**Response to reviewer 2:** We thank the reviewer for their very positive words and constructive suggestions. We agree with the suggestion regarding the velocity anomaly fields and will present the data in that way in the revised paper. We expand this point below within the Specific Comments section.

In their TCD manuscript "A decade of winter supraglacial lake drainage across Northeast Greenland using C-band SAR", Dean et al. present a database of winter supraglacial lake drainage events in Northeast Greenland derived from SAR observations. They combine data from both Sentinel-1 and the Radarsat Constellation Mission, providing a 10-year record of winter lake drainage activity. To improve temporal coverage, the authors apply a normalization approach that enables the joint use of multiple sensors and acquisition geometries. Using this merged dataset, they identify between four and eighteen drainage events per winter.

**General Comments**

Overall, the manuscript is very well written, and I enjoyed reading it. The authors successfully integrate multiple datasets and present their interpretations in a clear and well-structured way. I did not identify any fundamental flaws in the methods. However, the velocity analysis could be improved. I suggest calculating velocity anomaly fields relative to a seasonal or annual baseline velocity map. This approach would allow for a more spatially consistent analysis of velocity changes compared to interpreting arbitrarily chosen profiles.

Below, I list some specific comments that should be addressed before publication.

**Specific Comments**

**Abstract**

**L18:** Please explain what cascading events are, as not all readers may be familiar with the term.

We agree that an explanation will be useful for reviewers. We will move when we first define them (L85), to when they are first mentioned in the abstract (L8).

**L20:** You mention that there are more winter drainages when there are fewer summer drainages. Could this imply that drainage events are largely independent of the season, and instead controlled by a threshold pressure condition?

We explain the inverse relationship between lines 545-553. There are two likely reasons. In light of a comment from Reviewer 1 we plan to clarify the 2nd reason, which, as this reviewer suggests is related to the idea of threshold pressure condition.

**Introduction**

**L37:** Although you defined that "lakes" refer to supraglacial lakes earlier (L25), the phrasing reads awkwardly here. Consider using supraglacial lakes and lakes interchangeably throughout.

We agree. We will revise the definition at L26 to read “Supraglacial lakes – hereafter often referred to simply as lakes – …” to allow more flexible wording, and we will adjust the sentence around L37 for improved flow. Throughout the manuscript, we will use “supraglacial lakes” where it improves clarity/readability and “lakes” where the meaning remains unambiguous.

**L45:** WorldView imagery has a much higher resolution than 10 m, but access is limited, making it less suitable for time series analysis—perhaps better for case studies.

You raise a good point. We did not include reference to studies using high resolution sensors such as WorldView because they almost exclusively have been used for case studies rather than regional or ice sheet wide time series analysis. We will include some text briefly mentioning the use of high-resolution sensors to make the literature review more comprehensive.

**L58:** Note that Sentinel-1C is now operational.

We will rephrase this sentence to include mention of Sentinel-1C and its launch date.

**L121:** A short paragraph describing seasonal ice dynamics in the study region would improve context.

We agree that this would improve context and will include a short paragraph in the revised version.

**Methods and Data**

**Figure 1:** Please clarify what the sampling points (yellow triangles?) represent. Also, is the 10-year melt season mask derived from Landsat?

The Figure 1 caption will be revised to clarify that the yellow sampling points were used for ice velocity sampling, and that the 10-year melt season mask was derived using Landsat.

**L166:** You mention that all data were acquired in HH and HV polarization but only HV was used. Why? Schröder et al. (2020) demonstrated reduced ambiguity when combining HH and HV.

Schröder et al. (2020) indeed demonstrated that the inclusion of HH polarization can aid in the detection of supraglacial lakes. However, a primary objective of Schröder et al. (2020) was the mapping of lake area, which differs from the focus of our study. We do not attempt to delineate lake extent but instead analyze backscatter time series to detect lake drainage behaviour. We therefore chose to follow previous studies in which both HH and HV polarizations were available (e.g, Benedek & Willis, 2021; Hossain et al., 2024) and use only HV. In the context of our time-series–based approach, HH is unlikely to provide additional information, whereas HV offers superior penetration and greater sensitivity to volume scattering, making it particularly well suited for detecting winter drainage events. We do not rule out the potential usefulness of HH polarization in future methodological developments like ours, and this remains an interesting avenue for further investigation.

**Figure 2:** Does column 4 show the backscatter mean within the summer lake polygon? Please clarify. It would also help to indicate which SAR satellite (S1 or RCM) is used in panels (ii) and (iii).

Column 4 indeed shows the mean backscatter time series extracted within the summer lake polygon, and we will clarify this in the Figure 2 caption in the revised version. We note that the sensor is already indicated by the inclusion of "S1" in the titles of the panels in columns (ii) and (iii). However, for clarity, we will explicitly define the abbreviation "S1" as Sentinel-1 in the revised Figure 2 caption.

**L204:** Would using the non–terrain-corrected $\sigma^0$ values change your results?

It is likely the use of non–terrain-corrected $\sigma^0$ would result in errors associated with poorly georeferenced data. Greater spatial inconsistency of the backscatter data would result in noisier time series.

