Authors' Response to Reviews of

Multi-scale variations of hydro-mechanical conditions at the base of the surge-type glacier Kongsvegen, Svalbard

Coline Bouchayer et al.
The Cryosphere,

AR: Author's Response

Dear Editor and Reviewer 2,

We thank you for the thorough reviews that helped improving the manuscript. We have revised the manuscript to account for the comments of the reviewers and summarize the main revisions as follows:

- We have revised the reference list such that the statements are well supported by appropriate references.
- We have revised the discussion around the till rheology avoiding the outdated hypothesis of a viscous till rheology. We now explore the processes that can explain our results assuming the Coulomb-plastic rheology of the till.
- We have added detail to support our use of the framework to derive R and S. We refer to a sensitivity study by Gimbert that suggests that this framework is largely insensitive to the assumptions about nature of the bed substrate, the degree of fullness of the channels, the geometry of the conduits and the number of channels.
- We now better justify our usage of modelled surface runoff as a proxy for the subglacial discharge variations.
- We have modified the title, abstract and introduction to reduce the impression that the presented manuscript focuses on surge processes.

Further revisions include special attention to referencing, added detail and precision in our descriptions of data processing and interpretation of till rheology. To reflect these revisions, we have also rewritten the abstract. We have documented our revisions in an attached document that highlights all changes where additions are marked in blue and removals are crossed out in red. Below, we provide a detailed response to all comments, where our detailed Author Responses are labeled AR and shown in blue font.

Best regards,

Coline Bouchayer, on behalf of the authors.

1. Reviewer #2

1.1. General comments

The manuscript by Bouchayer et al. describes a series of novel subglacial and glacier surface observations collected on Kongsvegen Glacier during 2021 and 2022. These observations, including subglacial pressure, ploughing force, seismic power, ice surface velocity, and surface meltwater runoff are used to characterize the behavior of the subglacial system during two contrasting melt years. To do this, the authors utilize seismic processing methods to derive subglacial channel characteristics and classify the relationship between meltwater runoff and variables descriptive of the subglacial environment over seasonal, event, and daily timescales. Overall, they conclude that during the low melt year (2021), the subglacial system was able to readily adapt to changes in runoff availability, while during the high melt year (2022), runoff variability frequently overwhelmed the efficient subglacial system resulting in ice velocity acceleration events. However, the relationships examined on shorter timescales and those related to till behavior suggest a complicated and time evolving subglacial environment.

The observations presented within Bouchayer et al. are novel and the derived relationships between a range of variables on multiple timescales are thought provoking. In addition, the quantitative approach to characterizing the relationship between different variables is commendable. The manuscript is generally well written and conveys the complexity associated with multiple observations and over multiple time and space scales. However, I do wonder if the ambiguity in the results is more related to how the methods were applied than in the observations themselves. As such, I have some concerns about how certain methods were applied and some suggestions regarding analysis and interpretation of the datasets. Below are general comments pertaining to analysis and interpretation, comments about the manuscript structure, and line and figure comments.

AR: We thank the reviewer for the overall positive evaluation of our work. Below, we respond to the comments point by point.

1.2. General comments on analysis and interpretation

- Much of what is being described (as illustrated in Figure 10), is reminiscent of the 'preferential drainage axes' of Haut Glacier d'Arolla (e.g., Sharp et al. 1993; Mair et al. 2001; Mair et al., 2003), and it would be highly relevant to include a discussion of PDAs when considering how the Kongsvegen surface velocities, relate to subglacial pressure, seismic power, and till behavior.

AR: We now use the term 'preferential drainage axis' when referring to the previously called 'active part of the subglacial drainage system'. We have now added some discussion about the potential location of the PDAs on Kongsvegen glacier, based on the study by Scholzen et al. (2021) and the preprint by Pramanik et al. (2020) where these authors estimate the position of the PDAs. These estimates have motivated the selection of the drill site prior to the field campaign. We have also added the following sentence in the method section: 'The borehole location has been chosen based on the work of Scholzen et al. (2021); Pramanik et al. (2020) who suggest the existence of a preferential drainage pathway in close proximity to this site.'

- Overall, there is very little discussion of surface ice velocities. Ultimately understanding the subglacial system is necessary to inform our understanding of glacier motion, surges, seasonal, etc. More discussion of the link between subglacial conditions and surface velocities is warranted, including plots examining the relationship F, Q, and p vs us. An exploration of F, us, and uplift really is warrented.

AR: To enhance the visualization of the complex interplay between the involved quantities, we have now superimposed time series of velocity and runoff on the plots showing the evolution of phase relationships in Figs 5 and 7. However, we do not discuss uplift due to a lack of vertical velocity data. We agree that it would be interesting, but at the current stage we cannot include this without exceeding the scope of the paper. Furthermore, since the GNSS stations are not co-located with the borehole, we think that such discussion would be afflicted with considerable uncertainties.

