

Dear Authors,

Please accept my sincere apologies for the extremely long review time of your Discussion paper. Reviewer 1 submitted his/her report promptly after I had invited him/her, however, I had large difficulties finding more independent reviewers. I take full responsibility for this delay (and hope that you understand that some of this is not entirely in my hands). I decided to go ahead with an Editor Review. This review will include some of my own comments but also refer to the concerns/answers of the existing review.

Reviewer 1 has raised a number of critical concerns and did not suggest to publish the paper in ESSD. Some (but not all) of the criticism can be traced back to the history of this paper. Nevertheless, publication in ESSD requires a rigorous error analysis and a substantial (and not incremental) novelty compared to previously published datasets. The last point is particularly critical, as the relation between the ESSDD manuscript and the publication in Scientific Reports (Shen et al., 2018) was inadequately described in the initial submission. Some of these shortcomings can be excused, as the papers were likely simultaneously submitted to different journals. However, having a number of near identical paragraphs in both submissions (as revealed by the new similarity report) is a red flag for many scientific journals. As it stands now, I will follow Reviewer 1 and don't recommend publication of the paper.

There is no question that the L8 velocity map provided by the authors was a herculean effort and is an important baseline dataset for many applications in Glaciology. I am also happy to see this done by multiple groups, providing an independent cross-check of the applied methods. Unfortunately, it seems that the bulk part of this achievement has already been accredited for by a previous publication (Shen et al., 2018). If the authors believe they can make a clear case that this dataset is a substantial improvement compared to what has been published by Shen et al. 2018, I invite a response to the criticism raised. Below, I provide some comments/suggestions on how this could be done. However, given that the straight-forward comparison between Shen et al. 2018 and the new product from esd-2018-149 has not been done in the first place, I am not overly confident that this will be successful.

Kind regards,

Reinhard Drews

Line numbers refer to original submission unless stated otherwise.

Similarity between esd-2018-149 and Shen et al., 2018, Nature Scientific Reports

The initial similarity report of esd-2018-149 (Dec. 06) did not come up with significant overlap compared to other publications. However, I requested a new similarity report and now the overlap with the Shen et al. 2018 paper is significant. Flagged paragraphs are for example starting in l.51, l. 81, l. 136 and elsewhere. I understand that both papers cover the same scientific topic and use similar methods, however, using weakly paraphrased but in essence the same paragraphs in both papers is not the way to deal with this. Reviewer 1 is correct in pointing that out. Methods that are similar between both papers should be

succinctly mentioned and outsourced in the new paper with a reference to the older paper. Differences, on the other hand, should be explicitly stated and indicate the improvement over the previous approaches.

Some statements appearing word for word in both papers (e.g. “These velocity data have the highest spatial resolution of 100m achieved to date..”) are mutually exclusive (only one of the two papers can claim the highest spatial resolution).

Again, I understand that these kind of things happen when papers are submitted simultaneously. However, similarity reports which show a high similarity index are a reason for ESSD to reject papers up front. I apologize, that the initial report did not point that out (supposedly because the Shen et al. 2018 paper was just published at the time). If a revision is attempted the significant overlap between both papers must be removed completely.

Novelty of the essd-2018-149 compared to Shen et al., 2018, Nature Scientific Reports

As pointed out above, I see value in multiple groups deriving baseline datasets used by an entire discipline. I, therefore, don't expect necessarily that the authors present significant improvements/differences compared to, for example, the Gardner et al. 2018 dataset. A thorough comparison is required and I appreciate the replies that you have provided regarding this point.

However, the replies regarding the differences between essd-2018-149 and the data presented in Shen et al. 2018 do not yet have enough detail. Examples are:

Section 2.3 Largely reiterates (flagged by similarity report) the processing strategy already outline by Shen et al. 2018. However, the general strategy here should only briefly be mentioned, and differences more highlighted.

Section 2.4 same as above (also flagged by similarity report)

The Methods section would need a dedicated and detailed paragraph highlighting the differences between essd-2018-149 and Shen et al. 2018. I understand that more displacement vectors were used, and also that the post-processing has changed. However, (1) what are the processing changes in detail, and (2) what is their impact on the velocity map presented here compared to Shen et al. 2018? Improvements and differences between both datasets must also be detailed in the results. Your proposed revisions (e.g. l. 616 of revised paper) are not enough to answer this point.

Validation / Error Analysis

Figure 4 in essd-2018-149 and Figure S2 in Shen et al. 2018 seem to show the same information content (but with different colorscales). Is that correct? What additional information is provided here? Inclusion of the LISA error maps in the revise version is ok, but more importantly differences to Shen et al. 2018 must be shown.

Section 4.2: I appreciate your effort of assembling ground-truth data for an external validation, and yes I understand it is difficult to deal with the different timings. However, the comparison with in-situ GPS data has already (in parts?) been presented by Shen et al. 2018 (Supplementary Information I. 97). What additional data has been assembled for validation in this study? This needs to be explicitly stated, and results from the previous study should not be reiterated here.

Minor Comments:

I. 290: I agree with your comment that ENVI binary files are not a proprietary format and can be read by many programs. However, the majority of scientists is probably more used to GTiffs etc. and providing different file formats may increase usage of the data.

I. 344 double reference Leprince et al. 2017

I. 350 stacking reduced random but not systematic errors.

I. 358 explain n in formula (amount of data available for stacking).