The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-223-AC1, 2021

The Cryosphere Discussions

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Interactive comment on "Brief Communication: Ice Sheet Elevation Measurements from the Sentinel-3A / 3B Tandem Phase" *by* Malcolm McMillan et al.

Malcolm McMillan et al.

m.mcmillan@lancaster.ac.uk

Received and published: 26 February 2021

We thank the reviewer for the time they have dedicated to reviewing our manuscript, and are grateful for their comments, which we believe have substantially improved the manuscript. We address each of the reviewer's comments in turn; reviewer's comments are prefixed by an asterisk.

Reviewer 1 Comments

* This "Brief Communication" paper follows validations of the Sentinel-3 over Antarctic performed by McMillan et al. (2019). It uses observation data of the tandem phase of

S3A and S3B to cross-check both missions. The study is well structured, very focused and nicely illustrated. It summarizes that there are no significant differences between S3A and S3B. In principle I agree with that statement, however, some of the analysis made do not totally convince me. In Sect. 3 I would have expected some more data being analyzed and in Sect. 5 I miss the numeric results that prove the statements made. Before publication, I suggest some modifications of the analyses as described below.

As requested, we have now added significant additional content to both Sections 3 & 5. For Section 3, we have extended the analysis to a continent-wide evaluation, generating statistics for $\sim$ 4 million waveform pairs. We have also included a quantitative assessment of the relationship between waveform correlation and ice sheet surface slope. In addition to updates to the text, Figure 1 has been completely redrawn to reflect these new analyses. For Section 5, we have added more quantitative results. Further details are provided in response to the specific comments below.

Specific comments:

* l.38 If the across-track control range is $\pm$1km, why are the shots co-located within $\sim$150m?

We agree that our wording here was confusing. The control range is the *maximum* across track tolerance allowed. Usually, however, the separation is much less, and 150 m was the typical separation distance we found on the track analysed in our original Figure 1. We acknowledge that this was not at all clearly written and – as this text relates to the overall operation of the tandem phase, we have removed the reference to ‘150 m’. This is also more applicable now that we have expanded the analysis associated with Figure 1.

* l.62 I think it is well known that surface topography is a major factor for conventional pulse-limited waveforms and the repeat measurements are supposed to match very well (especially in absence of other factors as temporal changes in surface properties

or track orientation). Nevertheless, this comparison is very important to show how well the Doppler-stacking works in complex terrain. However, in my opinion, such an investigation should include a larger amount of data, not only 8 waveform examples of one track. As you have tandem data for more than one cycle, could you maybe find a quantity for “how well the waveforms of S3A and S3B match” (as maybe a correlation coefficient) and show this quantity on a larger scale (maybe even all over Antarctica)? This would prove that the stacking is reliable all over the ice sheet, not only for this specific location of this track. It would be also very interesting if there are places (e.g. in mountainous terrain) where the waveforms do not match.

We agree that this is an interesting and worthwhile analysis, and adds significantly to the results presented in the manuscript, so thank you for this suggestion. We have now undertaken a continental-scale analysis of $\sim$ 4 million S3A-S3B waveform pairs. We have computed the correlation between each pair, discussed the results within the text, and plotted the spatial distribution so the reader can see the performance in different sectors of the ice sheet. We have also undertaken a quantitative assessment of the relationship between the correlation coefficient and the surface slope of the ice sheet. The new results are presented in this section and also in a redrawn Figure 1.

* Fig. 2a) I suggest using a color with more contrast for the track.

As suggested, we have changed the colour of the track to one with greater contrast.

* l.82 If I understand that right, this assessment combined all measurements from S3A and S3B (of the selected track) to calculate a mean elevation profile and then compared the measurements of each mission separately to that mean profile. This, however, would include a possible small offset between the missions as well and interpret it as random measurement noise. I suggest to do this assessment for the 3 tracks of each satellite separately. This allows to compare the precision of each mission and could also identify a possible offset in comparing the mean profiles.

We agree with the reviewer, and we confirm that this is already the approach we have taken, as described in the text: “We selected a track that crossed above the central part of the subglacial lake and, *for each satellite*, we accumulated consecutive cycles. . . we computed the standard deviation of all measurements *made by each satellite* within 1 km intervals along the satellite track”.

* l.93 Could you provide some numeric results for the non significant differences? This would be an important quantity for an estimation of how significant obtained results could be.

As requested, we have added such statistics to give the reader information relating to the magnitude of these differences.

* Moreover, I understand that this “Brief Communication“ is just an update to McMillan et al. 2019 and that the investigation should, therefore, be similar to this paper. However, the only accuracy result shown here is a single OIB-profile from 2013 as a reference. In McMillan et al. 2019 this profile (I guess it is the same at Dome-C) is the only reference which is studied in the interior (only) and provides a standard deviation below several meters. How well is this profile itself quality checked? For Lake Vostok, there exists a wealth of independent in situ GNSS profiles (https://tc.copernicus.org/articles/11/1111/2017/), covering several years and cross-controlled at many crossover locations. I suggest considering these profiles for such an accuracy assessment.

We agree that the GNSS profiles are, in principal, a very valuable dataset. However, for the purposes of this specific study, we would prefer to keep with the OIB comparison for the following reasons, (1) this is a Brief Communication (as the reviewer notes) and is therefore specifically designed to follow on from the analyses presented in McMillan et al., 2019 (and therefore for consistency we would prefer to keep the same validation datasets where possible), (2) here we are looking at *relative* differences between S3A and S3B, and as such the *absolute* accuracy of the validation dataset it not critical (as long as it is kept the same in both comparisons), and (3) the ground footprint of the

OIB ATM is closer to the resolution of the SAR footprint than a GNSS measurement, and therefore provides a closer like-for-like comparison (i.e. less sensitivity to small scale undulations in the surface topography than a point measurement).

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