**L222:** In Figure 2 (iv), consider including side-by-side imagery from S1 and RCM for the same lake and approximate date, both before and after normalization. This could serve as a clear visual validation of your correction approach.

In the revised version we will consider including S1 and RCM imagery in the Figure, though this will be challenging as it is already quite large. We are also unsure of how useful including a comparison of both sensors would be as they were never used together in the same time series.

**L230:** See previous comment.

**L233–L234:** Please elaborate on the cause (e.g., lids?).

Thank you for pointing this out. The purpose of this additional 14-day pre/post filter is to reduce false positives by excluding transient backscatter excursions (i.e., short-lived spikes or noisy periods) and retaining only clear, sustained step-like increases in $\sigma°HV35$ (post-event minimum > pre-event maximum). Potential causes of these transient excursions include radiometric/geometric noise and residual preprocessing issues, which we described earlier (L224–226). We will add a brief clarification at L233–L234 to make this explicit.

**L237:** Please define how the end of a drainage event is determined; this is not obvious from Figure 2 (iv).

We agree that this is not clear and will be rephrased. We define the end of a drainage event as the acquisition at which the normalized HV backscatter reaches its maximum within a short transition window following the start date (up to the next three acquisitions). In the revised version, we will elaborate on our definition of how the end of the drainage event is determined. We note that the dashed vertical line shown in Figure 2 (iv) is the single representative date described at lines 240-241.

**L254:** Why was manual delineation required? Couldn't the Landsat lake masks be used here?

We found this step necessary to capture lake extent at the end of the melt season, after some lakes may have partially drained or contracted during late summer. Using Landsat-derived masks would more likely reflect near-maximum summer extents and would therefore bias our end-of-season area (and associated volume) estimates high.

**Results and Discussion**

**L365–L377:** This section is particularly interesting -- do you have a hypothesis or possible explanation for this observed behavior?

We found this to be quite interesting as well. We just present the results here in this Section 4. In our discussion (specifically Section 5.1 L540-555), we discuss two potential mechanisms that may be playing a role to explain this behavior.

**L398:** The statement seems self-evident, since summer drainages are far more numerous than winter ones.

We disagree with this point. Just because summer drainages are more numerous than winter drainages, a positive correlation between summer drainages and melt index does not necessarily mean there has to be a positive correlation between annual drainage and melt index. A negative correlation with winter drainages could offset it. The observed increase in annual drainage frequency therefore reflects a net seasonal redistribution rather than a trivial consequence of summer dominance.

**Figures 9 & 10:** These velocity plots are difficult to interpret. I suggest showing relative velocity anomalies compared to monthly or annual baselines. Importantly, note that apparent velocity increases coinciding with lake drainage (e.g., Fig. 10b ii at ~12 km) could reflect vertical displacement rather than true horizontal acceleration. SAR offset tracking cannot separate vertical and horizontal motion, and these velocity fields are not corrected for vertical effects. See for example Joughin et al. (2016) on this issue.

Thank you for the suggestion on these figures. We agree with your comment here and in general comments that showing velocity anomaly field relative to a baseline would be easier to interpret than the plots. We will include these in Figure 9 and 10 of the revised reversion. In addition, we will clarify in the revised manuscript that apparent velocity increases associated with drainage events may reflect vertical displacement rather than true horizontal acceleration, and we will review and revise related statements in the Results and Discussion accordingly. Your comment also prompted us to consider that abrupt backscatter changes associated with lake drainage may influence offset-tracking performance, and we will acknowledge this as an additional source of uncertainty when interpreting localized velocity anomalies.

**L600:** The reference to L1C is confusing; please clarify this and adjust the velocity plots accordingly.

Thank you for catching this. This is a typo. L600 should refer to L4C rather than L1C. This will be fixed in the revised version.

**L605:** If lake L5B drained first, this event would not qualify as cascading. Please clarify your terminology. You mention "basal uplift" — this may indeed be visible in the velocity fields, but again, vertical motion needs to be treated carefully (see comment above).

Given these issues, I recommend replacing profile-based analyses with velocity anomaly maps relative to an annual baseline. Such maps would better reveal spatial patterns in velocity changes and/or uplift events.

We disagree that drainage of L5B occurring first would preclude this event from being considered cascading. We use the term “cascading” to describe a chain-reaction drainage sequence, consistent with the framework of Christoffersen et al. (2018), in which drainage events may propagate upstream and/or downstream depending on stress transmission and hydrological routing. See our definition on L85 which, as we state above, we will also add to L8, the first time we refer to cascading events in the Abstract. Accordingly, if an upstream lake (L5A) subsequently drained in response to the drainage of a downstream lake (L5B), we interpret this as a coupled, chain-reaction sequence. Regarding our mention of basal uplift, this is intended as a hypothesis based on interpretation rather than a reference to the velocity fields shown in Figure 10, which, as we mention above, are horizontal velocities.

**L730–L732:** I fully agree with the statements here and the following paragraph.

We’re excited to see this area of research grow.

Additional Reference

Joughin, I., Shean, D. E., Smith, B. E., & Dutrieux, P. (2016). Grounding line variability and subglacial lake drainage on Pine Island Glacier, Antarctica. Geophysical Research Letters, 43, 9093–9102. https://doi.org/10.1002/2016GL070259