Author Response for “The impact of surface melt rate and catchment characteristics on Greenland Ice Sheet moulin inputs”

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Reviewer Comment	Author Response	Manuscript changes

Reviewer 1

General Comments

In the paper entered “the impair of surface melt rate and catchment characteristics on greenland ice sheet moulin inputs” the authors apply the SaDS surface meltwater routing model to a group of catchments located in west Greenland. The authors compare low and high intensity melt seasons to determine the relative importance of surface melting on meltwater inputs to moulins and the impact of supraglacial drainage system evolution. The authors find that supraglacial drainage system develop has a more pronounced influence on meltwater delivery to moulins in years with lower melt rates, while going on to provide recommendations for when to apply the computationally expensive SaDS model over other less expensive models. The authors have addressed many of the concerns raised in the last round of review and as a result the manuscript is much improved. I have included a few major concerns that should be addressed before publication, however once addressed I think he manuscript will make a significant contribution to the literature. A robust model such as the SaDS model will be increasingly significant as our in situ glacial hydrology observational record continues to expand.

We appreciate the reviewer’s detailed comments and have responded individually below.

We would also like to highlight a change to the statistical analysis in Table 2. While investigating the reviewer’s comments to Line 126-136 and Table 2, we found a mistake in the calculation of R^2 and p values. This has been corrected, all other code has been double-checked, and Section 4.1 has been updated accordingly.

Major Comments

In this paper lag times are presented as between solar noon (~15:22) and peak moulin inputs, rather than the lag time between peak melting and peak moulin inputs. Using the later definition would be inline with previous studies (). As is written now, it is unclear whether the timing of solar noon is allowed to change (presumably due to the approximate time stated in the manuscript), nor is it clear if there is a lag between the timing of peak melting and solar noon. This may become problematic for example if peak melting during transient melt events did not coincided with local solar noon (ref to the results presented on L113-115). If so, the longer lag time in moulin input would be an artifact of the timing of peak melt rather than caused by the supraglacial drainage system.

We have compared the timing of peak moulin inputs to solar noon due to the limited temporal resolution (3 hours) of the RACMO data used to drive SaDS. Smith et al. (2021; Fig. 3) and Mejia et al. (2022; Fig. 2) suggest that the range in timing of peak melt is less than three hours (the resolution of the RACMO data), so we can not resolve these slight variations. Fortunately, Mejia et al. (2022; Fig. 2) suggests that comparing peak moulin inputs to local time vs. peak melt does not make a significant difference in the interpretation of the timing of moulin inputs. Furthermore, our approach is not unusual, for example Muthyala et al. (2022) compare the timing of peak stream discharge to local solar noon. We have acknowledged this limitation in the revised manuscript when comparing our modelled lag times to the time between peak melt and peak stream discharge from Smith et al. (2017, 2021) (Line 267-272):

However, these differences in lag time should be interpreted with caution since Smith et al. (2017) and Smith et al. (2021) report lag times relative to peak melt rather than solar noon. This difference could be important, for example, if local weather conditions modulate the timing of peak melt relative to solar noon (e.g., Smith et al., 2021). On the other hand, the difference in timing between solar noon and peak melt reported by Mejia et al. (2022) for 0.2 km^2 and 16.7 km^2 catchments is less than three hours, so we would not be able to resolve these differences with our three-hour resolution surface melt forcing data.

Statistical Analysis
A few things regarding the statistical analysis presented on lines 126-136 and in Table 2 are unclear. First, the R^2 typically represents the coefficient of determination whereas r^2 would be the square of the Pearson correlation coefficient. I assume the text is referring to the former as it is stated that R^2 is equal to the proportion of variance explained by the independent variable. I know this is very in nit-picking but it is important to be precise here. The Pearson correlation coefficient (r) should also be included in this analysis.

