Dear Adrian Luckman,

Thank you very much for your detailed review and constructive comments and suggestions, which we believe have improved the manuscript. Please find below your original comments in black text and our responses in blue (quotes from the updated manuscript are in *blue italics*).

The authors use Sentinel-1 ice velocity products, DinSAR-derived grounding lines, and a variety of surface elevation data sources, to understand build up to the next surge of two important glaciers in Greenland, and document evidence of subglacial drainage events and the dynamic response to them.

This paper brings up to date the work by Mouginot et al., (2018) and provides a small advance in understanding the quiescent phase and predicting the likely year of the next surge. It is well written and very well illustrated and, although it could be considered incremental, I believe it is worthy of publication as a Brief Communication in the Cryosphere subject to some revisions:

GENERAL

1) The paper focusses on SAR and InSAR methods and observations, demonstrating very well developed data analysis skills and figure-making. In contrast, the glaciological discussion (page 6) is rather brief and pays no attention to the literature (surge-related, glacial hydrology-related, or otherwise), which is a potential missed opportunity for influencing the topic and picking up citations. There could be lots to discuss here about where the water goes (ground-water? - this is a growing topic), whether the glaciers are frozen to their beds (see lots of papers about surge initiation, and subglacial water outbursts), and what actually triggers a surge. If the authors do not have the appetite for a literature review, could they co-opt someone (e.g. a well-known surge specialist), to add this extra bit of informed (and referenced) discussion? If not, it may be better to couch this section purely in terms of observations and leave out the under-developed glaciology.

We acknowledge that the discussion of our findings in relation to surge theory was quite brief and lacked context and references. We have revised this section, providing some more context between our findings and the existing literature. The final part of the Discussion now reads (starting at line 158):

"*Using interferometric satellite radar measurements from the past decade, we find evidence of multiple supraglacial and ice-dammed lake drainages, showing that high inputs of water are regularly provided to the subglacial environment. The drainage events all occur outside the melt season, when we would generally expect a less efficient subglacial drainage system and thus a greater increase in basal water pressure, but lead only to transient flow accelerations over timescales of weeks to months. Within the general theory of glacier surges, meltwater inputs to, and subsequent changes in, the subglacial drainage system have frequently been linked to surge initiation (Kamb et al., 1985; Lingle and Fatland, 2003; Dunse et al., 2015; Haga et al., 2020). In a recently proposed generalized surge model based on enthalpy balance, an influx of water to the subglacial system is associated with an increase in enthalpy (Benn et al., 2019, 2022). While the rapid drainage events presented here clearly did not initiate a surge for either glacier, it is possible that similar events may contribute to future surge initiation, once the pre-surge configuration, and thus a state of* *mass/enthalpy imbalance, has been reached. Alternatively, the external forcing from these episodic, transient inputs of meltwater to the glacier bed may play a lesser role in initiating surges of Storstrømmen and L. Bistrup, which instead may be controlled by a more gradual evolution in basal water pressure and subglacial drainage configuration.*

A common theory is that surge initiation occurs once enough basal water is accumulated to raise water pressure above ice overburden pressure, enhancing basal motion through sliding and commencing a velocity-frictional heating feedback (Clarke, 1976; Benn et al., 2019). Our observations indicate downstream propagation of water through the subglacial system over timescales of weeks to months, however, it is unclear how much (if any) of this water is stored in the subglacial system. We do note that for several of the identified drainage events, downstream propagation of subglacial water appeared to cease 25 km upstream of the Storstrømmen grounding line (Figs. 3a-c, Fig. S6), suggesting that the drained water volume might not have been fully evacuated. Investigating similar surface-to-bed drainage events (including their frequency) in the time up to and during the next Storstrømmen surge may reveal detailed changes in the subglacial drainage system (in the form of spatial uplift/subsidence patterns - see Figs. S2-S3 - and the temporal propagation of the dynamic response). Continued close monitoring of hydrology-dynamical effects could then help establish the impact of supra- and subglacial drainage events on the surge cycles of Storstrømmen, L. Bistrup Bræ, and other surge-type glaciers."

While we have made a concerted effort to improve the context and interpretations of our observations, we also recognize that some aspects may have been left out. Ultimately, our goals with this paper are to 1) extend the time series of Mouginot et al. (2018) to update the timing prediction of pre-surge configuration, and 2) to present and share observations of rapid drainage events and highlight the potential of using similar measurements (particularly DInSAR) to study the hydrology-dynamic effects of melt drainage events on surge initiation (and the surge cycle in general).

