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

1. Reviewer #1

1.1. General comments

The revised manuscript is a significant improvement from the original submission. It's really great to see these complementary datasets from contrasting melt seasons, and a good idea to classify relationships amongst variables. The authors have done some good work. The structure of the discussion could still be tweaked to make the interpretation more consistent with observation. The authors take us on a journey to show that the various instrument records don't align with each other when interpreted in the context of transient vs steady-state drainage or water pressure and force on the ploughmeter. The authors later suggest that the discrepancy in observations can be explained by drainage system heterogeneity and the spatial scale of instrument detection. The latter is not novel and the results should be interpreted from this lens at the outset, while still paying attention to drainage system evolution to reconcile the seismological record. In my opinion, the paper is close but still requires a few changes.

AR: We thank you for the thorough reviews that have helped improve the manuscript. We have revised the manuscript to account for the comments of the reviewers. We have documented our revisions in an attached document that highlights all changes with additions marked in blue and removals 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.

1.2. Line comments

- Line 1. The abstract does not say anything about seismic power, which is one of the main techniques used in the paper

AR: We have now included the following precision in the abstract: Additionally, we derive subglacial hydraulic gradient and radius **from seismic power, recorded at the glacier surface**.

- Line 61. i think it's appropriate to also mention glacier surges in response to a change in water pressure regime since you're working on a surge-type glacier

AR: -We have now included the following precision in the introduction: Alterations in subglacial hydrological and mechanical conditions have the capacity to change the overall glacier dynamics, sometimes leading to partial or complete destabilization, **such as glacier surges** (Thøgersen et al., 2019; Zhan, 2019).

- Line 76. Due to, missing to.

AR: We have changed the text as suggested.

- Line 191. can you state here what is meant by open flow. Intuitively it seems hard to understand that the relationship between P, Q and S isn't sensitive to whether the channel is full, but i guess i need to dig into the supplementary material in Gimbert (2016) to convince myself .

AR: -We have now included the following precision in the text: For open **channel** flow, **i.e. flow with a free surface, as opposed to pressurized flow, where the channel cross-section is filled with water in all directions**, R scales with flow depth and S with slope along the flow direction.

- Line 381. perhaps use "channelized drainage" because cavities are also ice-walled and show the opposite behaviour

AR: We have changed the term to ' **Röthlisberger-type drainage**'

- Line 392. [The response ...] maybe there's a different way to say this? written this way, it seems like you've set the reader up to think that R and S can be disentangled from P and Q and we go through a lengthy results and discussion only to read that it's complex and the interpretation is difficult. I'm not saying you should change your analysis, perhaps just the wording on this sentence.

AR: We agree with the reviewer that having this sentence at the end of the discussion was wrongly placed. Indeed, we have already described and disentangled the relationship between P, R and S in the method section so that it is not necessary to recall it here. Instead, we have now recalled at the beginning of the discussion that R and S are derived from the observation of P and Q: Here, we first consider the expected responses of R, S **(Eqs. 1 and 2)** and p to changes in Q for different typical stages of subglacial channel evolution.

- Line 401. yes, but i think that the complexity of the relationship between F and p does not depend only on the relationship between till strength and water pressure of water pressure and sliding speed, but equally importantly on whether the glacier bed is coupled to the till (a good illustration of this is Iverson et al 2007..."soft bedded experiments").

AR: We thank the reviewer for the suggestion, and we have added this precision: However, more complex $F - p$ relationships can occur, arising from the non-linearity of p-sliding speed relationship (Alley, 1989; Boulton and Hindmarsh, 1987) **and from the degree of ice-till coupling** (Iverson et al., 2007) .

- Line 405-408. an added complexity that should be noted is that the p-F relationships is interpreted for local conditions at the instrument site, but the relationship between variables might be further governed by a forcing distal from the sensor. For example, suppose the till fails 1km upstream and the longitudinal coupling of the ice forces sliding of the downstream ice by a decoupling of the ice with the till, the till might not be in a state of failure at the bed, but the ploughmeter will still be dragged through a till that will be forced to fail at the contact of the ploughmenter and the till.

AR: We thank the reviewer for highlighting this aspect and have now added the following precision: p **and** F **can also be influenced by potential distal forcing, e.g. longitudinal stress-transfer, complicating their interpretation.**

- Line 413. this seems like it should be discussed in the methods section as a justification for RSS<2

AR: We have now moved this to the Method section as suggested.

