**Author response to Reviewer 1**

Dear Dr Tedstone,

Thank you for taking the time to provide a detailed review of our paper. Our response below builds on our initial, brief response submitted on 15 Apr 2024. Your comments are copied below in black text, and our responses are in blue.

**Summary of study**

This study maps the seasonal evolution of supraglacial meltwater features in a surface drainage catchment on the western margin of the Greenland ice sheet during two melt seasons by digitising the features from multispectral satellite scenes. Next, it seeks to attribute meltwater disappearance to either drainage or refreezing. Finally, it examines the case for links between surface meltwater drainage and basal sliding.

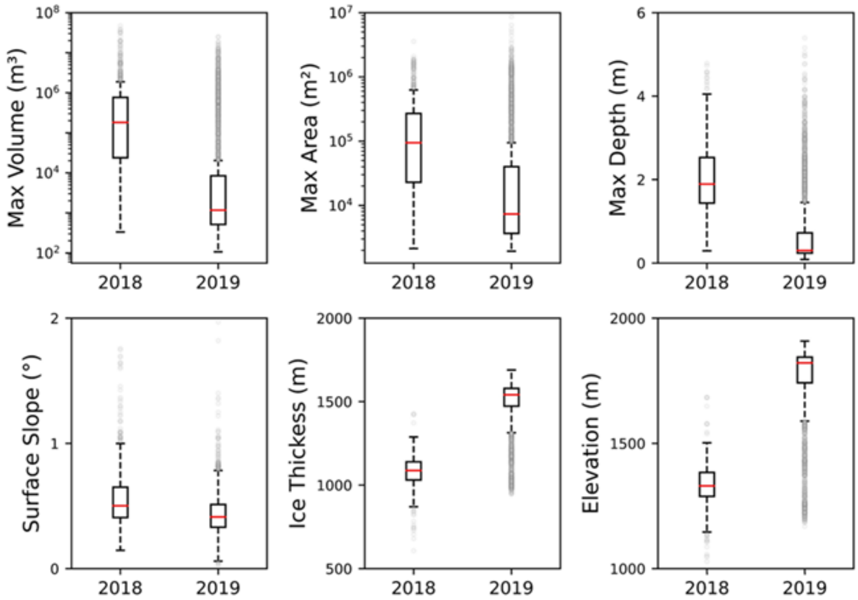
For clarity, the main aim of our study is to *compare* the distribution, morphology and evolution of supraglacial meltwater features in the Russell/Leverett glacier catchment in the low melt year of 2018, to the extreme high melt year of 2019. This is important as the frequency of high melt years like 2019 will likely increase in the future due to rising air temperatures.

In particular, we emphasise here that we, to the best of our knowledge, are the first to include small lakes (< 0.0495 km²), large lakes (> 0.0495 km²), AND slush in a single inventory. Now in our revised paper, following Reviewer 2's suggestion, we also map supraglacial channels to produce an even more inclusive inventory of surface meltwater evolution over the two melt seasons (for further detail, see our comments on page 7 of our response letter to R2).

In our revised paper, we will ensure that our study's key, unique, objectives are highlighted more clearly. For example, we will make our introduction clearer to include a revised paragraph along the lines of the following: '*Our unique dataset includes small (i.e., <0 .0495 km²) and shallow meltwater features (including slush), which are important, but have to date been overlooked in previous mapping studies, as well as the previously more commonly included, large lakes (i.e., > 0.0495 km²) and channels (i.e.,* all linear meltwater features > *1000 m long). We compare the seasonal evolution of supraglacial meltwater features in low and high melt seasons, with a focus on meltwater feature characteristics and drainage dynamics, and partitioning features into those that drain and refreeze. We then assess the potential implications of differences between the high and low melt year by introducing ancillary data (i.e. ITS_LIVE velocity and PROMICE proglacial discharge), enabling us to gain an insight into how a warming climate - where the high melt year becomes the norm - might impact meltwater throughput and ice flow, as a result of changes to surface meltwater characteristics and lake drainage dynamics.*'

The purported main findings are that (i) surface meltwater features evolve depending on surface melt, draining from higher elevations in warmer summers and (ii) the drainage of these features, even very small ones, can lead to transient ice velocity responses, which may therefore be important to the future stability of ice flow in response to enhanced melting.

We agree with you that these are *some* of our findings. However, more importantly, our findings relate to differences between the two melt years, including that in the higher melt year (2019): 1) surface meltwater features form and drain at higher elevations, 2) small lake formation and drainage is more prevalent and 3) slush is more widespread. The implication of this is that these patterns are likely to become the norm in future, higher melt years. Interestingly, we also have the unique finding that meltwater features in 2018 (the low melt year) tend to be deeper and more voluminous than in 2019 (the high melt year), as shown in the new figure below, which we will add to the Supplement of the revised paper. This is a finding that we will discuss in our revised results/discussion.



*NEW supplementary figure: Box plots showing (a) maximum volume, (b) maximum area, (c) maximum depth, (d) mean depth, (e) ice thickness and (f) elevation of surface lakes in 2018 (N = 1011) and 2019 (N = 1495). Red is the median and the edges of the box are the 25th and 75th percentiles ($q_1$ and $q_3$, respectively).*

## Major comments

My overall view is that this study is weak. The basic methodology is not novel: mapping of supraglacial meltwater features is well-established in the literature, indeed as highlighted by the references in the study. This would not be a problem if the rest of the analysis made solid contributions on top of this approach, but I did not find this to be the case. I am unconvinced by the partitioning between drainage and refreezing, while the links with catchment wide ice velocity are in principle novel but appear to suffer from some major methodological flaws which I fear are unfixable. Please find more reasoning for my conclusions below. I am sorry that I cannot be more positive about the manuscript at this time. I am open to discussion if the authors feel I have mis-interpreted aspects of the study.

