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

Response to Samuel Doyle, Referee #1

The authors’ replies are in bold blue

General Comments

Well-written, timely, and based on a substantial body of recent work this manuscript would make an excellent contribution to The Cryosphere and the topic in general. The manuscript presents the results from a moulin-channel model - the methods for which are described in a previous study (Andrews et al., 2022). The model does not include any glacier hydrological systems other than a single moulin and a single Rothlisberger channel and depending upon your perspective this represents the main strength or the main weakness of this study. The conclusion that there is additional “base flow” that contributes to and damps flow within moulin-connected channels is uncontroversial given that many moulins occur on the Greenland Ice Sheet and many appear to be connected (e.g. Andrews et al., 2014). However, this does not take away from the contribution this study makes in very neatly explaining observed moulin head variability using a numerical model. The assertions made regarding differences in “base flow” in the lower and upper ablation areas (Section 5.3.3) are intriguing and demonstrate recent advances in our understanding of the moulin-connected drainage system.

The main finding of this manuscript is that channel growth is too slow - and channels are therefore too small - to explain the observed damping of diurnal moulin head variations, when the model is forced by local moulin inputs alone. This finding is somewhat similar to that in Dow et al. (2014) but a different interpretation is given in that study: that channels are unlikely to form or persist at high elevations. Current evidence (e.g. Covington et al., 2020; Chandler et al., 2021) as well as the modelling presented in this manuscript suggests channels do form at high elevations and that connectivity to other channels may help them persist. Can the discussion of this be expanded slightly?

> **We thank Samuel Doyle for the thorough review and for all the useful comments. We are pleased to see that the summary above describes the findings in the manuscript well. We will expand the discussion on the subglacial channel at higher elevations with the Chandler et al. (2021) reference suggested, which is indeed independent corroborating evidence to what we find (the markings of non-local inputs to the channelized subglacial network).**

Specific Comments

L36 - Water pressure in moulins was also measured by Holmlund & Hooke (1983) and Vieli et al. (2004).

> **We added the two references.**

L396 - Section 5.3.2 and Sections 5.3 and 5.4 in general - As mentioned above, a limitation of this study is its application of a moulin/channel model without including other components of the subglacial drainage system. This is stated clearly in Section 5.3 (L375) but isn't mentioned in Section 5.3.2 which deals only with subglacial channels. It would be clearer if Section 5.3.2 was renamed from “Subglacial network connectivity and base flow” to something more specific to channels e.g. “Subglacial channel network connectivity and base flow”.

> **We renamed the Section as suggested.**

Note that filling of a moulin from the base by subglacial water was observed by Holmund & Hooke (1983) and has been observed in boreholes by several studies (e.g. Gordon et al., 2001) as well as being the focus of Mejia et al. (2021). This suggests that “reverse flow” into moulins does occur and is unlikely to be limited to flow within channels. I appreciate that the model cannot reproduce this - and I don't suggest this is attempted in this paper - but can this limitation be discussed alongside the direct evidence listed above?

> **We discuss the reversal of flow caused by a change of hydraulic gradient in Section 5.3.2. We added “This flow reversal or external basal flow has been observed in boreholes (Gordon et al., 2001; Holmlund & Hooke, 1983) and moulins (Mejia et al., 2021) .”**

Section 5.3 appears to overlap with Section 5.4. Is Section 5.3 only concerned with damping caused by storage within the unchannelised system and not damping caused by recharge from the unchannelised system? Can this be made clearer? Can these sections be combined?

> **We changed the title of Section 5.3 as suggested above. Section 5.3 does focus on the channelized system. As each of these sections are quite lengthy already (Section 5.3 is three pages with 3 subsections, and Section 5.4 is a full page), we prefer to keep them separate for better signposting and smaller topics.**

In the Section 5.4 heading what does “external” relate to? Is it the same as “non-local”? Does it mean from an unchannelised system.