- The seismic processing methods require a number of assumptions that may or may not be met by the data. Indeed, the authors state as much on line 472. Kongsvegen is not hard-bedded and the assumptions that the number of channels is constant and that these channels are full are not necessarily met suggest that the calculation of the hydraulic gradient and radius are not robust. This issue in addition to the one below suggests that the authors should consider a simpler way to consider seismic power and its relation to both the surface forcing and ice motion.

AR: This comment is largely identical to one of the remarks by Reviewer 1 and we reiterate our response here: The channel fullness does not substantially affect our results as described in the supplementary information of Gimbert et al. (2016) who conclude that "$P_w \propto Q^{5/4}$ is thus a good approximation of seismic power changes with discharge for channels evolving at constant pressure gradient, regardless of conduit shape and degree of fullness. We note, however, that uncertainties on conduit shape and fullness preclude us from confidently interpreting seismic power changes smaller than $10 log_{10}(1.25) = 1$ dB". The shallow slope of Kongsvegen, the long distance of our instrument site from the terminus (>12 km) and the ice thickness (>350 m) promote large creep rate thus fast closure of the channels Nye (1976) as well as comparatively little enlargement due to melt because of the shallow hydraulic gradient Röthlisberger (1972). As a results, channels might tend to close fast enough to prevent significant open-channel condition. This argument is further supported by observations of consistently high borehole water pressures, with a minimum value still representing $\sim$70% of the ice overburden pressure. We have added the sentence: "Here, we neglect changes in conduit shape, fullness and number as they have limited impact on the derivation of R and S (Gimbert et al., 2016; Nanni et al., 2020)."

- The authors are correct in using 'Runoff' for modeled glacier surface runoff, but the abbreviation Q is somewhat misleading as Q is typically shorthand for Discharge – which is the volume of water that passes through a cross section per unit time. However, my main concern on this point is that the modeled surface runoff is used to calculate the hydraulic radius and hydraulic gradient within inferred subglacial channels. Gimbert et al. (2016) and Nanni et al. (2020) calculate the discharge of the subglacial channels using the Manning Strickler equation. To argue that surface runoff = subglacial channel discharge, a number of assumptions are made, including that all surface runoff flows within subglacial channels (or at least flows turbulently), for the entire time period, within a region that can be monitored by the seismic station. It is quite possible that this assumed equivalency between surface runoff and subglacial channel discharge can at least partly explain the ambiguity of the results, and the authors should carefully consider whether the calculations of R and S are robust enough to use in the analysis.

AR: This comment is largely identical to one of the remarks by Reviewer 1 and we reiterate our response here: We agree that our simulated surface runoff does not represent subglacial discharge through a given cross-section. The model accounts for a time delay within the porous snow and firn but does not account for delays due to vertical transfer from the surface to the base of the glacier nor for downstream routing of water from its origin. In addition, the number of flowpaths and their size are unknown. However, the modelled runoff has been evaluated against proglacial discharge observations and although the observations in Svalbard are sparse, daily values of modelled runoff show good agreement with measurements. This agreement suggests that this delay is negligible for time scales exceeding one day and relatively short horizontal distances (see also in details, Schmidt et al., 2023, , Sect. 4.2.1.). Our analysis considers the **relative** variation rather than the absolute values of Q. Therefore, even if the local discharge in a given flowpath may numerically differ from the simulated runoff, our findings remain robust. To render this aspect more precisely, we have added the following in our manuscript: "Using simulated surface runoff to represent local discharge through a given cross-section implicitly assumes transfer of water between the surface and the base within short time, which is supported by in-situ observations from other Svalbard glaciers similar to Kongsvegen (Benn et al., 2009; Gulley, 2009; Bælum and Benn, 2011; Irvine-Fynn et al., 2011) and the good agreement between daily values of simulated runoff and measured proglacial discharge at the catchment scale (Schmidt et al., 2023). Therefore, we consider relative variations in surface runoff to represent those of subglacial discharge, even though large uncertainties on the magnitude of the subglacial discharge remain. "

- I am somewhat surprised that the borehole is in an active part of the drainage system during relatively low melt year, but in an inactive part of the drainage system during the high melt year – would typically be reversed and patterns of channelization tend to be consistent, though more or less extensive, from year to year. Could this be due to the initial hot water drilling? Changes in till characteristics? It would be nice to see the explanation. It is hard to see where the borehole would be located, both theoretically and in Figure 10, based on the analysis.

