

36. Line 329-331: this should be elaborated as the primary content in an ESSD section on error sources. You seem to omit it by only citing a few citations. Questions: 1) are you using the same parameter setting as the citations? 2) even though you might have used the same setting as the citations, the settings across various sensor data are very different as Table 1 shows. How would this error due to different parameter setting propagate to the final differential map? Also refer to the major comment #4.

We agree to increase the detail on error treatments along the lines raised by the reviewers and the Topical Editor.

37. Line 331: QA-IV-1 seems too subjective to be included in a scientific data set.

Agreed, this quality aspect is subjective, but it is only included in very particular and uncommon cases (see "if applicable" in the manuscript), e.g. when only a sector of a frame is available for one of the images of the pair.

38. Line 334: QA-IV-1/2/3 are not found in the tables of Appendix A as the authors stated in the text. Please fix.

QA-IV-2 and QA-IV-3 are found in Appendix A as columns 8 ("Percent (%)") and 7 ("Stdev (m/a)"). This will be better explained in revised manuscript. For QA-IV-1 see answer before.

39. Line 339-340: here you seem to have much more Sentinel-1 data for the short-term variability analysis. Why not including them for the Sentinel-1 2020-2021 mosaic that is used to compare with the 1990's and the ALOS PALSAR one? Instead, you only used one or two time epochs, which is too few.

See Point 1: 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.

40. Line 418-419: should be Fig. 13 not 14 (typo). If more (not two) surging glaciers are included, the winter data variability could further increase with respect to the annual mean. Given the 20-year time difference, it is risky to assume the same behavior of winter data variability especially when including more surging glaciers with several surging events. Also refer to the major comment #2. Thanks for spotting the mistake, it should be indeed Fig. 13. Agreed about the surging glaciers and winter data variability. This point will be better explained in the in revised manuscript.

41. Line 463: "submitted" is an invalid citation

Agreed, this will be changed by the revision of the manuscript when the cited paper will be hopefully published.

42. Line 469: please refer to the major comment #2

See answer of major comment #2.

43. Line 473-475: please rewrite the sentence to make it clearer

Agreed: "We conclude that winter velocities provide a better idea of long-term velocity trends, even if the spatial extent of summer acceleration events that may be affected by long-term changes is overlooked."