Review of Glen et al. "A comparison of supraglacial meltwater features throughout contrasting melt seasons: Southwest Greenland", submitted to The Cryosphere.

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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.

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.

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.

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.

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.

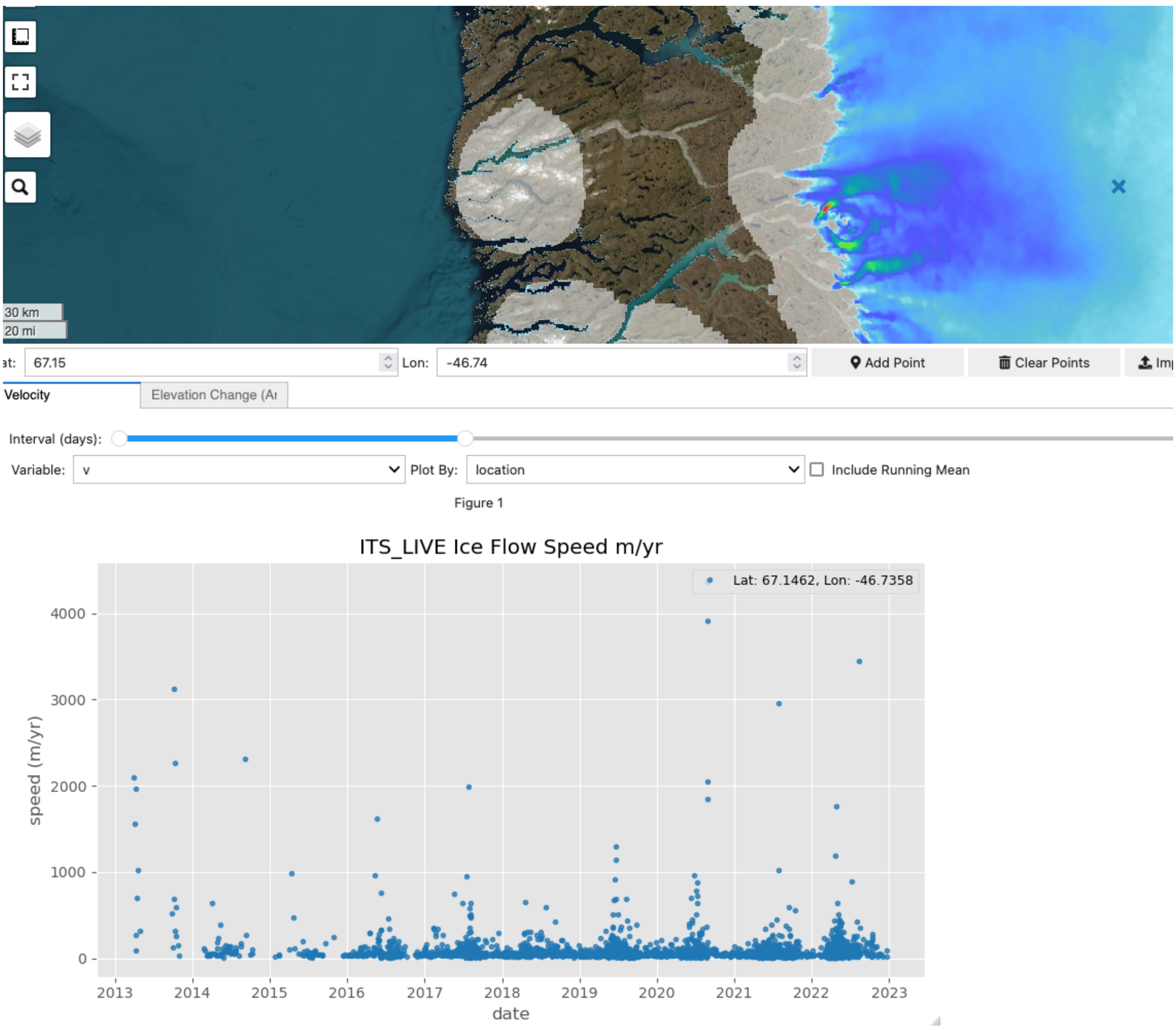
Scientific quality/rigour

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 ~1,200 m 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.

(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. 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 still 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.

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.

My fixation with ‘higher elevations’ may seem pedantic, but the reality is that the vast majority of this study’s geographic area of interest lies at these ‘higher elevations’, so my concerns are relevant to a large percentage of the catchment.

Significance/impact

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.

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.

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

> https://www.the-cryosphere.net/submission.html
>
> *Files for the review process & preprint posting*
>
> *After the manuscript registration, you are kindly asked to upload those files which are necessary for the peer-review process. The following files are required:*
>
> *…*
>
> *data sets, model code, video supplements, or other assets to your manuscript should be submitted to a reliable repository receiving a DOI, cited in your manuscript, and included in your reference list. Reviewers can then access these relevant sources;*

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?

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.

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.

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

L540-1: ‘perturb ice velocity at lower elevations…this is unexpected’. I disagree. Other studies, for example Doyle et al. (2014), 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.

References not in original manuscript

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