AR: We thank the reviewer for this interesting comment. Although the hot-water drilling might have disturbed the subglacial environment (excavation of fines, volume of water pushed to the bed), the volume of water injected through hydraulic connection of the borehole to the bed is limited (≈ 0.5 m^3 for the observed 30 m drop of water column at connection). In addition, the borehole was drilled beginning of May 2021, and in the absence of surface melting before late June 2021, it seems unlikely that a potential initial connection could be maintained. Geometrically controlled patterns of channelization on a hard bed may be persistent, but a soft sediment bed provides less geometrical controls on the spatial patterns and year-to-year variability of channel location within a few meters seems plausible. Furthermore, over the course of the observation period, the borehole location has moved down-glacier at a rate of > 30 m yr^{-1}, hence it seems plausible that the location of our instruments has moved relative to the channel. To address this comment, we have added the following paragraph to the text: "Depending on the hydraulic connection of the borehole and the ice-till coupling, p may be representative for about 1 m^2 in case of hydraulic isolation, or for a several orders of magnitude larger area in case of a direct connection to a preferential drainage axis (Murray and Clarke, 1995; Mair et al., 2001, 2003). In addition, the hot-water drilling operation might have disturbed the subglacial environment (excavation of fines, volume of water pushed to the bed), influencing the water pressure observation. However, the volume of water injected through hydraulic connection of the borehole to the bed is limited ($\approx$ 0.5 m^3 for the observed 30 m drop of water column at connection) and the borehole has been drilled beginning of May 2021. In the absence of surface melting before late June 2021, it seems unlikely that a potential initial connection could be maintained. Geometrically controlled patterns of channelization on a hard bed may be persistent, but a soft sediment bed provides less geometrical controls on the spatial patterns and year-to-year variability of channel location within a few meters seems plausible. In addition, several studies report that water pressure records displayed regime changes and suggest that these may reflect reorganization of the drainage system (Gordon et al., 1998; Kavanaugh and Clarke, 2000; Schuler et al., 2002; Andrews et al., 2014; Rada and Schoof, 2018)."

- The combined use of reanalysis and forecast data give me pause. While both use a similar model configuration and give regionally similar results, there are differences in the forcings and configurations that could impact temperature (including SW and LW radiation) and precipitation and there are documented local differences between the two systems. Kongsvegen has a weather station; could confirmation of similarity or differences be determined? Alternatively, because CARRA ends in 2021, could the AROME-Arctic analysis (not forecasts, I believe the analysis is the MET Nordic Analysis) be used for both years. Whatever tack is taken, more clarity on any differences between the met forcings used for CryoGrid between each year need to be included.

AR: These points have been discussed in the paper published by Schmidt et al. (2023), where the potential impact of using the CARRA reanalysis and AROME-ARTIC forecasts as model forcing have been assessed and found to be small. Schmidt et al. (2023) estimate that the area-averaged CMB in the AROME-ARCTIC-forced simulations differed by up to 0.1 m w.e. yr^{-1} from the CARRA-forced simulations. The average difference in glacier runoff between the AROME-ARCTIC- and CARRA-forced simulations is 0.03 m w.e. yr^{-1}, equivalent to only about 2% of the total runoff. To address this comment, we have added the sentence: "Differences in model results due to different forcing datasets are small in our study area (i.e., < 2% of the total runoff, Schmidt et al., 2023)."

1.3. General comments on the manuscript structure

- The title, abstract and introduction emphasize that Kongsvegen is a surge type glacier, but there is no discussion of how the observations inform our understanding of surge mechanics. Either the thrust of the Discussion needs to change, or the introductory materials should be adjusted.

AR: We have now changed the title to "Multi-scale variations of subglacial hydro-mechanical conditions at Kongsvegen glacier, Svalbard', we have removed the focus on surging also from the introduction as well as from the abstract of the paper.

- I empathize with the authors' desire to be succinct, but using many abbreviations and numbers to identify different classes makes reading the discussion challenging and requires multiple references to previous figures and text. Could more descriptive terms for the different classes be used?

AR: We have followed the suggestions and have changed the Class number as follow throughout the text and in the figures: Class I: Preceding class; Class II: Lagging class; Class III: In-phase class; Class IV: anti-phase class.

- I'd like to see the number and breadth of references expanded. There are multiple areas where there are no pertinent references.

AR: This comment is largely identical to one of the remarks by Reviewer 1 and we reiterate our response here: We have reworked our reference list to ensure that each statement is referenced to its origins, sometimes we also added newer references for additional support. We have removed the references where suggested by the reviewer and added 'e.g.' where the list was not exhaustive.

1.4. Line comments

1. **L10**: I would add here that this information is used to derive hydraulic gradient, and subglacial channel hydraulic radius.

AR: We have considerably revised the abstract of the paper and so this comment is not relevant any longer.

2. **L13**: Consider being specific here: water pressure and force and measured, hydraulic gradient and hydraulic radius are inferred/calculated from previously determined relationships.

AR: We have modified the abstract accordingly: 'To characterize the variations in the subglacial conditions caused by changes in surface runoff, we investigate the variations of the following hydro-mechanical properties: measured water pressure, measured sediment ploughing forces and derived hydraulic gradient and radius, over seasonal, multi-day and diurnal time-scales."

3. **L14**: modeled **surface runoff**. The ambiguity seems to be mostly between the inferred variables vs the measured variables (e.g. direct vs seismically inferred). It might be worth being more specific here.

AR: We refer the reviewer to the answer of the previous comment.

4. **L35**: Add an 'e.g.' to this citation. There are many papers suggesting this.

