Response to reviewers

Reviewer comments in black *- Answers in blue italic, changes in the original manuscript is in green italic*

Line numbers referenced in this document correspond to the manuscript version with tracked changes.

Reviewer 1 (Samuel Doyle):

Togaibekov *et al*. report detailed measurements of glacier surface uplift inferred to be caused by hydraulic ice-bed separation from a dense array of GPS receivers on Glacier d'Argentiere. The results show the seasonal pattern of subglacial hydrological development and induced changes in basal sliding from winter-time distributed "weakly-connected" cavities to a system dominated by efficient subglacial channels in summer. The paper makes a strong and original contribution to the large body of work on the topic of subglacial hydrology and basal sliding, and in particular to the specific topic of ice-bed separation. The results are tentatively and appropriately put in to the context of similar studies in Greenland. I have four general comments; two of which request more detail on the GPS methods and a number of specific comments and technical corrections.

We would like to thank the reviewer for the overall positive feedback and the valuable recommendations, all of which have been incorporated into the manuscript and have improved its quality.

General Comments

1. Further justification for the use of static over kinematic carrier phase positioning should be provided. It is likely that a reference station is located closer than 110 km to Glacier d-Argentiere and this could allow kinematic position to be used, at least for comparison. The time period over which the receiver is assumed to be static is unclear from the methods and should be stated: from the text of the methods it appears to be daily but then the time series appears to be sub-daily. It is unclear whether the resulting position estimates were filtered in any way prior to differentiation to calculate velocity. I expect there will be good reason for using static processing and this should be stated.

We agree with the Reviewer that this methodological detail is important. We have now added Figure S1 to the supplementary material to provide a direct comparison between static and kinematic solutions on both a seasonal time scale (Fig. S1a) and a short-term *time scale (Fig. S1b). The entire dataset was processed in kinematic mode using TRACK software relative to a base station 3 km away; these results were previously used to analyze short-term speed-up events (Togaibekov et al., 2024). We applied an 18-hour Gaussian low-pass filter to the kinematic time series, while the daily static position estimates remained unfiltered (Fig. S1). As shown in the supplementary material, 24-hour static solutions attenuate short-term diurnal cycles (physical signals) but reduce high-frequency noise over seasonal timescales while preserving long-term trends. This information has been also added to the manuscript (L157-L161).*

2. As (nearly) stated on L118 of the manuscript using the standard deviation or RMS of a static site does not give an estimate of the precision of a kinematic site due to biases associated with the assumption that the moving site is static (King, 2004). On the next lines (L124-125) the RMS of a static bedrock-mounted site (ARGB) is used to estimate the velocity error of the moving on-ice sites. I think it needs to be stated that the error estimates based on a static site may be smaller than the true uncertainty of a kinematic site, as errors for example within ambiguity fixing may be caused by motion of the antenna.

We thank the Reviewer for pointing out this nuance. We have added a sentence acknowledging that error estimates derived from the static bedrock site likely underestimate the true uncertainty of the kinematic sites due to motion-induced errors during ambiguity fixing (L169-L171). We have also rewritten this paragraph to improve its clarity based on the reviewer's comments below.

3. Was vertical displacement resulting from sliding along the bed corrected for velocity (i.e. faster sliding results in faster vertical displacement)? Can you expand on the methods for linearly detrending to correct for sliding along the bed? What does this assume about the bedslope (e.g. that it is constant). These methods are central to the paper and conclusions and should be expanded on.

We thank the Reviewer for this important comment. Indeed, we did not correct vertical displacement for the variations due to sliding speed changes. We corrected the vertical displacement time series by removing a linear annual trend calculated from the annual GPS-derived vertical component. This empirical correction accounts for the mean vertical velocity component resulting from both the average bed slope and mean emergent velocities. We acknowledge that variations in horizontal sliding speed over a sloped bed can technically induce seasonal fluctuations in vertical motion. However, given the seasonal variation in horizontal velocity of about 10 to 15 m/yr and the bed slope of 3 degrees, which is low, vertical motion linked to sliding along the bed is expected to be of the order of 0.5 to 0.8 m/yr. This is well below the observed vertical velocity variation amplitude (4 m/yr) and we thus treat these variations as negligible in *this study. The revised manuscript has been clarified regarding this point and the method description about the vertical velocity detrending expanded (L176-L180).*

4. In Section 4.3, it would be good to see more discussion of the measurements of Andrews et al. (2014) in the context of the subglacial hydrological processes inferred from the spatial patterns of ice-bed separation.