> **External does relate to non-local. However this does not specifically mean unchannelized system. “External” describes sources aside from surface meltwater in the ablation zone - e.g. basal melt, or multi-annually stored water (e.g. in the unchannelized basal system, or in the englacial system). “Non-local” refers to faraway (>10 km?) meltwater from the ablation zone. We will clarify this in the manuscript by formally defining it as we have done here.**

As in previous studies (Dow et al., 2014; Meierbachtol et al., 2014) Shallow surface and bed slope have a critical role in channel development. How was the slope ratio of 0.01 (equivalent to ~0.6 degrees) measured? Is the bedslope assumed to be the same as the surface slope in Table 1? If so, state this in the methods. If you were to increase the surface and/or bedslope would this change the results?

The surface slope is calculated along a flow profile going through the moulin area, which drops 400 meters over 50 km (slope of 0.01) (see Figure 1 in Mejia et al. 2021). The slope does locally vary, but this value is a good representation of the whole area. The surface slope in the model influences the moulin shape, but not the hydraulic gradient. It is true that in reality, the local hydraulic gradient will be influenced by the surface topography. However, on a larger scale, we assume that it is mainly controlled by the length of the channel and the ice thickness. In the subglacial channel model, the bedslope from the moulin to the margin is negligible considering the distance to the margin compared to the actual bed variation.

More locally, in the moulin area, it is true that the bed slope is more variable. For isolated moulins, flatter areas will likely decrease local hydraulic gradients and reduce basal flow in subglacial channels, while steeper areas will enable faster evacuation. We will add this point in the Discussion Section 5.3.2.

L128 - Stating the p-value for correlation as a measure of accuracy is inappropriate. The low p-value suggests that the correlation between modelled discharge and measured water level is unlikely to be due to random variation. The p-value does not tell us the degree of accuracy as a statistically significant correlation is plausible for any variables that co-vary regardless of the magnitude (or units) of the variables, or whether there is a causal relationship. For the same reason the statement of "agreement" on L124 between the same variables as above is not strictly speaking supported by the coefficient of determination given, which is a measure of correlation rather than accuracy, though this depends on the intended meaning of "agreement" which is relatively vague. Strictly speaking, the coefficient of determination of 23% suggests 23% of the modelled discharge can be explained by variation in stream level. Reporting the correlation between measured stream level and discharge may be useful but it should not be described as accuracy. A better measure of accuracy would be the root mean square deviation between modelled and measured discharge. To be clear, I see no problem with the modelled discharge imperfectly matching the measured discharge given how difficult it would be to model rainfall and turbulent heat fluxes.

This is a good point, we will calculate the root mean square deviation between modeled and measured discharge. We will also modify section 3.1.2 to correct our statistical analysis and interpretation, and expand on the relative accuracy of the modeled meltwater input in section 3.1.2.

L129 - Can you state that m and c are linear regression coefficients and state which variables are the subject of the regression? Is this the melt model calibration mentioned on L123? It's unclear why day of year 205 was included in the regression even though it was affected by rainfall. Overall, this section needs expanding and revising, possibly with the addition of a figure showing the linear regression.

m and C_R are not results from linear regression, they are empirically set values (Equations (2) and (4)) that we tuned manually. We reorganized section 3.1.2 to make it more clear, improved the naming convention of certain parameters, and made a new table (Table 1) containing the parameters used for the modeled stream discharge. We removed the mention to the rainfall event, which actually happened in 2018 but is not covered in this manuscript. Instead, the discrepancy between the measured melt and the modeled melt is caused by the cloud coverage. This is the reason why the melt model is calibrated manually. We will expand on this in the method section.

L216 - can you revise this sentence? The melt model reproduces melt not “particular weather conditions”. It’s unclear what is being underestimated. If correct, can you revise to make it clearer that melt is underestimated by the model under cloudy conditions and consider adding an appropriate citation.

We restated the sentence to “...limitations of the melt model in reproducing melt caused by particular weather conditions such as cloud coverage, which can underestimate melting … “

L230/L251, Fig. 8 and Fig. 8’s caption - consistent description of the error bars would help the reader.

We fixed the description. This was not accurate. The bars represent 1 standard deviation from the mean. Not errors.

L244 - Is “ranges” required here because you refer to the ranges in amplitude or is range already implicit in amplitude?

Removed “ranges”. Ah already refers to an amplitude.