AR: We have followed this suggestion.

5. **L38**: Add an 'e.g.' to this citation.

AR: We have followed this suggestion.

6. **39-48**: Surges are an interesting transient event that can be used to understand basal conditions, but they are not discussed within the content of the observations. Consider revising this paragraph to be more general.

AR: In response to the criticism of both reviewers, we have removed this paragraph from the introduction and adjusted the title to avoid a potentially misleading focus on surges.

7. **L60**: It's worth mentioning isolated cavities since these are invoked later in the manuscript (e.g., Iken et al., 1983).

AR: We have followed the suggestion and added this information in the text as follow: "These include water sheets (**??**Creyts and Schoof, 2009), cavities in the lee of bedrock obstacles (Lliboutry, 1968; Iken, 1981), linked cavities (Kamb, 1987; **?**) and channels incised into the ice or subglacial substrate (Röthlisberger, 1972; Nye, 1976; Hooke et al., 1990; Walder and Fowler, 1994). "

8. **L84**: 'Truffer'.

AR: We have corrected the reference.

9. **L92**: New paragraph.

AR: We have followed this advice.

10. **L145**: Is the geophone installed in the borehole? It's unclear. It might be worth including a subheading 'near surface instrumentation.'

AR: We have added a new sub-section under Method called 'Near-surface instrumentation'. We have as well clarified that the surface geophone is not located in the borehole but in its close vicinity: 'At $\sim$ 100m from the borehole, a three-component geophone (DiGOS, 4.5 Hz) was installed $\sim$1.5 m into the ice to ensure good coupling and prevent melt-out during summer.'

11. **L154-160**: The position information at least need general uncertainties.

AR: We have calculated more precisely the locations of the GNSS stations compared to the borehole location and added uncertainty estimates: "The stations are located at distances of 740 $\pm$ 10 m (KNG6, 78.78067^{o}N, 13.15153^{o}E) and 3100 $\pm$ 10 m (KNG7, 78.76770^{o}N, 13.23962^{o}E) upstream of the drill site."

12. **L162**: Westermann et al. (2023)?

AR: We have updated the reference.

13. **L165**: See my general note. Also, is the forecast the ensemble mean or the single unperturbed member?

AR: Please, see our answer to the general comment. Also, AROME-ARCTIC is a single member forecast model. For each day of the year, the forecast from 18:00 on the previous day is downloaded - the first 6 hours are considered a spin-up and not used, and then the midnight-midnight values (at a 3 hour timestep) are saved for each day. If this forecast is not available, we use the nearest past timestep. Schmidt et al. (2023) found that the effect of the forecast timestep used was small over timescales of months/years, but on certain individual days there was a large difference between the timesteps. We refer the reviewer to the article of Schmidt et al. (2023), Section 6.2.2. for further details. To address this comment, we have modified the text as follow: "The model is forced by 3-hourly fields of near surface conditions from the Copernicus Arctic Regional Reanalysis (CARRA, Schyberg et al., 2020; Yang et al., 2021) for 2021 and single-member forecasts by AROME-Arctic (Müller et al., 2017) for 2022."

14. **L170**: This is a big assumption. Is there any justification for this that could be included?

AR: This comment is largely identical to one of the remarks by Reviewer 1 and we reiterate our response here: We agree that our simulated surface runoff does not represent subglacial discharge through a given cross-section. The model accounts for a time delay within the porous snow and firn but does not account for delays due to vertical transfer from the surface to the base of the glacier nor for downstream routing of water from its origin. In addition, the number of flowpaths and their size are unknown. However, the modelled runoff has been evaluated against proglacial discharge observations and although the observations in Svalbard are sparse, daily values of modelled runoff show good agreement with measurements. This agreement suggests that this delay is negligible for time scales exceeding one day and relatively short horizontal distances (see also in details, Schmidt et al., 2023, , Sect. 4.2.1.). Our analysis considers the **relative** variation rather than the absolute values of Q. Therefore, even if the local discharge in a given flowpath may numerically differ from the simulated runoff, our findings remain robust. To render this aspect more precisely, we have added the following in our manuscript: "Using simulated surface runoff to represent local discharge through a given cross-section implicitly assumes transfer of water between the surface and the base within short time, which is supported by in-situ observations from other Svalbard glaciers similar to Kongsvegen (Benn et al., 2009; Gulley, 2009; Bælum and Benn, 2011; Irvine-Fynn et al., 2011) and the good agreement between daily values of simulated runoff and measured proglacial discharge at the catchment scale (Schmidt et al., 2023). Therefore, we consider relative variations in surface runoff to represent those of subglacial discharge, even though large uncertainties on the magnitude of the subglacial discharge remain. "

15. **L197**: In theory, I don't think there is a problem assuming a constant number of channels for short periods of time, but one of the final conclusions is that the borehole is connected to the efficient system

in 2021 and in an isolated(ish) region in 2022, suggesting a different number of channels.