- Line 430-431. perhaps mention what those other mechanisms could be

AR: We have now added the following precision: As mentioned above, due to the composite nature of turbulent-water-flow-induced seismic power, the $P - Q$ relationship is more difficult to interpret, but comparison to idealized behavior suggests that system adjustment is dominated by different mechanisms **(e.g., adjustments of geometry or hydraulic gradient)** during different stages [...].

- Line 434-435. this should be expected to some extent, right? the noise comes from the channelized drainage while the pressure in borehole is likely a measure of the distributed system because it's hard to hit a channel and the probe goes into till

AR: We have now added some precision on this aspect to discuss the cause of the disagreement, indeed expected by the means of observation: **We explain this apparent disagreement by the different spatial scales of sensing of the different instruments: while the geophone records are likely dominated by seismicity generated by turbulent flow in the channelized system (Nanni et al., 2021) , the local pressure record is representative for the borehole, which is more likely to sense the local distributed system (Rada and Schoof, 2018).**

- Line 439. add space after channel

AR: We have made the suggested change.

- Line 441. again, as mentioned above, p and Q are likely measuring different systems and p may not be representing the steady state or transient behaviour of the channel

- Line 452. again, as mentioned above, p and Q are likely measuring different systems and p may not be representing the steady state or transient behaviour of the channel

AR: We think that adding the precision after the previous comment, that is, that p and S are sensitive to different systems, also answers these concerns. Additionally, we further discuss these aspects later in the discussion in a dedicated paragraph 'Ambiguous interpretation from borehole and cryoseismic records'.

- Line 458. remove bending over to

AR: We have changed to 'shifting to'.

- Line 460. remove space before P

AR: This was changed.

- Line 469. remove space before Q

AR: This was changed.

- Line 475 - Discussion section. I would suggest reorganizing the discussion. In the previous few paragraphs the only thing that I could think of was "you're measuring p in the distributed system, why are you spending so much time talking about reconciling p with the P, Q, R, S variables for transient vs channelized?". Because the first part of the discussion focuses on transient vs steady state, the p record seems ambiguous, but really, it's not ambiguous if the focus is on whether the records represent distributed vs channelized.

AR: Following the previous suggestion (comment on Line 434-435), we have now precised at the beginning of Sect. 5.2 that our measurement of p and S might not be sensitive to the same system. However, we still think that it is important to first describe our observations with no-a priori and then discuss the potential reasons for the observed behaviors. In fact, we argue that the contrasting responses of p during the two melt seasons are ambiguous. Thus, assuming the position of that the sensor remains sensitive to the same part of the subglacial drainage system during the entire recording period would lead to an erroneous interpretation of our data.

- Line 483. remove relation with P-Q

AR: We have changed relation for **relationship** for consistency.

- Line 528. if you assume that the bed is coupled and basal motion responds to p at the bed only locally. Similar to the comment that I made above above reorganizing the structure of the discussion, I would suggest setting up the discussion so that the p-F relationship is not seen as ambiguous if positively correlated. I think that you're justified in suggesting that p-F can be positive if the bed decouples, or as I suggest, slip is not exclusively dictated by conditions locally. That could be stated in the introduction and that would give you more freedom to suggest that you observe till weakening as well as decoupling without tying to justify the apparent discrepancy.

AR: We thank for the comment that raised many conversations among the authors, however we think that there is no straightforward explanation for a positive p-F relationship based on our instrumentation settings. Indeed, the till layer might be saturated for the ice-bed decoupling to occur. In this configuration, the till is then weakened and we should observe a decrease of F while increasing p. Additionally, as stated in the Discussion already (lines 555 to 570), previous studies with ploughmeter device have observed such positive relationships but the interpretation of such behavior remains inconclusive. We therefore suggest that such relationships should be explored further with a new design of the ploughmeter (involving tracking the instrument vertical position), currently a work in progress project.

- Line 552. I initially suggested that you take out some of the focus on glacier surges, which was quite elaborate in the introduction but not mentioned in the discussion. I still think that it's okay to leave out the extensive literature review on surges in the introduction, but somewhere in the discussion it would be a good idea to talk about whether your interpretation is in line with the state of the glacier at present, i.e., do the data make sense with the glacier starting to accelerate toward a surge phase? nothing elaborate, only a sentence or two.

