This paper demonstrates mosaics of winter ice surface velocities for the 1990's over the Eastern Arctic (Novaya Zemlya, Franz-Josef-Land, Severnaya Zemlya and Svalbard) through using the offset tracking approach on historical SAR data. Both the JERS-1 SAR data (primary) and the ERS-1/2 SAR/InSAR data (secondary) are used to generate the 1990's velocities. The authors also studied the long-term variability of winter ice surface velocity from the 1990's by comparing to mosaics derived from ALOS PALSAR in 2008-2011 and Sentinel-1 in 2020-2021. The paper generally reads well and compensate the existing ice velocity products on the knowledge of the ice surface velocity in 1990's. However, we found a few fundamental problems and also suspect the paper in its current form is not fitting the scope of ESSD well. Please refer to our following comments.

Thank you for the positive statements about the quality of the presentation and the usefulness of the ice surface velocity products. Please find below (Comment 1) our reply regarding the suitability of our paper for ESSD.

1. ESSD mainly requests the description, processing methods and validation of the dataset, rather than the data interpretation or analysis. Please refer to the website: https://www.earth-system-science-data.net/about/aims_and_scope.html. The third paragraph is copied here (with the specific line highlighted): "*Articles in the data section may pertain to the planning, instrumentation, and execution of experiments or collection of data. Any interpretation of data is outside the scope of regular articles. Articles on methods describe nontrivial statistical and other methods employed (e.g. to filter, normalize, or convert raw data to primary published data) as well as nontrivial instrumentation or operational methods. Any comparison to other methods is beyond the scope of regular articles.*" This paper in its current form only has Section 3 on the data description, which is not very clear on its complex data structure either. However, the majority of the paper is on the data interpretation or analysis in the field of glacier changes (e.g. frontal line retreat/advance, surge, instability, etc) over the four test sites, respectively, e.g. almost the entire Section 4 and Section 5 are on such glacier analysis. We thus recommend the authors to consider publishing this paper in a regular research journal such as Cryosphere not in a data journal like ESSD.

We agree that this point is open to discussion but we have recently learned that the editors have some flexibility in deciding what is suitable for ESSD. Although we think this flexibility is a good idea, it can lead to inconsistencies and misunderstandings, for example for referees. For example, doi.org/10.5194/tc-16-489-2022 was not accepted for ESSD but doi.org/10.5194/essd-2022-60 and doi.org/10.5194/essd-13-2923-2021 are. All three studies present new glacier datasets and change assessment, but the latter two for ESSD also include a climatic interpretation of the observed glacier changes. In our interpretation such analysis would be beyond the journals scope. In the present study we also describe observed changes, but we do not analyse their reasons. It is a largely descriptive part that does not include any new science to make it suitable for a more topical journal. Also the methods applied are around since decades and would not justify this. On the other hand, a presentation of glacier information from three different points in time without any description of the observed changes would also be strange in our opinion. Moreover, the datasets refer to different periods within a year and velocities vary seasonally, so there is a need to show possible users of the datasets that they are indeed comparable and representative. We thus think this part is mandatory for the description of the dataset.

We also considered publishing the paper in a more topical research journal such as The Cryosphere. However, by taking into consideration the available literature, we came to the conclusion that ESSD is better suited to enhance the value of past ice surface velocities computed after the recent release of historical SAR data by various space agencies. Moreover, the general increase of winter velocities from the 1990's to present along with a retreat of glacier fronts in the Eastern Arctic has already been published previously, although without such a comprehensive coverage, and is thus not really novel. The analyses of the dense Sentinel-1 time series to infer the representativeness of winter ice surface velocities with respect to mean annual value is rigorous, but again, in our opinion, not sufficient for a more topical journal. The major aim of our work is really to present the data of past ice surface velocity to potential user communities and for their applications, complementing the existing ice velocity products with knowledge of velocities in 1990's.