We thank the Reviewer for this suggestion. We agree that the measurements from Andrews et al. (2014) provide a critical empirical baseline for our discussion. In the rewritten Section 4.3, we have expanded our discussion to explicitly connect our finding with the observed isolated cavities by Andrews et al. (2014) and other studies in Greenland (L375-L390). Furthermore, we have integrated this into our existing comparison with Moon et al. (2014).

Specific Comments

L7 – I was initially unsure whether it was the vertical velocity or the correlation that was positive or negative. Although it became clear when reading the corresponding section in the main text this should be clarified here. The directions of positive and negative can be defined either way.

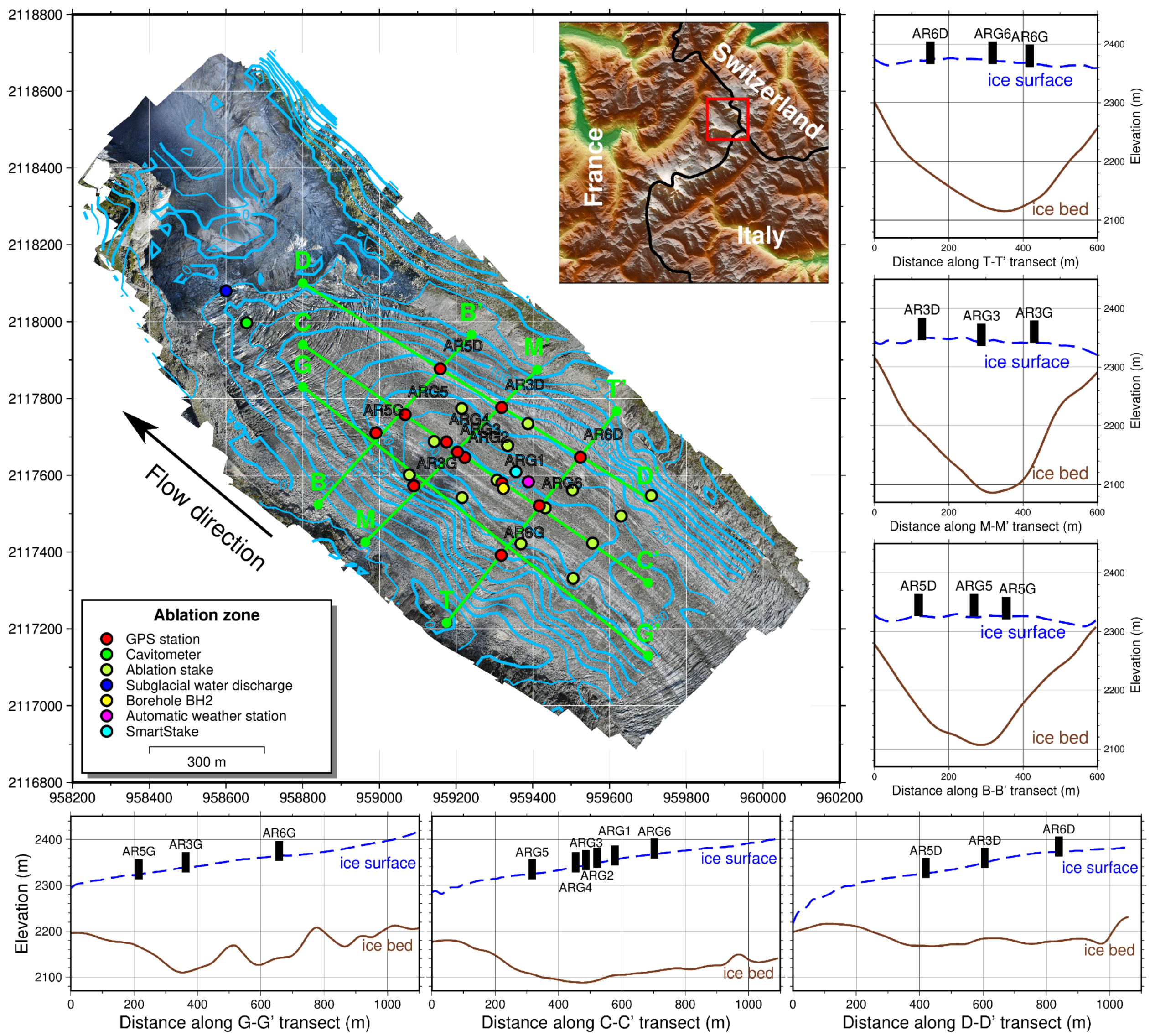
Yes, we agree with the review that this sentence is ambiguous, therefore we have changed "anti-correlated" to "in anti-phase" in L7. Now the sentence reads: "We find that the ice–bed separation velocity is in anti-phase with subglacial water discharge, being positive in winter in the absence of surface melt and negative during summer melt."

