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 Reviewers,

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- We have revised the discussion around the till rheology avoiding the outdated hypothesis of a viscous till rheology.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 #1

1.1. General comments

In this paper the authors explore the relationships between subglacial water pressure, seismic power, ploughmeter force, surface velocity and modelled surface meltwater input for a large surge-type glacier in Svalbard for the 2021 and 2022 melt seasons. Based on recently established relationships between seismic power and discharge by Gimbert et al. (2016), the authors use the seismic power data and modelled meltwater input to derive channel cross section and pressure gradient to understand the evolution of these variables for the two contrasting melt seasons. The derived and measured subglacial variables are filtered into diurnal, multi-day and seasonal windows and plotted against each other in phase space with implicit dependence on time. From the phase space relations and the filtered signals, the authors categorize the time periods into four domains to understand the state of channelized flow in relation to Rothlisberger theory for steady channel flow. They have identified periods of time when channel flow is in steady state vs transient. Unrelated to the seismic power, they explain ploughmeter and water pressure data by proposing that sometimes till is coulomb-plastic and sometimes it is viscous. The authors have carried out an impressive campaign to instrument and analyze a rich dataset and their work will contribute to a better understanding of subglacial processes.

AR: We thank the reviewer for the overall positive evaluation of our work.

The major concerns that I have with the work are listed here while minor comments are annotated on the attached PDF and listed below.

For much of the paper, the references are not accurate or pertinent. I stopped commenting on the references halfway through the introduction, but the authors should consider revising the appropriateness of references throughout the paper. In general, the authors need to consider using more original citations and be sure that the citation supports the statement.

AR: 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.

For a discussion of subglacial processes, the authors need to do substantially more work to fit their observations with the literature, especially for till mechanics. My expertise is not in glacier seismicity so I cannot offer much feedback that way. My personal take on till mechanics is that, as a community, we've moved passed the idea of a viscous rheology for till. There are many basal processes related to till mechanics that are not considered and could really change the interpretation of the results, including ice-till coupling, cavitation around clasts within the till, sheet flow at the ice-till interface, regelation infiltration, water pressure fluctuations in the till, etc.

AR: We agree that there is widespread agreement about the Coulomb-plastic rheology of till while viscous behavior is no longer considered. We have carefully reworked our discussion to reflect this. The point we make here is that we sometimes observe quasi-viscous behavior, which is in line with several previous studies (e.g., Murray and Clarke, 1995; Rousselot and Fischer, 2007; Thomason and Iverson, 2008). As the reviewer correctly points out, there are multiple processes that have been thought to be responsible for the observed behavior. However, none of them provides a straightforward explanation for the respective episodes in the ploughmeter record. To address this comment we have added to 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 (e.g., Iverson et al., 1998; Tulaczyk, 1999; Tulaczyk et al., 2001; Truffer et al., 2000; Truffer, 2004). 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."

The work focuses heavily on subglacial hydrology through the lens of channel flow and channel geometry, and in the discussion the authors recognize that the water pressure and ploughmeter data cannot be explained by channel flow characteristics and that distributed flow is probably important. It would therefore make sense that the authors do more to fit all results, including seismic power, into existing theories that describe the seasonal evolution of the drainage system and the relationships between channelized and distributed flow. At present, all observations are being analyzed through the lens of channel flow where the number of channels

(N) is fixed. The authors should focus more on explaining the measured variables rather than the derived quantities.

AR: We have revised the manuscript to provide a more detailed analysis and discuss how the derived quantities may be affected by the underlying assumptions. In this regard, we have included a discussing of the sensitivities to the degree of channel fullness, the characteristic of the glacier bed, the number of channels, and the cross-sectional geometry. This follows the detailed sensitivity analysis by Gimbert et al. (2016) and Nanni et al. (2020). In conclusion, we find that the derived quantities R and S are not very sensitive to these characteristics, therefore relative changes that we analyse in our manuscript should provide an appropriate picture of the overall behavior of the drainage system at the considered scale. Further details are discussed in the detailed answers provided to the specific comments below.

As mentioned above, the paper relies heavily on the interpretation of derived variables (channel cross section and pressure gradient) and discusses the variables as though they're measured quantities. It is intriguing that the phase space portraits can show log-linear correlations that correspond to end-member states of fixed geometry or fixed gradient, but the authors should approach this with caution. There may be many reasons for log-linear correlations amongst variables and the limitations to the equations used are not discussed with relevant detail.