The reviewer is correct to point out the general distinction between the coefficient of determination (R^2) and the squared correlation coefficient (r^2). However, for the comparison as described here, we have computed these two quantities independently and verified they are identical. For clarity, we explicitly refer to the coefficient of determination (R^2) throughout the text. For example, Line 133 now reads

The extent to which surface melt rate controls these features can be quantified by comparing the coefficient of determination, R^2, between melt rate and each of the moulin input rate, diurnal amplitude, lag time, and lake water level (the coefficient of determination is equal to the square of the Pearson correlation coefficient, r^2, for linear regression).

In the same regards, I am confused by the stated maximum and minimum values for R^2 as there should be a single value given for each of the correlations.

We have stated that the comparisons are carried out for each of the seven sub-basins, meaning that we obtain seven R^2 and p values. We have chosen to report the minimum and maximum of these seven R^2 values in Table 2 to measure how these relationships vary by basin. The caption for Table 2 has been expanded to explain these values.

Table 2. Coefficient of determination (R^2) and p-values for the null hypothesis that there is no relationship between the specified variables. Coefficients R^2 and p-values are computed independently for each of the seven sub-catchments and for each year. The tabulated min and max R^2 values represent the minimum and maximum R2 values taken across the seven catchments for a given year, and the p values represent the maximum value across the seven catchments. Coefficients R2 and p-values are computed for model outputs at native 2-hour resolution and binned into 24-hour increments

Table S1 does not give statistics or p-values as stated in L134-L136.

We believe this confusion was caused by an old version of the supplementary material being provided to the reviewer. Table S1, accessible as described in the code and data availability statement, provides the stated statistics.

Additionally, even though p-values are small <10^-6 they should not be represented as 0 it is in violation of the definition of a p-value which is a probability.
The magnitude of all values are provided in the table.

And finally, there should be a figure added to the supplement showing these relationships as graphs are essential to correctly interpret regression analysis results.
We have added figures to Appendix B for each of the comparisons (Fig. B3–B7).

Regarding the interpretation of the statistical tests it is unclear how the p-values alone are being used to determine there are good correlations between variables while R^2 values range from 0.09—0.9, this issue here is not a low R^2 value but the variance between variables, years, and smoothing choices (e.g., diurnal vs daily). Moreover, I wonder if the lower R^2 values for the diurnal variables are a result of the lag time between variables. This is a common problem that is either solved by imposing a lag-time adjustment (e.g., Smith et al., 2021), or by instead analyzing forcing-response plots (e.g., Extended Data Figure 4 in Andrews et al., 2014). Due to the significant amount of text in the Results and incorporation within the Discussion (e.g., L165-170), I recommend explaining this analysis in more detail.

The additional scatter plots that we have now included (Fig. B3–B7) aid in the interpretation of the R^2 and p values. We agree that part of the reduced R^2 from two-hour resolution model outputs is due to the previously computed lag time. However, given the relatively little Discussion text devoted to the two-hour relationships and the additional context provided by the scatter plots, we believe it is interesting to evaluate the relationships as-is. The influence of lag time on these R^2 values is acknowledged on line 149:

The lower R^2 values obtained with two-hour model outputs may in part be due to the time lag between peak melt and peak moulin input rates.

In section 4.1 the manuscript states internal variability is most important on timescales shorter than one day, as evidenced by the statistical analysis. Is this conclusion supported by model results? Specifically, how are model parameters (e.g., channel water depth, incision depth, density, flow, etc.) changing on daily vs. sub daily timescales? In high melt vs low melt years (e.g., 2012 vs other years)? Figure C1 shows that there is diurnal variability in channel length, so how does this fit in? What is the breakdown between catchments (e.g., is this only important for large or small catchments? What controls this variability and how does this vary between years?
Channel flow metrics (water depth and discharge) change with similar characteristics as the moulin hydrographs, while sheet metrics (water depth, discharge) change with similar characteristics as the lake water level curves. Figure C1 shows the incision depth of supraglacial channels. This is similar for all catchments. While we appreciate the reviewers interest here, since this section is dedicated to interannual changes, we have removed this statement as it distracts from the intention of the section.

In the discussion comparing model outputs to other works from Rio Behar catchment lag times are compared to work by Smith et al., 2017. It is important to note here that the lag time reported in that paper are the time between peak melting and peak moulin inputs (this is different from how lag times are described in the present manuscript), and are accordingly not directly comparable.

