In their TCD brief communication: ‘Storstrømmen glacier, Northeast Greenland, primed for end-of-decade surge’ Andersen et al., use ice velocity, double difference interferometric SAR, and DEM data to study the state of two large surge glaciers in the north-east of the Greenland Ice Sheet. The work builds on the earlier work of Mouginot et al., (2018) who reviewed the surge history of the two glaciers and used recent and historical data to suggest that the apparent conditions for the next surge of the Storstrømmen Glacier could be met in the 2027 – 2030 time period. Andersen et al., added recent data, from 2017 to 2023, and exploited all the height change data to suggest that the next surge of this glacier could occur in the 2027 – 2040 period. They also use some double difference interferograms to show change in the line-of-sight ice surface position over particular time periods and suggest that these changes present evidence for lake drainage events leading to changes in ice movement.

My review is in three parts. Firstly, I try to answer the questions suggested by TC in their guidance to reviewers, secondly, I add some detailed comments on the text and figures, and thirdly present a summary of my thoughts on the paper.

1. *Does the paper address relevant scientific questions within the scope of TC?*

Yes.

2. *Does the paper present novel concepts, ideas, tools, or data?*

The recent ice velocity, double difference interferometry and elevation change data included in the paper are new and of interest in the context of when the two glaciers may surge next. However, the methodology is not unique.

3. *Are substantial conclusions reached?*

The answer to this question is inevitably somewhat subjective. Remembering that the submission is in the form of a ‘brief communication’ my response to this question is that the results and conclusions in the communication are sufficiently useful that they can be considered as ‘substantial’.

4. *Are the scientific methods and assumptions valid and clearly outlined?*

Yes.

5. *Are the results sufficient to support the interpretations and conclusions?*

Yes, although I think that some of the discussion and figure labels on ‘inferred drainage’, ‘subsidence’ and ‘flow acceleration’ should be qualified.

6. *Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?*

Yes. But as outlined below some of interpretation could benefit from improved figures and methods.

7. *Do the authors give proper credit to related work and clearly indicate their own new/original contribution?*

Yes.

8. *Does the title clearly reflect the contents of the paper?*

Yes.

9. *Does the abstract provide a concise and complete summary?*

Yes.

10. *Is the overall presentation well structured and clear?*

Yes.

11. *Is the language fluent and precise?*

Yes.

12. *Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?*

There are suggested changes to the use of the terms 'accumulation' and 'ablation' rates.

13. *Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?*

There are suggested improvements to some of the figures, see below.

14. *Are the number and quality of references appropriate?*

Yes.

15. *Is the amount and quality of supplementary material appropriate?*

Yes, but some improvements are suggested.

More detailed comments.

Lines 30-31.
“Contrary to all other marine-terminating glaciers… decreasing distance to the ice front”. I’m not sure that this is always correct, so unless you have a credible reference for the statement, I would suggest that you simplify the sentence to just describe the situation for the two glaciers.

Re 2.2 ‘Double-difference interferometry.’ Note that any interferometric technique is by definition ‘differential’.

Lines 55 – 56. *“DInSAR measures phase change between two image acquisitions, which is proportional to displacements in the radar line-of-sight direction.”* This is strictly not correct; I would prefer something like the following…

Interferometric SAR (InSAR) allows the estimation of the phase change between two image acquisitions which, when the phase is unwrapped (ref), provides data on the difference in slant range to the registered pixels. When the unwrapped phase is then corrected for the different viewing positions and terrain topography (using the baseline and a DEM, add another reference), then the change in the unwrapped phase is proportional to the change in the surface displacement in the radar line-of-sight direction.

I don’t think using ‘DDInSAR’ is necessary. ‘DInSAR’ is now often used for double differencing interferograms. e.g., Wild et al., ‘Differential interferometric synthetic aperture radar for tide modelling in Antarctic ice-shelf grounding zones’, The Cryosphere, 13, 3171–3191, https://doi.org/10.5194/tc-13-3171-2019, 2019.

Line 65. I suggest you drop ‘DInSAR’… ‘We generate interferograms using …’

And line 68; e.g., ‘A time series of double difference interferograms is then generated by differencing all temporally neighbouring interferograms.’

2.3 Elevation change measurements.

Line 83. Here you refer to ‘accumulation rate’ and ‘ablation rate’. These are well defined parameters in glaciology neither of which account for height change due to ice motion (in particular along-flow strain rates). So, I suggest you change the wording here, and elsewhere, to make sure that readers understand you are referring to surface height change and height change rate in two areas, one thickening and the other thinning. In fact, I doubt that the elevation of the upper area is high enough to be in the true ‘accumulation zone’, i.e., with a positive surface mass balance.

3. Results

You have used two 5 km sections of the NASA ATM flight track over Storstrømmen to monitor height change in what you describe as in the 'accumulation' and ablation' zones. Here you appear to follow the work of Mouginot, et al., who also used this data. Is the 5 km section the same as used in the Mouginot work? Maybe add a comment or reference?

Line 100. The noise in the velocity anomaly sub-images for the summer periods is so large that I think you may want to rethink the estimate of summer speed up in... *'... ice flow is observed to be quite stable, except for a reoccurring summer speed-up on the order of < 40 m/y (although it should be noted that measurement noise generally increases during summer, likely due to increased surface melt).'*

Lines 105 – 112. The text here is a mixture of results and conjecture as to the cause of the observed changes. I would prefer that the conjecture re lake drainage, surface speed up based on phase, subsidence, etc., go in the next section. Some more results or clear evidence showing that there are 'lakes' which could periodically release water would be helpful (see below for one example).