AR: We agree that caution must be taken when interpreting the behavior of R and S. Both quantities have been derived from seismic power and runoff using relationships for specific assumptions in terms of geometry, flow conditions, system structure etc. However, the sensitivity analyses by Gimbert et al. (2016) and Nanni et al. (2020) show that deviations from the underlying assumptions have limited effect and do not fundamentally alter the results. Therefore, we interpret our results for instance in terms of drainage system capacity rather than assigning a numerical value to a particular geometry. We have revised the wording to better reflect our caution. In addition, we would like to emphasize that our method analyses **relative** changes and we do not base our interpretation on absolute values.

- Bedload transport and fluvial erosion should play a very important role in generating noise, and bedload transport can show its own hysteresis with discharge.

  AR: We agree that that bedload transport generates seismic noise. This was indeed observed in many locations, both non-glacial rivers (see also, Burtin et al., 2008, 2011; Hsu et al., 2011; Schmandt et al., 2013) and glacial rivers Bartholomaus et al. (2015). However, this process tends to have a higher frequency signature (typically above 15 Hz) than turbulent water-flow-induced seismic noise (typically below 10 Hz), as supported by observations (see above) and modeling (Gimbert et al., 2014, 2016). Given the frequency band investigated in our study [3-7] Hz, the seismic tremor is dominated by turbulent water-flow induced seismic noise. In order to refine this argument in the manuscript, we have added the following sentences: "We calculate the seismic power from the vertical component of the ground velocity using Welch's method over a two-second time window with a 50% overlap (Welch, 1967; Beyreuther et al., 2010) within the frequency band 3 to 7 Hz. Our choice of this frequency band is based on the dominance of turbulent-water flow-induced seismicity in this band (Bartholomaus et al., 2015; Gimbert et al., 2016; Nanni et al., 2020, 2022), as opposed to bedload transport that generates seismicity at higher frequencies (Gimbert et al., 2016). This has been previously observed in other glacial settings (e.g., Preiswerk and Walter, 2018; Lindner et al., 2020; Labedz et al., 2022b; Clyne et al., 2023)."

- The Q being plotted against subglacial variables is also derived and does not represent the discharge for the Rothlisberger equation. The time delay and changes in the time delay throughout the season should have a very large impact on the diurnal filter window.

  AR: 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.). We as well assume that the transfer between supra-glacial to subglacial drainage of the surface water is done efficiently and at a short time scale by englacial features as observed in other polythermal glaciers similar to Kongsvegen (Benn et al., 2009; Gulley, 2009; Bælum and Benn, 2011; Irvine-Fynn et al., 2011). 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. "

- One of my main concerns is that all the equations are describing the channel evolution at a point. At line 495 the authors state that noise is picked up from a 1 km2 area and detecting the loudest noise, but wouldn't the noise be integrated over this area where channels can show a large variation is size and shape, through time and space?

AR: We agree that the equation describes channel evolution at a point, and that our measurement likely integrate an area of c. 1km2. In the presence of one major preferential flow path within the footprint area of the sensor, the seismic power will be dominated by the signal from this flow path and not reflect other, potentially hydraulically less active or even dry parts of the base (see for instance Nanni et al., 2020, 2021). Our argument is that seismic power characterises the drainage system at the scale of $\approx 1km^2$ but within that area there is variation (Gimbert et al., 2016). Measurements conducted through a borehole are more representative of the scale of these variations. This scale mismatch can therefore explain the apparent disagreement between seismically-derived and borehole-measured evolution of hydraulic conditions. To address this comment we have added the following in the main text: "This approach allows to estimate the evolution of R and S of the dominating drainage system over an area of c. 1 km^2 around the seismic station (Gimbert et al., 2016; Nanni et al., 2020; Lindner et al., 2020; Nanni et al., 2021; Labedz et al., 2022a)."

- Using the seasonal averaging window, the filtered data suggests that channels are in a transient state for a long period of time, so why would daily or multi-day time windows be used over the same time period investigate the possibility of steady state?

AR: We think there must be some misunderstanding. We investigated both long and short term evolution of the drainage system. Our analysis over shorter time scales does not target a two member classification (steady-state or not), but a four member classification of phase relationships which are subsequently interpreted. The analysis is performed on each time scale independently, hence not excluding apparent ambiguities. However, we are not aware of such conflicting interpretations on different time scales. We have added in the text to clarify: 'Our analysis does not target a two member classification (steady-state or not), but a four member classification of phase relationships which are subsequently interpreted.'

