Author response to

Interactive comment on “Controls on Greenland moulin geometry and evolution from the Moulin Shape model”

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In review at *The Cryosphere*
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Response to Anonymous Referee #2

Authors’ responses are inline in blue text.

Apologies for taking a long time to produce this review.

This is a very interesting paper that presents a new model for the evolution of moulin geometry and explores how the results of this model for moulin shape and water level depend on various model parameters. It is argued that moulins comprise a sizeable fraction of the englacial-subglacial drainage system in Greenland, and that the time-evolution of their volume is a potentially important feature to include in englacial/subglacial models, offering improvements over a model that assumes a static moulin volume.

The study is an interesting one and I believe it deserves publishing in some form. However, I do have quite a lot of detailed questions, and some concerns, about the ingredients that go into the model. I will focus this review largely on these model details, from section 2 of the paper. Some of these may be sorted out by clarification as to what equations have actually been solved. As a general comment, there appears to be quite a lot of duplication of notation, which overcomplicates the presentation of the model and causes some confusion. I think it would also be helpful to express the physics in terms of differential equations rather than discrete increments that implicitly include time-steps.

We thank the reviewer for a thorough and helpful review, particularly the close reading of our mathematical formulations. Overall, we make an effort to revise the equations to ensure consistency and simplicity. We also reduce the number of subsections (there were a lot!).

In response to comments below and from Reviewer 1, we now provide several simplifying options in the model code and test their impact on model results in a new suite of sensitivity runs. This sensitivity study examines the impact of using (1) a fixed cylinder geometry, (2) a circular vs egg geometry, (3) elastic deformation calculated with or without surface deviatoric stresses, (4) a fixed subglacial channel size, (5) different subglacial channel lengths (i.e., differing hydraulic gradients), and (6) different forms of subglacial base flow. While not as exhaustive as our parameter sensitivity study, we believe that these additions provide insights into what components of the model are important to moulin water level and moulin shape, and what components can be used in a simplified form.

We respond to each comment below. We also want to note that we have carefully considered the reviewer's comments on our application of elastic deformation. We still conclude that elastic deformation is an important component of moulin shape evolution, and we now make a concerted effort to better describe our parameterization of elastic deformation both throughout the main text and in a new Supplement. We believe that these improvements clarify the results and clarify the comparison between elastic deformation, viscous deformation, and phase change in the Discussion and in several figures.

Major comments

Section 2.2.1 - the rationale for modelling the moulin cross-section with this strange egg shape was weak for me. It significantly complicates the model to do this, rather than to assume it has a circular cross-section, and it was not at all clear to me that there was any great advantage in doing so. It is also not clear how r_1 and r_2 are separately evolved, and this needs to be made clearer. I ended up with the impression that the difference is likely because the open channel flow above the water line gives rise to a change in one of these but not the other; but below the water-line it seemed that r_1 and r_2 would evolve identically and therefore stay the same, assuming they start the same? However, this should be made clear by telling us what exactly are the equations that govern the evolution of r_1 and r_2. I would, at the same time, encourage the authors to think about simplifying things and assuming circular symmetry, since I think many of the results would still apply, and I think it would give a model that is more likely to be adopted by others.

The reviewer makes a good point about our choice of horizontal coordinates. As deduced, the primary reason we chose an 'egg' geometry is to adequately represent the evolution of the moulin above the water line, however, with the goal of preserving the chosen model geometry while permitting simplicity, we have 1) included a description of why we chose a 'egg' geometry in the *Moulin shape coordinate system* section, 2) included a comparison of water level and moulin water storage in a new set of model runs (see our introductory response above), and 3) included the necessary module and run script changes in MouSh to allow users to select the desired geometry (see our Github repository and the *Model description* section).

Section 2.2.2 - I had great difficulty following the treatment of elastic deformation, and am slightly concerned that this is not dealt with correctly. In particular, a number of figures (figure 6, figure 9) compare viscous and elastic 'deformation' as a *rate*, with units m/d. Elastic deformation is not a rate - it is an instantaneous deformation and it results in a displacement (relative to some reference state) that is fixed, for fixed stress - in this context, that is the change in radius given by (4). Presumably this must be viewed as relative to some 'reference' radius that can evolve in time due to viscous deformation and phase change. The elastic displacement in (4) does itself evolve in time due to changes in water pressure, and therefore gives rise to a deformation rate that is d(\Delta r_E)/dP * dP/dt, i.e. proportional to the rate of change of water level, and perhaps that is what is being plotted in these figures, but I did not really have this impression. If that is indeed what is meant, note that the elastic deformation rate depends on the rate of change of P, not on P itself, so whether the pressure is above or below overburden is irrelevant to the sign of the deformation rate (it is instead a question of whether P is increasing or decreasing).

