Handling Editor:

I do not want to see you 'chasing' various mountain glaciers as requested by individual reviews but - at the same time - you will need to deal explicitly with issues of quality of velocity estimates for complex glaciers in difficult terrain.

Just an idea:

Choose mountain glacier(s) from among the list of (30?) reference glaciers at WGMS (https://wgms.ch/)? Certainly one to show optimal outcome of velocity techniques, but perhaps also one to show challenges?

Entirely your choice, you will know best how to present your work. Just ideas from this end.

We genuinely thank the Handling Editor for suggesting both ways above to present the data quality description over small mountain glaciers, which is an important part of the data validation. Since the manuscript is already very lengthy, we prefer to add a paragraph detailing all the possible limitations and errors that could be found in the dataset over high-relief small mountain glaciers. Please note that no single small mountain glacier can have all of these limitations, so the elaborated validation over various mountain glaciers (as the editor says "chasing various mountain glaciers") would be incomplete anyway. The newly added paragraph (line 750-761 in the revised text) is also copied here:

> *"Below we provide the implications of the data quality over smaller mountain glaciers. For global processing, autoRIFT uses square search chip sizes ranging 240 m × 240 m to 1920 m × 1920 m. Only a single velocity vector is returned for a single search chip. This means that when a single search chip covers a surface with steep spatial gradients in surface velocity (e.g. shear margins, glacier margins, nunataks), only a single velocity vector will be returned. The returned vector represents the displacement between features that provide maximum correlation. Rock often dominates the correlation for mixed search chips that contain rock. This can cause glacier velocities to be negatively biased for narrow valley glacier and along shear margins. This same issue occurs for features that advect with the glacier (e.g. medial moraines) but present as stationary features. When a search chip samples these features the correlation can be dominated by the advecting moraine that appears as stationary. Lastly, Sentinel-1 is a side-looking SAR that is impacted by layover (i.e., multiple targets at the same range distance from the sensor are overlaid with each other causing their velocities mixed together) and line-of-sight shadowing (i.e., no targets appear at the side of a high mountain glacier facing away from radar resulting in missing velocity data) effects. Both of these issues are magnified in areas of high-relief where mountain glaciers are often located."*

Reviewer #1:

The revised manuscript is substantially improved and brings more clarity. Well done. I have two remaining concerns and a few minor corrections that may be quickly addressed in the final version.

1. Although the authors included a mountain glacier - Malaspina Glacier in the revised manuscript to show that their global dataset is good enough for mountain glaciers. To be honest, Malaspina is not a good representative of most of the mountain glaciers. Most mountain glaciers are small, even the largest mountain glacier from high-Asia is much smaller than Malaspina. It is quite clear that the ITS_LIVE velocities should be diligently used for these glaciers before any scientific use, mainly because of the spatial resolution. This must be addressed in the revised manuscript - A line in the Abstract and/or a few lines in the Conclusions and Outlook.

By the way, this was also raised by another reviewer.

Please refer to the response to the handling editor at the beginning of this response.

2. Another concern is for the scientific community who has to choose which among (e.g. FAU, ITS_LIVE, PROMICE) these data is the most appropriate (e.g. resolution, addressed uncertainty etc.). Therefore a table showing parameters such as Temporal and Spatial Resolutions, Period, Method, Uncertainty, Global/Regional etc. of these data would be a useful addition. I think the authors have already included them in validation, so adding a table like this won't be too much of a task.

We thank the reviewer very much for this constructive advice. We have included the new Table 1 in the revised text, with the screenshot attached below.

Table 1. Sentinel-1 based data product specifics of PROMICE, FAU and ITS_LIVE.

Data Product	Temporal Resolution	Spatial Resolution	Period	Method	Uncertainty	Global/ Regional
PROMICE	24 day (temporally averaged)	grid spacing 500 m (effective resolution: 800-900 m)	2016-present	Normalized Cross Correlation	20-27 m y^{-1} (with in-situ GPS) and 8-12 m y^{-1} (over stable ground)	Greenland only
FAU	6-12 day (no temporal averaging)	grid spacing 200 m (effective resolution: 800-900 m)	2016-present	Normalized Cross Correlation	14.6 m y^{-1} (with TerraSAR-X) and 87.6 m y^{-1} (with Landsat-8)	Global
ITS_LIVE	6-12 day (no temporal averaging)	grid spacing 120 m (effective resolution: 240-1920 m)	2016-present	Normalized Cross Correlation	50-70 m y^{-1} (over rocks and stationary surfaces)	Global

Other minor suggestions:

L235: One image can't be contaminated by temporal decorrelation.

Removed "one of". The revised sentence is

> *"if the input images were largely contaminated by temporal decorrelation"*

L900: Unnecessary comma after Jakobshavn Is.,

Removed comma.

L915 or elsewhere: The texts on Qualitative comparisons are unnecessary and do not add value here - quite clear in the maps already. The time-series comparisons make more sense and this was nicely done.