L56 – Does Rothlisberger discuss glacier slow down? Adding an additional citation that does is worthwhile.

The reviewer is right, Röthlisberger (1972) does not discuss glacier slowdown. In the revised manuscript, we have moved this reference to the beginning of the sentence where subglacial channels are introduced; and then we now cite Nye (1976), Spring and Hutter (1982), and Tedstone et al. (2015) for glacier slowdown (L32-L35).

L83 - do you specifically mean V-shaped here. Is the Argentiere valley not U-shaped?

Yes, Glacier d'Argentière is known to have more a V-shaped valley. Below, we present bed topography derived from ground-penetrating radar (GPR) across the GPS network that demonstrates a characteristic V-shaped valley profile.



L86 – is this water equivalent melt or ice equivalent? State either way. It is usual to give melt as water equivalent.

Actually, it is neither one. This value of surface melt rate was obtained from the GNSS-IR technique, which measures SMB in meters. Now, we converted this value to m w.e. using compacted snow density and ice density (Togaibekov et al., 2025), resulting in an average surface melt rate of 0.05 m w.e. d⁻¹. We have updated this value in L118.

L103 – it's unclear here that this threshold is an upper, rather than lower, measurement limit. State upper.

We have rewritten it as: "with a maximum measurable discharge of approximately 10 m^3 s^{-1} due to collector capacity limitations" L140

L104 – Expand on the vague phrase “more advanced measurement device” to state precisely how discharge was measured. Make clear here that this new device also allowed lower values to be measured (i.e. during winter, and as stated later in the results on L182).

We have rewritten it as: “This limitation was eliminated in summer 2020 after an upgrade to a high-precision discharge monitoring system. The new installation utilizes laser altimetry to measure water surface elevation within a stable, concrete-lined conduit, with a rating curve established using dye-tracing experiments. This system also enables the measurement of low discharge values below 1 $m^3 s^{-1}$, which is important for capturing minimum flow rates during the winter periods.” L142-L147

L113 – Revise to “with double differencing and the ionosphere-free linear combination (LC; Bock et al., 1986) using the geodetic software GAMIT/GLOBK (Herring et al., 2018)”. Note the acronym LC is not used so could be omitted.

We have incorporated all these suggestions, thank you for the reformulation (L154-L156).

L115 – Move ‘13’ to before well-determined. Strictly speaking its not stated what is well-determined; can be assumed that this is the station position but this could be stated.

We now use “13 well-constrained” stations instead of “well-determined” in L156 to better reflect the high precision of the IGS station coordinates.

L121 – Add something along the lines of “Instead we estimate the precision by …” to make it clearer.

We have rearranged the structure of the paragraph so the reason for using the ARGB station is clearer (L160-L165).

L130 – omit ‘bed-separation-induced’ as interpretation is better suited to the results and no evidence for this has yet been presented.

Done - L183

L146 – You switch between transmissivity and conductivity and while some readers may know that $T = kh$ others might be able to follow the equations easier if this is defined, or if the term conductivity is used throughout.

We have replaced all instances of transmissivity to conductivity.

L173 – this paragraph starts and focusses on the difference between sliding velocity and surface velocity and they do differ, but there are also periods of agreement that are worth highlighting, perhaps even before focussing on the differences.

While the overall magnitude and timing of the velocities differ significantly, we agree it is important to first acknowledge their shared seasonal behavior. We have revised the opening of this paragraph to explicitly state that both datasets exhibit seasonal variations. It now reads: "Both the sliding velocity measured by the cavitometer at the terminus of the glacier and the GPS-recorded surface velocity several hundred meters upglacier (Figure 1) exhibit distinct seasonal variations. Both independent measurements capture a series of synchronous short-term speed-up events and a similar deceleration trend during the late melt season from July through November. Despite this shared seasonality, they differ notably in overall magnitude and specific timing (Fig. 2b)." L239-L243

L178 – frame this inference regarding the difference in basal shear stress: "with a different amplitude which previous studies (citation) have explained as due to higher basal shear stress". Worth also commenting here on the difference in basal slope angle and surface velocity between the cavitometer and the GPS. It's worth emphasising these differences as they are important to the interpretation

Following the Reviewer's suggestion, we have adjusted the phrasing to state that the difference in amplitude is attributed to higher basal shear stress at the cavitometer location, citing Gilbert et al. (2022). We also have added a sentence highlighting the physical differences between the two sites: "It is important to note the differences in the physical settings of these instruments: the cavitometer is located in an icefall area characterized by a significantly steeper basal slope angle and higher mean surface velocities compared to the flatter, slower-moving region monitored by the GPS network." L247-L250

L182 – modify this sentence after including these methods details in the methods.