The reviewer is correct. We did not initially realize this subtlety (we're really calculating dR_E/dP * dP/dt) until pointed out here. Thank you. We now include a clarification of this point in the *Elastic deformation* section and within the derivation of elastic equations in the Supplement. (Note we also include a dR_E/da * da/dt term, where "a" is the moulin radius as calculated by the other modules.) The elastic Supplement includes an extended description of the assumptions made, the derivation of the elastic equations, including versions with zero deviatoric and shear stresses, and our parameterization of instantaneous elastic deformation over our model timestep (the information plotted in Figures 6 & 9).

This concern is tied up with the question above of how exactly r_1 and r_2 are evolved. It seems to me that you would want to have 'reference' values of these that evolve according to the viscous processes; they satisfy an equation of the form dr/dt = melt-back - viscous closure (very similar to the subglacial channel in (28)); and then you want to add the elastic deformation given by (4) on top of those evolving reference values to get the actual radius at any instant in time.

Indeed, we do evolve the moulin radius by using a reference state to which the phase change, viscous deformation, and elastic deformation are added after each time step. In the revised manuscript, we made an effort to express the equations in the text to be uniform and to clearly describe the use of this reference state. To include elastic deformation, we re-express elastic deformation as a 'rate' according to Eqn S9.

In equation (4), I would be inclined to simply ignore the deviatoric stresses, which I expect are relatively small in most cases compared to the effective pressure P (it was not clear to me what you have actually assumed for them in the examples). Given that you are comparing with a null model which contains no moulin physics whatsoever, I think there is some advantage in not making this one overly complicated! Note that there are in any case some missing brackets in this equation. In equation (5), the first term is presumably set to zero for z larger than h_w (i.e. above the water line)? This equation could be made more consistent with (6), which is essentially the same thing, but where P is now called sigma_z. (6b) should again by zero for z larger than h_w, I think.

We thank the reviewer for detailed consideration of the model equations. We reformulate equation 4 to provide the initial and time evolution of the elastic radius and include an option without deviatoric stresses: $\Delta r_E = \frac{r(1+\nu)}{Y}P$, in the Supplement. We agree that in our current model, deviatoric stresses can over-complicate the model and their values can be difficult to constrain. To examine the impact of deviatoric stresses, we complete a comparison between the model with and without deviatoric stresses in the elastic module as part of the new sensitivity study. We now include this option in the model code.

Equation (9) is a strange way of discretising the time-derivative and this is where confusion starts to arise as to how r is actually evolved, because this gives an incremental change in r (both r_1 and r_2 ?) due to only viscous processes, and it is not clear how this is combined with the changes due to phase change and elastic deformation. The viscous closure of a moulin due to (7) is essentially identical to that for a subglacial channel as described in (27) and as described by Nye (1953) for the closure of a borehole. I think it would be helpful to express it as a contribution to the time-derivative dr/dt, as (effectively) done in (27).

We rewrite Equation 9 as $\frac{dr}{dt} = \frac{r}{dt}(e^{\epsilon} - 1)$(we have also substituted $\epsilon = \dot{\epsilon} dt$), although this requires the RHS to have a time differential (dt) without a corresponding radial differential (dr). We note that this was the case in Catania and Neumann (2010), although their variable *t* is really a time interval (~dt), so it is hidden.

Section 2.2.2.2.2 (I don't think I've ever seen quite so many subsections!)

We eliminated two levels by removing the remote sensing section in response to Reviewer 1 and simplifying the ordering. The maximum subsection depth is now three and much more manageable.

The downstream deformation of the ice is interesting, but it wasn't clear to me how it is incorporated into the model. It seems like it translates the 'centreline' of the moulin? But doesn't affect r_1 and r_2? So does it actually have any effect on the rest of the model or is it just relevant for the visualisations like in figure 8? The formula in (10) assumes no slip at the bed, which is presumably not always going to be the case?

The shear deformation does not affect the moulin radius, so it is (as the reviewer says) just relevant for visualization. We discretize the moulin as a stack of planes with holes in them; one can see how shear between adjacent planes does not affect the radii of the holes. We add a sentence to clarify this point.

