Author response to

Review of “Controls on Greenland moulin geometry and evolution from the Moulin Shape model”

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Response to Anonymous Referee #2

Authors’ responses are inline in blue text.

Thank you for addressing my previous comments on the manuscript. Many areas have now improved, including the focus on moulins rather than the subglacial system, the description of the elastic equations, and reporting of the sensitivity tests/results.

Thank you and thank you for the second careful review. We very much appreciate the attention to detail. Responding to your comments has improved the manuscript substantially. We, again, carefully considered how elastic deformation is treated in the model. This resulted in several changes to Supplement 1 and an update to elastic.m. We reperformed all experiments with updated elastic deformation, correct subglacial deformation, and a more realistic enhancement factor. The figures and text reflect these updated results. Although the overall conclusions are similar, there are differences: the substantial reduction in elastic deformation increases the time for the model to reach quasi-equilibrium; the correction to subglacial deformation results in an increase in diurnal variability within in the subglacial system and a decrease in diurnal variability within the moulin. These changes also minimized differences when compared to a static cylindrical moulin. The results and discussion have been revised to reflect these changes.

The figures, figure references, equations, and constants are now consistent. We apologize for any confusion arising from inconsistencies arising through the review process.

The line number issue is odd. We made the numbers smaller and tighter to the text to resolve this issue.

I have two major comments remaining, the first of which is again related to elastic deformation. The new explanation for elastic deformation is certainly clearer to follow and it makes much more sense to me now that you have added in equation 7 demonstrating how you convert from instantaneous elastic deformation to a rate based on the rate of change in pressure in the borehole. However, I was surprised that none of your figures or results changed following application of this method, and that you mentioned that this was how you were calculating elastic deformation in the initial round. I had a look at the code you provide in the git repository and from that it looks like you’re still just plotting cumulative instantaneous elastic deformation rather than

taking the change in pressure into account (elastic.m, moulingeom_fcn.m and simpleplots.m). Here it seems like you calculate the absolute elastic deformation for each time step based on the current water level, and then use each of those timestep outputs as the deformation rate. Instead, according to your equations, you should be calculating the deformation rate based on the change in pressure since the previous time step, not the absolute pressure. From some quick calculations it looks like it should be about an order of magnitude smaller than you've plotted.

You are correct. We made a mistake. This comment forced us to reconsider the role of elastic deformation in moulin evolution (again) and revisit to our derivation. We made some modifications to Supplement 1 and the equations in the main text and reperformed all the model experiments with the correct formulation in elastic.m. Elastic deformation now has a negligible impact on moulin geometry and evolution (see Supplement S2.2.3). This result is discussed in Section 4.5.

Another potential issue I spotted in the online code is in moulingeom_fcn.m where c2 is listed as 1An^-n but in your manuscript it's 2An^-n (P11 line 18).

Fixed! Even though we feel a bit silly, this is the benefit of open code development. All model runs completed with this and elastic changes.

Section 4.5: In the first round of reviews I suggested that focusing on englacial void ratios while discussing the role of moulins in the hydrological system isn't directly useful and your response was that it puts your results in the context of adjacent ideas. My concerns still remain but I will cede that it is useful to include if you're clear that, if the time-varying and evolving moulins are used in a subglacial model, their capacity for storage could remove the necessity of applying an englacial void ratio. However, in lines 59-77 (P25) it seems that you're suggesting calculating an englacial void ratio for entire catchments based on parameterized moulin-like responses? I'm unclear how this would be achieved with the results that you've presented and this goes beyond what you are able to discuss in the manuscript. In general, as the subglacial model would only represent reality if the moulins were used as water inputs/storage features, having a catchment-wide englacial void ratio parameterization seems moot. This argument stands even if using a static moulin shape. Lines 59-76 therefore do not add to your argument so I suggest you remove them.

It was not our intention to suggest that we could parameterize the evolution of englacial void ratio. We removed the suggested lines and made some modifications to the text to soften the language and suggest that more work needs to be done before concrete conclusions can be drawn.

I list my line-by-line comments below but something happened with the manuscript line numbers which cutoff the first number. So from page 5 onwards I list the page number and associated line number that I can see.
Sorry. We fixed this issue in this round.

13: instead of 'and models melting', just 'and melting'
Done

19: repetition of ‘representing’
Changed the first to ‘Implementing’

98-99: you should refer to the Table where you list these values.
Done.

P5 53. You have F* as the enhancement factor but this is still listed as E in the table. You also need to define ‘n’.
Changed E to F* and included n in the table. We also updated Table 1.

