Review of the study by Strozzi et al.

The study presents glacier flow velocity derived from SAR data for several subregions of the Eastern Arctic. The dataset primarily focuses on filling the gap in the 1990s, making it valuable for future investigations to monitor glacier changes in the highly sensitive Arctic under global warming. Most of my concerns, however, fall on the presentation of the paper and the data availability. I also found the discussion about error sources and the representativeness of winter velocity interesting. Still, the related content might need further elaborated or trimmed depending on the authors' goals. I have listed all of my major and technical comments below.

Major Comments

Data sharing and availability: I appreciate that the authors have already uploaded the "main plate," but this paper presents more data than those available on Pangaea. In addition to the velocity datasets derived from JERS-1, ERS-1, and ALOS-1, the authors produced Sentinel-1 glacier velocities, digitized frontal positions, new glacier outlines, and velocity differences clipped to those outlines. It would greatly help improve the data availability and reproducibility of this paper if authors can make these additional data (especially the Sentinel-1 derived product and the unpublished outlines), as well as plotting scripts, readily available to readers

For Appendices A&B, I suggest providing the data using a machine-readable format (e.g., CSV, excel files…) instead of making large tables in the PDF file.

Here's also a thought for the authors' reference. For data previously published with a proper license (e.g., CC-BY), I think it is okay to redistribute them with appropriate credits. So, readers can access and download the necessary data all at once and start their analysis.

Section structure: Section 3 presents the derived velocity data set this study plans to contribute. However, the section title ("3 Data") could be confusing as readers don't know whether it is for raw or derived data. Maybe the authors could combine Sections 3 and 4 into a bigger Results section and rename Section 3 as "3.1 velocity datasets," Section 4.1 as "3.2 mosaicked datasets and velocity differences," and so on for a clearer section structure.

Winter scene representativeness: There are two closely related comments.
1. At the end of each subsection in Section 4.2 (e.g., L207-209, L222-224, etc.), the authors compared the interannual changes (essentially the difference between the Sentinel-1 mosaic and the other mosaic from the 1990s) with seasonal variability (the amplitude of summer speedups seen in the Sentinel-1 data) and assessed whether the former is representative for the long-term change. I found this a bit problematic because:
   A. How did the authors quantitatively assess that? Unfortunately, the method is not

mentioned in the paper.

B. There are many areas where the speed difference is below ± 50 m/yr. How did you assess this representativeness of the long-term change while taking care of its spatial variation?

C. If both Sentinel-1 and other mosaics are made using winter velocities, then the seasonal signal should be already canceled out, so there is no need to worry about seasonal variability. Right?

2. In Section 5.1, the authors attribute seasonal variability as an error source of glacier velocities. I think this is only true for the mosaicked products when they are assigned a nominal date. A single offset-tracking pair gives us the average velocity between the acquisition period. We know the actual date for this measurement well, and there is nothing to do with seasonal variability as an error source.

Yes, the errors include the difference between the measurements and the actual velocity on the nominal date for a mosaicked velocity product. However, the amount of this error should be significantly smaller than the amplitude of the seasonal variability if the nominal date is in winter. Again, there is a lack of quantitative discussion about this error source in the paper.

Because of the issues and challenges above, I have started thinking about whether it is necessary to assume that winter mosaics should represent annual velocity patterns. One more reason is that summer speedups can increase the annual average significantly (e.g., Figure 12e). Since the summer velocity from the 1990s cannot be derived from the SAR data due to surface decorrelation (L96-97), we may never know how representative the winter mosaics are to annual velocities. Can't we just say they are winter mosaics (with a reasonably calculated uncertainty range regarding the nominal date) and leave the readers to decide how they use the data?