Basal slip is likely in the real world, and in a complete model of the englacial and subglacial system, basal slip could be relevant - though perhaps really only on timescales longer than the one melt season we model here. Our current parameterization does not include slip at the bed. Our coordinate system starts at the bed: the origin is where the uphill wall of the moulin meets the bed. We add text clarifying this in the *Shear deformation* section.

Section 2.2.3 - I was a bit confused why melting and refreezing are treated separately - you could simply write down an energy balance that allows for either to happen automatically, depending on the relative magnitude of turbulent heating and the conduction into the ice, without having to have any 'switch' between melt season and not. In (11), I would have thought that the dT/dx should really be a dT/dr, i.e. the radial temperature gradient away from the (roughly) cylindrical moulin; the distinction between them is quite important because conduction around a point source in two dimensions (ie. in the x,y plane) is very different from conduction in one dimension (i.e. in x alone). That said, solving the heat equation in the ice for each different z seems a lot of work for a model of a single moulin, and I wonder if a reasonable approach would be to simply *estimate* the temperature gradient at the moulin wall, dT/dr, as (\Delta T)/r_m, where \Delta T is the temperature difference to the far-field ice and r_m is the moulin radius. That would be consistent with the way you incorporate the estimate of sensible heat in (18) when considering melting.

We choose to separate freezing and melting primarily because we initially developed the model with only melting, following many subglacial models, but then included refreezing. While we see the point of the reviewer's comment, we choose to keep them separate, so that they retain consistency with the current, widely used subglacial equations. However, we do appreciate the reviewer's comment about the temperature gradient and make the relevant changes in our model code and manuscript.

The salient question - in regard to the temperature gradient across the monulin's radius - is how long does refrozen ice persist inside the moulin. In a calm environment with no summertime melting, refrozen ice would persist arbitrarily long, and the refreezing rate would drop over time as simultaneously (a) the refrozen layer thickened, and (b) the latent heat conducted into the ice sheet warmed the ice, lowering dT/dx (or dT/dr). On the other hand, in a turbulent environment with significant summertime melting, each winter's refrozen ice may quickly melt away, exposing cold ice to the moulin water. This should allow a fairly uniform pattern of refreezing each winter (Eqn. 8-9 in Alley et al., 2005). In our original paper, we incorrectly assumed the first scenario. In fact, the thickness of refrozen ice (~0.5 m over one winter) is much less than the thickness of ice melted off the moulin wall in summer (~0.1 m/day). Thus, the second scenario applies, and we now calculate the refreezing rate based on a simple equation (Eqn. 8-9 in Alley et al., 2005 and Eqn 13 in the *Refreezing* section of our revised manuscript) proportional to the square root of time elapsed since the end of the turbulent melt season (start of the refreezing season). Note that in response to Reviewer 1, we indicated that we would remove this parameterization; however, upon consideration of this comment, we now choose to retain it.

Section 2.2.4 - Equation (22) needs to include Q_base, similarly to equation (24). In fact, there seems to be some inconsistency and duplication between (22), (24) and (29). These equations are all expressing mass conservation, and (22) and (29) are really the same equation (I assume that the m in (29) must include the freezing rate -delta as well). But I think they should include Q_base if you're going to include Q_base in (24). And I think (24) should really include some terms to account for the rate of change of the cross-sectional area (it comes from inserting V_m as the integral of A_m from 0 to h in (29)).

Yes! Eqn 22 should include base flow and now it is consistent with Eqn 24; Eqn 29 now includes refreezing (this was an oversight in the original manuscript). We made an effort to revise, simplify and ensure consistency across all equations.

Section 2.2.4.2 - I think it would help to have a schematic picture of the moulin and the subglacial channel showing some of the various variables. It is slightly frustrating - but I can see that it may be unavoidable - to have the moulin shape model coupled so tightly to a subglacial channel model; ideally you'd like to be able to model the moulin separately. In this case, it seems that the subglacial channel is assumed to run from the bottom of the moulin to the ice-sheet margin, along which length the channel cross-section would presumably vary in reality, but I think that you assume a single value of S (the value at the bottom of the moulin?) is sufficient to describe how the flow evolves? This seems a reasonable simplification here, but I think could be explained a bit better, and as I say, a diagram might help. The 'b' in the hydraulic gradient on line 339 seems to disappear when this term is inserted in (28). The diagram might also help to explain Q_base, Q_in and Q_out. The use of Q_base seems fine to me, as for most moulins there will likely be water arriving at the bottom of the moulin from upstream as well as via the moulin.