It is correct that we base our methodology on previously established and validated methods used successfully in previous mapping studies, and indeed we reference these studies throughout our manuscript. It should be noted, however, that we do adapt the well-established NDWI method to add small and shallow meltwater features to our dataset. In our revised manuscript, we will also adjust the threshold to delineate slush in order to detect even shallower slush features than previously Additionally, in our revised manuscript, we will expand our analysis to also include delineation of surface channels (defined to include both rivers and streams > 1000 m long), following Yang et al. (2016). Please see our separate response letter to Reviewer 2 for more details about our additional channel analysis and results, as well as our revised slush delineation analysis and results (pages 3 -7) of the R2 response letter).

## Originality/novelty

The mapping of supraglacial meltwater features here uses established techniques which have been previously applied to a range of moderate and fine resolution imagery (non-exhaustively: Williamson et al., 2018; Smith et al., 2015). In this respect, I did not really learn anything new about the basic evolving pattern of supraglacial drainage in this area of the ice sheet that has not already been evidenced elsewhere.

Our study does not seek to develop new knowledge about the ‘basic evolving pattern of supraglacial lake drainage’ which as you say, is well documented elsewhere. Rather, as also mentioned earlier in this letter, we seek to discover differences in this pattern of supraglacial meltwater and drainage between high and low melt years, and we do find substantial differences, as explained in our response above. In our revised paper, we will ensure to make this overriding objective of our study clearer. Additionally, during both melt years, we also make the novel finding that small lakes - disregarded in previous work (e.g. Williamson et al., 2018, Miles et al., 2017) because they are thought to be too small to drain - are able to drain rapidly and thus form moulins that act as conduits for surface to bed meltwater transport in previously unexpected areas. This is especially important as these moulins will likely stay open, and hence will act as surface to bed connections, for the remainder of the melt season (e.g. Banwell et al 2016).

As I indicated above, the link to ice velocity has the potential to be novel, as does the attribution to meltwater disappearance between refreezing and drainage. However, I have major concerns about the quality of these two parts of the analysis.

**Scientific quality/rigour**

Here I concentrate on the two areas of analysis which I found most problematic.

***(i) Ice velocities***

The authors employ the ITS_LIVE velocity dataset, which is derived from feature tracking of optical satellite imagery. The details provided in this study’s methods are insufficient. For instance: what are the uncertainties/errors? Was any filtering (as opposed to smoothing) of the ITS_LIVE data carried out? 10 days is a very short baseline considering the expected ice displacement over this period in a “slow” land-terminating catchment and also in relation to the imagery pixel size, so is surely prone to high uncertainties.

Concretely, without these details I am especially concerned by the analysis of retrievals above ~1,200m asl. Among the several studies which have looked in detail at feature-tracked velocity retrievals in this area (e.g. Tedstone et al., 2015; Williams et al., 2020; Halas et al., 2022), it is clear that retrievals become very sparse to non-existent above 1,200-1,400 m asl, even when employing higher-resolution Sentinel-2 data, owing to a lack of features which can be reliably tracked. Of course, these studies examined annual net ice flow, not intra-annual flow, so are not directly comparable, but provide a conservative sense of the coverage of feature-tracked retrievals in this area of the ice sheet (or possibly even optimistic given the way in which they mosaic several acquisitions together)

In this light I was very surprised to see “unvalidated” use of ITS_LIVE velocities all the way up to the 2,000 m contour. Using the ITS_LIVE Binder (https://mybinder.org/v2/gh/nasa-jpl/its_live/main?urlpath=lab/tree/notebooks), I took a look at a single point of these data through time at roughly this elevation:

It is clear that at high elevations the short-baseline retrievals are exceptionally noisy. Previous GNSS observations in this sector (e.g. Sole et al., 2013; Doyle et al., 2014) have shown that ice speeds above 1,500 m asl are less than c. 100 m/yr and show maximum daily velocities of up to max. 300 m/yr, generally occurring over periods less than 10 days (based on Sole Fig. 2). So, there are lots of velocity retrievals above which are simply not supportable by reference to previous ground observations.

To be satisfied that the analysis presented in the present study is appropriate, I would need to see evidence of: (a) a robust filtering approach to treat the abundant outliers (not just a boxcar moving window as currently used to smooth the data); (b) ideally, examination of the underlying signal-to- noise ratio (i.e. does the ITS_LIVE algorithm even support these velocities?); (c) an error budget/uncertainty analysis. (d) At the highest elevations, I suggest to go further, interrogating the velocity fields with reference to the underlying imagery, as I suspect that spurious cross-correlations are being identified associated with ephemeral slush fields, which are much less ‘stable’ than the ice- incised supraglacial channels found at lower elevations. Put simply, at the moment I do not believe the velocity analysis for elevations higher than ~1,200 m/yr.

We appreciate your concerns about our use of the ITS_LIVE velocity data, however we suggest that you may have inferred greater weight imposed upon its inclusion than intended in the paper. To clarify, our aim is to use these ancillary data to support our analysis, as opposed to a primary dataset upon which we construct our original arguments; we will ensure this is also clarified in our revised paper. Our aim and methodology is similar to the use of ITS_LIVE data in numerous previous, published, studies (e.g. Otto et al., 2022; Wang and Sugiyama, 2024, Arthur et al., 2021; Boxall et al., 2022). We note that the data only appears in one of the five figures in the paper, and we are careful to use language such as ‘appears to perturb ice velocity’ within the text, which is in recognition of its uncertainties.

We do perform some filtering: we only include data within the 1st and 99th percentile in order to remove the outliers you mention, and we include the uncertainty estimates provided to us by ITS_LIVE in our revised Figure 5. We will refer to our filtering method and addition of uncertainty estimates in the revised manuscript.

