

Response to reviewers

Reviewer comments in black - *Answers in blue italic*

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. 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).”

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.

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.

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

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.

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

We have updated “exponent” to “fitting parameters” as suggested.

L191: include references to newer hydrology models (GIADS, 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."