AR: Although the surface velocity measurements indicate the build-up of a surge, our dataset does not allow us to interpret the processes driving surge build-up. Indeed, the focus of our study on one area of the glacier prevents us to look at spatial changes of the glacier dynamic and propagation of surge. Additionally, we only focus on short time period, i.e., two melt seasons, where the dynamic is dominated by seasonal changes rather than multi-annual dynamic. We have therefore added this sentence into the conclusion: **By constraining the spatial and temporal extents of our investigation, this study aims at investigating local subglacial hydro-mechanical conditions rather than exploring the multi-annual and likely large scale processes responsible of the surge build-up.**

- Line 555. while you have characterized the broad spectrum of channel behavior with the cryoseimology and discharge data, I don't think that you've covered the broad spectrum of sediment deformation behaviour.

AR: We have reworded as follow: **We synthesize the broad spectrum of relationships between the observed data series in four classes and** interpret them in terms of drainage system evolution and till rheology [...].

- Line 563. It is very well established that efficient and inefficiency systems can coexist in time given the spatial variability of sublglacial drainage, so rather than syaing "we suggest the co-existence", perhaps you could say something like, "the disagreement among seismic and water pressure and tilt meter data can be explained by the spatial heterogeneity of the subglacial drainage system, whereby..."

AR: We have added the precision: **The apparent disagreement between seismic data and borehole-sensed pressure records can be explained by the spatial heterogeneity of the subglacial drainage system,** supporting the concept of a discontinuous, spatially heterogeneous drainage system [...].

- Line 566. i don't think that it puts into question the rheology of till itself, but rather the extent to which the glacier is coupled to till and the extent to which slip results from water pressure conditions locally

AR: We have reworded as: **The relationship between the force experienced by the ploughmeter and the water pressure reveals complex behavior. As expected, the till behaves mostly as a Coulomb-plastic material but episodically, the p-F relationship might be indicative of ice-till decoupling, permitting slip of ice over the till.**

2. Reviewer #2

2.1. General comments

In the revised manuscript, 'Multi-scale variations of subglacial hydro-mechanical conditions at Kongsvegen glacier, Svalbard,' the authors explore the relationships supraglacial runoff and subglacial conditions, as derived from borehole and seismic monitoring. Overall, they find that their relationships can be reasonably explained by the canonical spatially variable subglacial hydrologic system.

AR: We thank you for the thorough review that has helped improve the manuscript. We have revised the manuscript to account for the comments. We have documented our revisions in an attached document that highlights all changes with additions marked in blue and removals 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.

While the authors made a reasonable effort to address many of my previous concerns, I do feel that there is still some work to be done. I have two substantial/major comments and multiple line comments, both indicated below.

Major comments 1. Because large portions of the manuscript were rewritten, there are often sentences that are incomplete or unclear. A careful reading for grammar and clarity is in order. In some cases, I have pointed out issues below. In addition, the referencing is both limited and occasionally ad-hoc (e.g. there are cases where a paper is germane to the immediate point yet is not included in the citation list despite being previously cited). Some effort on improving the referencing is necessary.

AR: We have re-read carefully the manuscript and corrected the ad-hocs sentences. As well, we improved the referencing as requested.

2. There is still limited to no discussion of the ice surface velocity data. Near the end of discussion, I was still waiting for some link back to the velocity data. These data can provide substantial context to the subglacial observations and will allow the authors to more readily link their observations with previous work.

AR: We have referred to acceleration events more often in the discussion to bring context. However, there are no clear patterns between acceleration events and the relationships observed (p-Q, S-Q and F-Q) and when we bring velocity context, it should be taken with care due to the integration of wider conditions into the glacier motion. We have added in the text the following sentences:

- In this situation, we expect constant R, unaffected by variations in Q (not classified) accompanied by acceleration events.
- In this situation, the glacier velocity is expected to remain constant or decrease.
- In that case, a high water pressure would weaken the till, facilitating its deformation
- The melt-induced runoff increase at the beginning of the melt season and a major rainfall event at its end (Fig. 3, 1 and 3) leave a clear impact on p at the multi-day time scale (Fig. 3d, Fig. 5a) and coincided with a considerable glacier acceleration. During these events, $p - Q$ relationship indicates that the drainage system evolves in a transient manner (*Lagging class* and *In-phase class*, Fig. 5a). At the beginning of the melt season, the drainage system is not yet developed when the melt begins leading the immediate increase in p. At the end of the melt season, Q was at low levels for a period of about 10 days (Fig. 3d), and presumably, the capacity of the drainage system had decreased, when it suddenly became overwhelmed by the arrival of new water volumes. This results in a sharp increase of p, provoking a short-term acceleration of the glacier. Similar late season events have also been reported in other studies (Andrews et al., 2014; Rada and Schoof, 2018; Nanni et al., 2023). We note that p supports the interpretation of the velocity data in this context, the p-Q relationship displays similar patterns throughout the 2021 melt season, which do not lead to acceleration events.
- This period coincides with a noticeable glacier acceleration event aligning with a subglacial drainage system out of equilibrium.
- The major acceleration event occur when p is above the overburden pressure indicating a direct link between glacier velocity and state of the subglacial drainage system as measured locally and possibly in isolated bed regions (Figs. 3h 7, 4i).
- However, as opposed to the acceleration events during the melt season 2021, there is no straightforward explanation between p-Q relationships and the acceleration events. As the velocity data reflect a wider area than the borehole, we hypothesize that the acceleration events are not directly caused by processes happening at the promiscuity of our borehole.
- As a consequence, interpreting the cause of glacier acceleration through cryoseismology or local basal water pressure might not be suitable. On one hand, cryoseimology is sentitive to the most efficient part of the subglacial drainage system that are often not the primary controllers of glacier velocity. On the ther hand, glacier velocity results from basal conditions over a larger area (scale of ice thickness) than what is locally sampled by the measured water pressure p.