We agree that there are research components in our investigation, but this is the aim of our work as scientists. Nevertheless, in line with the aims and scope of ESSD, we refrained to do interpretation of data. The focus of our manuscript, as indicated in the title, is on the historical data set. The discussion about the seasonal variability of ice surface velocity is considered to infer the representativeness of the winter data compared to annual means rather than investigating a research question per se. Obviously, for this analysis, all aspects of glacier changes (e.g. frontal line retreat/advance, surge, instability, etc.) had to be considered (see Comment 2). We nonetheless agree with Referee #1 that Section 5.2 contains an interpretation of the long-term trends to introduce examples of future use of the data, i.e. quantifying the regional decadal average calving flux and analysing characteristic patterns of time series of ice surface velocities over dynamic instabilities. We think that this short section together with the conclusions can better place our investigations in the general research framework, i.e. to demonstrate interest and utility of our data as explicitly required by the ESSD policy. We are therefore convinced that our manuscript is in line with the scope of the journal. Nevertheless, the title of Section 5.2 will be changed from "Interpretation of the long-term trends" to "Utility of past ice surface velocity data".

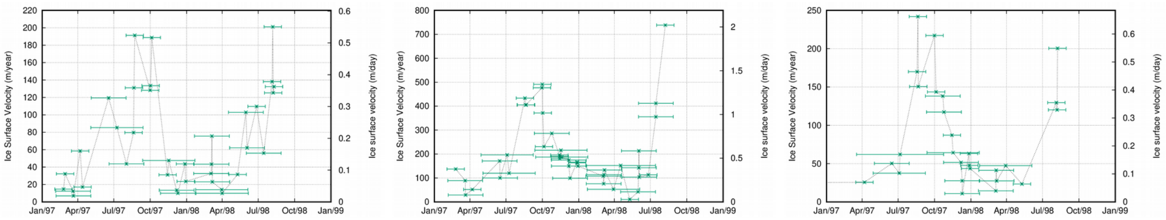
Note also that we had a similar discussion with the ESSD editor in charge of our paper before publication that then led to manuscript revision towards the current structure.

Regarding the formulation of Section 3 see Comment 3.

2. The key idea in this paper is to publish velocity mosaics in 1990's by grouping 7-year (1991-1998) velocity products derived from JERS-1/ERS SAR and InSAR data, and consider the winter velocities do not change much compared to the annual mean velocity, which is claimed by the authors to be justified by using Sentinel-1 time-series velocity products (since 2014). In our opinion, this is not a convincing assumption. Not to mention the paper shows the Svalbard site tend to violate the assumption when including more surging glaciers, even for the other three sites, the Sentinel-1 data that were used to justify the assumption were collected almost two decades later than the 1990's cases. It is thus risky to make this assumption. In contrast, why not just publish a time series of velocity mosaics (rather than a multi-year mean) for each of the four sites, which could have a temporal resolution of 1 year or shorter depending on the JERS-1/ERS data availability? In our opinion, this would be more meaningful to the cryosphere community facilitating more flexible needs of the researchers in this field?

We cannot publish time series of velocity mosaics (rather than a multi-year mean) for each of the four sites simply because the JERS-1/ERS data availability is not sufficient for this kind of analysis. Maybe there is just a misunderstanding here that we need to better describe in the revised paper. We are not grouping 7-year (1991-1998) velocity products under the assumption of constant winter velocities. We were forced by the sparse availability of historical satellite images in time and space to compute longer-term mosaics. The Sentinel-1 analysis was then used for the attempt to characterize the uncertainty of such an approach, i.e. to infer the quality of the winter data compared to annual means. We are in any case not claiming that our approach is justified or give our opinion about how convincing this assumption is. We strictly 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 (see e.g. Figure 13).

Although in many cases the number of available JERS-1 data is very limited, over eastern Austfonna a larger number of acquisitions is available. Over Basin 2, Basin 3 and Basin 7 we observe that summer speed up events were taking place also during the 1990’s, even if present day velocities are much larger. This confirms our Sentinel-1 observations that summer velocities are significantly larger than the annual mean because of the short and intensive summer speed-up events. Winter velocities, on the other hand, are a good representative of mean annual velocities with smaller fluctuations.



(a) (b) (c)

Time series of JERS-1 velocity for (a) Basin 2, (b) Basin 3 and (c) Basin 7.

Please note that this kind of analysis was not possible over other regions, where often only one good JERS-1 data set is available over the entire mission history. This point will be better explained in the revised version of the manuscript. In particular, we will provide the number of available satellite observations per orbit as indicated in the following table.

Numer of JERS-1 acquisitions available from the ESRIN archive per orbit.