- How do the authors address the noise generated from open channel flow vs. pipe full flow? (perhaps this is addressed and I misunderstood)

AR: In our case, this difference does not have a strong effect on our interpretation. 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 in conduit shape and fullness preclude us from confidently interpreting seismic power changes smaller than $10log_{10}(1.25) = 1$ dB". 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 $\sim$70% of the ice overburden pressure. We have added the sentence: ”We neglect here 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; Scholzen et al., 2021).”

- The x axis for derived quantities is labelled as log (X/Xref) AND the scale is log, does that make sense? The x axis should show units.

AR: Thanks for catching this! The axis should have been labelled X/Xref instead of log(X/Xref) when plotted in log-scale. We have changed this in all figures where this applied.

The authors have an interesting dataset pertaining to hydraulic connectivity and basal sliding where continuous till is inferred. The paper would be a lot stronger if there was less focus on resolving the characteristics of channel flow that are derived with seismic noise, while focusing more on explaining the relationships amongst measured quantities and explaining the results based on a stronger foundation in existing literature for basal processes.

AR: Our revisions have hopefully clarified that we do not intend to resolve details of channel flow, instead we exploit the measured seismicity to derive an interpretation of drainage system evolution at a 1 km scale. This is then compared with the borehole measurements that represent processes on a smaller scale. The apparent contrast is then interpreted to reflect the high spatial heterogeneity of the subglacial drainage system, also suggested by others (e.g., Murray and Clarke, 1995; Rada and Schoof, 2018). In this way, we intend to maximize the information that we can gain from all our measurements.

Another minor comment is that the intro to the paper focuses a lot on surge-type glaciers and the basal processes of surge-type glaciers, but a discussion on the mechanics of surge-type glaciers is absent. Do the observations inform us of anything new about the dynamics of surging glaciers and how do the results fit in the context of this glacier building up to a full surge?

AR: To reduce the impression of a misleading surge-focus, we have removed "surge-type" from the title (now: ”Multi-scale variations of subglacial hydro-mechanical conditions at Kongsvegen glacier, Svalbard”), as well as we have removed detailed and unnecessary descriptions of surge processes from the introduction and from the abstract.

1.2. Specific comments, made in annotated manuscript

1. **p2**: Flowers 2015 does not comment on the deficiency of models for explaining surges. Benn and Thogerson refs also praise their modelling work for explaining surges. Perhaps the latter two refs discuss how their models miss certain observations pertinent to hydrology, but their models are good at capturing dynamics of surges.

AR: We have now removed the surge-focus paragraph from the introduction, so this comment does not apply any longer.

2. **p2**: don’t need refs for these widely accepted statements

AR: We have removed these references.

3. **p2**: consider using a more pertinent and original reference here

AR: We have added (Kamb and Engelhardt, 1991; Kamb, 2001) references.

4. **p2**: doesn’t have to be the ablation area, it could be entirely in the accumulation zone

AR: We have now removed the surge-focus paragraph from the introduction, so this comment does not apply any longer.

5. **p2**: only steepening at the boundary of the reservoir area. surface could be flattening up glacier

AR: We have now removed the surge-focus paragraph from the introduction, so this comment does not apply any longer.

6. **p2**: agreed that a threshold is reached with driving stress, but is it thickness or slope? not clear from how you have written it

AR: We have now removed the surge-focus paragraph from the introduction, so this comment does not apply any longer.

7. **p2**: iverson 1995 suggests the opposite, the zoet and clarke papers do not suggest surging results from till failure

AR: We have now removed the surge-focus paragraph from the introduction, so this comment does not apply any longer.

8. **p2**: schoof ice modelling paper perhaps not the appropriate ref for linking surging to changes in drainage

AR: We have now removed the surge-focus paragraph from the introduction, so this comment does not apply any longer.

9. **p3**: water flow through till is not mentioned but is of equal importance, especially for storage. you address this in the discussion and so it should be introduced in greater detail here.

AR: We have added a paragraph describing the subglacial hydrology in case of soft-bed glaciers. ”Glaciers resting on a till base exhibit a complex subglacial drainage system. While some water drains through the pore space of the granular material, also drainage along the ice-till interface has been described and various drainage structures have been proposed. For glaciers lying on fine grain sediments, water is expected to flow through distinct flow pathways termed canals. These canals are incised into the sediment and/or ice by erosion and close through the creep of ice from above and sediments from below (Walder and Fowler, 1994; Ng, 2000). Flowers and Clarke (2002a,b) proposed a macro-porous horizon as a continuum concept to comprise inter-granular pore space, thin films, cavities, or larger gaps”.