2.2. Line comments

- 4. Consider using the month, spring and summer may have ambiguous meaning.

AR: We have added the months.

- 5. Channel? Hydraulic gradient and radius or something different. Either way this should be defined.

AR: We have reworded as : Additionally, we derive subglacial hydraulic gradient and radius **of channelized subglacial drainage from seismic power, recorded at the glacier surface**.

- 12. Instead of 'In this configuration' I would use 'In the latter configuration' to be very clear.

AR: We have made the suggested change.

- 28. I am pretty sure this reference should be Schoof (2010), not Schoof (2005) which is related more to cavitation, but I may be wrong.

AR: We have made the suggested change and referred to Schoof 2010, which was indeed the appropriate reference.

- 37. The sentence starting on this line should be referenced.

AR: The references are already cited above when we first introduce the terms: These include water sheets (Weertman, 1972; Walder, 1982; Creyts and Schoof, 2009), cavities in the lee of bedrock obstacles (Lliboutry, 1968; Iken, 1981), linked cavities (Kamb, 1987; Walder, 1986) and channels incised into the ice or subglacial substrate (Röthlisberger, 1972; Nye, 1976; Hooke et al., 1990; Walder and Fowler, 1994).

- 39. The 2nd sentence starting here sort of loses the map and should be rewritten.

AR: We have reworded as: **For glaciers resting on deformable sediments, i.e. till, the subglacial drainage system is more complex. Flowers and Clarke (2002a,b) proposed a macro-porous horizon as a continuum concept to comprise inter-granular pore spaces, thin films, cavities, or larger gaps. Additionally, water may circulate through channel-like structures called canals that are incised into the sediment and/or ice by erosion and close through the creep of ice and sediments(Walder and Fowler, 1994; Ng, 2000).**

- 65. I am not sure that the statement 'Such process are yet poorly…' is completely true anymore. I point you to Sommers et al. (2023; J. Glac.), Downs et al. (2018; JGR Earth Surface), and Gilbert et al. (2022, GRL).

AR: We have now reworded as: Adequately representing such heterogeneity remains a challenge for subglacial drainage models (Flowers, 2015) , although some progress have been made recently (Downs et al., 2018; Gilbert et al., 2022; Sommers et al., 2023).

- 87. Instead of 'request', would 'necessitate' or 'dictates' be better?

AR: We have changed to **necessitate**.

- 173. A bit more description would be useful. What topography was used, routing algorithm? These are important for characterizing the surface catchment for this study. Reproducible details are important.

AR: We have now simplified the description as the use the topography model was not necessary since we integrate all the runoff produced in the glacierized catchment upstream of our location. We have reworded the text as: **The surface runoff is modeled on a 2.5 by 2.5 km grid. We determine the surface runoff routed through our study area by summing all surface runoff generated upstream from our study site within the glacierized catchment of Kongsvegen's glacier.**

- 187. I am assuming that 'This' refers to flow induced seismicity? It would be useful to be clearer here.

AR: We have rephrased as: **This turbulent-water flow-induced seismicity has also** been observed in other glacial settings [...]

- 212. Something is off with the use of the 'i.e.' here.

AR: We have simplified the text as: To characterize subglacial conditions, we analyze the responses of the measured (force F and water pressure p) and derived (turbulent-water-flow-induced seismic power, P, the hydraulic radius, R and the hydraulic gradient S) subglacial variables in terms of phase relationship to Q.