See the response to the first major comment. This limitation has been acknowledged in Lines 267-272.

The discussion also describes the models limitations on refining the potential influence of supraglacial lakes on moulin inputs, is there a way to look at the outlet channels that drain the lakes and compare changes within those to other parts of the supraglacial drainage system to see if there are localized effects there on the draining lake? Alternatively, how do the lag times for catchments with lakes compare to a simple parameterization such as that used in Smith et al., 2017 (already cited within the manuscript)? I think understanding the influence of lakes on lags and meltwater inputs to moulins is very interesting and would be a significant contribution to the field of supraglacial hydrology. While I understand that lakes cannot be disentangled from your model domain, I wonder if comparison with a synthetic unit hydrograph could help parse out the lake's influence on lag times. (As stated previously, I would suggest redefining the lag determination used in the text to be consistent with other models (e.g., SUH/UH models).

We also believe there is more work to be done in evaluating the impact of lakes on moulin inputs, and we hope the current work and model can provide a starting point for such analysis. However, we struggle to constrain the relationship further with the currently available data. In particular, it is difficult to assess changes in the timing of peak flow given the RACMO data resolution. Given the model mesh used here, where each lake consists of several mesh elements, a lake-focused analysis may be best served by a domain consisting of just one small lake basin to better resolve detailed processes such as changes in lake surface area and how this impacts the additional time lag imparted by the lake. For these considerations, we believe it's most appropriate to limit the current analysis to basin-scale features.

Channel density is spoken of throughout the entire manuscript yet there are no figures showing channel density evolution (only Figure 1)

References to "channel density" throughout the Introduction and elsewhere have been changed where the sentence is made more clear by instead referencing certain aspects of the channel network (e.g., Line 21, 35). We have added a sentence in the Model subsection to explain how local channel processes impact channel density (Line 59):

The density of supraglacial channels therefore changes as individual channel elements melt out if stream incision is insufficient to balance surface ablation.

Minor Comments

L2-6: Runon sentence

This sentence has been changed to explicitly enumerate the points being made:

We apply the Subaerial Drainage System (SaDS) model, a physically-based surface meltwater flow model, to a ~20 x 27 km^2 catchment on the southwestern Greenland Ice Sheet for four years of melt forcing (2011, 2012, 2015, and 2016) to (1) examine the relationship between surface melt rate and the rate, diurnal amplitude, and timing of surface inputs to moulins, (2) compare SaDS to contemporary models, and (3) present a framework for selecting appropriate supraglacial drainage models for different modelling objectives.

L4: change to "and the timing of surface meltwater inputs to moulins"

We appreciate the suggestion for clarity and precision, but since we have stated "surface meltwater inputs" twice already in the abstract, we believe the shorthand (surface inputs) is easily understood.

L13: Add citations to Smith et al., 2021, and Mejia et al., 2022. (Full citations at the end of this document).

We have added a citation to Smith et al. (2021) here and to Mejia et al. (2022) on Line 266 where we believe it is most relevant.

L16: Add citations to Yang et al., 2020. Added
L20: Define "efficient" here. We have added a definition for efficient "(i.e., faster)".

L21: Define what you mean here by “evolution of drainage density”, the processes you describe typically control the evolution of a single channel (e.g., hydraulic capacity of that single channel), from the text it is not clear how these processes modify drainage density.
References to “drainage density” throughout the Introduction and elsewhere have been changed where the sentence is made more clear by instead referencing certain aspects of the channel network (e.g., Line 21, 35). We have added a sentence in the Model subsection to explain how local channel processes impact channel density (Line 59):

The density of supraglacial channels therefore changes as individual channel elements melt out if stream incision is insufficient to balance surface ablation.

L28-29: It is not clear what “supraglacial drainage characteristically acts to reduce the diurnal amplitude of moulin inputs” means, would the concentration of flow by supraglacial drainage systems not increase the amplitude of diurnal meltwater inputs to moulins?