All great points. We now include a detailed schematic of the model as a part of Figure 1. It includes a visual description of model terms and variables.

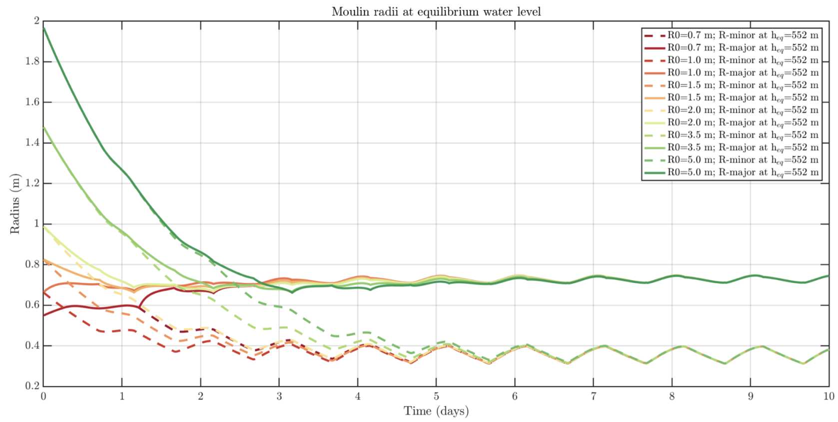
We initially developed MouSh with a fixed radius subglacial channel; however, identifying an appropriate constant subglacial channel radius was an impossible exercise, since the channel size significantly affects moulin water level/variability and could not be used for an entire melt season. For this reason, we needed to include an evolving subglacial channel. We share the reviewer's frustration about not being able to isolate the impact of moulin variability, so we now include a model run with a fixed subglacial channel geometry in the new sensitivity study.

Subglacial channel cross-sectional area is taken to be representative of the channel halfway between the moulin and terminus - this is now clarified in the model schematic. We acknowledge that the simplified subglacial channel model is not fully realistic, but feel it is a reasonable middle ground between a non-evolving subglacial channel (see problems listed above) and a full subglacial model (which would shift the focus and free parameters away from the moulin and into the subglacial system). Coupling with a full subglacial model is part of our future plans.

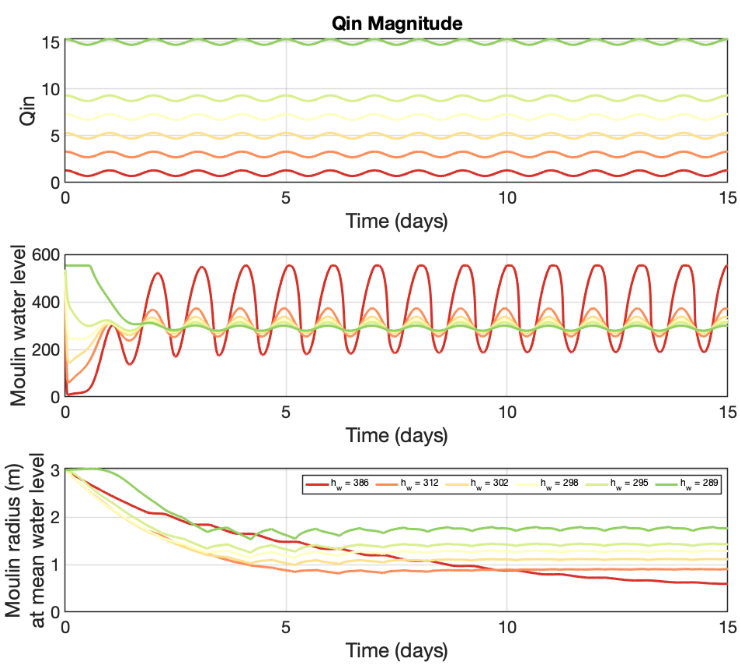
We are happy that the reviewer finds our Q_base parameterization adequate. We do include a small suite of different base flow types on moulin geometry in response to Reviewer 1.