AR: We refer the reviewer to the detailed answer above, when responding to the general comment concerning this point.

16. **L244**: velocity should have the abbreviation u_s.

AR: We have followed this advice.

17. **L254**: Only total precipitation is included on Figure 4. It would be useful to have both rain and snow fall.

AR: We have now made the changes and the figure shows both rain- and snowfall instead of precipitation.

18. **L257**: Sometimes the second number in the figure references is circled and sometimes it isn't.

AR: We could not find where the number should have been circled and that it was not. Maybe the confusion comes from the fact that in Figure 4, 6, 8, the circled number corresponds to specific period (rainfall, warm period) but these periods differ from the un-circled number in the Figure 5, which corresponds to changes in the observed dynamic. These numbers (circled and uncircled) therefore do not correspond to the same time period. We have clarified this in the caption of Figure 5: "The numbers do not correspond to the same periods between each panels and are unrelated to the periods identified by the circled numbers in Figure 3".

19. **L293**: If dates are referenced here, they should be clearly identifiable in Figure 5.

AR: We have now changed the phrasing as these dates are approximate and we rather refer to the first half of the melt season. To new sentence now is: "The linear relationship between F and p during the first half of the melt season indicates that the two subglacial variables are anti-correlated..."

20. **L304**: Figure 5i doesn't seem to indicate a linear relationship… perhaps this is because the axis ranges are vastly different.

AR: Our remark concerns the general co-evolution of p and Q displaying increase of p with increasing Q and vice versa, although there is some hysteresis. Therefore it is not appropriate to describe this as a 'linear' relation, instead we have changed the wording to '...p and Q are positively related...'

21. **L306**: The figure seems misplaced.

AR: We do not understand this comment. The Figure 6 comes after the sub-section "Analysis at the multi-day time scale", in which it is referenced.

22. **L344**: What is the overburden pressure at the borehole location? The lack of diurnal variability in p and F and us, suggests that any subglacial channels are not completely water filled except during melt/rain events. This has a number of implications for the analysis.

AR: The overburden pressure at the borehole location is ∼3.2 MPa, corresponding to an ice thickness of ∼ 350 m. The velocity is derived daily, and does not resolve sub-diurnal variations. We do not observe pronounced diurnal variations in p and F except for a few, short episodes (Appendix F, F1). To clarify the original sentence, it has been reworded: "Except for during short episodes, p and F do not display pronounced diurnal variations (Appendix F, Fig F1)". The other reviewer commented on the potential impacts of open-channel flow and we reiterate our response here: Given the shallow slope of Kongsvegen, the long distance of our instrument site from the terminus (>12 km) and the considerable ice thickness (>350 m), non-pressurized conditions are highly unlikely, since an open channel would quickly close and a shallow hydraulic gradient would be insufficient to cause melt enlargement overcoming the closure. This argument is further supported by consistently high observed borehole water pressures, with a minimum value still representing ∼70% of the ice overburden pressure.

23. **L365**: A constant R would be expected, if the channel is water filled. The lack of diurnal variations suggests that this might not be true. In a partially filled channel, R would increase with increasing S.

AR: We refer to the detailed answer provided to the general comment concerning this point and the modified text.

24. **L363-381**: Some references would be beneficial.

AR: We have added the following references for the paragraph Röthlisberger (1972); Schoof (2010); Werder et al. (2013).

25. **L429**: p didn't exhibit diurnal variations, so this statement seems a bit misleading.

AR: As answered in a previous comment, p exhibits episodically some diurnal variations. We have changed the misleading sentence to: "Except for during short episodes, p and F do not display pronounced diurnal variations (Appendix F, Fig F1)".

26. **L430**: Could this rapid adjustment of R be the result of subglacial channels that are not filled?

AR: We refer to the detailed answer provided to the general comment concerning this point.

27. **L460**. There seem to be more diurnal variations in p during 2022 than in 2021, indeed it looks like at least 60% of the days have enough variability to assign a class.

AR: We did assign classes for p-Q relationship where the diurnal filter did not fail. Therefore, we have more classified events in 2022 than in 2021 (see Fig 7 in the revised manuscript). However, we meant in this statement that this is hard to depict a consistent response of p to changes in Q as the classes are all mixed. We have clarified the point as followed: "The same analysis on the diurnal scale reveals that there is more diurnal variations in p during the melt season 2022, compared to 2021 (more events are classified). However, the diurnal analysis renders a blurry picture since all classes occur and no clear pattern can be depicted."

28. **L472**. See general note.

AR: We thank the reviewer for pointing this out. We have removed this paragraph since, as explained in a detailed answer to the general comment, the theoretical scaling by Gimbert et al. (2016) and the derivation of R and S have been developed for both hard- and soft-bed glaciers. See also the detailed answer provided to the general comment concerning this point.

29. **L497**: See the literature on preferential drainage axis. This is what is being described in Figure 10.