Svalbard		**Novaya Zemlya**		**Franz-Josef-Land**	
Orbit #	# Acquisitions	Orbit #	# Acquisitions	Orbit #	# Acquisitions
0305	10	0219	3	0241	1
0306	10	0220	4	0242	0
0307	10	0221	2	0243	0
0308	10	0222	2	0244	0
0309	11	0223	2	0245	1
0310	10	0224	3	0246	0
0311	12	0225	4	0247	2
0312	10	0226	1	0248	1
0313	8	0227	3	0249	12
0314	12	0228	3	0250	13
0315	0	0229	4	0251	13
0316	0	0230	3	0252	17
0317	2	0231	3	0253	14
0318	1	0232	2	0254	12
0319	2	0233	0	0255	12
0320	1	0234	2	0256	14
0321	0	0235	3	0257	13
0322	1	0236	3	0258	13
0323	2	0237	4	0259	14
0324	0	0238	1	0260	4
0325	2	0239	5	0261	6
0326	1	0240	4	0262	5
0327	1			0263	4
0328	6			0264	1
0329	0			0265	3
0330	3			0266	4
0331	2			0267	7
0332	0			0268	6
0333	1			0269	7
0334	0			0270	5
0335	1			0271	1
0336	0			0272	0

0337	1	0273	0
0338	1	0274	0
		0275	2
		0276	3
		0277	1
		0278	2
		0279	4
		0280	0
		0281	2
		0282	0
		0283	3

3. Data description section (Section 3) is not clear on the complex structure of the dataset. It would be great to rewrite it using graphs or tables for the readers' convenience.

We agree that Section 3 can be better organized to highlight the structure of the datasets used. For this purpose we will also make better use of the table in the Appendix, describing the statistics of how many pairs are used for the velocity mosaics, including temporal baselines.

4. The processing methods as described in Section 2.1 and 2.2 are too general. As shown in Table 1, the JERS-1 and ERS data products have different setting of processing parameters, as well as compared to the ALOS and Sentinel-1 data products. You need to clarify why the processing parameters were chosen as such and what impact these different parameters (e.g. template size) would have on the accuracy in the final comparison.

Section 2.1 and 2.2 make reference to consolidated methodologies described in many published papers. This is why they are kept short and more general here. Experienced users should immediately 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, a discussion about the impact of the different parameter settings on the accuracy of the products and how results with different effective spatial resolution can be analysed will be included in the revised version of the paper.

5. Inter-comparison of the dataset with other similar products is required by ESSD. Please see Sect. 3.5 of https://essd.copernicus.org/articles/10/2275/2018/. You seem to compare the 1990's mosaic with ALOS-1 and Sentinel-1 a lot in this paper, but that comparison was only to show the long-term variation. Even though there was a lack of other satellite data back to the 1990's, you still need to validate the product with some contemporary measurements (e.g. GPS) or over static flow and/or rocks. Without such validation, readers do not know how accurate or confident to use this data product.

We made reference to previous published work 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)." With these indications, readers should be aware of the uncertainty of the data products. In addition, the percent of valid information over ice and statistical measures over ice-free regions are given in the metadata information of each dataset. Unfortunately, contemporary in-situ measurements (e.g. GPS) or satellite data back to the 1990's are not available to us. It is frequently discussed in the community if measurements at a different scale (point vs. raster) over a possibly different period (e.g. with DGPS) can really be taken as a 'validation' of the satellite data. However, in the listed references we studied all kind of possible validations, including transformation of the offset estimation precision into a displacement precision, assessment over stable zones, inter-comparison with higher-resolution sensors, and comparison to GPS data (ALOS-1 PALSAR-1) and ground-based interferometers (Sentinel-1). We are confident that this is sufficient to characterize product uncertainty.

Below we list the detailed comments:

1. Line 14: in Appendix A, you only used the Sentinel-1 data at one or two epochs as the 2020-2021 maps. Unlike the abundant acquisitions from JERS/ERS and ALOS, why not include more data spanning the whole year of 2020-2021?
This is reflecting the approach followed for the JERS/ERS and ALOS mosaics, which were also computed by choosing for every point the value of the best available product, i.e. the one with the largest coverage and smaller errors over ice-free regions. This point will be better explained in the revision. More sophisticated methods to compute mosaics might be developed in future work.

2. Line 72-77: better to tabulate these with the numbers for each sensor
Agreed.

3. Line 81-82: the sequence you mentioned the steps sounds like you have geocoding done before coregistration and offset tracking. Do you run offset tracking over already-geocoded images or the other way around?
No, offset-tracking is run in slant-range geometry. Geocoding of the reference image is first done in order to consider the terrain in the co-registration of the slave image to avoid baseline dependent co-registration errors. This point will be better explained in the revision.