We have made a reference to Section 2.2 in L255 where the upgrade of water discharge equipment was mentioned (please see our answer above)

Fig. 2 – shading the melt seasons would make the plot easier to interpret. A horizontal line at 0 should be added for the vertical velocity axis.

Done

L192 – Start sentence with ‘Although’ and change ‘but’ to ‘at’ and ‘the one’ to ‘those’. Summer is by definition a period so ‘period’ can be omitted. Make question plural. Finally expand on how the model and GPS measurements are different.

We have revised the paragraph to incorporate all the suggested grammatical and stylistic changes. We have expanded on the differences by adding these sentences: “Specifically, the GPS records show an earlier peak in bed separation coinciding with the spring event, followed by continuous subsidence throughout the melt season. In contrast, the model predicts a delayed peak and a more sustained level of uplift.” L269-L271

L197 – Expand this first sentence and show evidence for the second point.

We have rewritten this section and divided it into two paragraphs for the two separate points as the Reviewer indicated. We now provide a more quantitative description for the relationship between winter uplift and speed-up (L??). To prove the second point, we have added a sentence that explicitly points out summer subsidence while the glacier maintains high horizontal velocity: “This is evident in Figure 3c, where the summer months (July to August) exhibit a steep vertical drop in the scatter plot: bed separation decreases precipitously from approximately 0.6 m to 0.2 m, yet horizontal velocity remains high between 45 and 55 m a^{-1}.” L287-L289

L200 – you state two equations are used to plot a single line on Figure 3b. Expand on how these equations were combined.

We agree that the derivation of the dark gray line in Figure 3b required more explanation. We have now included a new equation (Eq. 5) which represents the analytical combination of the basal sliding velocity and water sheet thickness (Eq. 1 and Eq. 3, respectively) in L214:

$$h = h_r * (1 - (\tau_b^{\ m} A_s/u_b))^{p_1}$$

L206 – mention stake measurements in methods.

Indeed, we did not mention the stake measurements in methods. We have now added this sentence: “We also incorporate a network of 27 ablation stakes from Vincent et al. (2022). The stakes were measured 5–8 times per year between 2018 and 2020 with an intrinsic accuracy of +-0.01 m.” L136-L138

L220 and L274 – change to ‘Figs’ with no full stop as it’s a contraction.

Done

Figure 4 – include methods used to create this figure (i.e. what type of interpolation was used). HVC needs to be defined in the figure caption.

We have added these explanatory sentences in Figs. 4 and 5: "Contour maps were generated using the surface module of the Generic Mapping Tools (GMT), which uses a continuous curvature spline in tension to interpolate the data (Wessel et al., 2019). HVC denotes the horizontal velocity change, defined as the difference between the winter and summer extrema within the horizontal velocity time series."

L223 – I see an increase in HVC gradient to the left bank which is the opposite to that stated in the text.

We meant a decreasing value of HVC. We reformulated the sentence in the revised manuscript by: "We also observe a decreasing value toward the left bank in both uplift and HVC." L309

L231 – Just to highlight that this is a key point regarding HVC and uplift in winter that is worth emphasising in the abstract and conclusions.

We agree that the relationship between the horizontal velocity change (HVC) and winter uplift is a key finding of this study, therefore we have highlighted this point in the Abstract ("A key finding is that changes in horizontal velocity are well correlated …") and the Conclusions ("A key finding of this study is that the observed surface uplift provides strong evidence of variations in bed separation …").

L245 – Yes, depth-homogenous vertical strain rate may not be a valid assumption. The other potential problem here is that crevassing violates the continuity equation.

We agree that crevassing can also play a crucial role, therefore we have mentioned it in the revised manuscript: "This more likely indicates that the assumption of depth-homogeneous strain rate is not valid (Sugiyama et al., 2004) rather than the estimated surface strain rate is inaccurate. Furthermore, the presence of crevassing may violate the continuity equation in this region; however, future measurements of internal vertical strain in boreholes are needed to observationally confirm this" L333-L335

L259 – the conclusion regarding the requirement for running water here seems tenuous and would be best framed as speculation.

We now start this sentence with, 'We speculate...'" in L347

L307 – Hoffmann et al. (2016) is a modelling paper so although relevant here it is not quite the right reference. Andrews et al. (2014) could be cited as well or instead of Hoffmann et al. (2016).