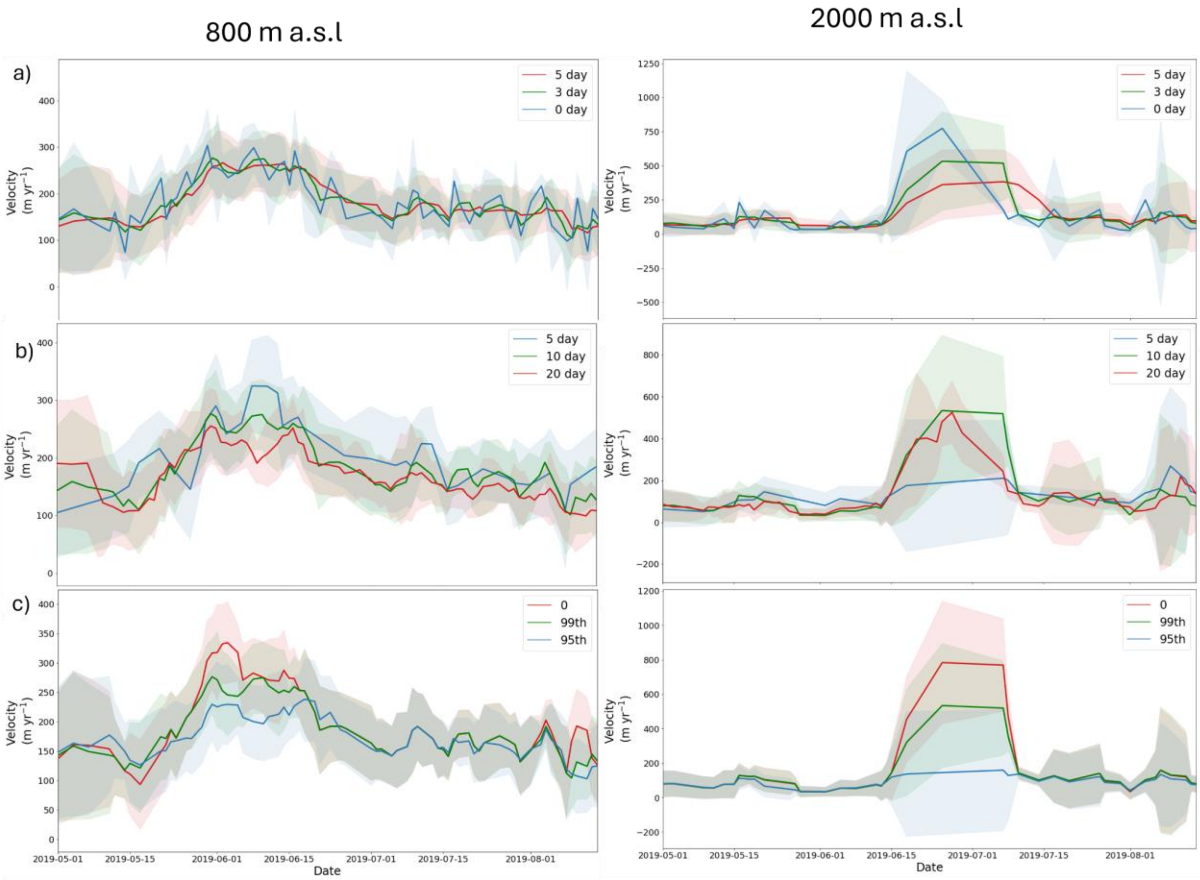
More specifically, in our revised manuscript, we will add the following text to the methods section:

*“In line with several other studies, we use ITS_LIVE data to support our analysis (e.g. Otto et al., 2022; Wang and Sugiyama, 2024, Arthur et al., 2021; Boxall et al., 2022).”*

*“We filter the data between the 1st and 99th percentile in order to remove the outliers and a three-day smoothing window was used to further reduce noise.”*

*“Uncertainty measurements are provided by ITS_LIVE and are detailed in the ITS_LIVE Regional Glacier and Ice Sheet Surface Velocities documentation (Gardner et al., 2023). ITS_LIVE velocity data is retrieved through auto-RIFT feature tracking methods and we note that ice velocities retrieved using this method at high elevation regions (i.e., > 1200 m a.s.l) are subject to high uncertainties (e.g., Tedstone et al., 2015; Williams et al., 2020; Halas et al., 2022) due to the lack of trackable features here.”*

Additionally, in response to your review comment, we have tested the sensitivity of the observed signals in the velocity data to our choice of baseline and filtering parameters and produced the new figure pasted below, which we will put in the Supplement. This figure compares a) filtering, b) image separation, and c) smoothing. However, an in-depth independent validation of the ITS_LIVE dataset beyond this is well outside the scope of this study.



*NEW supplementary figure: Sensitivity of 2019 ITS_LIVE velocity data to a) filtering with 0, 95th and 99th percentiles, b) image separation under 5-, 10- and 20-day time steps, and c) smoothing with 0-, 3- and 5-day moving windows. Data from 800 m a.s.l (left) and 2000 m a.s.l (right) are shown. Shaded regions indicate the uncertainty taken directly from the ITS_LIVE data product (Gardner et al., 2018; 2023).*

### *(ii) Surface hydrology partitioning*

Like with the ice velocities, my concerns particularly relate to higher elevations of the catchment. The study uses air temperatures to apportion the disappearance of surface meltwater into either drainage or refreezing. When meltwater disappears between two successive satellite acquisitions, if air temperatures were positive it is assumed to drain, whereas if they were negative it is assumed to refreeze. This is almost certainly overly simplistic. First, for instance, going all the way back to Holmes (1955), there is evidence that meltwater can continue to flow in open channels for up to two weeks after the end of surface melting. There is therefore a substantial lag between the onset of negative air temperatures and the freeze-up of the surface.