Thanks to the reviewer for this suggestion. However, we prefer to keep some of these texts as some features in the figures need to be discussed to bring forward or induce the unique characteristics of our data product compared to the others. For example, when discussing Fig. 12, we mentioned "It can also be seen from Fig. 12 that the PROMICE product has been smoothed/filtered spatially as well as averaged temporally by mosaicking all the 6-day and 12-day image pair product within the 24-day temporal resolution window." Although readers can easily notice the strong filtering effect of PROMICE compared to ours, we wanted to emphasize to the readers and thus convey the idea that this phenomenon is because our data product has a much shorter temporal resolution of 6-12 days (without temporal averaging) compared to PROMICE's 24-day (with strong temporal averaging). We thus appreciate the reviewer's understanding that such texts are kept as they are.

Figure13: These comparative plots clearly show the strength of Sentinel-1 based velocities. You may indicate one of the periods when ITS_LIVE Optical data based velocities are not available in order to show this. Just an addition.

Per the reviewer's comment, we added the following sentence both in the text and Fig.13's caption:

> *"Note for each year of 2016-2022, there are periods that ITS_LIVE optical data are unavailable, which strengthens the adding value and competitive edge of using ITS_LIVE SAR data (currently Sentinel-1)."*

L980: date >> data

Fixed

Figure 17: R2 >> R square

All R^2 in figure labels are marked as “R2”, which was also used by other scientific articles. We appreciate the reviewer’s understanding on this.

Reviewer #3:

The paper by Lei et al presents a processing methodology for Sentinel-1 imagery as part of the ITS_LIVE project, which mainly focused on Landsat data in the past. The paper presents in details the processing methodology of the processing chain, and SAR radar processing. My main comment is similar as one of the other reviewers, about the lack of application in mountainous areas. Here most of the applications are provided for Greenland, Antarctica and recently Alaska. Although ITS_LIVE claims to provide data on all glaciated massifs, all applications here are shown on large glaciers (Jakobshavn, Malaspina and Pine Island), with relatively smooth ice surfaces and relatively high flow velocities, compared to the great diversity of terrestrial glaciers. On top of that, satellite image processing in these regions is much more complex, with extremely high topography, shadow /layover problems, slower and smaller ice masses. Hence, monitoring the evolution of surface flow velocity in these areas is much more complex (see Millan et al., 2019), and specifically with synthetic aperture radar. Moreover, the choice of correlation parameters can vary largely depending on the ice object to be examined, which will also impact the spatial coverage and accuracy of the measurements. Thus, the use of velocity data to track glacier dynamics in mountain ranges can be highly risky when taking raw data as presented here. It is notably subject to advanced post-processing, with stacking or temporal interpolation (Millan et al., 2019; Charrier et al., 2021; Derkacheva et al., 2019), in order to extract a meaningful signal from noisy data. Since ITS_LIVE claims to have global coverage of velocities, I think it is important that the authors reconsider their test regions, to be more representative of the diversity of glaciers they want to cover. Thus I suggest keeping Jakobshavn in Greenland (a large and very fast glacier) but adding the Khumbu glacier region in the Himalayas, which is well representative of mountainous terrain issues, and the southern ice field of Patagonia, which has fast marine glaciers, but in areas of high topography and harsh weather conditions. I think it is also important that authors compare their composite map with existing products, in order to see the difference in velocity pattern, which is important for modelers but also for reconstructing ice volumes, or assessing rates of glacial erosions. For that matter I suggest to compare with the velocity data from Friedl et al., 2021, and Millan et al., 2022 and specifically in mountainous regions, where getting precise velocity is the most difficult.

We thank the reviewer for suggesting various test regions that are more focused on mountain glaciers. Rather than selecting a complete different set of test regions, please refer to our response to the handling editor at the beginning of this response for our addition on any possible problems of the dataset over small mountain glaciers. We appreciate the reviewer's understanding and clarify other concerns and/or points raised by the reviewer as below.

First, Millan et al. (2019) does not deal with radar data and we were unable to identify any specific approach presented in that paper that is relevant to the image-pair data being presented in our manuscript. Millan et al. (2019) does however provide a great discussion on optical image correlation and data syntheses, two topics not covered in our study. Charrier et al., (2021) discusses a new method for post-processing of image-pair data, an important topic but not one covered in our study. We were unable to locate Derkacheva et al., (2019) but did find a Derkacheva et al., (2020) that discusses filtering approaches for post-processing image-pair data, a topic that we do not cover in our study. Our processing of the Sentinel-1 data can be viewed as an enhanced version of the approach taken by Friedl et al., (2021; https://essd.copernicus.org/articles/13/4653/2021/). In agreement with the findings presented in our study, Friedl et al. (2021) find that "Overall, we find that both [Landsat-8 and Sentinel-1 velocity] data sets are in good agreement. Absolute velocity differences are generally less than 0.02 $m\,d^{-1}$ over stable ground and slow-moving ice with enough surface features that can be successfully tracked.". Lastly, we did cross-validated our data product with Friedl et al. (2021) as shown in our Fig. 20-21, as well as other similar products (PROMICE and other MEaSUREs along with ITS_LIVE Landsat-8 and Sentinel-2).