AR: We have re-phrased the sentence and we include references related to PDA/transfer of mechanical support as follow: "Depending on the hydraulic connection of the borehole and the ice-till coupling, p may be representative for about 1 m^2 in case of hydraulic isolation, or for a several orders of magnitude larger area in case of a direct connection to a preferential drainage axis (**?**Mair et al., 2001, 2003)" and "Sufficiently high water pressure in connected bed areas can cause the expansion of the connected subglacial drainage system (Murray and Clarke, 1995). [...] Conversely, when the connected areas of the bed operate at low water pressure, areas of the bed adjacent to preferential drainage axes are hydraulically isolated, resulting in areas of the glacier bed switching back and forth between connected and isolated."

30. **L555**: One thing to consider is how the behavior illustrated in Figure 10 transfers mechanical support of the overlying ice and how that might impact till behavior or measured force on the ploughmeter.

AR: We agree with the reviewer that other mechanisms can take place and influence the reading of F and p. Positive correlation between F and p has been previously observed (Murray and Porter, 2001; Rousselot and Fischer, 2007; Thomason and Iverson, 2008) but no straight forward answers have been yet provided. To address this comment, we have added in the discussion: "However, a positive relationship between p and F as pictured in Figures 8b and d does not agree with Coulomb-plastic rheology. The illustrated episodes apparently do not coincide with periods of high surface velocity. Similar $p - F$ correlations have been observed previously (e.g., Murray and Porter, 2001; Rousselot and Fischer, 2007; Thomason and Iverson, 2008) but not extensively discussed. A range of mechanisms have been proposed to explain such behavior, such as the sediments loaded towards their yield point (e.g., Murray and Porter, 2001), the state of the mechanical coupling between the ice and the till and its influence on pore-pressure variations (Iverson et al., 1995; Fischer and Clarke, 1997; Boulton et al., 2001; Mair et al., 2003; Iverson, 2010), the varying mobilisation of the till at depth (Iverson et al., 1998; Tulaczyk, 1999; Tulaczyk et al., 2001; Truffer et al., 2000; Truffer, 2004, e.g.,). However, a direct explanation on how these mechanisms would explain the correlation between F and p is not straightforward." In addition, we propose that the observed behavior may be partly caused by changes in the instrument-till coupling, for instance by changes in the attitude (tilt or vertical motion) of the ploughmeter relative to the till, but these effects cannot be disentangled from till behavior without further accompanying measurements. Such measurements will be subject for future designs of ploughmeter deployments. We have added this information in the text as follows: "We further point out that the attitude of the ploughmeter relative to the till may have changed, for instance through changes in tilt or vertical position, but these effects cannot be disentangled from till behavior without further accompanying measurements. Such measurements will be subject for future ploughmeter deployments."

1.5. Figures

1. **Figure 1**

   (a) Data source for panel b?

   AR: We have added that these data come from a personal communication from Jack Kohler, a co-author of the paper.

2. **Figure 2**:

   (a) could easily be in an appendix.

   AR: We have followed the suggestion.

3. **Figure 3**:

   (a) The class colors here and in the other figures are hard to distinguish, could the be more distinct?

   AR: We have changed the color scale and hopefully now the figures are more readable.

   (b) It would be useful to have the same color scale as in Figure 5, etc.

   AR: We have updated the color scale in the other figures accordingly.

4. **Figure 4**:

   (a) Rainfall is discussed multiple times in the text, so rainfall and snowfall should be parsed in panel a.

   AR: We have now added rainfall and snowfall in the panel a.

   (b) The winter period isn't analyzed, could it be cut out (and possibly included in the Appendices) to make the summer seasons bigger?

   AR: We have followed the suggestion.

(c) I don't see any blue or grey shaded areas on my printed version.

AR: We have made the requested modification by displaying only the melt seasons in two separate panels, as suggested in the previous comment. This change has resolved the issue of problematic color shading..

(d) Are there diurnal variations in ice velocity?

AR: The velocity is derived per day so we cannot answer this point.

5. **Figure 5**:

   (a) It would be useful to include how to read figure 5 a-c, f-h in the caption including how the curves relate to the bounds to determine behavior.

   AR: We have added a description on how to read the figure as follow: 'For the panels a to c and f to h, we interpret our observations as aligning with one of the scenarios detailed in Gimbert et al. (2016); Nanni et al. (2020), where the slope of the hysteresis curve is parallel to the theoretical scaling.'

   (b) Scale the color bars to be the same number of days such that it's clear that the 2022 data doesn't go to the end of the melt season and they should be the same across Figures (right now Figure 7 has a different color scale for 2022).

   AR: We have modified the figure so that the color scale is similar for the melt season 2021 and 2022.

   (c) The vastly different ranges on the x and y axes make it difficult to interpret behavior (see line comment 304). These should be standardized as much as possible, in ways that highlight the main points of the analysis.

   AR: We have changed the figure such as the x and y axis are comparable between the melt season 2021 and 2022 and we have normalised the data from the borehole for the same purpose.

6. **Figure 6**:

   (a) Could the windows be plotted on subpanels b and d?