Fig. 9 is difficult to interpret. The y-axis label gives two versions of A_h with the modifier in brackets meaning different things (0 relates to amplitude without baseflow, while n relates to time lag). Could A_h(0) be written as A_h minus A_bf? A different symbol for time lag to n, which is usually sample size, would be more intuitive. The symbol for time lag needs to be used consistently (e.g. in the text, legend and caption). A simpler y-axis label could be used and defined in the caption e.g. something like “normalised diurnal head amplitude”. The plot could be labelled with arrows showing where on the x-axis A_bf exceeds A_in and vice versa.

We will improve Fig. 9 and integrate the reviewers comment.

L274 and other occurrences - consider using the past tense for things that were done in the past.

We will do as suggested.

L367 - This is the orthodox view that crevasses are unlikely to be sufficiently open below a few tens of metres below the surface but there is not that much observational evidence to support this view. It is worth remembering that the moulin would have originated from water flowing into a fracture and that englacial conduits often follow such fractures (e.g. Gully, 2009). I don't think storage within englacial fractures can be definitively ruled out. See also evidence for fractures at depth presented in Hubbard et al. (2020) and evidence for energy released by refreezing meltwater which potentially occurs in fractures (Luthi et al., 2015).

We agree that water storage in deep fracture cannot be conclusively ruled out. However, 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 10 m long and 8 cm wide, reaching a depth of 150-200m. At our specific site, there is no visible crevasse passing through or around the moulin (see Figure 1e). Also, the general topography of the area suggests that we are more in a compressive zone, while the Hubbard et al. (2021) study is situated in a faster flowing region that is more likely to be in extension. In addition, Andrews et al. (2022) show that any moulin formation process shorter than two weeks is unlikely to influence the moulin shape. Englacial conduits following fractures have been found in temperate glaciers, and cut-and-closure formation reported by Gulley et al. (2009) are formed on longer timescales and could produce englacial storage, but this process has not been observed in our study area, which has cold ice (~ -10°C). The refreezing observed by Luthi et al. (2015) took place over multiple centuries (Poinar, 2016) as the ice advected through areas of various stress states- in contrast to the seasonal or sub-seasonal storage we seek here. We will discuss potential englacial planar storage in the moulin in Section 5.2.

L451 - reference Figure 10

Referenced as suggested.

L452 - discrete in what sense, temporal or spatial or both?

Changed to "Spatially discrete .. "

L459 - Nienow et al. (2005) comes to this conclusion by interpreting velocity data on the basis of pressure measurements presented elsewhere (e.g. Hubbard et al., 1995; Gordon et al., 1998). It may be better to cite the studies with hydrological observations of this process.

Modified as suggested.

L484 - Can you reconcile the assertion that at lower altitudes subglacial water flow is steadier while at higher altitudes it is lower in magnitudes but greater in diurnal oscillation amplitude, with the opposing observations in Covington et al. (2020)? Perhaps this would apply if fixed moulin and channel sizes were assumed. Could you expand this assertion in the conclusions slightly to better reflect how it is described (very nicely) on ~L445?

We changed "in the main subglacial channel" to "This contrasts with low-elevation moulins, whose greater number density and variation in lag should produce a large,

minimally oscillating baseflow in the main subglacial channels, similar to Sim~EMb." in Section 5.3.3 to avoid confusion. We expect the steadier flow in the main subglacial channels fed by multiple moulins, while the observations made by Covington et al. (2020) are from moulins and not subglacial channels. The lower diurnal range in head compared to a lower altitude moulin could be caused for example by damped surface meltwater inputs, and connectivity with a moulin with very different phase lag. We will expand this assertion in the conclusion, as suggested.

Technical Comments

L17 - Move citation beginning "(Yang …" to before "that"

Moved as suggested.

L74 - delete second citation to Morlighem et al. (2017)

Deleted as suggested.

L90 and other occurrences - check whether "Fig. 3" should be "Figure 3" when referenced in the main text outside of brackets.

The rule is 'The abbreviation "Fig." should be used when it appears in running text and should be followed by a number unless it comes at the beginning of a sentence, e.g.: "The results are depicted in Fig. 5. Figure 9 reveals that...".' We fixed the occurrences of Figure that should have been Fig.

Fig. 3 and other occurrences in Figures - units should not be in italics.

Fixed the italics in all figures.

L112 - the first "(Q_p)" is unnecessary.

Removed Q_p.

