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.

We thank the reviewer for the positive and constructive comments, highlighting the usefulness of our ice surface velocity products.

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.

We welcome the long list of constructive suggestions of the reviewer and will improve the manuscript in the indicated direction as much as possible. Please find below our specific replies to the major and minor comments.

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of this paper if authors can make these additional data (especially the Sentinel-1 derived product and the unpublished outlines), as well as plotting scripts, readily available to readers.

The focus of our manuscript, as also indicated in the title, is clearly on the historical velocity data set. Those are indeed the datasets that, after quality check, were stored on the Pangaea repository. On the other hand, Sentinel-1 is still a running mission and ice surface velocities are continously computed. Therefore, rather than a repository such as Pangaea, we think that an ad-hoc developed project to provide automatically generated products would be more appropriate. This is however beyond the scope of our project. Reference to other projects providing automatically generated products such as Golive, ITS-live and the FAU-Glacier Portal is given in our paper. Regarding the unpublished outlines, we plan indeed submission to GLIMS.

Our datasets of ice surface velocity in the Eastern Arctic from historical satellite SAR data are available for all image tracks as original research data in vector format (comma-separated values file .csv) with metadata information in extensible markup language format (.xml) and additional GeoTIF files of the three-dimensional ice surface displacement maps and as velocity mosaics in GeoTIF format. Datasets in GeoTIF format can be readily ingested and displayed by any GIS package (e.g. the popular open-source GIS package QGIS). Most software packages are also capable to import text-delimited files such as csv. Further, the csv can be converted to a shapefile for viewing the dataset in geospatial software (e.g. ArcGIS, GRASS, QGIS). Regarding plotting scripts readily available to readers, we think that for this kind of tailored analysis the conversion should be rather left to the users of the data because we cannot anticipate which kind of analysis and filtering they are interested in.

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.

The tables, prepared in Word, can be easily translated into another machine-readable format such as Excel. Depending on the journal's policy, we can make Appendices A&B available as pdf or other formats.

Here's also a thought for the authors' reference. For data previously published with a proper licence (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.

Thank you for the suggestion. This is for instance the case for the past Svalbard ERS-1/2 InSAR maps.

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.

Agreed, we will rename Section 3 from Data to Results, include new sections as "3.1 Velocity datasets", "3.2 Mosaicked flow velocities for specific periods" and "3.3 Long-term variability of ice surface velocity".

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.

We are unsure what the referee means with "quantitatively assess that"? (a) interannual changes, (b) seasonal variability or (c) difference between (a) and (b)?

For (a), as indicated in Section 4.1, difference maps between the 1990's and 2020/2021 velocity data were computed for all study regions only where the 2020/2021 Sentinel-1 ice velocities are larger than 50 m/a.

For (b), as indicated in Section 5.1, we computed the differences between annual average ice surface velocity and winter (October-May, blue) and summer (June-September, red) averages for many glaciers and statistically analysed these results. In this analysis, a linear trend was not subtracted from this comparison, although there are some signs of changes. Figure 12 graphically summarises the percentage differences between winter and summer velocities compared to annual averages for the selected glaciers over the four study regions.

As far as (c) is concerned, our statements in Section 4.2 are indeed not quantitative:
→ "Over Novaya Zemlya the inter-annual changes of winter ice surface velocity between 1998 and 2021 exceed seasonal variability (Figure 11) and can be considered a significant representation of the long-term variability of ice surface velocity over this region."
→ "Over Franz-Josef-Land we did not detect any clear sign of destabilisation and the inter-annual changes of winter ice surface velocity, which exceed seasonal variability, can be considered representative of the long-term variability of ice surface velocity."

→ "Over Severnaya Zemlya we detected two glaciers with clear signs of destabilisation. Also for these, however, the inter-annual changes of winter ice surface velocity, which exceed seasonal variability, again represent the long-term variability of ice surface velocity rather well."
→ "Over Svalbard, the long-term variability of ice surface velocity from a mosaic computed over short-time intervals from several years is much less representative of yearly averages than over the three other study regions, even without taking into account that over this region a large number of glaciers underwent surging events in recent years."

The reviewer is right in finding this a bit problematic, because, on one hand, it is indeed not quantitative and, on the other hand, we also noticed that the discussion about seasonal variability is not yet introduced in Section 4.2 and will only be discussed in Section 5.1 We will take into consideration this point in the revision of the manuscript, rearrange the discussion about the comparison between interannual changes with seasonal variability, and also provide more quantitative assessments (including a description of the applied methodology).

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?

Taking into considerations our tracking errors as indicated at ll. 329-331 ("Our SAR-derived velocities have uncertainties of ±20 m/a for JERS-1 (Strozzi et al., 2008), ±40 m/a for ERS-1 (Dowdeswell et al., 2008), ±10 m/a for ALOS-1 PALSAR-1 (Paul et al., 2015) and ±20 m/a for Sentinel-1 (Strozzi et al., 2017)) and our ERS-1/2 InSAR errors as indicated at l. 120 ("In most cases errors are assumed to be smaller than 7 m/a"), we cannot study changes for areas where the speed difference is below ± 50 m/a. Hence, we cannot analyse the representativeness of the long-term changes for these areas. This remark will be included in the revision of the paper.

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?