Indeed, Andrews et al. (2014) is a more appropriate reference, so now we have replaced it as suggested.

L319 – consider introducing Moon's Types II and III in the introduction or expand on what is meant slightly here. Only the informed reader will follow this.

We agree that the study of Moon et al. (2014) is essential that provides context on ice dynamics in Greenland. Therefore, we have added a paragraph to the Introduction presenting the three distinct types of seasonal velocity behavior (L46-L56). Furthermore, we have expanded Section 4.3 in the Discussion to provide a direct comparison between our specific observations and the findings of their study, highlighting where our results align with these established seasonal patterns.

Technical Corrections

L51 – Reorder citations so 2011 citation comes before 2016 citation.

This sentence is no longer present in the revised manuscript.

L57 – add 'usually' before 'cannot be measured'.

This sentence is no longer present in the revised manuscript.

L89, L113, L115, – change 'are' to 'were' to use past tense.

We have used past tense as suggested

L102 – move bracketed text later in the sentence where you describe the location of the gauge.

We thank the Reviewer, the bracketed text has been moved later in the sentence where its place

L129 – write out '… Figure 2'.

Done: Figure instead of Fig.

L135 – quantify number of decades

We have added "three decades" in L188

L142 – change to ‘the distance’

We have added the missing article “the” L195

L143 – here and elsewhere fix the brackets for in text citations.

We have corrected the citation brackets in five instances throughout this section.

L204 – replace ‘right after’ with ‘immediately after’ and give a figure reference here.

We thank for these suggestions which we have incorporated in L287

L205 – state which year or state in both years.

Done: “... which usually encompass most of the summer months in 2020 and 2021…”

L205 – change to “cavities shrunk when surface velocity was constant”. Note change to past tense.

Thank you, we have implemented this change as suggested in L290

L256 – revise ‘is required the transition’

The sentence now reads: “Summer subsidence concomitant with the onset of melt suggests that cavities connect in response to meltwater input; this indicates that a threshold of extra subglacial discharge is required to drive the transition to, and maintenance of, a connected cavity network.” L346

L273 – delete bracketing comma.

We have deleted

L296 – delete ‘the’ and specify ‘surface ice motion’.

Done: “We monitored surface ice motions in …” in L397

L316 – fix citation brackets.

We have put the citation in brackets

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Reviewer 2:

This manuscript presents data gathered at Glacier d'Argentiere over a three year period in order to explore relationships between uplift, water discharge, and horizontal velocity fluctuations. The work follows similar studies at the same study site, now bringing together simultaneous observations of uplift and discharge.

We thank the reviewer for the careful reading and corrections, which improved the quality of the manuscript.

General Comments

Some of the data is compared to a numerical model for sliding speed; the model description is not complete without a description of what the model inputs are, and associated assumptions that go into the model. Based on the description in Gilbert et al 2022, I assume the results presented here are also forced with the measured discharge, but I couldn't find a mention of this in the present paper. If so, then perhaps it could be that the assumption of constant hydraulic pressure gradient (required to link discharge to effective pressure - Gilbert2022) is what leads to the model-observations discrepancy in spring, noted in section 3.1.3 but not discussed in depth.

The modeling results presented in this study were produced using the fully coupled model described by Gilbert et al. (2022). This means that the only input to the model is local surface melt, which is computed from the air temperature and degree-day model. Therefore, this study does not make any assumptions about the hydraulic pressure gradient. The description of the model and its forcing data have been extended and clarified in the revised manuscript (L215-L219). We also now discuss the model-observations discrepancy in more detail in Section 3.1.3.

In Section 3.2.2 it's a bit unclear what HVC is defined as. Is it the difference between the current season and the season beforehand? So summer HVC is summer minus winter before, winter HVC is winter minus summer before? (Similarly for winter uplift).

We agree that the description of the Horizontal Velocity Change (HVC) required further clarification. The HVC is calculated based on seasonal extrema (the 'spring peak' and 'autumn minimum') to capture the full magnitude of seasonal acceleration and deceleration:

- Winter HVC (Acceleration): Calculated as the difference between the spring peak velocity (typically in May) and the minimum velocity from the previous autumn (typically October–November).

- Summer HVC (Deceleration): Calculated as the difference between the autumn minimum velocity (typically October–November) and the preceding spring peak (typically May) of the same calendar year.