4. Line 88: what is the effect of using various DEM with differing resolution?
The DEMs used in our analysis have the same resolution (3 arcsec), but different accuracy. The effect is small and will be indicated in the revision of the paper.

5. Section 2.1: you should clearly mention the product is temporally averaged across few years, e.g. 1991-1998.
Agreed.

6. Table 1: refer to the major comment #4. What is the reason behind the selection of processing parameters? Given the various processing parameters (e.g. template size), how do you analyze the results with different effective spatial resolution?
See Comment 4 before.

7. Line 110: when to use each of 1/2-pass and 3/4-pass?
If a good DEM is available, 2-pass InSAR is preferred. This is the case for the Nuth et al. (2019) data set. 3/4-pass InSAR was used for the Dowdeswell et al. (2008) which was produced earlier. These explanations will be included in the revision.

8. Line 111: the TanDEM-X DEM is almost two decades later than the JERS/ERS missions. What is the effect of using such DEM in your data product? Please quantify the possible error source for the readers' convenience.
Agreed. Potential geocoding errors of JERS/ERS tracking results and displacement and geogoding errors of ERS InSAR results (JERS-1 InSAR is not used to estimate ice surface motion) will be reported.

9. Line 124: median and standard deviation of what? Velocity over stable terrain? How do you guarantee their result over other regions is applicable to yours without actual error analysis or validation?

Median and standard deviation of the velocity refers to stable terrain. This point will be better clarified. We don’t understand the second comment because the results of Nuth et al. (2019), i.e. “their results”, are exactly those we considered in our paper.

10. Section 3: refer to the major comment #3. This section needs to be rewritten with graphs and tables. Since this data product is based on historical not operational satellite data, for each site, you can provide a table of the statistics of how many pairs used for JERS and ERS (how many InSAR pairs were used), temporal baselines, etc. Basically, you could analyze the tables in Appendix A for the readers’ convenience.
Agreed, we will in particular provide the number of available satellite observations per orbit.

11. Line 141: why saturation at 300 m/a?
To highlight the fastest moving outlet glaciers.

12. Line 142: what is phase coherence? Is it just the InSAR coherence?
Yes, InSAR coherence.

13. Line 144: missing “)” ?
OK.

14. Line 145: the grid spacing of 100 m is not matching the smallest template size in Table 1.
It is nearly matching the Tracking Step in Table 1.

15. Line 147: how to define “best”? Please mark those ones in the tables of Appendix A.
The ones with the largest coverage and smallest errors over ice-free regions. The “best” products used for the mosaics are those listed in the tables of Appendix A. This will be better explained in the revised version.

16. Fig.1: what does the “red” color mean?
Red is the GLIMS inventory of glacier outlines. This explanation will be included in the revised version of the paper.

17. Line 161 and Line 166: why do you give priority to one data over the other? Please clarify.
Because of the higher accuracy (see ll. 329-331). This will be clarified.

18. Line 162: any suggestion how to automate the manual adjustment of the outline?
No, more sophisticated methods are not the topic of the paper. The current method was considered sufficent and efficient enough.

19. Fig. 2,3,4,5,6: those figures should move to the relevant places.
Yes, will be done after revision during typesetting.

20. Line 171: as in detailed comment #1, only 1 or 2 pairs of Sentinel-1 data were considered to be 2020-2021 maps, which is not representative of the whole year. Please consider averaging multiple products throughout the year 2020. It is problematic to compare a multi-year mean (sampled at a few epochs) in 1990’s with one or two epochs in 2020. Better to compare the results sampled at the same time of the year, otherwise, it is not clear if seasonal variation plays a role or not.
See the answers regarding major Comment #2 and minor Comment #1 before.

21. Line 178: why using 50 m/a as a threshold?

In particular to remove the noise caused by inaccurate JERS-1 and ERS-1 results over slow moving areas. But also to better highlight the changes over the outlet glaciers rather than residual noise on the interior of the ice caps. This point will be clarified in the revision.

22. Line 180: please do not use “unpublished” as citations.
Agreed. Hopefully, related publications will be soon available.

23. Line 183: the previous ALOS product has different parameter setting. See the major comment #4.
See reply above.

24. Line 186: “masked out” means “removed”. Please reword it.
Agreed. Instead of saying “glaciers masked out from land and sea” we will say “land and sea removed”.