Correct. But consider that for JERS-1 and ERS data we were forced by the sparse availability of historical satellite images in time and space to group 7-year (1991-1998) velocity products under the assumption of constant winter velocities. The Sentinel-1 analysis was then used to characterize the uncertainty of such an approach, i.e. to infer the quality of the winter data compared to annual means and the potential variability of winter velocity over several years. This point will be better clarified in the revision of the paper.

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.

Agreed, we will better specify that attributing seasonal variability as an error source of glacier velocities is only true for the mosaicked products when they are assigned a nominal date, but not for the single offset-tracking pairs.

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?

Agreed, we will revise the paper by avoiding to mention the representativeness of winter mosaics compared to annual means. We will nevertheless keep the Sentinel-1 time-series to let the readers decide how they use the data and to study the potential variability of winter velocity over several years.

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).

Agreed.

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.)

The Svalbard glacier outlines will be submitted to and available from GLIMS after some final edits. The front positions were adjusted only for a small number of glaciers (indicated in the manuscript). We don't think that a subsection specifically describing glacier outlines and front position is necessary, also considering that the focus of our study is on historical velocity data.

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.

For every point on every glacier we strictly computed the annual average of the ice surface velocity and the winter (October-May, blue) and summer (June-September, red) averages. We don't think that providing the associated equations and rules explicitly is needed.

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?

The InSAR Multi-Looking, the Tracking Template Size and the Tracking Step are indicated in ground-range geometry (e.g. 8.8÷sin39×4 = 55m). This will be better explained in the revision of the paper.

Figure 1: Would be helpful to put regional names in one of the panels.

Agreed, we will add regional names in the panels of Figure 1.

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.

Agreed, for panel c we will 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. Regarding panels a and b, we made of course different attempts using different color tables and were quite satisfied with the final maps. Nevertheless, if you can suggest a different color table we will be happy to try it.

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.

Agreed, we will revise the aspect of Figure 13 to better resemble a boxplot.

Appendix A: What is the percent column?

Percent of valid information over ice. We will specifically indicate in the caption of Appendix A what the different columns are.

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.)

Agreed, we will change "Long" and "Lat" to UTM X and Y and specify the zone.

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 2.1 and 2.2 make reference to consolidated methodologies described in many published papers. This is why they are kept general here. Experienced users can understand which processing methods are used and which parameters were applied. More specific details about the choice of the processing parameters for less experienced users can be found in the provided references. In any case, the reference to the workflow will be better specified in the revised version of the paper.

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?

Agreed, we will spell "3/4-pass InSAR" as "3- or 4-pass InSAR" and specify which "pass" Dowdeswell et al. (3-pass InSAR and 4-pass InSAR) and Nuth et al. (2-pass InSAR) 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.

Agreed, we will use "...by compiling the best results over several years..." instead of "winter scene pairs".

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.

Agreed, we will replace radar speckle by radar scatterers. We won't remove that word because when phase coherence is retained over an image pair, then also intensity tracking works fine even if we don't see any crevasses or debris in the radar intensity images.

L89: needs a formal citation for the data from ViewfinderPanoramas.

Agreed.

L125: Is this data from the Dowdeswell or Nuth paper?

This data was processed by one of the authors following the same procedure as described in Nuth et al. (2019) but never published. It is now on https://doi.pangaea.de/10.1594/PANGAEA.938381.

L144: a ")" is missing.

Thanks for spotting this.

L145: What is the resampling method for making these 100-m grids?

Data were geocoded at 100 m resolution with use of the Gamma Software. This will be indicated in the revised version of the paper.

L161: To mask out means to hide or conceal – maybe use "glaciers were clipped" or "unglacierized terrains were masked out" instead?

Agreed, we will use "Glaciers were clipped from land and sea".

L161 and 166: Can you justify this priority?

Taking into consideration our estimated uncertainties (smaller than 7 m/a for ERS-1/2 InSAR, ±20 m/a for JERS-1 offset-tracking and ±40 m/a for ERS-1 offset-tracking) priority was given to ERS-1/2 InSAR over JERS-1 offset-tracking and to JERS-1 offset-tracking over ERS-1 offset-tracking. This will be indicated in the revision of the paper.

L209: Should be Figure 9 instead of Figure 11?

Correct, it should be Figure 9. However, we noticed that the discussion about seasonal variability and the reference to Figure 9 should be moved from Section 4.2.1 because not yet introduced. This will be accomplished in the revision of the paper.

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.)

Yes, we investigated different levels of saturations and were mostly satisfied with 100 m/a.

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)?

Yes, similar parameters. In past studies (see e.g. Section 4.1. Error assessment of https://www.mdpi.com/2072-4292/9/9/947) the uncertainty of the ice surface velocity product is estimated as the mean and standard deviation between the differences from the "true" velocity (i.e. higher-resolution or ground-based radar), if available, with statistical measures of ice surface velocity that are computed over ice-free regions (again mean and standar deviation), or by assuming a precision of 1/20th of a pixel in the offset estimation. This will be better explained in the revised version of the paper.

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.

Thanks for double-cheching these numbers, we will also double-check all other numbers.

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

Agreed, this sentence will be removed in the revised version of the paper.

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.

Agreed, this sentence will be removed in the revised version of the paper.

L596: needs to fix the reference format.

Agreed, this will be done by the revision of the paper.