10. **p3**: Additional?

AR: We have changed ’further’ for ’additional’.

11. **p3**: No hyphen

AR: We do not see this hyphen.

12. **p3**: Remove further

AR: We have removed ’further’.

13. **p4**: should mention that cryoseismology is also inferring noise from discrete slip events, not just turbulent flow in channels (see works by Graff or Lipovsky)

AR: We have added the following sentence explaining more broadly how cryoseismology is used and added references. ”Recent studies have shown the potential of near-surface cryoseismology to bridge the gap between observations at different scales (Podolskiy and Walter, 2016), for instance to detect brittle fractures related to crevasse opening (e.g., Roux et al., 2008; Nanni et al., 2022), stick-slip motion at the glacier base (e.g., Wiens et al., 2008; Gräff et al., 2021; Köpfli et al., 2022; Hudson et al., 2023), iceberg calving (e.g., Köhler et al., 2015; Sergeant et al., 2018), or to infer hydraulic conditions across various temporal (sub-daily to multi-year) and spatial (decametric to kilometric) scales (Bartholomaus et al., 2015; Nanni et al., 2020; Lindner et al., 2020; Nanni et al., 2021; Labedz et al., 2022a).”

14. **p4**: It seems like this can be labelled in a more traditional way, with ;Field Area; and Methods; separately. sections 2.2 and 2.3 seem like especially good candidates for the methods section

AR: We have restructured the article by separating field description and a new Method section that contains five sub-sections: 1. Borehole, 2. Near-surface instrumentation, 3. Surface runoff and meteorological conditions, 4. Derivation of subglacial variables, 5. Processing of time series, catalog of events and classification.

15. **p4**: is the till just sand/silt-sized or is it a more traditional till that is poorly sorted across the range in size from boulders to clay?

AR: The granulometry of the till under Kongsvegen glacier is not known. Hjelle (1993) made geological maps of Kongsfjorden area and extrapolated the lithology under the glacier. To address this comment, we have added in the main text: "The glacier rests on fine-grained sandstone and sand/silt glacio-marine sediment (Hjelle, 1993; Murray and Booth, 2010)."

16. **p5**: Location

AR: We have made the change.

17. **p5**: might be good to credit the source of these data in the caption

AR: These data are provided by Jack Kohler, a co-author of this paper, as mentioned in the author contribution and are not published elsewhere.

18. **p6**: s

AR: We have removed the s.

19. **p6**: reword this sentence for clarity

AR: We have modified this sentence : 'The exact insertion depth of the device into the till is unknown. However, based on previous experiences with identical devices, we estimate the penetration depth to be around 10 to 40 cm, which is sufficient to ensure that all strain gauges are immersed in subglacial material."

20. **p6**: more detail is needed in the section. It's clear that you rely on the results from Schmidt, but more justification and clarity are needed here. partitioning the volume that get trapped in the snow vs. treated as runoff seems like it would come with a lot of uncertainty that needs to be addressed. I'm also not clear on what is meant by ;all runoff produced upstream is conveyed...; what fraction of that water is going into the borehole?

AR: We have added more details in this section and error estimations. To address this comment, we have changed the paragraph into: "The available surface water in a grid cell is either retained in snow or firn, or runs off under the influence of gravity. The retention is governed by the hydraulic conductivity of the snow, parameterized based on snow grain size, density, and effective water saturation. Depending on temperature conditions, retained water may refreeze, thereby releasing latent energy. Once the retention capacity of a layer is reached, excess water may run off with a time scale depending on surface slope (Schmidt et al., 2023). Schmidt et al. (2023) estimated a standard error of runoff of 0.12 m w.e.a^{-1}. The surface runoff is modelled on a 2.5 by 2.5 km grid and we assume that all runoff produced within an area of 6.25 km^2 upstream of our borehole is conveyed at the base without any delay. Since our analysis considers relative changes, only the timing but not the absolute magnitude are of interest. "

21. **p7**: The strain gauges to strain and are to is

AR: The new sentence is: "Strain on the ploughmeter is measured using a Wheatstone bridge for each pair of strain gauges in two perpendicular axes (Hoffmann, 1974)."

22. **p7**: at

AR: We have removed the extra 'at'.

23. **p7**: This seems like it should be sensitive to the bending moment and so the depth of the ploughmeter into the till should influence F, no?