We have modified the text to better define HVC: "We compare the spatial patterns of winter uplift and summer subsidence with the average horizontal velocity and the horizontal velocity change (HVC). Here, HVC is defined as the magnitude of velocity change between the seasonal extrema: the spring peak (typically in May) and the autumn minimum (typically in October–November)." L304-L306

It's a bit concerning that strain-rate derived thinning/thickening is predicted to be an order of magnitude larger than uplift/subsidence. Can the sign of uplift even be known given this uncertainty? What are the uncertainties in strain given the number of GPS sites?

We agree with the reviewer on this point. However, these differences do not arise from uncertainty in the observed surface strain rate, but rather from its vertical integration. The strain-rate derived thinning/thickening is estimated here by assuming that the surface strain-rate is constant through depth. This assumption is clearly inaccurate, which explains the order-of-magnitude difference. In the manuscript, we argue that if strain rate were to explain the observed vertical movement, then a strain rate pattern similar to the observed one should be observed at the surface. Providing a value of the vertically integrated strain rate was only to highlight that it leads to unrealistic values that are unable to explain the observations in terms of amplitude. We revised the manuscript to better explain our rationale for using observed surface strain rate to discard strain rate as an explanation for the observed uplift (L332-L334).

The cavitometer is in a drained cavity by necessity; therefore there is no basal water pressure acting in this region of the glacier. Approximately how far from the terminus do you expect "basal water pressure" to be a well-defined property?

The reviewer is correct, this cavity is atmospheric and water-free. However, the reason why it is so essential is that there happens to be, locally, a sharp break in bed slope that facilitates ice to detach from the bed without requiring further help from the basal water pressure pushing the ice up. We believe such a local setting is rather unusual, such that *other cavities located in the surrounding tens to hundreds of meters may be filled up by water and experiencing changes in basal water pressure, explaining the observed changes in sliding speed (Gimbert et al., 2021). We have added several sentences in Section 2.2 to clarify these points (L134-L136).*

Specific Comments

L7: separation velocity is strange wording - rate of uplift perhaps better

We agree with the Reviewer so we have changed it to "rate of uplift" as suggested in L7

L18: put into a geographical context - specifically alpine glaciers? or also Greenland - but not Antarctica!

We have substantially rewritten the Introduction to provide a clearer geographical and physical context. While the fundamental mechanics of ice-bed separation discussed are applicable to various glacial environments, we now explicitly distinguish between the dynamics of Alpine glaciers and the Greenland Ice Sheet in the revised manuscript. We agree with the reviewer that Antarctica is quite a different setting, thus we have not included any reference to it.

L147: h acts as an effective water sheet thickness averaging over some region

The Reviewer is correct. We have clarified in the text that h represents an effective water sheet thickness averaged over the study area (L202-L203).

L148 (or L153): p_1 and p_2 are fitting parameters

We have updated "exponent" to "fitting parameters" as suggested in L208.

L191: include references to newer hydrology models (GlADS, SHAKTI)?

Yes we agree that the references are here not the most appropriate. This statement is actually better illustrated using GlaDS in Downs et al. (2018):

Downs, J. Z., Johnson, J. V., Harper, J. T., Meierbachtol, T., & Werder, M. A. (2018). Dynamic hydraulic conductivity reconciles mismatch between modeled and observed winter subglacial water pressure. Journal of Geophysical Research: Earth Surface, 123, 818–836. https://doi.org/10.1002/2017JF004522

Fig2b: not a colorblind-friendly plot

We thank the reviewer for this important suggestion. We have revised Figure 2b using a colorblind-friendly palette.

Fig3bc: use same axis limits for consistency

We have considered the reviewer's suggestion to use identical axis limits for Figures 3b and 3c. However, we have chosen to keep the current axes because the scales are already identical across both plots to allow for a direct comparison of the magnitudes.

L257: I am not sure I understand the importance of this distinction. Water pressure gradients = flowing water, no?

Here, we wanted to distinguish between cavities that form mechanically from channel-like conduits that open through melting. In the revised manuscript, we clarified the sentence by stating: " This suggests that, at this site, cavities cannot connect mechanically (i.e., through growth driven by increased water pressure and sliding speed) but instead require channel-like conduits that open through melting to establish connections." L349

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Reviewer 3:

Overview

This manuscript presents observations collected continuously over a few consecutive years at a mountain glacier, combined with modeling, to try to understand the spatial and temporal patterns in ice velocity and subglacial hydrology.

In general, the paper is well written with nice figures. Please see below for general and specific comments. With minor revisions to further improve clarity and strengthen the paper, I feel this work will make an excellent contribution to the glaciological literature.

We appreciate the Reviewer's positive assessment of our work and his comments, which have helped clarify the manuscript.