What we intend to convey is that the diurnal amplitude of moulin inputs is typically much less than the diurnal amplitude of surface melt rates, and that the timing of peak moulin inputs lags the timing of peak surface melt rate. We have tried to make this clear by changing this sentence to (Line 28-29)

Water flow through the supraglacial drainage system characteristically acts to reduce the diurnal amplitude and delay the timing of moulin inputs relative to the diurnal cycle of surface melt [...]

L105: Do you mean diminished diurnal amplitude for smaller catchments? Over time? Be specific.
We mean diminished diurnal amplitude compared to surface melt rate (Line 109):

For all seven catchments, moulin inputs generally track surface melt rate (Fig. 2), diminished diurnal amplitude relative to the amplitude of the surface melt rate (Fig. 3) and a phase lag of ~2 to ~8 hours (Fig. 4e, f)

L105-111: consider combining paragraphs.

We believe it is easiest for the reader to keep these paragraphs separate since each paragraph describes a separate feature of the modelled drainage system.

L108-111:
L119: change & to “and” Changed here and throughout the manuscript.

L126: Figures 2—5 Corrected.
L128: R^2 is the coefficient of determination.
The reviewer is correct, and we have clarified this throughout. See also the response to the general comments.
L173-176: runon sentence

L172-177: Hard to understand paragraph

We have reorganized this paragraph to improve its clarity (Line 190-197):

Continuous seasonal trends in the amplitude and time lag of moulin inputs, as suggested by synthetic modelling (Yang et al., 2018; Hill and Dow, 2021), are not clear except in a few atypical cases. For example, in 2015, the diurnal amplitude of inputs to moulins M1--M5 steadily decreases with a statistically significant trend ($p<0.01$) from the onset of surface melting on 13 June until 2 July. Since this period (12 June to 2 July) is characterized by relatively steady surface melt rates (~1 to ~2 cm w.e. day^{-1}), this trend may be a result of a reduction in the extent of small supraglacial channels (Fig. B2). The end of the decreasing trend coincides with a rapid increase in melt rate from ~2 to ~4 cm w.e. day^{-1}

L295 (and elsewhere): I understand the use of the normalized or relative moulin input amplitude but this line is misleading, because the actual amplitude of moulin input variability is larger for your large catchments (it is only smaller/lower when you normalize by the overall larger discharge rates)

We have acknowledged this caveat here, and explained that the absolute diurnal amplitude is larger for the large catchments. The remaining references to diurnal amplitude are explicitly referenced as relative to the average moulin input rate (Line 320).

The four large catchments with supraglacial lakes within our domain have consistently lower relative diurnal amplitude in moulin inputs (however, a larger absolute diurnal amplitude given the larger magnitude of moulin inputs) [...]

Supplement

Figure S1: Please add a legend corresponding to the colors used in the plots as to not make the reader flip back and forth between the main text and the supplement. It is also not clear what you mean by bold colors vs. light colors, do you mean the black line here? Please make this more clear in either the legend or in the figure's caption. Further, it appears the colors used in the main text are different from those in the supplement.

We apologize for the confusion about the supplement. We had submitted a supplement as a companion to a previous version of the manuscript, however we no longer have a supplement associated with the current version. All additional figures are in the Appendix, and Table S1 is available as described in the code and data availability statement.

References

Andrews, L. C., Catania, G. A., Hoffman, M. J., Gulley, J. D., Lüthi, M. P., Ryser, C., Hawley, R. L., & Neumann, T. A. (2014). Direct observations of evolving subglacial drainage beneath the Greenland Ice Sheet. Nature, 514(7520), 80–83. https://doi.org/10.1038/nature13796

Smith, L. C., Andrews, L. C., Pitcher, L. H., Overstreet, B. T., Rennermalm, Å. K., Cooper, M. G., Cooley, S. W., Ryan, J. C., Miège, C., Kershner, C., & Simpson, C. E. (2021). Supraglacial River Forcing of Subglacial Water Storage and Diurnal Ice Sheet Motion. Geophysical Research Letters, 48(7). https://doi.org/10.1029/2020gl091418