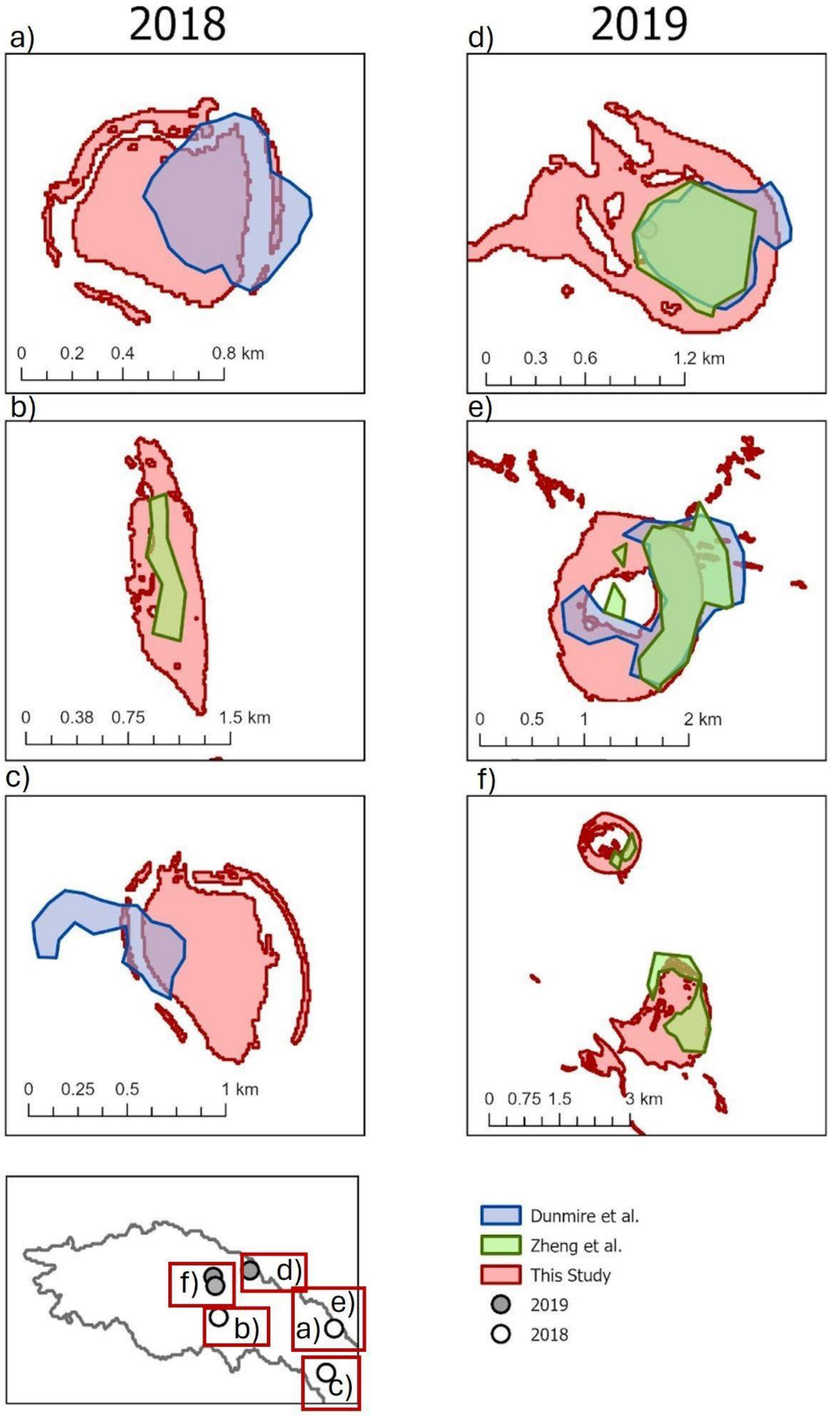
We agree that our previous methods that we used for partitioning between drainage and refreezing, based on air temperature alone, were rather simplistic. In our revised manuscript, we will take two additional measures to improve our method of partitioning lake drainage vs. refreezing, and to account for associated uncertainty:

1) We now class a meltwater feature as refreezing not only based on air temperature, but also on the pattern of lake volume decline, following Selmes et al. (2013). In our original manuscript, lake drainage and refreezing were classified as such if > 20% of volume is lost over any time period within a melt season. And refreezing (rather than drainage) was previously assumed to occur if the air temperature was < 0C +/- 1C for at least three days prior (following *Wan et al., 2002 and Selmes et al. 2013)*. Now, in our new methods, we also

implement a > 72-hour time requirement, whereby refreezing is only assumed to occur if a lake takes at least 72 hours to lose 20% of its volume AND the air temperature stays below (or equal to) 0C. This additional measure reflects the fact that refreezing does not happen instantaneously after the onset of negative temperatures, as you state above.

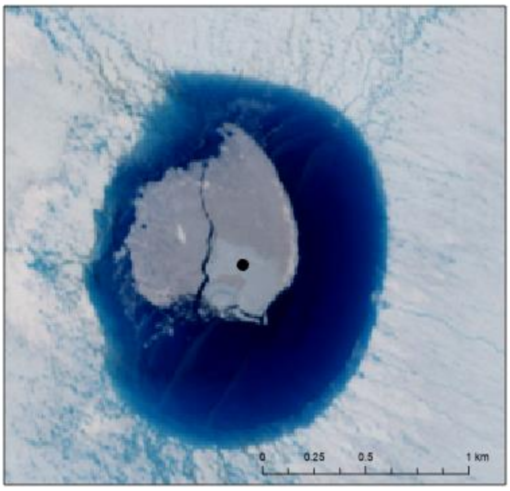
2) We have also added an ‘unknown’ class to our lake behaviour partitioning (i.e. in addition to ‘drain’ and ‘refreeze’), A lake is classified as ‘unknown’ if the pattern of area and volume decline is not in accordance with that of a draining or refreezing lake.