Section 3 - The results section focuses a lot on parameter sensitivity, and it is great that this has been explored so thoroughly, but I found this hard to follow without it having first been outlined some of the general behaviour of the model. In particular, I think it would be helpful to see some sort of figure showing the periodic states to which the moulin apparently evolves. Just the fact that the modelled moulin approaches an 'equilibrium' does not seem an obvious result, and I think that equilibrium could be described a bit more fully. Presumably it involves the water level moving up and down on a diurnal timescale, and the moulin opening and closing? It would also be useful to know how this depends on the moulin input Q_in (for me that would seem more of interest than dependence on drag parameters etc, which we don't know very well). It seems quite surprising to me that if such an equilibrium is really reached, it depends on the initial moulin radius. Also, has the moulin model been run over the course of multiple years (with melt season and a winter), and how does it behave? This has implications for what an appropriate 'initial' moulin radius is, presumably. I think it would be helpful to have some general discussion along these lines, and figure(s) (perhaps like figure 6 or 8) that show the general behaviour of the model, before going into detail about how certain outputs depend on the parameters, since it would help give those more context.

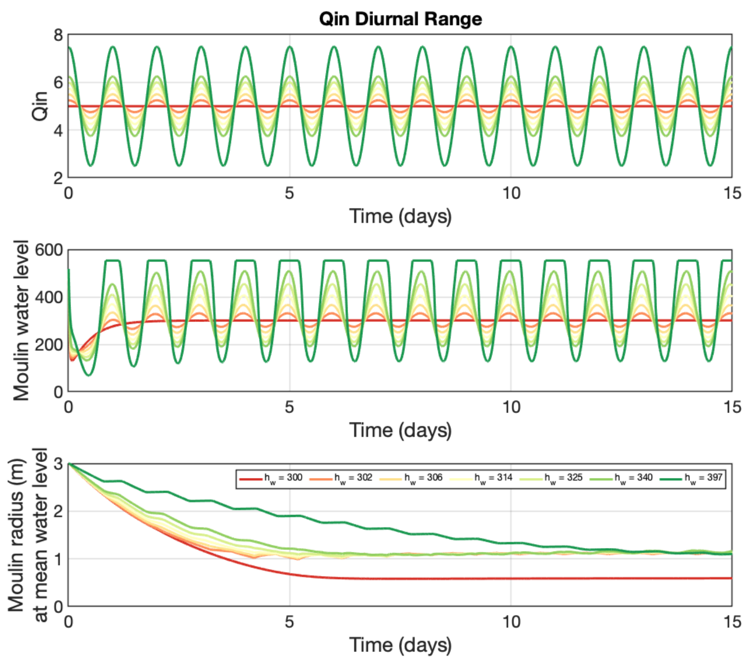
Great point: the result that the moulin approaches a quasi-equilibrium state (with “quasi” to denote the diurnal fluctuations around a steady mean state) could be a significant result. This quasi-equilibrium state is not dependent on the initial moulin radius (figure below). We now make sure to explicitly state that the moulin’s quasi-equilibrium state is independent of the initial given geometry.



We now include sections in the Results and Discussion that highlight the quasi-equilibrium state reached by the moulin, including how the characteristics of Q_in affect that state. As part of this, we include a figure highlighting how the magnitude and diurnal range of Q_in impact the moulin water level and geometry. We also include a new figure that examines the behavior of the model components. In summary, when we vary the absolute magnitude of Q_in but not its diurnal range (panel 1 below), we find that the quasi-equilibrium water level decreases, as does the diurnal variability (panel 2 below). This occurs in concert with an increase in the major moulin radius at the equilibrium water level (panel 3 below). The geometry and water level adjust such that elastic and viscous deformation are generally equal to melting over a given day.



We find that increasing the diurnal range of Q_in (panel 1 below) results in an increase in the quasi-equilibrium water level and its daily range (panel 2 below). Interestingly, the moulin radius at the mean water level is generally very similar, though with small diurnal differences, suggesting that the moulin exhibits a relatively constant geometry even with different diurnal ranges (panel 3 below). The only exception to this is when Q_in has no diurnal variability, due to the differences between water-filled and open-channel turbulent melting. These results indicate that Q_in does affect the quasi-equilibrium state of the moulin, but in a predictable fashion. Further analysis is included in the revised manuscript.



We stand by the importance of testing sensitivity to uncertain parameters like the drag coefficients. We found that modeled moulin geometry, unfortunately, has high sensitivity to the most uncertain parameters (drag coefficients, especially above the water line). This is an important result because it suggests the importance of moulin exploration above the water line in order to constrain moulin shape and water flow there.