AR: Fischer and Clarke (1994) argument that as long as the strain gauges are in the till, the force is independent of the penetration depth since the distance between the tip and the strain gauges is constant (hence the bending moment remains the same. We have added in the text: "The exact insertion depth of the device into the till is unknown. However, based on previous experiences with identical devices, we estimate the penetration depth to be around 10 to 40 cm which is sufficient to ensure that all strain gauges are immersed in subglacial material".

24. **p7**: does noise come from bedload transport? is is assumed that bedload transport is coupled to discharge?

AR: We refer to our answer above on the general comment regarding this point.

25. **p8**: within the band of the window being investigated?

AR: We have clarified the text as follow: "We define an event by two subsequent minima of Q within the bandwidth investigated."

26. **p10**: how did you come up with a threshold of 2 and are your results sensitive to that value?

AR: We motivated our classification scheme by noticing that phase relations may be linearly positive or negative or exhibit some transitional stage (preceding or lagging). To account for uncertainties symptomatic for observations of natural systems, we allow some deviation from strictly linear behavior and accept RSS ≤ 2 still representing linear behavior. The choice of this threshold is motivated from visual impression of clustering of phase relations. In Sec 5.1 (p18) we discuss the influence of this somewhat deliberately chosen threshold on the classification. Such classification uncertainty is symptomatic for any classification of behavior that may occur along a continuous scale. We have added in the discussion: "In addition, the definition of the four classes is motivated by noticing that phase relations may be linearly positive or negative or exhibit some transitional stage (preceding or lagging). To account for uncertainties symptomatic for observations of natural systems, we allow some deviation from strictly linear behavior and accept RSS ≤ 2 still representing linear behavior. The choice of this threshold is motivated from visual impression of clustering of phase relations."

27. **p13**: the ratios are shown as the log of the units, the axes are also distributed by logarithmic values, does that mean that the units are log AND the axis is log, or is it sufficient to have a log scale, but the units are in Q/Qref or R/Rref? My take would be that the axis is log, and axis label should simply read Q/Qref or R/Rref. Or, you could show log units with a linear scale.

AR: Thanks for catching this. Yes, your impression is correct, we have removed the 'log' from the label.

28. **p13**: so the label should just be R/Rref or Q/Qref

AR: We have removed the 'log' from the label.

29. **p15**: as to and

AR: We have made the suggested change.

30. **p15**: and on the other hand?

AR: We have rephrased as follow: "On the other hand, p always precedes Q during the melt season 2022".

31. **p15**: why use such a large buffer in window below and above a 24 hour cycle?

AR: Diurnal variations occur on a daily basis, with a period of approximately 24 hours. However, they are not strictly confined to exactly 24 hours due to external factors, e.g., time of the year, local environmental conditions. The 6 to 36-hour frequency range captures the primary diurnal frequency (around 24 hours) and also accounts for some variations and fluctuations around this period. To address this comment, the main text has been modified into: "To examine the glacier response to changes in Q at a diurnal scale, we filtered the time series using a band pass filter, cutting off variations beyond the lower and upper limits of six hours and 36 hours, respectively to capture the primary diurnal frequency (around 24 hours) and also account for some variations and fluctuations around this period."

32. **p16**: more space here

AR: We have modified the figure accordingly.

33. **p17**: Since you're suggesting that they are closely correlated, can you show that result? even though pressure might be fixed at the terminus, it likely varies immensely down glacier as the drainage system evolves and so pressure at one location might not correlate with S.

AR: Indeed, as S is an integrated measurement and p, point-wise, the records might not be correlated, as observed in our record. We have reworded our sentence: "Since we always measure p at the same location and the glacier terminus is fixed at sea-level, for a spatially homogeneous drainage system, we expect that variations in S are closely correlated to those of p. However, spatio-temporal complexity in the drainage system downstream of our borehole may lead to incoherent relations between local p and spatially integrated S."

34. **p19**:but only for some parts of the record filtered by long term windows, right?

AR: We have modified the sentence to specify that this applies to the long-term filtered part of the records: "While long-term variations of R and S suggest that the system capacity reaches an equilibrium with Q, the variations of p..."

35. **p19**: though your trying to match all observations to steady state equations that all describe channel full conditions

AR: The framework is that of Gimbert et al. (2016) (see also Supp. Mat.). For a detailed answer, we refer to the answer of the general comment above.

36. **p19**: assuming that all the noise can be accounted for by changes in channel size.

AR: We think this is a misunderstanding, the evolution of seismic power is partitioned between R and S and no assumption about predominance of one over the other is made. Our text discusses the phase-relation of R and Q on short time-scales and the co-evolutions of S and p.