General Comments

1. First, I will say that this is a valuable dataset of observations to understand the system that can serve as an excellent modeling target. It is especially interesting to see the documented observations of the winter hydrological influence as measured through the persistent winter discharge (lines 181-182) and the evidence of winter subglacial water pressure controlling winter velocity (Fig. 6), also demonstrating that summer behavior is more complicated to predict.

We thank the Reviewer for his positive feedback of our dataset as a valuable modeling target and for highlighting the importance of our observations regarding winter hydrological controls on glacial velocity.

2. I have some questions about the nature of surface meltwater inputs to the bed at this glacier: Can you provide more details about location, timing, and magnitude of water inputs that likely reach the bed? The right bank of the surface is described as being heavily crevassed (line 225), but otherwise I don't see any description of surface water features. Are there moulins? Supraglacial ponds? How do these features change in different regions of the glacier, and over time (seasonally and

interannually)? Without measurements to constrain meltwater inputs beyond the discharge measured downstream, even a qualitative description of this possible distribution would be helpful, to give the reader a sense of where, when, and how much meltwater might drain from the surface to reach the bed.

We agree with the Reviewer that providing a qualitative description of the nature of surface meltwater inputs to the bed of the glacier is important information. While we do not have a direct inventory of surface water features, we have observed numerous small moulins across the study area, and as noted in the manuscript, the right bank is heavily crevassed, along with the terminus as can be seen in Fig. 1. Based on our fieldworks, we can confidently say that supraglacial streams do not remain on the surface for significant distances; rather, they percolate englacially through these crevasses and moulins. The absence of long-lived supraglacial streams support our model's primary assumption: a direct and relatively instantaneous connection between surface meltwater production and the subglacial system. We constrain the timing and magnitude of meltwater inputs using a degree-day model based on air temperature. To clarify this for the reader, we have added a qualitative description of these drainage features to Section 2.4. (L215-L219)

3. In Eqs. 3 and 4, how are the values chosen for average bed bump height, intrinsic conductivity, and the exponents? This would be helpful to explain and justify the choices, as well as discuss the implications and limitations of these assumptions.

We thank the Reviewer for pointing this out. The values for the average bed bump height (h_r), intrinsic conductivity (k_0), and the associated exponents (p_1 and p_2) were initially selected to best fit the high-resolution direct measurements of sliding velocities from the subglacial cavitometer site (see figure S2 in Supplementary Information of Gilbert et al. (2022)). However, there is a trade-off between bump height and intrinsic conductivity, since both parameters play a similar role in the sheet discharge formulation. This is why the height of the bed bump has been fixed at the realistic value of one meter. We have added a brief justification for these choices and a discussion of the associated assumptions in Section 2.4. (L208-L213)

4. Discussion and interpretation of subglacial geometry refers to cavity size and cavitation rates, yet the summer velocity and pressure observations suggest that a highly connected or channelized drainage system likely develops. Do you have any observations of channelization? What does the outflow at the terminus look like?

We thank the Reviewer for this insightful question. Previous studies at Glacier d'Argentière using seismic (Nanni et al., 2021) and borehole (Roldan-Blasco et al., 2024) observations have documented the existence of a single large efficient channel that forms in the center of the glacier. We interpret that during periods of high summer *melt, water likely drains from the isolated or weakly connected cavities into this low pressure primary channel. We have now explicitly written the role of channelization and its relationship to isolated cavity drainage in the Discussion (Section 4.1) in L349.*

5. I think it would be useful to add a "Limitations" section to the paper. This is currently missing, but would be a good place to discuss some of the assumptions involved in the observations and modeling methods, as well as opportunities for further research.

We thank the Reviewer for this suggestion. We agree that acknowledging the assumptions and limitations of the study is essential for a balanced interpretation of the results. Rather than creating a standalone 'Limitations' section, we have chosen to integrate these discussions directly within the relevant portions of the Methods (Section 2) and Discussion (Section 4). This way the limitations are contextually linked to the specific observations and modeling steps they affect.

6. I am interested to see the documentation of short-term velocity pulses in the summer (line 311). More discussion or interpretation about what drives these pressure variations would be insightful. For example, can you connect them with higher temperature or rainfall events that contribute large amounts of water to the system?

We agree with the Reviewer that the drivers of these short-term velocity pulses play a central role in summer subglacial dynamics. These high-frequency (mostly diurnal) variations are often triggered by daily meltwater input or rainfall events. We have now added Figure S1 to demonstrate the correlation between diurnal velocity cycles and meltwater input. Additionally, a detailed analysis specifically of these rainfall events is provided in a separate study (Togaibekov et al.,2024). Then, Section 4.2 in the Discussion interprets the role of these short-term pressure pulses and how they relate to transient surface water inputs.

