation, and potential errors.

Regarding the stream water level: Stream water level is controlled by the cross-sectional area of the supraglacial stream, as well as the distance between the instrument and the bed of the stream, which evolves through time. The lowering trend of the stream water level time series after day 206 is artificial; it is due to the deepening of the bottom of the stream beneath our sensor after we left the field. In

addition, the curvature of the daily amplitude of the stream water level cannot be directly compared with the stream discharge. Usually, hydrology methods use a simple regression curve to interpolate corresponding discharge for specific water levels. Unfortunately, we cannot use those methods because of the constant lowering of the stream bed and the cross-sectional area variation over time. However, the stream water level does reflect the increase in supraglacial stream discharge caused by the cloud coverage. We will describe the stream gauge better in the Methods section.

I also wonder why most simulations (Tab 2) use the modelled Qin, even though it looks like a worse fit than the idealized?

The idealized input might be a good fit for the specific days we measured discharge, however it does not represent the seasonal variability of the meltwater input throughout the season as well as the modeled Q_{in} does. This is why we use the modeled Q_{in} for the first sets of simulations.

This poor match is not necessarily a problem when constraining a model with observations, if it is clearly acknowledged and if the uncertainty it introduces is more rigorously accounted for.

We will improve the Methods section by further describing the meltwater input calibration and providing a range of uncertainty based on the 2017 melt season.

I think to address this first weakness we would need some detailed error analysis before any comparison between the simulations and observed head time series can be interpreted in a useful manner. I would envisage an ensemble (e.g. Monte Carlo or latin hypercube sampling) or more advanced statistical approach to account for the very many uncertainties in the MouSh model, the treatment of baseflow, and the stream inputs. The long discussion section (which is currently very speculative without even a basic error analysis) should then focus on how much of the record can be confidently interpreted in terms of real variations in subglacial water pressure (or drainage characteristics), and how much cannot be untangled from uncertainty in the simulation and inputs.

This is an interesting comment. However, for this study specifically, variations in meltwater input do not affect the main results of this manuscript. As mentioned above, even if there are some uncertainties with the tuning of the melt model and with the melt model itself, it reproduces relative meltwater amplitude in a similar range as other measured streams in Greenland. The simulations in Figure 8a demonstrate that the amplitude of oscillation relative to the mean discharge needs to be ten times smaller than the measured diurnal variability in supraglacial streams in Greenland (Table 3). We will clarify this point in the manuscript.

Regarding the treatment of baseflow, we think an involved ensemble approach would be incongruent with the simplistic nature of typical baseflow. Our forward-model approach, where we prescribe constant, sinusoidal, and lagged-sinusoidal baseflow (i.e. a maximum of 3 parameters: mean, range, and phase) is better suited here.

> **Finally, the reviewer has a fair point that the MouSh model contains many processes that have inherent uncertainties. Rather than rehash the extensive sensitivity testing done by Andrews et al. (2022) -- 24 pages! -- we instead suggest that we will discuss the effects of the more poorly constrained aspects of MouSh (e.g. above-water-line melt, viscous deformation, below-water-line melt) in the Discussion. Note that we already address potential biases from the above-water-line melt module in Section 5.2.1.**

The second weakness is its relevance, which of course I acknowledge is limited by where field data are available. In Fig 5 it is evident that the study is conducted mid melt season when there is relatively little variation in moulin head (range looks like 250 to 400m, but much of period close to 300m), and as such Pw is always well below the ice overburden pressure. In fact the authors choose a period mid-season when h is around 60% of H and varies diurnally by about 10%. In these conditions we would expect the moulin head to have a minimal effect on ice motion. Probably there are some data for this region that could answer that more precisely. What the community needs more, I would suggest, is for the study to simulate the early season formation of moulins as part of efforts to simulate the duration/extent of ice acceleration in spring. What role do moulins play in the evolution to channelised drainage & associated ice deceleration? It's not clear how the results would help that aim, at the moment. However, some simulations of moulin head very early in the season could provide valuable pointers for interpreting moulin pressure records in the more dynamically important part of the cycle, or in thicker ice. Similarly, experiments simulating extreme melt pulses mid or late season could be useful. But, related to the first point, we would need to know how the subglacial channel model is driven by / coupled with the larger-scale hydrological evolution.