37. **p20**: this statement has become quite repetitive at this point in the paper

AR: We have removed the sentence.

38. **p20**: at this point in the paper I'm still just a bit confused. does rigid pipe simply mean not channel full or does it mean that cross sectional area is fixed while discharge varies?

AR: A rigid pipe describes an end-member case where the cross-sectional area of the channel is fixed and so every variations in runoff leads to a variations in hydraulic gradient rather than hydraulic radius. We have clarified this point in the Section 5.1 as followed: "For a channel with a fixed cross sectional area referred to as rigid pipe, increase in runoff Q results in increase in water pressure p that translates ...".

39. **p20**: R and S are not records

AR: That is right. We have added 'and derived variables'.

40. **p21**: good to add rada and schoof (2018 and 2023) here

AR: We have added these references.

41. **p22**: that is one possibility, but the other possibility is that the drainage system evolution is in reality dictating the borehole water pressure and is well connected, but the equations derived to infer channel geometry from seismic power from channels alone are not appropriate. If there is a continuous till layer, my first instinct would be that water pressure variations are dictated by the pore water pressure in the till, as a complex function of the state of the till and its connectivity to nearby channels.

AR: We do not see a disagreement between our interpretation and the one suggested by the reviewer. In our text, we explain the apparent discrepancy between the seismic and borehole records in terms of spatio-temporal complexity of the drainage system. This interpretation entails the view of the borehole being indirectly (less efficiently) connected to the dominating flow pathway which is dominating the observed seismicity. This seems consistent with the view proposed by the reviewer.

42. **p22**: This statement needs to be referenced. There are theoretical and experimentally derived reasons for true or perceived rate strengthening or rate weakening in a coulomb-plastic till. Even if sediment was shown to be viscous, why would the p-F relationship be linear and positive? increased water pressure would still weaken a viscous till. the complex relationship between p and F depends a great deal on ice-till coupling, as noted in the previous papers referenced in this manuscript that analyze ploughmeter data

AR: This behaviour would indicate a viscous rheology if we assume a positive relationship between water pressure and basal velocity. For a viscous till, F would increase with u_b; using our assumption, this translates to a positive $p - F$ relationship. We have clarified this in the main text as follows: 'However, during winter 2021/22, the $p - F$ relationship exhibits a positive slope which is unexpected for Coulomb-plastic rheology. Quasi-viscous behavior entails a velocity dependency of basal resistance which results in a positive $p - F$ relationship. At the same time, this requires accelerated glacier speed, however, this is not observed during winter 2021/22. Quasi-viscous behavior has been observed and discussed by Murray and Porter (2001); Rousselot and Fischer (2007); Thomason and Iverson (2008)".

43. **p22**: or water pressure fluctuations that do not diffuse to the depth of the probe tip in the till (e.g. Truffer 2004), or the bed becoming rigid with the ice decoupling to allow for a very small background level of slip

AR: We refer the reviewer to the answer to the general comment as this comment is largely similar.

44. **p22**: this feels like the right train of thought away from viscous rheology, but consolidation happens from a decrease in water pressure and as the water drains. again, consider in more detail the depth dependent nature of pore water fluctuations in till (in addition to Trapridge work, see works by Tulaczyk, Iverson, Truffer, Rose/Hart, etc.)

AR: We refer the reviewer to the answer to the general comment as this comment is largely similar.

45. **p22**: again, think more about ice-till coupling and depth-pressure variations in till than on the constitutive law for the till

AR: We refer the reviewer to the answer to the general comment as this comment is largely similar.

46. **p22**: If you have to stick with the idea of till viscosity perhaps try using apparent viscosity. even in aggregates, the grain bridges and clasts in the aggregates fail by a more-coulomb law (plastic)

AR: We refer the reviewer to the answer to the general comment as this comment is largely similar.

47. **p23**: the complex interplay between till water pressure and till properties has been shown experimentally, theoretically and from field data on several occasions, but not just through ploughmeters, through drag

spools and tilt meters and sliding speeds too.

AR: We have removed this statement which was indeed incorrect.

2. Reviewer #2

2.1. 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.

2.2. 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 $10log_{10}(1.25) = 1$ 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 ∼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)."

2.3. 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.

2.4. 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 ∼ 100m from the borehole, a three-component geophone (DiGOS, 4.5 Hz) was installed ∼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 ± 10 m (KNG6, 78.78067^{o}N, 13.15153^{o}E) and 3100 ± 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."

2.5. 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

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