To provide some validation for our approach, we have cross referenced a subset of our refreezing lakes with two independent datasets of subsurface lakes acquired from SAR imagery in both years (Dunmire et al., 2021; Zheng et al., 2023), under the assumption that refrozen lakes essentially become subsurface lakes after refreezing. We find good agreement in the locations of refreezing lakes in our dataset when compared to the other two datasets (See NEW supplementary Figure on page 7 of this letter). We suggest that differences in the locations of these refrozen lakes are due to differing NDWI thresholds between the three datasets.



*NEW supplementary figure: Cross referencing a subset of refreezing lakes in this study with maps of subsurface lakes acquired from SAR imagery in both 2018 (left) and 2019 (right). Blue: dataset from Dunmire et al. (2021); green: dataset from Zheng et al. (2023); red: Data from this study. Coordinates of lakes in WGS 1984 UTM Zone 22N: a) 47.3861245°W 66.9952765°N, b) 48.4755826°W 67.0147779°N, c) 47.4456300°W 66.8319809°N, d) 48.2075289°W 67.1934572°N e) 47.3873357°W 66.9953815°N, f) 48.5521200°W 67.1620352°N.*

Using the optical satellite record, we have also manually identified ice formation on large lake surfaces that we classify as refreezing (e.g. image below). For these lakes, we also manually check that surrounding meltwater features also appear to decrease in area, which points towards a common forcing mechanism (most likely refreezing).

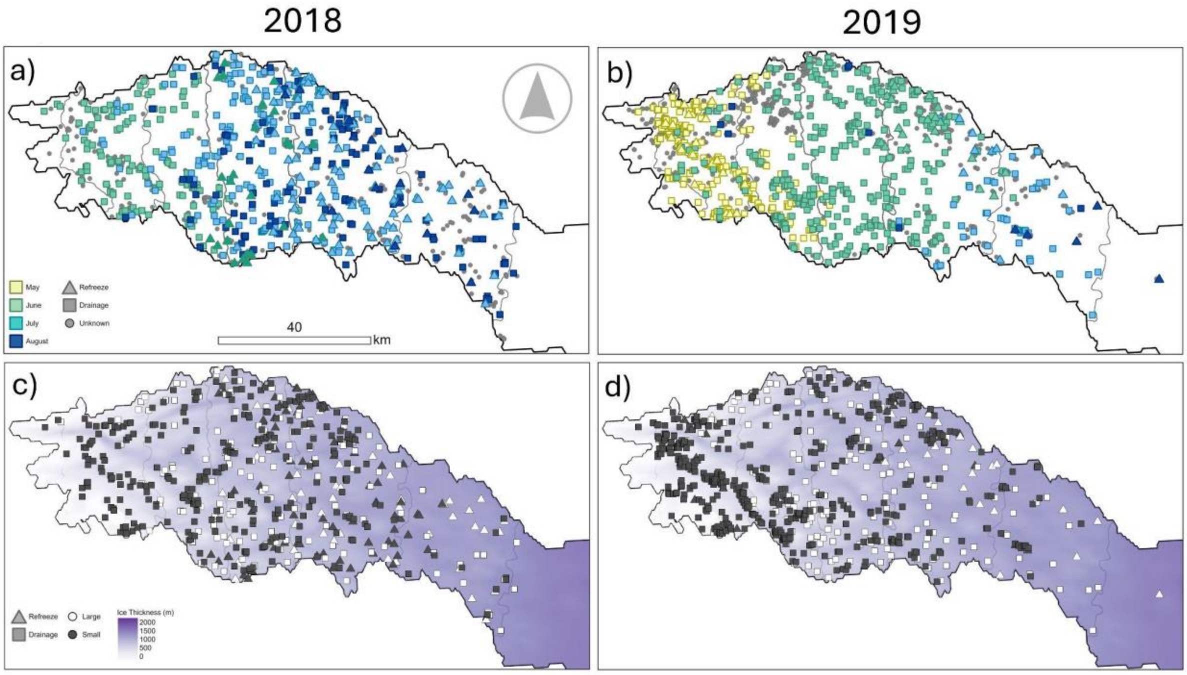


*Example of a refreezing lake (for the purposes of this letter only) identified in a Sentinel-2 image from 17/08/2019. Note the formation of an ice lid and the formation of thin ice on the surface. Lake situated at 47.3873357°W 66.9953815°N (WGS 1984 UTM Zone 22N).*

In general, we consider our approach for identifying lakes that refreeze to be conservative relative to other studies. For example, as indicated by our new statistics (new Table 1 below as well as in our revised Figure 4 below), we calculate that 13% and 4% of lakes refreeze by the end of the 2018 and 2019 melt season, respectively. In contrast, Selmes et al. (2013) determined that on average, over multiple melt seasons, ~46 % of lakes froze. We will also state this point in the revised paper.

