

Interactive comment on “Multisensor validation of tidewater glacier flow fields derived from SAR intensity tracking” by Christoph Rohner et al.

Anonymous Referee #1

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General comments

Rohner et al. present a detailed validation study of ice velocity measurements derived from intensity feature tracking using Sentinel-1A and RADARSAT-2 SAR images. The study area is a tide-water glacier in Western Greenland (Eqip Sermia) which showed substantial fluctuations in recent years. The derived velocity maps are compared with field data, derived from 3 GPS stations, a Terrestrial Radar Interferometer (TRI) and repeated UAV surveys, in an effort to demonstrate the accuracy and limits of the method and data sets. Statistics are provided of the outcomes and are visualised in histograms, scatter plots and difference maps. The authors report a good agreement with the in-situ data with minor differences near the calving front ascribed to the characteristics of the different techniques. The ice velocity maps are also compared with Greenland-wide

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products, produced as part of the NASA MEaSUREs and ESA CCI programs, revealing significant differences. According to the authors these products underestimate ice flow, resulting in an underestimation of ice discharge and an overestimation of mass loss when using the products for mass balance assessments.

The subject of this paper, the assessment of inherent uncertainties in satellite data products with field derived data, is a topic of great relevance for quantifying the quality of these products. The combination of different contemporaneously acquired and independent (field) measurement techniques, as described in this study, provides hereby the highest level of validation. The paper also exemplifies the added value for ice velocity maps with high spatial and temporal resolution. From these perspectives, this study is a relevant and welcome contribution to the existing literature and within the scope of The Cryosphere. However, there appears to be a serious flaw in the core data set, i.e. the Sentinel-1 and RADARSAT-2 derived ice velocity maps, which necessitates major revisions and re-analysis of the data. To clarify, the authors claim to have produced ice velocity maps at a spatial resolution of 5x5m. This, however, is not possible with the SAR data sets that they have used (or any spaceborne sensor for that matter). At best, one can produce a velocity map with a 5m grid spacing (which is not the same as resolution), but the required step-size would lead to a ~99% overlap of image patches, making them strongly correlated and they can therefore not be treated as independent measurements. With Sentinel-1 SAR in IW mode, it is likely not possible to achieve a much higher resolution than 100m using intensity feature tracking. Unfortunately, this issue renders most of the subsequent analysis futile and precludes drawing any major conclusions from the study. There are also several other issues and logical fallacies in the manuscript which I detail below.

Specific comments

* As mentioned, the authors claim to have produced Sentinel-1 and RADARSAT-2 derived velocity maps with, in their words, a spatial resolution of 5m (see Pg. 5 & Pg. 14, Ln 13). This is surprisingly at a similar or even higher resolution than the satellite sen-

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sor. As mentioned above, with the selected patch sizes (250mx250m) it does not make much sense to produce maps with a spacing lower than 100m, the resolution is not increased but you basically end up with a smoothed dataset with no extra information and not suitable for for example modelling purposes.

* Pg. 5 Ln 11-12: “for each of the 133 image pairs available between 2014/10/11 and 2018/03/18”. In fact, many more image pairs are available when considering tracks in both ascending and descending direction.

* Section 2.1 & 2.2: The data and methods section seem to miss some essential information required for a careful interpretation of the study results. For example, how was the coregistration performed? Were any ground control data used or only orbit data? Was the SLC data deramped for the azimuth phase ramp? How and when is the burst and sub-swath stitching performed? It appears that offset tracking was performed on terrain corrected geocoded images. Why not in SAR geometry as errors in the DEM are less of a concern and this approach would cause the least distortion as no resampling is required. Also, it is not clear if the SLC data was oversampled before converting to amplitude. Which components of velocity are provided in the output, what assumptions are made (horizontal, vertical, slope parallel?). How is dealt with radar shadow, which could be a concern in steep terrain, in particular when using only ascending data as done here?

* Section 3: Much effort is spent on intercomparing the acquired field data with the satellite derived velocity maps but it seems to focus only on comapring velocity magnitude. Because the accuracy might differ in different directions, it would be useful to do an intercomparison component wise. Also, a concluding section on the final error estimate of the ice velocity maps, integrating the outcome of all the independent estimates, is missing.

