Dear Dr Tedstone,

Thank you for taking the time to provide a review of our paper. Whilst we will provide a full, detailed, response to all of your comments in due course, we thought it would be helpful to provide an initial response to your main points here.

Your comments are copied below in normal text, and our response is given 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 and morphology 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. As such, we undertake detailed mapping of meltwater features to elucidate differences in their meltwater characteristics and dynamics under expected climatological conditions now (i.e. 2018) and in the future (i.e. 2019). In particular, we emphasise here that we are the first to map lakes < 0.0495 km^2 and - to the best of our knowledge - the first to include small lakes, large lakes, rivers/streams AND slush in a single inventory. In our revised paper, we will ensure that our study's key, unique, objectives are highlighted more clearly.

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.

Our main findings are that in the higher melt year (2019): 1) surface meltwater features form and drain at higher elevations, 2) that small lake formation and drainage is more prevalent and 3) that slush is more widespread. The implication of this is that these patterns are likely to become the norm in future, higher melt years. In order to investigate the potential impact of this, we introduce ancillary data (i.e. ITS_LIVE velocity and PROMICE proglacial discharge) which suggest that these surface melt and drainage patterns do influence ice velocity and proglacial discharge in a different way in high melt years (our future analogue) than in low melt years.

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 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.

During both melt years, however, we do 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 act as conduits for surface to bed meltwater transport in previously unexpected areas. This is important, as these conduits (aka moulins) will likely stay open for the remainder of the melt season.

Scientific quality/rigour

(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 think you may have inferred greater weight imposed upon its inclusion than intended in the paper. We will ensure this is clarified in our revised paper.

To clarify here in this letter, our aim is to use this as ancillary data to <u>support</u> our analysis, as opposed to a primary dataset upon which we construct our original arguments. This is in a similar manner to the use of ITS_LIVE data in numerous other, 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 '<u>appears</u> 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 can certainly include the uncertainty estimates provided to us by ITS_LIVE. Both of these will be elucidated in the revised manuscript.

We are also happy to test the sensitivity of the signals we see to our choice of baseline and filtering parameters, and to include details of this testing in a revised supplement to the main paper. However, an in-depth independent validation of the dataset beyond this is well outside the scope of this study.

(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 using air temperature to partition between drainage and refreezing is rather simplistic, especially with respect to the larger lakes which possess sufficient thermal inertia to resist refreezing for some time. However, manual inspection of the imagery suggests that it is effective. In the revised manuscript we are happy to validate the partitioning by cross referencing the 'refreezing' lakes with SAR imagery, which will enable us to confirm where liquid water persists below the surface and has not drained (e.g. Miles et al., 2017; Benedek et al., 2021; Dunmire et al., 2021).

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 subsurface, as shown by Clerx et al. (2022), in a water table that is below the height of the snow surface.

Regarding slush in 2019, we agree that this can drain via the two mechanisms you mention. We do not see any consolidation into discrete channels in our data so 'drainage' is likely to be downstream discharge through the water table. We do discuss the downstream discharge of slush on lines 485-488 of the paper, but we accept that this could be made clearer with respect to the physical mechanism, and we will edit this accordingly in the revised manuscript.

We think that our characterisation of 22% of slush area as refreezing using the comparison with RACMO is reasonable given that a) it occurs high up in the percolation zone where overland flow is limited and thus if melting stops then water flow stops, b) water held within an ice matrix (i.e. slush) is likely to refreeze much more readily than water within a large open-water body and c) the timing of this is associated with the onset of refreezing of the entire catchment at the end of the melt season, which begins at these high elevations.

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 the radiative transfer approach to measuring lake depth is suboptimal in terms of measuring *absolute* lake depth, and indeed many of our team were involved in a recent study examining just this (Melling et al., 2024). We think it is reasonable however to assume that *relative changes* to volume between successive scenes (i.e. required by the FASTER algorithm to identify drainage) are relatively robust. Regardless of this, many of the patterns we see are obvious in the lake area data alone (e.g. in figure 5), so we are happy to remove reference to the volume data in the revised manuscript where we use it in the absolute sense and where it is redundant with respect to associated area data.

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

We have uploaded our dataset here: https://zenodo.org/doi/10.5281/zenodo.10949984. This dataset will be referenced in the revised manuscript.

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