L112 - Consider using a different letter for coefficients, or being consistent with the subscripts, or otherwise reducing the potential for confusion; currently there are two C_x's for concentrations (L95), a C_p for the peak discharge coefficient (L112), and a C for the runoff coefficient (L115).

We now use S for the stream concentration and D for the injection dye concentration, and C_p for peak discharge coefficient, and C_R for runoff coefficient.

L137 - delete "and" and add a comma before phi

Changed as suggested.

L152 - Figure 4 and its description could be easier to follow. In the text, figure, and caption can the same terms be used for “turbulent-underwater” and “open-channel” melt be used? The description of moulin deformation modelled as viscous and elastic needs to be separated from the shear deformation of the ice modelled using Glen’s flow law. Can you mention that the former is modelled using a Maxwell model. Overall the description of the model needs to be expanded to briefly introduce all of the components without the reader needing to refer to Andrews et al. (2022) to make sense of the modelling approach.

We will improve the Moush model description using these suggestions.

Figure 5 caption - In “(d-e) Moulin shape evolves with surface input.” does this mean that other processes affecting moulin shape were excluded?

We changed to “Moulin shape is free to evolve through time”, as opposed to simulation Fa-Fc which have a fixed moulin radius through the simulation.

Should “Simulations EMa”be singular? And should “head of oscillation” be “oscillation of head”?

Changed to “Simulation EMa:...”

Generally interpretation of figures should be left to the main text and not included in the caption.

Removed “This simulation illustrates that a large volume is needed to reproduce realistic head amplitude.” from the caption in Figure 5.

Figure 5 c,e,g y-axis labels - Symbol z has not been defined in the text, and in Figure 4 “Height above the bed (m)” is written out in full.

Updated figure 4 to show z and x instead, and added “The horizontal position (x) varies along the moulin height above the bed (z).” in the caption of Figure 4.

L193 - specify the sim for the fixed 5 m model run.

We added “(Sim Fb)” in : “For our model to reproduce measured head oscillations (black lines), a fixed moulin radius of 5 m (Sim Fb) was required …”

L206 - plural ‘radii”

Corrected radius to radii.

L214 - Specify the sim before referred to.

We added “with Sim EMb” in : “While the general diurnal range of moulin head is reproduced with Sim EMb, the match between the simulated and measured head is imperfect.”

L254 - Specify Sims E1a-d.

Moved “(Sim EMa)” after “without baseflow” to avoid the confusion.

L259 - “of oscillation” could be omitted.

Removed “of oscillation”.

Fig. 8 - Units incorrectly in italics.

Removed italic in all units in all the figures.

L290 - is it necessary to specify normalized here when discussing in general terms? Is this A^*_in?

Yes, the “normalized” is important to specify, because the ratio between the mean meltwater input and the diurnal range controls the variability, not the diurnal variability itself. And yes, it is A^*_in. We added the symbol.

Table 3 - Can you use a narrower dash to indicate that a variable is unitless to avoid ambiguity with the minus sign, or perhaps just leave the units for unitless variables blank as on L353? (A narrower unitless symbol is used on Figure 9).

Made the dash narrower as suggested

L333 - add “moulin” after “FOXX”

Added moulin as suggested

L380 - add “respectively” after “base flow”

Modified as suggested

L477 - revise to avoid the apparent contradiction in dismissing “subsurface inputs” which would dismiss “basal inputs”.

Added “shallow” in : “With surface and shallow subsurface external inputs dismissed at our site…”

L468 - the following sentence needs revising to make sense “with only a small portion of the water can”

Changed the verb tense of the verb “to travel”: “In reality, before it can make it to the subglacial channel system, some portion of this available basal melt is stored in the inefficient portion of the drainage system with only a small portion of the water traveling through the linked cavity system …”

Additional References

Gordon S et al. (2001) Borehole drainage and its implications for the investigation of glacier hydrology: experiences from Haut Glacier d'Arolla, Switzerland. Hydrological Processes 15, 797-813. doi:10.1002/hyp.184.

Gulley J (2009) Structural control of englacial conduits in the temperate Matanuska Glacier, Alaska, USA. Journal of Glaciology 55, 681-690.