MINOR

line 50: "Contrary to all other"→ "In contrast to non-surge-type"
Changed to "*In contrast to non-surge-type…*" (line 32).

line 51: "decrease with decreasing distance to the ice front"→ "decrease up-glacier"
Changed to "*...flow speeds decrease up-glacier*" (line 33).

section 2.1: I got very confused over all of the time periods, so I recommend revising this section. Where do the 24-day averaged velocities come from and why are these used to create the quarterly mosaics? Why not go from the natural periodicity (which is unacknowledged as 6-days for part of the time-period) direct to 3-months?
The PROMICE velocity product is distributed as 24-day averaged velocity mosaics (with a 12-day overlap between subsequent mosaics). These 24-day mosaics are generated from individual 6- and 12-day image pairs - we now mention this in line 50 ("*...utilizing all available 6- and 12-day image pairs*"). The additional averaging, down to 3 months, was done for two reasons: 1) to provide

additional noise reduction, and 2) to be able to illustrate a full 2D time series of velocity variation through the entire 2016-2023 period in a somewhat reasonably sized, digestible figure. Ultimately, the goal of including this data/figure is to document that ice velocity in this region is relatively stable, with the exceptions of the summer months (please see our response to your "line 100" comment below) and a few other instances, which we later link to drainage events.

Line 58: "displacement anomalies". This doesn't seem like a suitable term. Anomalies are normally related to long timeseries. I think the term differences or variations is more suitable here.
We rephrased "displacement anomalies" to "displacement changes".

Line 71: Remove "roughly". I'm sure you were as careful as you could be.
Done.

Line 83 (and later): The term "accumulation" here is used to describe vertical uplift, which I guess is the net result of ice inflow, surface snow input and minimal surface melt. So it is not exactly wrong, but might be misinterpreted as simply the surface snow input. I suggest you find another way of expressing this.
We agree that the use of "accumulation" and "ablation" was confusing, as we are indeed referring to surface elevation change. Consequently, we have replaced all instances of "accumulation/ablation zone" with either "upper/lower zone" or "thickening/thinning zone" (throughout the text as well as in Figure 1c).

Line 100: Here I think you are referring to the highly noisy velocity maps for each jun-aug period in figure 2. These appear to be too noisy to make sense, and the apparent (but clearly wrong) signals dwarf those that you are drawing attention to (the non-summer speed-ups). I suggest you either properly filter these data (I am surprised that the PROMISE processing chain has allowed these through), or just remove the jun-aug panels and say that the summer data is not reliable because of surface melt.
We recognize that the Jun-Aug measurements are indeed very noisy, and concluding a "*summer speed-up on the order of 40 m/y*" is putting too much confidence in these measurements. That being said, we find it likely that the measurements (although admittedly noisy), likely do measure a real speed-up signal (at least locally), based on the fact that a re-occurring summer speed-up has been measured in the past (Vijay et al., 2019). As for applying additional filtering or omitting the panels from the Figure, we prefer to show the measurements "as is", as the filtering/culling procedures of the PROMICE product are well described in the Solgaard et al. reference, and then caution the reader that the summer measurements are less reliable (likely due to a heavy influence of surface melt).
We have re-formulated the sentence as (lines 105-107): "*An apparent re-occurring speed-up is observed during summer, however, as measurement noise drastically increases during this period (likely due to enhanced surface melt), the confidence in this signal is reduced.*"

Lines 140-148: Have a rethink of the order of explanation here. It is confusing that you talk about the 2027 date, then the 2040 date, then mention them both again later. This could all be made much clearer with reference to each date, its source, and implication only once.
We have rephrased this paragraph. We want to convey the year in which we expect each of the elevation/grounding line parameters to reach their pre-surge configuration (2027 for the grounding

line and lower zone elevation and 2040 for the upper zone elevation). At the same time, we also wish to point out that, since the grounding line retreat and ice thinning in the lower reservoir seemingly persist at constant rates, the total mass imbalance between the upper and lower zones should continually increase. Therefore, we anticipate that surge initiation is more likely in the earlier parts of the 2027-2040 time frame. The whole paragraph now reads as follows (starting at line 148):

"*Compared to Mouginot et al. (2018), we thus predict that the Storstrømmen grounding line location and lower zone elevation will meet pre-surge (1978) conditions around year 2027 (agreeing well with previous estimates), while mass build-up in the upper reservoir will likely occur later (around year 2040 vs. the previous estimate of 2029-2030), assuming a continuation of current trends (Figure 1c). A presumed requirement for surge initiation is an ice mass imbalance between the upper and lower reservoirs of Storstrømmen (Reeh et al., 1994; Mouginot et al., 2018). Although thickening in the upper reservoir has recently decreased, thinning in the lower zone and retreat of the grounding line appear to persist at steady rates, resulting in a continuous increase in driving stress. Thus, while the precise pre-surge conditions of 1978 are unlikely to be fully reestablished by 2027, surge initiation is anticipated to be more probable in the earlier part of the 2027–2040 time frame. Inferring the timing of a coming surge would provide a valuable opportunity for acquiring in-situ and remote observations in the years up to, during, and after a glacier surge.*"

Line 149: "decrease in back pressure". This would be better expressed as "increase in driving stress"
Changed to "*continuous increase in driving stress…*" (line 154).

As a final note, we discovered a minor error in the date labels of Figures 3, S6, and S7. The acquisition dates of the second image pair in the double-difference interferograms were shifted by 6 days, but have now all been corrected. The error arose from the parsing of dates from the interferogram filenames (in the script used to generate the plots).

References

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