- 222. I think in this sentence you need to define that X represents a given variable. Since it is used in Figure 2.

AR: This is now added.

- 223. no dash in subglacial.

AR: This was changed.

- 245. Could this sentence just say that "We interpret a four-member classification of the phase relationship to better understand…" Or something similar?

AR: We have now removed this sentence as the information was already given just before.

- 299-303. This is a nice description.

AR: We thank the reviewer for such a positive comment.

- 305. To avoid a double negative, drop the 'not' or change to neither/nor to either/or

AR: We have reworded as: Then, the preferential drainage axis briefly leaves this regime to follow an evolution that is **not indicative for either constant R or constant S.**

- 369. I think it is relevant to have a sentence indicating that you discuss why relationships might not fit current theory (line 410ish).

AR: We have reworded as: **In addition, we discuss the expected responses of$F$ for the Coulomb-plastic rheology and discuss why relationships may not differ from current theory.**

- 385. Is rigid pipe the best term since it is unknown whether the channel has a pipe like structure?

AR: We use here the term rigid pipe to refer to a channel of static cross-sectional, as defined in the Methods section. We consider the term "pipe" as neutral in terms of shape and free from implicit assumptions.

- 418. I think it is also relevant to mention the variability in Q, not just magnitude.

AR: We have now added: The two observed melt seasons considerably differ in terms of duration, **variability** and intensity.

- 435. This sentence is incomplete.

AR: We have simplified the sentence as: While long-term variations of R and S suggest that the system capacity reaches an equilibrium with Q, the variations of p indicate a transient evolution in response to changes in Q.

- 445. drop the 'already' and make it clear over what timescale the roughness response is active.

AR: We have now removed this sentence as it was not correct.

- 485. The system is not only a microporous sheet, there are channels too.

AR: We have added: Perceiving the drainage system as a macroporous sheet, **intersected by channels**, goes along with the spatial heterogeneity of the subglacial drainage system [...].

- 490. To me, it seems that seismic power in the provided range is primarily related to turbulence. In the case of a channel + microporous sheet, it would be expected that the vast majority of the signal would originate in the channel – and to some extent this is what is assumed from the framework used in this paper (e.g. hydraulic radius and channelized description). But here it seems that, by invoking the sheet, that the attribution of seismic noise may be broader, but then the last sentence confuses that again. I think it would be best to be very clear. Something like: while the geophone is sensitive to

signals within 1km2; the frequency range of interest is primarily generated by turbulent flow; therefore, we expect that the vast majority of the observed signal is generated by large channel(s).

AR: We have rephrased as: Accordingly, the cryoseismologically derived variables P, R and S are sensitive to an area of $\sim$1 km^2 around the geophone location. In addition, since the seismicity in this frequency range is mainly generated by turbulent water flow, we expect that the observed signal is mainly indicative of large channels (Nanni et al., 2021).

- 512. with channels?

AR: Our expression "spatially heterogeneous and discontinuous drainage system" already accounts for channels, so we do not specify each potential component individually.

- 513. Actually, limited diurnal variability wouldn't necessarily signal hydraulic isolation, but an inverse relationship between pressure and ice velocity would.

AR: We have now added that we draw this interpretation by combining the lack of diurnal variability and the high values of pressure, which has been used in several studies as indication for hydraulic isolation (Rada and Schoof, 2018; Rada Giacaman and Schoof, 2022). We have added this precision: Although the minor diurnal variability **and high values of** p during the melt season 2022 may be interpreted as symptomatic for hydraulic isolation.

- 563. Also, Andrews et al 2014 and Hoffman et al 2016.

AR: We have added the suggested references.

2.3. Comments on Figures

Figure 1. The personal communication citation is not included in the references.

AR: We have removed the personal communication as the dataset comes from a co-author of this paper but we have added the description of the method as follow: **For the long term averaged velocity from 2005 to 2022 (Fig. 1b), annual (April to April) glacier surface velocity was derived from annual GNSS surveys of mass balance stakes (Nuth et al., 2012).**

Figure 3. What does the p stand for in the rain and snow panel? Are these rain and snow rates or accumulated rain and snow? The figure caption suggests rates, if not this should be clarified, and the timescale of accumulation should be indicated.

AR: We have now changed the p label that was confusing for a$_{rain}$ and a$_{snow}$. We have as well clarified the unit in the figure and the caption since, as the reviewer pointed out, these quantities are rates.

Figure 4. The caption is very long. It seems that some of this description may need to be in the main text.

AR: We have shortened the caption as suggested.

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