   AR: We have followed the suggestions.

7. **Figure 7**:

   (a) See notes about color scale and axes for Figure 5.

   AR: We have followed the suggestion.

8. **Figure 9**:

   (a) How are the melt seasons combined in panel a?

   AR: We have normalised the variables and then plotted them in the same panel with the color bar indicative of time. We have clarified the caption.

9. **Figure 10**.

   (a) Where would the borehole sit in the subglacial plan view maps?

   AR: We have indicated the borehole location in the figure. Additionally, we have modified the figure (mechanical properties) to better reflect our discussion that has been considerably revised.

References

Andrews, L. C., Catania, G. A., Hoffman, M. J., Gulley, J. D., Lüthi, M. P., Ryser, C., Hawley, R. L., and Neumann, T. A.: Direct observations of evolving subglacial drainage beneath the Greenland Ice Sheet, Nature, 514, 80–83, 2014.

Bælum, K. and Benn, D.: Thermal structure and drainage system of a small valley glacier (Tellbreen, Svalbard), investigated by ground penetrating radar, The Cryosphere, 5, 139–149, 2011.

Benn, D., Gulley, J., Luckman, A., Adamek, A., and Glowacki, P. S.: Englacial drainage systems formed by hydrologically driven crevasse propagation, Journal of Glaciology, 55, 513–523, 2009.

Boulton, G., Dobbie, K., and Zatsepin, S.: Sediment deformation beneath glaciers and its coupling to the subglacial hydraulic system, Quaternary International, 86, 3–28, 2001.

Creyts, T. T. and Schoof, C. G.: Drainage through subglacial water sheets, Journal of Geophysical Research: Earth Surface, 114, 2009.

Fischer, U. H. and Clarke, G. K.: Stick–slip sliding behaviour at the base of a glacier, Annals of Glaciology, 24, 390–396, 1997.

Gimbert, F., Tsai, V. C., Amundson, J. M., Bartholomaus, T. C., and Walter, J. I.: Subseasonal changes observed in subglacial channel pressure, size, and sediment transport, Geophysical Research Letters, 43, 3786–3794, 2016.

Gordon, S., Sharp, M., Hubbard, B., Smart, C., Ketterling, B., and Willis, I.: Seasonal reorganization of subglacial drainage inferred from measurements in boreholes, Hydrological Processes, 12, 105–133, 1998.

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

Hooke, R. L., Laumann, T., and Kohler, J.: Subglacial water pressures and the shape of subglacial conduits, Journal of Glaciology, 36, 67–71, 1990.

Iken, A.: The effect of the subglacial water pressure on the sliding velocity of a glacier in an idealized numerical model, Journal of Glaciology, 27, 407–421, 1981.

Irvine-Fynn, T. D., Hodson, A. J., Moorman, B. J., Vatne, G., and Hubbard, A. L.: Polythermal glacier hydrology: A review, Reviews of Geophysics, 49, 2011.

Iverson, N. R.: Shear resistance and continuity of subglacial till: hydrology rules, Journal of Glaciology, 56, 1104–1114, 2010.

Iverson, N. R., Hanson, B., Hooke, R. L., and Jansson, P.: Flow mechanism of glaciers on soft beds, Science, 267, 80–81, 1995.

Iverson, N. R., Hooyer, T. S., and Baker, R. W.: Ring-shear studies of till deformation: Coulomb-plastic behavior and distributed strain in glacier beds, Journal of Glaciology, 44, 634–642, 1998.

Kamb, B.: Glacier surge mechanism based on linked cavity configuration of the basal water conduit system, Journal of Geophysical Research: Solid Earth, 92, 9083–9100, 1987.

Kavanaugh, J. L. and Clarke, G. K.: Evidence for extreme pressure pulses in the subglacial water system, Journal of Glaciology, 46, 206–212, 2000.

Lliboutry, L.: General theory of subglacial cavitation and sliding of temperate glaciers, Journal of Glaciology, 7, 21–58, 1968.

Mair, D., Nienow, P., Willis, I., and Sharp, M.: Spatial patterns of glacier motion during a high-velocity event: Haut Glacier d'Arolla, Switzerland, Journal of Glaciology, 47, 9–20, 2001.

Mair, D., Willis, I., Fischer, U. H., Hubbard, B., Nienow, P., and Hubbard, A.: Hydrological controls on patterns of surface, internal and basal motion during three “spring events”: Haut Glacier d’Arolla, Switzerland, Journal of Glaciology, 49, 555–567, 2003.

Müller, M., Homleid, M., Ivarsson, K. I., Køltzow, M. A., Lindskog, M., Midtbø, K. H., Andrae, U., Aspelien, T., Berggren, L., Bjørge, D., Dahlgren, P., Kristiansen, J., Randriamampianina, R., Ridal, M., and Vignes, O.: AROME-MetCoOp: A nordic convective-scale operational weather prediction model, Weather and Forecasting, 32, 609–627, , 2017.