*REVISED Table 1: Meltwater statistics of draining and refreezing lakes as well as those with unknown behaviour in 2018 and 2019, where draining and refreezing features were identified using the FASTER algorithm (Williamson et al., 2018a) and subsequently partitioned using RACMO 2 m air temperature data (Noël et al., 2019), with a threshold of < 0 °C identified as refreezing. DOY is the 'day of year' in 2018 and 2019. DOY sampling is calculated by averaging the start drainage DOY and the end drainage DOY. Percentage values are proportions of the sum of the total meltwater areas or volumes for each melt season.*

| | **2018** | | | **2019** | | |
|---|---|---|---|---|---|---|
| **Statistic** | *Drainage* | *Refreeze* | *Unknown* | *Drainage* | *Refreeze* | *Unknown* |
| Frequency (n) | 432 | 129 | 450 | 650 | 59 | 786 |
| n (%) | 43 | 13 | 44 | 44 | 4 | 50 |
| Total Volume (km$^3$) | 0.54 | 0.065 | 0.0035 | 0.50 | 0.12 | 0.0015 |
| Total Volume (%) | 89 | 11 | < 1 | 80 | 19 | 1 |
| Mean Volume (km$^3$) | 1.3e-3 | 5.0e-4 | 7.9 e-6 | 7.7e-4 | 2.8e-4 | 1.8e-6 |
| Total Area (km$^2$) | 59 | 12 | 3.5 | 99 | 23 | 3.5 |
| Total Area (%) | 80 | 16 | 4 | 79 | 19 | 2 |
| Mean Area (km$^2$) | 0.14 | 0.091 | 0.0079 | 0.15 | 0.40 | 0.0044 |
| Mean Depth (m) | 1.7 | 1.4 | 0.73 | 1.4 | 1.8 | 0.53 |
| Mean event DOY | 195 | 200 | n/a | 159 | 166 | n/a |
| Mean DOY sampling (± days) | 6 | 6 | n/a | 2 | 5 | n/a |



*REVISED Figure 4: Drainage and refreezing within the Russell/Leverett glacier catchment in 2018 (left) and 2019 (right). (a) and (b) depict the timing of lake drainage (square) and lake refreezing (triangle) events in 2018 and 2019, respectively. Lakes of unknown drainage/refreezing behaviour are represented as small grey circles. (c) and (d) depict small (<0.0495 km$^2$; black) and large (>0.0495 km$^2$; white) lake drainage (square) and lake refreezing (triangle) events in 2018 and 2019, respectively. Light to dark purple gradient represents ice thickness in metres. Lakes of unknown drainage/refreezing behaviour are not shown in panels c and d.*

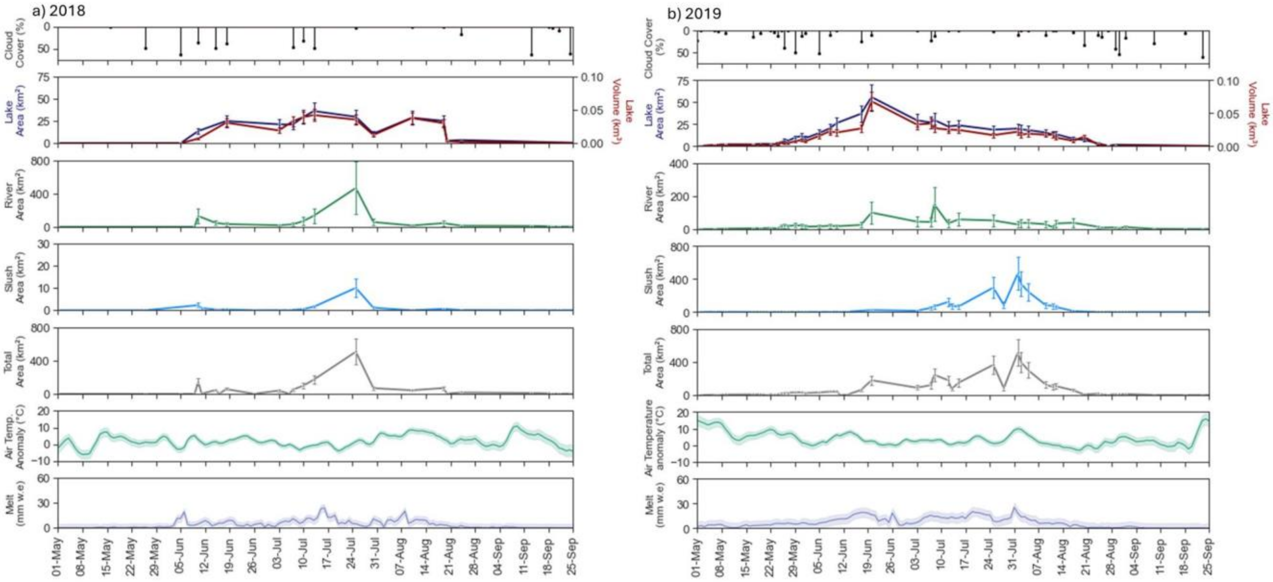
Second, I suspect that there is some antialiasing of the evolution of surface drainage features with ‘drainage’. This particularly concerns slush fields, which can either collapse/incise into more spatially discrete, efficient supraglacial channels – thereby presumably allowing water to be evacuated more quickly – or never evolve as far as incision, instead allowing water to continue to flow through the matrix. In this case, just because the water disappears from view, does not mean that all that water has refrozen. Instead, it may be discharging via the sub-surface, as shown by Clerx et al. (2022), in a water table that is below the height of the snow surface.

We agree with your concern, so we now only focus on partitioning the drainage and refreezing of lakes; not slush. Although we still analyse the seasonal evolution of lake, channel and slush features, we remove slush features from our drainage/refreezing dataset to avoid any confusion between the evolution of surface features with drainage.

Secondarily, I am concerned about the suitability of the water depth retrieval algorithm to drainage at higher elevations. These algorithms were developed on the basis of a solid ice substrate, which is often not the case above the ELA. In particular, slush fields are composed of a porous water-filled snow matrix perched on top of an ice slab formed of refrozen meltwater (see e.g. Clerx et al., 2022). Thus, neither are they spectrally similar to supraglacial ice-incised channels, nor do their depths correspond to an entirely liquid column. This makes the water volume retrievals from these features problematic.