P6 81. Define Lf and ki
Done in text and in Table 1

P7 83. Using ‘thus’ doesn’t follow here.
Removed.

P7 01. ‘Although’ instead of ‘though’
Changed here and throughout the text.

P7 11 – Symbology for both ODE and PDE in this sentence.
Changed to ordinary on line 214.
P10 85. ‘near the margins of the Greenland Ice Sheet’?
Added ‘of the GIS margins’

P11 eq 27. You defined N on page 3 as Pw-Pi. Usually N is Pi-Pw and would be applied as such in equation 27 so you’ll have to redefine it as something like Ns for the subglacial system. Alternatively change ‘N’ to ‘P’ for the moulin pressure as you have in the supplement.
We more carefully define P and N in the text and also add a line “Note that P is not effective pressure, which is defined as $P = P_{\mathrm{i}} - P_{\mathrm{w}}$ (Cuffey and Paterson, 2010)” to clarify this point to readers.

P11 18. Lf should have been defined further up in the manuscript so should be removed here.
Checked all equations and defined constants when first used. In some instances, we define them a second time for clarity if it has been several equations since a constant was last used.

P12 40 This is confusing where you talk about steady diurnal inputs then switch to talking about seasonal and diurnal melt variability.
Completely right! Removed that sentence.

P12 53 Isn’t this now deformation enhancement factor F*?
Yes, fixed.

P12 56. Which enhancement factor value did you choose when running these other tests and is there justification for it?
The Enhancement factors used are listed in the first sentence of the paragraph, but we add additional clarification about the range of enhancement factors expected in reality. “While the range of enhancement factors tested here cover a variety of ice conditions, including ice shelves and temperate glaciers, the Greenland ice sheet likely has values between 4 and 6 (e.g., Cuffey and Paterson, 2010). Outside of testing the model sensitivity to the enhancement factor, we assign F^* a value of 5.”

P13 81. The 2019 melt season? Above you say the 2015 melt season.
Apologies, not really sure where 2015 came from.

Section 2.5. Where are the corresponding figures for each of these sensitivity tests?
In each subsection, we now identify the location of the sensitivity results and associated figures.

P14 05-08. Which parameters do you change for these tests and to what value? The choices are now more clearly defined in the main text and direct references to the subsections are listed.
P14 09-11. I don’t understand how changing the supraglacial input to the moulin can test an evolving vs. fixed radius subglacial channel. The language here was unclear. It's now fixed and the description in the supplement is now directly referenced. Essentially, we used a simpler Qin when testing a fixed subglacial channel because it is extremely time consuming to find a fixed S that ‘works’ (e.g., the moulin isn't empty or always completely full).

P14 22. This should be ‘mean moulin water level’
Fixed

P15 35. Do you mean a larger than expected moulin compared to other runs with smaller magnitude of input?
Yes, clarified.

P15 45. Not sure what Figure S2.4 g is.
Neither are we. Sorry! Fixed S5d

P15 48. Ice flow enhancement factor listed as E again. Also later in the paragraph.
Fixed.

P16 82. Is this E Youngs modulus or the ice flow enhancement factor?
Went through and did a through check and now everything should be correct.

P16 83-85. A lot of ‘which’s’ in this sentence.
Completely fair, we eliminated most of them.

P17 97-00. It would be good to mention here again that the ice thickness and distance from terminus are proxies for hydraulic potential gradient which will be the main driver for these

parameters rather than assuming moulins in thicker ice always increases the moulin size and volume capacity. Done. Now we include this text: ”These changes act to directly modify the hydraulic potential gradient of the moulin system. The modeled ice thicknesses and distances from the terminus result in hydraulic potential gradient variation between 0.009 and 0.2 (~180%)...”

P17 05. When you say you use standard values, do you mean the median values that are listed in Table 1?
Yes, changed to be clearer.

P17 06-07. This is repetition from the previous section.
It is, we modified this section to more clearly discuss the roles of different model components on moulin shape.

P18. 29. Although (also line 32). Check rest of manuscript for this.
We changed most instances of though to although.

P18. 38-43. Something strange going on with Figure numbers here. I think you mean Figure 6 when you say Figure 7 and Figure 8 when you say Figure 9?
Yes, I thought I checked this, but with the removal of the remote sensing section, the figure numbers shifted and the reference to the figures got fumbled. Figure references are now correct.

P18 41. ‘Elastic’ rather than ‘ecstatic’.
Hahaha… fixed.

P18 53. I don’t think you mean Fig 7m-n here.
Fixed.