Minor comments and small points

- To promote equality, diversity, and inclusion in the InSAR community, COMET and WinSAR have released a joint announcement. They suggest changing the terms "master" and "slave" to "primary" and "secondary," see https://comet.nerc.ac.uk/about-comet/insar-terminology/. I recommend the authors use this new practice and address the old terms in the manuscript, such as in L81-82 (and potentially in other lines).
- For glacier outlines and front position, it seems that the authors used previously published data AND created their own. I would suggest that the authors add a subsection in Section 2 and reorganize the text to describe this dataset and relevant digitizing methods for better clarity. (Maybe also for better open-data practice, see my major comment #1.)
- How are winter average velocity and annual average velocity calculated? How did you take care of the data gap while calculating that (e.g., Figure 10b)? It should be worth writing down the associated equations and rules explicitly.

Table 1: the multi-looking parameter for azimuth makes sense to me (e.g., 4.5 m × 12 = 54 m for

JERS-1), but I can't figure out the math for slant-range spacing (e.g., 8.8 m × 4 ≠ 55m). The same issue also appears for tracking template size and tracking step. Are there some incorrect numbers?

Figure 1: Would be helpful to put regional names in one of the panels.
Figures 2-6: I suggest using a different colormap for panels a and b because they show non-diverging quantities (flow speed). Besides, there is a speed level rendered as near white, which is indistinguishable from no-data values. For panel c, I would mask the part of the color bar between -50 and 50 m/yr since the panel only shows the change that goes beyond ± 50 m/yr.
Figure 13: The x axis does not serve any purpose for visualizing the data. A boxplot might be a better way to visually present the y-axis distribution of each group here.

Appendix A: What is the percent column?
Appendix B: "Long" and "Lat" should be changed to UTM X and Y. (Better to put the information about UTM Zone in the heading too.)

Section 2.1: Readers may find it hard to reproduce the work by solely using the steps described here. Did you follow a workflow that has been described in detail in any previous papers? Or was that a standard or default workflow for offset tracking using Gamma (with more information in the user manual)? The workflow seems classic to me, and to address my questions above, it would be helpful to explicitly say that in this section or guide readers to look for the parameters in the provided XML files.
Section 2.2: (1) It would be great to spell "3/4-pass InSAR" as "3- or 4-pass InSAR" to avoid ambiguity. (2) Since you mentioned different passes, can you specify which "pass" Dowdeswell et al. (2008) and Nuth et al. (2019) used for their data?

L50: This is where the concept of using "winter scene pairs" first appears without further explanation or justification. Since all of the derived velocity products are between October and May, I would use "…by compiling the best results over several years…" and keep the entire winter-scene discussion in a later section.
L66: maybe replace "radar speckle" with radar scatterers, or remove that word? To me, radar speckle is an observation (interference pattern), not a physical substance/feature like crevasses and debris.
L89: needs a formal citation for the data from ViewfinderPanoramas.
L125: Is this data from the Dowdeswell or Nuth paper?
L144: a ")" is missing.
L145: What is the resampling method for making these 100-m grids?
L161: To mask out means to hide or conceal – maybe use "glaciers were clipped" or "unglacierized terrains were masked out" instead?
L161 and 166: Can you justify this priority?
L209: Should be Figure 9 instead of Figure 11?

L277: Do you consider changing the saturation level of Figure 8c? (And perhaps 5c and 6c since most glaciers have a speed change of > 100 m/yr and are saturated.)

L329-330 & 464: For the numbers cited here, did the originating velocity products have the same internal measures (e.g., matching window size, post-processing filter, etc., cf. L322-325)? What exactly are these uncertainties? (1- or 2-sigma when measurement is assumed normally distributed, or an absolute range for a non-normal distribution)?

L386: should be "397 m/a (-0.76%)"?

L402: should be "1189 m/a (-4.0%)"? Please double-check the other numbers presented in Section 5 – there might be others I didn't find

L483-486: This sentence seems irrelevant to the goals of the manuscript and the contributing dataset.

L487-489: This sentence seems to be a bit off-topic since I could not find any downsides of using Sentinel-1 IW data in this paper.

L596: needs to fix the reference format.