We do not run the model over the winter. The quasi-equilibrium state is independent of the initial moulin radius and there are a number of complexities to explore with regard to winter freezing, creep closure, and re-opening (e.g. Catania & Neumann, 2010). We hope to explore this in a subsequent manuscript.

Figure 6 - see my earlier comments about comparing elastic and viscous deformation. I just don't understand what is actually plotted in panel f and g. Could you express whatever quantity is being plotted in terms of variables in the equations? Similarly for figure 9, and the associated discussion in section 4.5

We now include a Supplement that describes how we take an instantaneous term and apply it over a timestep, making it comparable to viscous deformation in Figures 6/9. We also clarify this point in the *Elastic deformation* section. Both these changes allow for a better understanding of the comparison presented in the *The role of elastic deformation in moulin geometry* section.

More minor comments

L82 - why does taking k = 1 approximate likely channelized pathways? The usual thinking is that channels would tend to *lower* the water pressure and would therefore be associated with a lower value of k, if anything.

This section has been removed to focus the manuscript more clearly on moulin geometry, in response to Reviewer #1.

Figure 1 is very nice. It might be noted that the elastic deformation here is quite different from all the other ones, in that the others are all *rates* - they accumulate every timestep to give continued deformation - whereas the elastic one is just static.

We change the caption to briefly explain the difference between elastic deformation and the rest of the terms. We also include a new panel with a model schematic.

L185 - the small component of melting due to temperature differences between the water and ice seems to be ignored in the model, since it is later assumed that the water is at the melting temperature ?

Yes, we ignore the sensible heat of the moulin water. This is justified because (a) it is small (sensible heat from 1°C is ~1/160 of latent heat), and (b) moulin water temperatures are not well known. We now state this explicitly in this section.

L255 - you seem to use both hydraulic diameter D_h and hydraulic radius R_h and it would keep the notation simpler to just work with one or the other.

Done.

L262 - it sounds like in the end you take f_R to be fixed (and vary it's value) so I wasn't sure what the point of introducing (16) was.

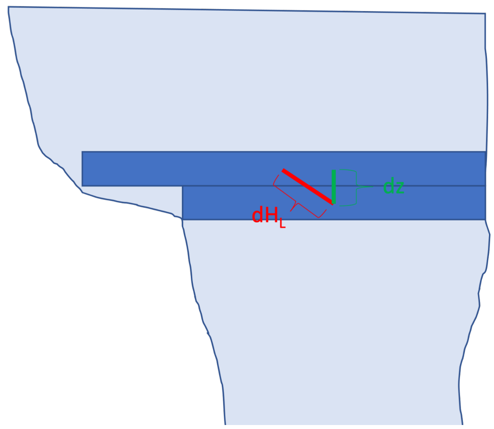
We simplify this section by removing the equation and reference to the Bathurst parameterization of friction. We agree it is unnecessary because in all model runs, we keep the friction factor fixed at a user-defined value.

In (18) presumably S is really A_m, the moulin cross-sectional area?

Yes. This was an error. We now replace S with A_m.

In (19) is dh_l/dz the same as dh_L/dL in (15), and is there significance in the change from lower case to upper case subscripts?

The reference to dh_L/dL on the line following the equation is in error, a holdover from a previous iteration. It should be dh_L/dz, where z is the vertical step height within the moulin and dh_L is the path length from the center of each vertical step, taking into account the offset between moulin elements (see below for a sketch). We fix it and include a small diagram in Figure 1.



In (23), time appears to be in hours, not days.

Great point, hours it is. We have now corrected this.

In (24), h is the same as h_w ?

Yes it is; we have now corrected this and ensured that equations are consistent with Figure 1b.

Figure 7 - should there be a purple line in panel (d)?

The line for the 741 m moulin (purple) is hidden behind the 533 m moulin (black). We will clarify this in the caption.

L662 - I wasn't able to see this statement about the fixed moulin frequently overtopping the moulin in Fig 11a. How does the figure show this?

Hmm, we aren't sure about what we meant by referencing Figure 11 here. We revised the text and now reference a figure associated with the new set of sensitivity runs.

References

Alley, R. B., Dupont, T. K., Parizek, B. R., and Anandakrishnan, S.: Access of surface meltwater to beds of sub-freezing glaciers: preliminary insights, 40, 8–14, https://doi.org/10.3189/172756405781813483, 2005.