Murray, T. and Clarke, G. K.: Black-box modeling of the subglacial water system, Journal of Geophysical Research: Solid Earth, 100, 10 231–10 245, 1995.

Murray, T. and Porter, P. R.: Basal conditions beneath a soft-bedded polythermal surge-type glacier: Bakaninbreen, Svalbard, Quaternary International, 86, 103–116, 2001.

Nanni, U., Gimbert, F., Vincent, C., Gräff, D., Walter, F., Piard, L., and Moreau, L.: Quantification of seasonal and diurnal dynamics of subglacial channels using seismic observations on an Alpine glacier, The Cryosphere, 14, 1475–1496, 2020.

Nye, J. F.: Water flow in glaciers: jökulhlaups, tunnels and veins, Journal of Glaciology, 17, 181–207, 1976.

Pramanik, A., Kohler, J., Lindbäck, K., How, P., Van Pelt, W., Liston, G., and Schuler, T. V.: Hydrology and runoff routing of glacierized drainage basins in the Kongsfjord area, northwest Svalbard, The Cryosphere Discussions, pp. 1–33, 2020.

Rada, C. and Schoof, C.: Channelized, distributed, and disconnected: subglacial drainage under a valley glacier in the Yukon, The Cryosphere, 12, 2609–2636, 2018.

Röthlisberger, H.: Water pressure in intra-and subglacial channels, Journal of Glaciology, 11, 177–203, 1972.

Rousselot, M. and Fischer, U. H.: A laboratory study of ploughing, Journal of Glaciology, 53, 225–231, 2007.

Schmidt, L. S., Schuler, T. V., Thomas, E. E., and Westermann, S.: Meltwater runoff and glacier mass balance in the high Arctic: 1991–2022 simulations for Svalbard, The Cryosphere, 17, 2941–2963, 2023.

Scholzen, C., Schuler, T. V., and Gilbert, A.: Sensitivity of subglacial drainage to water supply distribution at the Kongsfjord basin, Svalbard, The Cryosphere, 15, 2719–2738, 2021.

Schoof, C.: Ice-sheet acceleration driven by melt supply variability, Nature, 468, 803–806, 2010.

Schuler, T., Fischer, U. H., Sterr, R., Hock, R., and Gudmundsson, G. H.: Comparison of Modeled Water Input and Measured Discharge Prior to a Release Event: Unteraargletscher, Bernese Alps, Switzerland: Selected paper from EGS General Assembly, Nice, April-2000 (Symposium OA36), Hydrology Research, 33, 27–46, 2002.

Schyberg, H., Yang, X., Køltzow, M., Amstrup, B., Bakketun, å., Bazile, E., Bojarova, J., Box, J., Dahlgren, P., Hagelin, S., Homleid, M., Horányi, A., Høyer, J., Johansson, å., Killie, M., Körnich, H., Le Moigne, P., Lindskog, M., Manninen, T., Nielsen Englyst, P., and Wang, Z.: Arctic regional reanalysis on single levels from 1991 to present, Copernicus Climate Change Service (C3S) Climate Data Store (CDS), , 2020.

Thomason, J. F. and Iverson, N. R.: A laboratory study of particle ploughing and pore-pressure feedback: a velocity-weakening mechanism for soft glacier beds, Journal of Glaciology, 54, 169–181, 2008.

Truffer, M.: The basal speed of valley glaciers: an inverse approach, Journal of Glaciology, 50, 236–242, 2004.

Truffer, M., Harrison, W. D., and Echelmeyer, K. A.: Glacier motion dominated by processes deep in underlying till, Journal of Glaciology, 46, 213–221, 2000.

Tulaczyk, S.: Ice sliding over weak, fine-grained tills: dependence of ice-till interactions on till granulometry, SPECIAL PAPERS-GEOLOGICAL SOCIETY OF AMERICA, pp. 159–178, 1999.

Tulaczyk, S. M., Scherer, R. P., and Clark, C. D.: A ploughing model for the origin of weak tills beneath ice streams: a qualitative treatment, Quaternary International, 86, 59–70, 2001.

Walder, J. S. and Fowler, A.: Channelized subglacial drainage over a deformable bed, Journal of glaciology, 40, 3–15, 1994.

Werder, M. A., Hewitt, I. J., Schoof, C. G., and Flowers, G. E.: Modeling channelized and distributed subglacial drainage in two dimensions, Journal of Geophysical Research: Earth Surface, 118, 2140–2158, 2013.

Yang, X., Nielsen, K. P., Amstrup, B., Peralta, C., Høyer, J., Englyst, P. N., Schyberg, H., Homleid, M., Kpltzow, M. ø., Randriamampianina, R., Dahlgren, P., Stpylen, E., Valkonen, T., Palmason, B., Thorsteinsson, S., Bojarova, J., Körnich, H., Lindskog, M., Box, J., and Mankoff, K.: C3S Arctic regional reanalysis – Full system documentation, Tech. rep., 2021.