P18. 61-62. ‘To test this idea…’ This is confusing coming at the end of this section. Move to the next section.
Deleted since there is an introductory sentence in the following section.

P19 74-77. Long sentence. Consider splitting.
Done

P19 88-91. Clarify here which moulin type dampens the supraglacial input signal, moulin water levels and increases subglacial channel size.
Now reads: “Changing moulin capacity…”

P20 97. A new paragraph would be helpful here.
Done, but this section was also modified due to changes in the elastic formulation.

P20 05-06. What inputs resulted in runaway growth or collapse?

This now reads: Finally, we examine the impact of fixing the subglacial channel cross-sectional area *S*. Experimental results using a fixed *S* and a seasonally evolving melt curve resulted unrealistically low or zero water levels during low, early season Q_{in} and complete viscous collapse of the moulin if subglacial S was prescribed to be too large, or persistently high (always above the ice thickness) water levels and runaway moulin growth if subglacial S was prescribed to be too small. Therefore, we explore the impact of fixing *S* using a constant mean Q_{in} with an overlaid diurnal variability (Supplement Sect. S2.2.6).

P20 26. '…Maxwell time for ice, are more likely…'
Fixed.

P21 41. Remove one of the 'which's fixed
Fixed.

P21 44. Which differs from what?
Clarified.

P23 94. I'm not sure why 'nevertheless' follow from the previous sentences. Is that because Covington et al (2020) had a fixed geometry moulin? This could do with clarification.
We clarify this to read: (Covington et al. (2020) use a fixed moulin geometry), or that their measured water levels were not from the same moulin they mapped englacially. However, our results suggest that an evolving moulin capacity may be important to represent realistic moulin water levels. During much of the melt season, modeled water levels within an evolving moulin are lower and less variable than in a fixed, cylindrical moulin (Fig. 10).

P23 02. The primary closure mechanism.
Fixed, thanks.

P24 28. What do you mean 'the space of viscous and elastic parameter values'?
Clarified, but this section changed substantially.

Fig 1. Would be helpful to include the component symbol (e.g. ud, drv) in brackets after each colored label to make Figure 2 clearer.
Excellent point. Done.

Fig 3. Left hand side y axes should run 0 to 0.3 so the results are clearer. I mentioned this in my last review and your rebuttal was that it wouldn't leave room for the subpanel numbers. It would be better to have these subpanel numbers above each graph, or on the right-hand side to have the y-axis at a lower range.
Fair enough. We moved the panel lettering to the outside of the panels in Fig. 3 & 4. We also made an effort to improve the scaling on all axes and place the panel letters in a visible (though not always consistent) place.

Figure 3 and Figure 4. I'm unclear what the difference is between these figures. I see the differences in the subplots but from the captions it's not clear what they're each showing.
There was a mis-labeling in axes labeling and these are now corrected.

Fig 5 g. Would be helpful to have the moulin water depth also plotted in this figure to see how it relates to the phase change and deformation outputs. You also mentioned in your rebuttal that you now include the combined change in moulin shape in Fig 5 g, which I don't see here.
We updated Figure 5 to include the water levels (panel h). While we had initially proposed to include the total change but failed to include it, we now choose to not include it in order to prevent the plot from being too busy.

Fig 8. You say in the caption that all the grey lines are <1 but around day 35 it looks like they're above zero (as discussed in the text). I find it difficult to differentiate between the grey and purple lines in subpanel b. It would be clearer if the legend said 'Elastic:viscous ratio' and the y axis also refer to the ratio.
We removed the gray lines; with the modification of elastic deformation, it is substantially smaller than viscous deformation. We do keep the phase change: viscous ratio and change the y-label accordingly.

Fig 10. In the caption you say basin 1 is black and basin 2 is purple. They all look purple to me.
A remnant from the previous method of plotting. Now it is correct.

S1.2 why not have the hydrostatic stress in the same notation as the main manuscript (eq 3c)? This applies generally to the equations you present in the supplement.
We changed Eqs. 3a-c and the Supplement and main text now both use z.

S1.2.1 with v = 0.3, the difference for the plane stress solution is 9%.
This section was modified to test the impact of elastic deformation instead of surface stresses.

S2.2.2. Which moulin capacity is 47% smaller?
Fixed, the circular moulin is smaller (31%).

S.2.3 'Though' at the beginning of the sentence in the second paragraph is confusing – would be better if the sentence started with 'Overall'.
Fixed.

S2.2.4 'Reducing L reduces' should be rephrased.
Fixed.