Lines 120 – 123. *'... ice flow accelerations on the order of 50 – 120 m/y,'* The units are not appropriate for acceleration, do you mean change in speed of ....

Lines 151 – 154. While I think these statements are correct, the evidence is at best indirect. I think this should be acknowledged.

Figures.

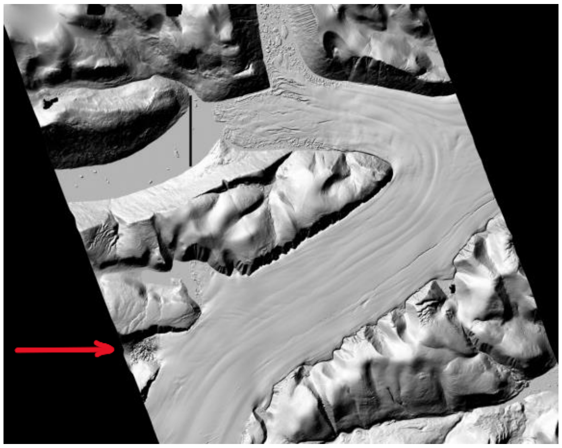
Figure 1. Good.

Figure 2. The top left sub-image uses a linear colorbar. A non-linear colormap, like Figure 1a would be preferable. A vector showing the zero-Doppler or line-of-sight direction would also be useful here.

Figure 3. Panels 3c and 3f include labels indicating areas with 'subsidence'. The arguments in the text are somewhat speculative and not proven. I suggest change to 'possible subsidence'.

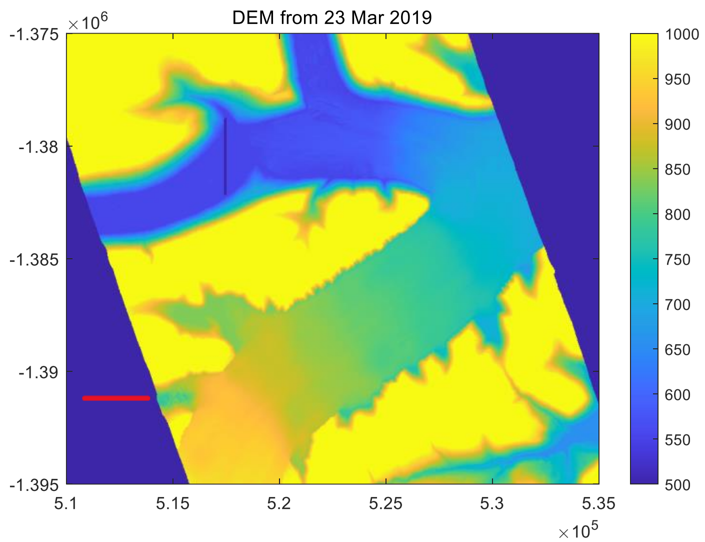
Supporting supplementary material.

There are 6 images in the supplementary material. The first three are in support of the text and the interpretation of the results. Figure S4 and S5 were intriguing and led to some investigation using date-specific ArcticDEMs. These supported the interpretation in the communication but provided much more convincing evidence that the glacier speed-up in Fig. S4e was due to significant water outflow from what appears to be a subglacial lake.

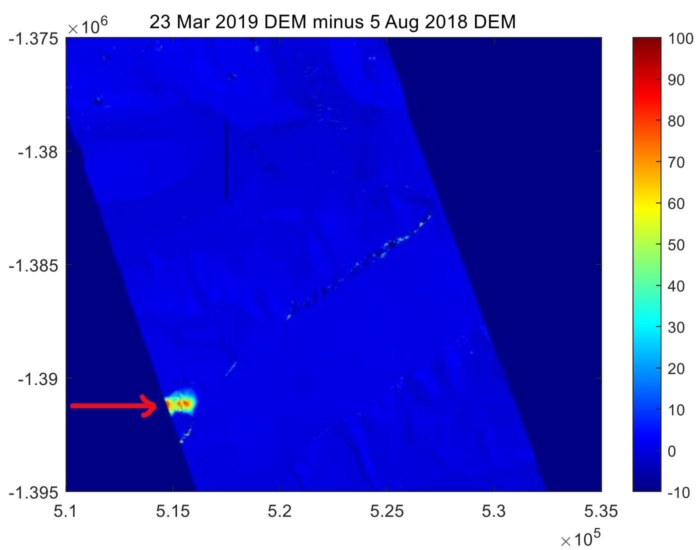


This image is from a shaded relief file for the PGC ArcticDEM from 23 March 2019. The red arrow points to the position where the ice surface dropped due to subglacial lake drainage.

Note the position of the arrowhead in Fig. S4a pointing to the area of 'inferred drainage' is not accurate and should be changed to point to the correct area.



This figure illustrates the DEM from 23 March 2019 after the water outflow from the subglacial lake.



This illustrates the height difference between pre- and post-outflow DEMs. The surface falls by up to around 85 m. This complements your observation that the SAR image brightness increased significantly between 11 and 17 Feb. 2019 as the ice broke up due to the surface collapse as the subglacial lake drained.

Presumably, the water outflow facilitates the surface ice speed-up seen in S4e and S4f.

Summary.

The brief communication contains interesting material which warrants publication. However, I would like the authors to consider the comments above, and the suggestion to use some time-specific ArcticDEMs. You are welcome to use the above example in your paper, I can provide the files, or you can download e.g., ...

SETSM_s2s041_WV01_20190323_1020010082E60600_1020010083934E00_2m_lsf_seg1.tar.gz

From the PGC directory ...

https://data.pgc.umn.edu/elev/dem/setsm/ArcticDEM/strips/s2s041/2m/n76w025/

I think the other two files I used are also in this directory (N76W25).