7. Finally, I am happy to see that you highlight the difference in seasonal behavior as observed at different parts of the glacier with different local characteristics. This shows that seasonal velocity and hydrology dynamics are more nuanced than how they are sometimes approached with different schemes of classification. This also raises the question of whether it is important to resolve these finer details across spatial and temporal scales, or not. That probably depends on the science question, but is something to consider, and this study nicely demonstrates that you can come to different conclusions about the nature of a glacier's subglacial system depending on when and where you look.

We thank the Reviewer for these concluding remarks. We agree that the spatial heterogeneity we documented highlights the glacier-wide behavior is indeed complex.

We hope that this work resolves these details and identifies the specific physical mechanisms, such as local morphology and cavity connectivity, that will ultimately determine how glacier dynamics respond to a warming climate.

Specific Comments

Lines 44-45: Remove either "although" or "yet" from this sentence

We thank the reviewer for identifying this error; however, the sentence has been removed during the revision of the manuscript.

Line 100-101: About how thick is the glacier at each location? This would be helpful to mention here.

Indeed, this information is very important. We have added the approximate ice thickness for each location in the revised manuscript: the glacier is approximately 250 m thick at the GNSS profile locations and approximately 55 m thick at the cavitometer site. The sentence reads: "The bed topography at these locations is distinct: the glacier is steep (20%) and thin (55 m) at the cavitometer location, whereas it is flatter (10%) and thicker (250 m) at the GPS network site (Gimbert et al., 2021a)" L132-L135

Fig. 1: You might consider adding an inset with a map showing the location of the glacier in a broader geographic area for reference, for those not familiar with the location.

We thank the Reviewer for this suggestion. We explored adding a broader geographic inset map; however, we found that it significantly crowded the figure and reduced the readability of the high-resolution GNSS station labels and local topography, which we think are more important. However, we have ensured that the glacier's location and coordinates are clearly stated in the text.

Equations 3 and 4: How do you choose the average bed bump height, intrinsic conductivity, and the exponents used in these equations? See General Comment 3.

As it was addressed above, the values for the average bed bump height (h_r), intrinsic conductivity (k_0), and the associated exponents (p_1 and p_2) were initially selected to fit the high-resolution direct measurements from the subglacial cavitometer site (Gilbert et al., 2022). L208-L213

Line 182: I'm not sure that "circulates" is the right choice of word here, as it might imply water moving around without flowing out, which is not the point being made with the observation of winter discharge. Perhaps it would be more accurate to say the water "moves" or "flows" or "travels" beneath the glacier year-round.

We agree with the Reviewer that the word 'circulates' is not appropriate in this context; we have replaced it with 'flows' as suggested in L254.

Figure 2 is great to see all the measurements together.

Thank you, we have further improved the figure by making it colorblind-friendly

Fig. 2b – interesting that the water pressure increases so much more significantly over the first instrumented winter (2019-2020) than the second (2020-2021), during which it remains more constant, also the big drop event during the first winter (any explanation?). How is water pressure measured?

We thank the Reviewer's attention to these details. Regarding the second year of the observations, we interpret the relatively constant and 'flat' pressure curve as that the borehole sensor became hydraulically disconnected from the active subglacial drainage network sometime in late summer 2020. This information is provided in the corresponding text in Section 2.2. We do not really know the nature of this sudden pressure drop in February 2020. It could be either a true physical signal or an instrumental malfunction, but the overall increasing seasonal trend is clear which is the main focus of this particular paper. Water pressure is measured in a borehole (yellow star in Fig. 1a) next to the GPS site ARG1 using a piezometer positioned 95 m above the bed. To obtain the basal water pressure, we add a constant pressure equivalent to the water column height of 95 m.

Fig. 2c – difference in vertical velocity and vertical GPS displacement. How is vertical velocity measured? Is that at the icefall with the cavitometer? It would be good to clarify in the caption here for easy reference.

We apologize for the lack of clarity regarding the labeling of our observables. We have updated the legend to explicitly include 'GPS separation velocity' and have expanded Section 2.3 to clarify how exactly we calculated bed separation and its rate.

Line 319: Rather than referring to "types II and III" seasonal patterns, you might consider briefly describing each pattern in a few words, for readers not familiar with that classification (or for readers who always forget which is which)

We thank the reviewer for pointing this out. We agree that the classification by Moon et al. (2014) was not introduced; we have now added a paragraph in the Introduction to define these categories and provide the necessary context for our results (L46-L56).