* Pg. 9 Ln 8: “which corresponds to an area of about 25×25 m.” Considering the chosen template size used for the tracking it actually corresponds to a much larger

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area.

* Pg. 16 Ln 10: “. Left uncorrected, these introduce biases in the estimated magnitude of surface velocities (Nagler et al., 2015). “. Did the authors apply any such corrections? If not, what is the estimated bias introduced by this?

* Section 4.2: A large part of the discussion concerns an intercomparison of an annually averaged version of the '5m' maps with products from MEaSUREs and CCI. The authors find substantial differences near the calving front and margins but also further upstream. These differences are reported as an underestimation of ice flow in the operational products and according to the authors this implies an underestimation of ice fluxes and an overestimation of Greenland mass loss when used for mass balance assessments. These claims are, however, unsubstantiated by the current work for a number of reasons, aside from the issue regarding resolution described before.

Firstly, the reported differences might as well imply an overestimation of the annual product. How is this distinguished? Although the authors mention their claim is supported by the intercomparisons (GPS, UAV, TRI), these only involve short term intercomparisons during a number of episodes in summer when ice flow is usually faster than the annual mean (Pg. 16 Ln 17). Also, looking in closer detail at the GPS intercomparison (section 3.2), there appears to be a systematic underestimation for most of the data points (Figure 5). In particular with Sentinel-1 there are large differences, that seem much higher than the reported 8.7% and are up to nearly 40% (Table 3). In contrast, NASA MEaSUREs reports much better agreement with in-situ GPS (Joughin et al., 2017, 2018).

Secondly, even if for this medium-sized outlet glacier ice flow is underestimated in standardized products, this cannot be generalized into a systematic and substantial underestimation of Greenland ice fluxes, as the authors assert (Pg. 17 Ln 21-30). The nuance seems to be missing here and it appears the paper is overreaching while downplaying uncertainties.

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Thirdly an underestimation of flow velocities on an outlet glacier would indeed lead to an underestimation of ice fluxes, but this would also lead to an underestimation of mass loss in the mass budget calculation as less ice is exported. It is unclear why or how this would lead to an overestimation of mass loss as stated by the authors.

In order to clarify the discrepancies addressed above, it is necessary to better explain the methods and assumptions used, and to check and revise the error estimates. It could be that perhaps different components are compared (e.g. surface parallel velocity vs horizontal velocity). Also, perhaps the authors are not aware that 250m CCI products are available (Nagler et al., 2015).

* In several places throughout the manuscript the authors claim their velocity maps as 'improved' over existing products. I have no doubt that these products can be improved in several meaningful ways, including by increasing the spatial/temporal resolution or for example by correction of ionospheric streaks. But, aside from the 'increased' spatial resolution there does not seem to be any further methodological improvement to warrant this claim. Concerning temporal resolution, the CCI project has also provided time series at high temporal resolution, with temporal sampling up to every 6 days, albeit only for selected glaciers (see: <http://esa-icesheets-greenland-cci.org>). The presented study provides only 12-day maps.

Figures

Figs 4, 6 & 8: Use same colour scale. The high density of the flow vectors in fig 4 & 6 obscures the velocity map.

Fig 9: Most of the data points seem to lie outside of the glacier (on bedrock?), is this data included in the in the intercomparison?

Figs 10-15: Difficult to distinguish between the red and orange lines, I would suggest using green instead.

Fig 14 & 15: The glacier margins where velocity goes down to zero are missing in this

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plot. These shear margins are good areas to show the improvement of the increased resolution.

Figs 14 & 15: The x-axis label mentions 'Distance from start of flowline', I assume 'start of profile' is mentioned as this concerns a cross profile.

References

Joughin, I., Smith, B. E., and Howat, I.: Greenland Ice Mapping Project: ice flow velocity variation at sub-monthly to decadal timescales, *The Cryosphere*, 12, 2211-2227, <https://doi.org/10.5194/tc-12-2211-2018>, 2018.

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Nagler, T.; Rott, H.; Hetzenecker, M.; Wuite, J.; Potin, P. The Sentinel-1 Mission: New Opportunities for Ice Sheet Observations. *Remote Sens.* 2015, 7, 9371-9389.

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