Mejia, J. Z., Gulley, J., Trunz, C., Covington, M. D., Bartholomaus, T. C., Breithaupt, C. I., Xie, S., & Dixon, T. H. (2022). Moulin density controls the timing of peak pressurization within the Greenland Ice Sheet’s subglacial drainage system. Geophysical Research Letters, 49, 1–13. https://doi.org/https://doi.org/10.1002/essoar.10511864.1

Yang, K., Sommers, A., Andrews, L. C., Smith, L. C., Lu, X., Fettweis, X., and Li, M. (2020)Intercomparison of surface meltwater routing models for the Greenland ice sheet and influence on subglacial effective pressures, The Cryosphere, 14, 3349–3365, https://doi.org/10.5194/tc-14-3349-2020.

Yang, K., & Smith, L. C. (2016). Internally drained catchments dominate supraglacial hydrology of the southwest Greenland Ice Sheet. Journal of Geophysical Research : Earth Surface, 121, 1891–1910. https://doi.org/doi:10.1002/ 2016JF003927

Reviewer 2

The authors have substantially revised and reorganized the manuscript following the review comments and I am satisfied with the new version of the manuscript. I find the new version well organized, complete and in sync with the title, abstract and introduction. The introduction and objective has been nicely rewritten. I am also pleased to see relevant additional tables and figures, and that the figures have been simplified and are now easier to read and interpret. The figure reorganization displaying the four years by data type makes sense. The model description in the appendix is a nice addition. The result and discussion section now thoroughly investigate changes in supraglacial discharge with drainage basin features for different melt years. The authors also interestingly compare their modeling results with a similar field site and provide relevant modeling recommendations.

We appreciate the reviewers comments and have responded individually below.

We would also like to highlight a change to the statistical analysis in Table 2. While investigating the first reviewer's comments to Line 126-136 and Table 2, we found a mistake in the calculation of R^2 and p values. This has been corrected, all other code has been double-checked, and Section 4.1 has been updated accordingly.

Minor comments

I have minor comments regarding the text:

Line 4: consider breaking the sentence after the parenthesis

This sentence has been changed to explicitly enumerate the points being made:

We apply the Subaerial Drainage System (SaDS) model, a physically-based surface meltwater flow model, to a ~20 x 27 km^2 catchment on the southwestern Greenland Ice Sheet for four years of melt forcing (2011, 2012, 2015, and 2016) to (1) examine the relationship between surface melt rate and the rate, diurnal amplitude, and timing of surface inputs to moulins, (2) compare SaDS to contemporary models, and (3) present a framework for selecting appropriate supraglacial drainage models for different modelling objectives.

L68-70: I was just wondering if you investigated how different smoothing of the ArcticDEM led to different routing and discharge results.

This is a good question. The 1.44 km moving average filter is the weakest smoothing for which we have achieved suitable numerical convergence. We agree that it is possible that modelled discharge is sensitive to the DEM smoothing we have applied. However, we believe the topography has not been overly smoothed, as evidenced by the persistence of supraglacial lakes and topographically controlled drainage pathways. Sub-grid scale roughness is likely to be an important control on moulin discharge, although this must be captured by the hydraulic conductivity rather than the surface topography. We have explained the reason for the DEM smoothing following line 70:

We first smooth the ArcticDEM with a moving average filter with an edge length of 1.44 km, and then average the pixels that lie within each triangular element to define the centroid elevation. This smoothing is required to achieve numerical convergence within the SaDS model. It is possible that moulin inputs would change with higher resolution surface elevation data. However, it does not appear that the topography has not been overly smoothed here, as evidenced by the persistence of supraglacial lakes and topographically controlled drainage pathways.

L74: replace contraction “don’t” by “do not”. Same for L82. Done.

L103: is it a season average or a daily average that is used to calculate the “relative diurnal amplitude” ?

We use the seasonal average moulin input. This sentence has been clarified as follows (bolded text added; Line 105):

[...] the relative diurnal amplitude of inputs to moulins (measured as the peak-to-peak range in moulin inputs normalized by the **melt season-averaged** moulin input rate) (Fig. 3) [...]

L212: replace “it’s” with “it is”. Corrected.