Holmlund P and Hooke R (1983) High-water pressure events in moulins, Storglaciaren, Sweden. Geografiska Annaler 65A, 19-25.

Hubbard B et al. (2021) Borehole-Based Characterization of Deep Mixed-Mode Crevasses at a Greenlandic Outlet Glacier. AGU Advances 2, e2020AV000291. doi: 10.1029/2020AV000291.

Luthi MP et al. (2015) Heat sources within the Greenland Ice Sheet: dissipation, temperate paleo-firn and cryo-hydrologic warming. The Cryosphere 9, 245-253. doi:10.5194/tc-9-245-2015.

Meierbachtol T, Harper J and Humphrey N (2013) Basal drainage system response to increasing surface melt on the Greenland Ice Sheet. Science 341, 777–779. doi:10.1126/science.1235905.

Vieli A et al. (2004) Short-term velocity variations on Hansbreen, a tidewater glacier in Spitsbergen. Journal of Glaciology 50, 389-398. doi: 10.3189/172756504781829963.

References

Andrews, L. C., Poinar, K., & Trunz, C. (2022). Controls on Greenland moulin geometry and evolution from the Moulin Shape model. *The Cryosphere*, *16*(6), 2421–2448. https://doi.org/10.5194/tc-16-2421-2022

Chandler, D., Wadham, J. L., Nienow, P. W., Doyle, S. H., Tedstone, A. J., Telling, J., Hawkings, J., Alcock, J. D., Linhoff, B., & Hubbard, A. (2021). Rapid development and persistence of efficient subglacial drainage under 900 m-thick ice in Greenland. *Earth and Planetary Science Letters*, *566*, 116982. https://doi.org/10.1016/j.epsl.2021.116982

Covington, M. D., Gulley, J. D., Trunz, C., Mejia, J., & Gadd, W. (2020). Moulin Volumes Regulate Subglacial Water Pressure on the Greenland Ice Sheet. *Geophysical Research Letters*, *47*(20). https://doi.org/10.1029/2020GL088901

Gordon, S., Sharp, M., Hubbard, B., Willis, I., Smart, C., Copland, L., Harbor, J., & Ketterling, B. (2001). Borehole drainage and its implications for the investigation

of glacier hydrology: Experiences from Haut Glacier d’Arolla, Switzerland. *Hydrological Processes*, *15*(5), 797–813. https://doi.org/10.1002/hyp.184

Gulley, J., Benn, D. I., Müller, D., & Luckman, A. (2009). A cut-and-closure origin for englacial conduits in uncrevassed regions of polythermal glaciers. *Journal of Glaciology*, *55*(189), 66–80. https://doi.org/10.3189/002214309788608930

Holmlund, P., & Hooke, R. LeB. (1983). High Water-Pressure Events in Moulins, Storglaciären, Sweden. *Geografiska Annaler. Series A, Physical Geography*, *65*(1/2), 19–25. https://doi.org/10.2307/520717

Hubbard, B., Christoffersen, P., Doyle, S. H., Chudley, T. R., Schoonman, C. M., Law, R., & Bougamont, M. (2021). Borehole-Based Characterization of Deep Mixed-Mode Crevasses at a Greenlandic Outlet Glacier. *AGU Advances*, *2*(2), e2020AV000291. https://doi.org/10.1029/2020AV000291

Lüthi, M. P., Ryser, C., Andrews, L. C., Catania, G. A., Funk, M., Hawley, R. L., Hoffman, M., & Neumann, T. A. (2015). Heat sources within the Greenland Ice Sheet: Dissipation, temperate paleo-firn and cryo-hydrologic warming. *The Cryosphere*, *9*(1), 245–253. https://doi.org/10.5194/tc-9-245-2015

Mejia, J. Z., Gulley, J. D., Trunz, C., Covington, M. D., Bartholomaus, T. C., Xie, S., & Dixon, T. H. (2021). Isolated Cavities Dominate Greenland Ice Sheet Dynamic Response to Lake Drainage. *Geophysical Research Letters*, *48*(19), e2021GL094762. https://doi.org/10.1029/2021GL094762

Poinar, K. (2016). *The influence of meltwater on the thermal structure and flow of the Greenland Ice Sheet* [University of Washington]. https://digital.lib.washington.edu/researchworks/handle/1773/35062