We agree that there are uncertainties when applying the radiative transfer model to slush, for the reasons you state. Therefore, as we also stated above, we no longer use the radiative transfer model to calculate the depth of slush (and nor channels); we only apply it to lakes.

Additionally, we appreciate that even though we now do not apply the radiative transfer model to slush, there will be some uncertainty when applying the radiative transfer model to lakes above the ELA, especially as these lakes are surrounded by slush. For example, many of our team were involved in a recent study examining the accuracy of this algorithm (Melling et al., 2024), which we will comment on in our revised paper. We also note that we do include the uncertainty associated with our lake volume calculations, as shown in our revised Figure 3, pasted below.



*REVISED Figure 3: Time series of total areas of lakes (dark blue), channels (green), slush (light blue) and all meltwater features (grey) in (a) 2018 and (b) 2019 from L8 and S2 imagery. Area error bars represent uncertainty of automatically delineated features compared to a manually delineated dataset. Lake volume is given in red error along with an estimate of uncertainty determined by comparing lake depth to Melling et al. (2023). Also shown is cloud cover percentage (black bars), RACMO 2 m air temperature anomaly (light green line) from the 1958 - 2019 catchment average with the spatial standard deviation (light green shading), and RACMO total daily melt (mm w.e.; light blue line) with the spatial standard deviation (light blue shading). Note that the y-axis ranges are different for the channel and slush areas between (a) and (b).*

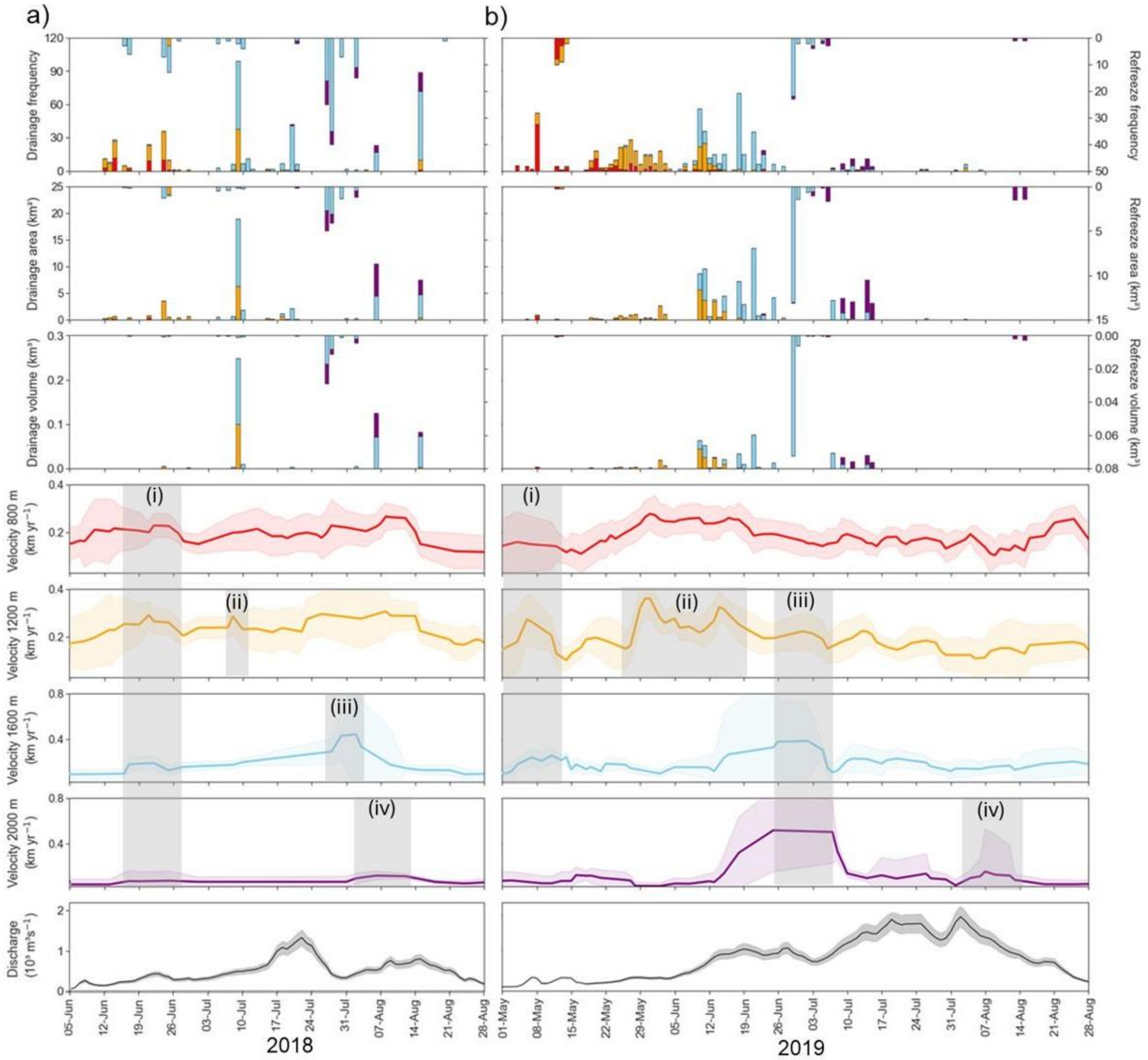
Given my perspective above, in my view this study is not able to make impactful insights into the fate of supraglacial meltwater on the Greenland Ice Sheet.

We hope that our detailed responses above help you to understand how our study, and in particular our revised paper, does give impactful insights into the fate of supraglacial meltwater on the Greenland Ice Sheet when a high and low melt season are compared.

