Response to reviewers

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

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.

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. 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.

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). 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.

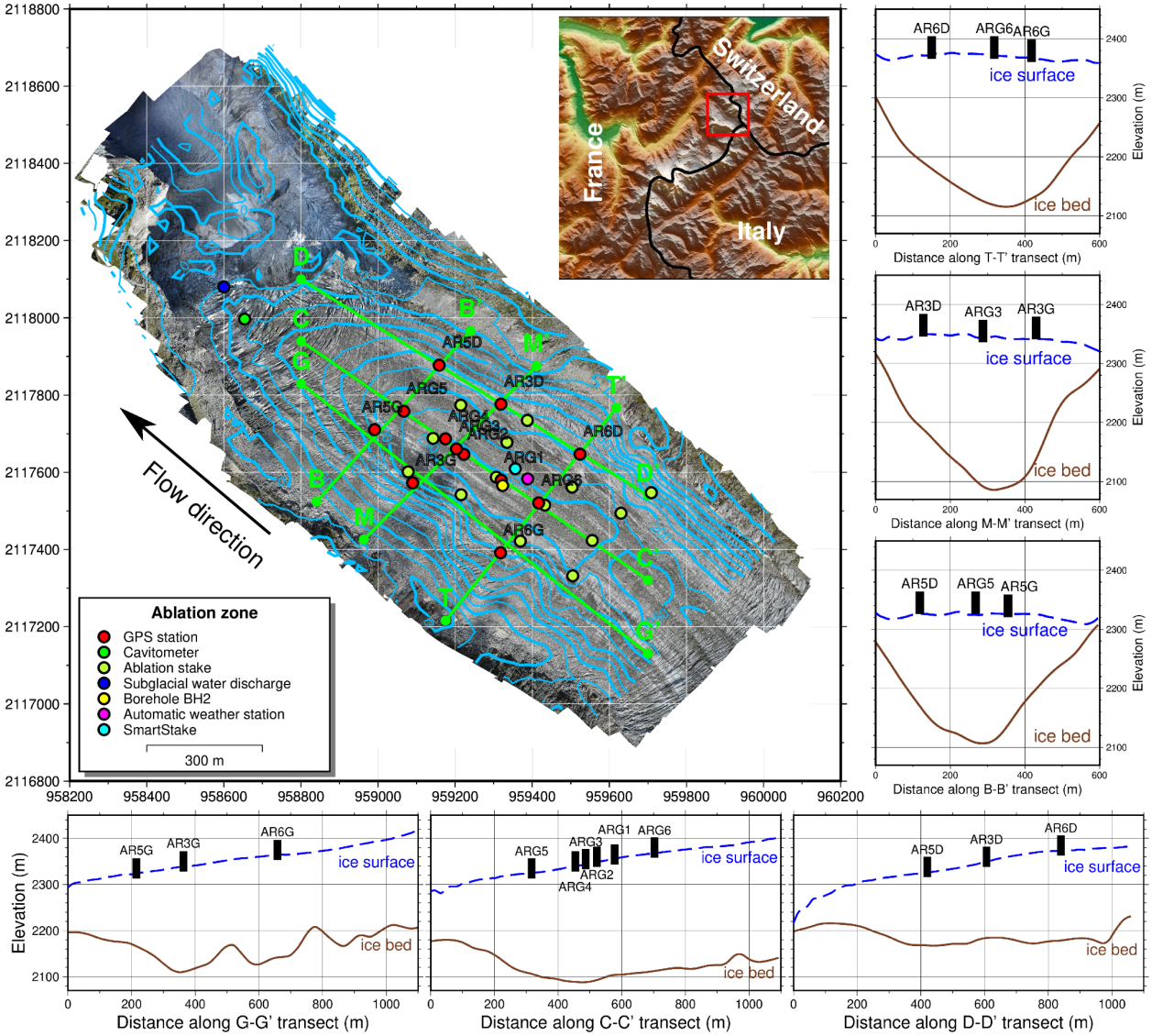
Yes, we agree with the review that this sentence is ambiguous, therefore we have changed "anti-correlated" to "in anti-phase". Now the sentence reads: "We find that the ice–bed separation velocity is in anti-phase with subglacial water discharge, being positive in winter in the absence of surface melt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

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.

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"

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.”

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.

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” 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.

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

Done

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)."

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."

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

We have made a reference to Section 2.2 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."

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}."

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):

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

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."

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."

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"

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...'"

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. 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"

L142 – change to 'the distance'

We have added the missing article

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

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

L256 – revise ‘is required the transition’

The sentence now reads: “Vertical 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.”

L273 – delete bracketing comma.

We have deleted

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

Done: “We monitored surface ice motions in …”

L316 – fix citation brackets.

We have put the citation in brackets