25. Line 202: better to quantify the retreat.
The spatial extent of the frontal velocity increase is clearly visible in Figures 2 and 3, but we will quantify this.

26. Line 205: could the seasonal variation play some role? Comparing the products at the same time of year would be more informative. Also refer to the above detailed comment #20.
The Sentinel-1 image pairs we used in the mosaics are those with the largest coverage and smallest uncertainties over ice-free regions are from the winter season, when short-term velocity variations are small.

27. Line 214: better to quantify the retreat.
Quantifying the glacier retreat over Franz-Josef-Land is out of the scope of our work. According to the ESSD policy we refrained to do interpretation of data and we think that to demonstrate interest and utility of our velocity data a qualitative statement about glacier retreat should be sufficient at this place.

28. Line 217: please provide a citation for this statement.
This is not a statement from the literature, we observed in our data the glacier advanced by nearly 500 m from 2017 to 2019.

29. Line 222-223: as mentioned a few times above, you only used one or two pairs of the Sentinel-1 data and the 1990’s are grouped by averaging a few years’ data. How do you guarantee it is not seasonal variation in 1990’s and/or in 2020/2021?
Of course, we can not guarantee that there were not seasonal variations in the 1990’s and/or in 2020/2021. However, we analysed time-series of Sentinel-1 data to infer the quality of the winter data compared to annual means. We reported a general underestimation of less than 10% of winter velocities compared to annual averages without strong speed-up events as observed in summer. In particular, over Franz-Josef-Land the detected inter-annual changes of winter ice surface velocity exceed the seasonal variability observed in winter with Sentinel-1. We thus considered these changes as representative of the long-term variability of ice surface velocity.

What do you exactly mean by “not detect clear sign of destabilisation”?
It should mean velocity increase by at least one order of magnitude, frontal advance and/or intense crevasses. This will be better described in the revision of the manuscript.

30. Line 228-231: not clear what you exactly meant? Please clarify what type of errors are you referring to and what methods (offset-tracking or InSAR) are you talking about?

Agreed. We will explicitly include in the revision of the paper the methods we are talking about. Tracking uncertainties are already 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)." ERS-1/2 InSAR errors are already indicated at l. 120: "In most cases errors are assumed to be smaller than 7 m/a".

31. Line 233-234: any reason for higher maximum speeds in 1990's? Also, the difference maps in Fig. 5 have the higher maximum speeds in 1990's masked out, due to retreat of the glacier's frontal line. In our opinion, this difference map is not a good graphical representation of the difference in ice flow flux. You might need to consider another graph for better representing the difference. Same problem to the difference maps in other figures.
We agree that in certain cases the higher maximum speeds in 1990's are masked out because for the Russian High Arctic glacier outlines are from satellite imagery acquired between 2000 and 2010 and for Svalbard glacier outlines are from satellite imagery spanning the period 2000-2010. However, for the glaciers that significantly retreated or advanced from the 1990's to the 2000's we manually adjusted the glacier outlines using the SAR backscattering intensity images. We recall again that in our work we refrain to do interpretation of data and these analyses were made to demonstrate interest and utility of our velocity data. In future work more precise quantitative analyses in this direction can be performed (and published in a more topical journal).

32. Line 245-248: same problem as detailed comment #29
See answer for comment #29.

33. Line 268: would be great to show a map of advance/retreat in meters
Of course, but quantifying the glacier advance/retreat is beyond the scope of our work.

34. Line 278-281: this statement by the authors relate to our detailed comment #1, #20 and #29
What we are saying here is not really the possible error introduced by selecting the winter data set with the largest coverage and smaller uncertainties over ice-free regions as representative of one winter season. We are rather directing attention of the readers to the large number of surging glaciers observed in recent years over Svalbard. This point will be clarified in the revision.

35. Fig. 7 and 8: Is Fig. 7c same as Fig. 4c? Similarly, is Fig. 8c same as Fig. 6c? As mentioned above a few times, difference maps are problematic both in space and in time, i.e. masking out speed changes due to retreat, and sampling too few Sentinel-1 pairs as the 2020-2021 maps.
Agreed, in the revision we will better explain this problem and we will also highlight that future work can go in the direction of reducing these problems. Despite the fact that not all technical issues are solved yet, we think with the methods and datasets at hand the multi-temporal velocity products presented here can (and should) be created and shared with the community.