**Presentation quality**

This is overall reasonable. The manuscript is mostly clearly written. I suspect it could be made more concise in places (i.e. length reduced). The figures are basically fine, apart from Figure 5, which presents ice velocities by elevation band but does not also segment the drainage/refreeze events/area/volumes by elevation band. This makes it very difficult if not impossible to independently verify the proposed links between surface hydrology “events” and ice velocity/basal sliding.

We have revised our previous Figure 5 as per your recommendations, and those of Reviewer 2. Our revised Figure 5 is pasted below.



*REVISED Figure 5: Time series of lake drainage and refreeze within the Russell/Leverett catchment in a) 2018 and b) 2019. From top to bottom: daily frequency of lake drainage/refreeze events (i.e., the number lakes that drained or refroze); total daily lake area loss; total daily volume loss; mean ice velocity at 800 m a.s.l (red), 1200 m a.s.l (orange), 1600 m a.s.l (blue) and 2000 m a.s.l (purple). Shading indicates the uncertainty taken directly from the ITS_LIVE data product (Gardner et al., 2018; 2023). Vertical grey shaded columns depict, and velocity perturbations discussed in the text (vertical grey shaded columns, labelled i, ii, iii, and iv); Daily values of meltwater discharge through the Watson River (black line) and associated uncertainty (grey shading). Note that the x-axis date ranges are different for each year, constrained by the first and last meltwater feature drainage events in each melt season. Also note that the y-axis for the velocity plots differ between elevation bands.*

I note that some data are indicated as 'on request'. According to TC submission requirements, this is not acceptable.

We have now uploaded our updated dataset here: https://zenodo.org/doi/10.5281/zenodo.11645884, which will be referenced in the revised manuscript.

**Minor comments**

In light of my major comments, I have only a small number of minor comments at this point. Clarification of terminology: particularly in the methods, in general ‘surface meltwater features’ is employed, but sometimes ‘lakes’ or ‘slush fields’ are used instead. This is particularly the case in sect. 2.6, which initially claims to track ‘meltwater features’ but then uses this term and ‘lakes’ interchangeably. See also L175, ‘accuracy of lake area estimates’ concerning delineations, was this actually only for lakes (and not also channels etc), and if so, why?

We will be clearer throughout our revised manuscript when referring to all meltwater features (now including lakes, channels and slush), and also when referring to separate lake, channel and slush features. We have undertaken an accuracy assessment of lake, channel and slush areas, indicated in the uncertainty calculations in our revised Figure 3 (page 10). We did this by comparing automatically delineated lake, channel and slush features with a fully manually delineated dataset.

L185: surface gradients of meltwater features were retrieved from ArcticDEM. Presumably this is highly sensitive to whether those features were water-filled at the time of data acquisition for the DEM? More details are needed to assess whether this is a valid approach.

We agree that the error in the DEM may be too high for this method to be a valid approach for calculating slopes of small surface meltwater features, so we have removed all reference to surface gradients of these features from the text.

Sect. 2.7, use of RACMO: Given the high quality of in-situ AWS measurements on the K-Transect, it is valid to consider/state the performance of RACMO along this transect. Referencing should be sufficient.

We will add: ‘*RACMO performs well compared to automatic weather station data acquired in the same region, i.e. along the K-Transect (Noël et al., 2018)*.’

L389 and around: references to panel a of Fig. 5, but this is to drainage only, without also referencing the velocities panel. Please improve.

We will edit this to refer to both drainage and velocity.

L540-1: ‘perturb ice velocity at lower elevations…this is unexpected’. I disagree. Other studies, for example, Doyle et al. (2014) and Ryan et al. (2024), show that transient velocity variations occur whenever the subglacial drainage system’s capacity is overwhelmed by the rate of meltwater supply . Rather than considering “drainage efficiency” to be an absolute quantity, consider it instead relative to antecedent and event melt supply.

Thank you for pointing this out. We agree with you, and will take this comment into consideration in our revised manuscript.

References not in the original paper (which we will add to our revised paper)

Arthur, J. F., Stokes, C. R., Jamieson S, S. R., Miles, B. W. J, Carr, J. R., Leeson, A. A.: The triggers of the disaggregation of Voyeykov Ice Shelf (2007), Wilkes Land, East Antarctica, and its subsequent evolution, *Journal of Glaciology*, 67(265):933-951, doi:10.1017/jog.2021.45, 2021.

Benedek, C. L. and Willis, I. C.: Winter drainage of surface lakes on the Greenland Ice Sheet from Sentinel-1 SAR imagery, The Cryosphere, 15, 1587–1606, https://doi.org/10.5194/tc-15-1587-2021, 2021.

Boxall, K., Christie, F. D. W., Willis, I. C., Wuite, J., and Nagler, T.: Seasonal land-ice-flow variability in the Antarctic Peninsula, The Cryosphere, 16, 3907–3932, https://doi.org/10.5194/tc-16-3907-2022, 2022.

Otto J, Holmes, F. A. and Kirchner, N.: Supraglacial lake expansion, intensified lake drainage frequency, and first observation of coupled lake drainage, during 1985–2020 at Ryder Glacier, Northern Greenland. Front. Earth Sci. 10:978137, doi: 10.3389/feart.2022.978137, 2022

Wang, Y., Sugiyama, S.: ‘Supraglacial lake evolution on Tracy and Heilprin Glaciers in northwestern Greenland from 2014 to 2021’, Remote Sensing of Environment, Volume 303, 2024, 114006, ISSN 0034-4257, https://doi.org/10.1016/j.rse.2024.114006, 2024.

Glen, E. (2024). Dataset for: A comparison of supraglacial meltwater features throughout contrasting melt seasons: Southwest Greenland [Data set]. Zenodo. https://doi.org/10.5281/zenodo.11645884