> **We agree that the early melt season would be very interesting to investigate. Because we do not have field data from the beginning of the melt season and because the model limitation shows that we are missing a basal influx component, investigating the early season might be off-topic for this particular research. We thank the reviewer for the thoughtful ideas for future research, which we agree with and would be excited to enact in the future. We believe that our results from the melt season are relevant, as our work combines, for the first time, field data constraints for meltwater inputs and moulin water level, as well as moulin shape constrained by a physically based model, and shows that for this particular case, englacial storage might not be the answer to moulin water level damping.**

Overall I think this could go one of two ways – (i) keep the focus on the link with available field observations, by carrying out some detailed error analysis, or (ii) accept that the field observed melt inputs are perhaps too limited/uncertain at present, and instead focus on a conceptual study that is not tied to that one location and can explore controls on head variations across a wider (more interesting) range of sites/conditions. Either of these directions could turn this into a really useful study to help interpret or design experiments with moulin water pressure records.

> **We thank the reviewer for the recommendations. Suggestion (ii) is included in Andrews et al. 2022. This present manuscript instead, focuses on a region where we have the most in-situ constraints, in order to identify some system properties which we may be able to broaden more regionally. For suggestion (i) we think that detailed**

> **error analysis is not necessary. This is because the field observations of stream flow, while uncertain in an aleatoric sense, is a far lesser contributor than the epistemic uncertainty inherent to our simple one-moulin, one-channel model. The calibrated meltwater input greatly reduces the uncertainty and this uncertainty is orders of magnitude lower than the uncertainty in the subglacial processes. The moulin size error is also an order of magnitude lower than what would be required to dampen the moulin head; Andrews et al. 2022 conducted a detailed sensitivity analysis of the MouSh model that shows this. We recognize that some of the meltwater input descriptions were badly formulated and created some confusion. We will improve our description of the meltwater input model and demonstrate how the uncertainty does not matter so much because the general ratio diurnal range/mean discharge is constrained enough for the purpose of this manuscript.**

Minor comments from the introduction

L2: I believe water pressure also influences motion in some marine terminating glaciers (not just land terminating).

> **Subglacial flow does also affect marine terminating glaciers. However, in this manuscript, our focus is on land terminating glaciers for 2 main reasons: 1. Subglacial water pressures are the main driver of ice motion and 2. the moulin we study is in a land terminated area. In marine terminating glaciers, the relationship between tidal influence, subglacial water pressure, and freshwater plumes represents a complex environment that we believe cannot be compared with our current study. So, we have left the mention of marine-terminating glaciers out of our abstract.**

L19 I think some observation based papers showed the influence of temporal melt inputs, before Schoof 2010 – worth to cite these here too.

> **We change the sentence in the Introduction "... and temporal (Bartholomaus et al., 2008; Iken & Bindschadler, 1986; Schoof, 2010) supraglacial meltwater input variability can …"**

L28 again there are earlier papers describing drainage evolution in Greenland (Bartholemew et al 2011 EPSL?), and of course even earlier elsewhere (1980s/90s work on alpine glaciers).

> **We add "While earlier studies suggested that the efficiency of the subglacial drainage system controls the seasonal pattern of velocities (Bartholomew et al., 2010; Iken & Bindschadler, 1986), field observations of basal water pressures in Greenland demonstrated instead the prominent role of the weakly connected drainage system (Andrews et al., 2014; Hoffman et al., 2016)."**

In general there is a lot of self citation in the intro that could be expanded to include earlier work from other groups.

> **We did our best to cite the relevant literature, but doubtless there are papers we do not know about. We will spend some time searching for earlier work to cite.**

Fig 8 confusing that red dots can be either observations or simulations. Can sims not be blue or some other colour?

> **Changed the sim to orange instead of red and updated the figure caption to match the change.**

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