

## ***Interactive comment on “Remote-sensing estimate of glacier mass balance over the central Nyainqentanglha Range during 1968 – ~ 2013” by Kunpeng Wu et al.***

### **Anonymous Referee #1**

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In their TCD manuscript “Remote-sensing estimate of glacier mass balance over the central Nyainqentanglha Range during 1968 – 2013” Wu et al. estimated glacier surface elevation and area changes in the central Nyainqentanglha Range between 1968 and 2013. The authors build their analysis on remote sensing data and older topographic maps. The study has the potential to contribute to our understanding of glacier changes in this area, where glaciological mass balance measurements are lacking. Especially, the long time series to the mid 1960s could be of great value.

However, I have the feeling that many parts of the study are just a methodological replication of an earlier study of the authors (Wu et al., 2018). In its present form the

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manuscript does not present any novel concepts, ideas or tools. Having in mind that the methodology is not really new, large parts of the manuscript should be summarized with a reference to the earlier study of the authors. This would certainly increase the readability of the manuscript and the authors could focus on new aspects and interpretation of the data. I further have several aspects concerning the processing and analysis of the data which would certainly involve much interaction of the authors to make the manuscript acceptable for TC.

An idea to increase the novelty of this study would be to process the original aerial imagery (to which the authors state that they have access) with new software solutions to obtain better surface elevations and ortho-images from the mid 1960s (see for example Magnússon et al., 2016).

I will not correct for all mistakes in the English language as there are too many. The manuscript needs to be corrected by a native English speaker before resubmission.

Specific comments:

Abstract: page 1 line 16: please change “those” to glaciers.

Abstract: page 1 line 17-21: this sentence sounds awkward and could be separated into two sentences, one dealing with glacier area changes and one with glacier elevation changes. I further suggest to keep this separation throughout the abstract with a possible synthesis at the end.

Page 2 line 8: typo, hence.

Page 2 line 21-22: please rephrase.

Page 2 line 24-27: what do the authors mean by this sentence? Please rephrase.

Page 2 line 27-31: I think this paragraph is not really necessary in this place.

Page 2 line 29: not only bistatic data is available from the TanDEM-X mission, there are also monostatic acquisitions.

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Page 2 line 37-39: please rephrase.

Page 2 line 11-39: I am wondering why the authors did not include the recent and important study of Brun et al., 2017 in their introduction.

Page 2 line 40-Page 3 line 1: this paragraph could start with: In this study, we... and should be better organized. Here the authors should mention the research questions they want to address and the anticipated way to get there.

Page 3 line 3-4: please provide geographic coordinates here as many readers of TC are probably not familiar with Chinese counties.

Page 3 line 6-23: I am wondering if the authors are aware of the study of Loibl et al., 2014 who give a thorough overview of the study area.

Page 3 line 32-34: please rephrase.

Page 4 line 2-4: rephrase.

Page 4 line 4-6: this statement is quite outdated and should be formulated more carefully or even removed here, see for example Berthier et al., 2018 on this issue.

Page 4 line 10-12: language.

Page 4 line 13: what data points? Please rephrase.

Page 4 line 16-18: good point.

Page 4 line 18-19: I think this sentence is more related to data processing. I suggest to keep the data and processing sections clearly separated or to generate one Data & Methods chapter which includes each data source and the associated processing strategy in the same subsection. I leave this to the authors but it should be clearly structured in any way.

Page 4 line 22: yes, but why? This could be moved to the introduction.

Page 4 line 21-30: so in the end only Landsat-8 data is used? Not clear.

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Page 4 line 29: almost? Quantify or remove.

Page 4 line 34-35: these lines indicate that the authors have access to the original aerial images. So why not using them directly? Did the authors try to process the data? If this is done successfully it would clearly raise the novelty and impact of the study. For inspiring purposes I suggest to read Magnússon et al., 2016 and Korsgaard et al., 2016.

Page 5 line 5-9: this paragraph is not clear to me. Please rephrase.

Page 5 line 25: is this really a new method implemented in this study? It sounds very similar to the one used in a previous study of the authors (Wu et al., 2018).

Page 5 line 36-Page 7 line 8: I am also wondering in how far this processing scheme differs to the one applied by Wu et al., 2018? I think it would be better to explain the differences to Wu et al, 2018 rather than rewriting everything. This applies not only for this section but for large parts of the manuscript.

Page 7 line 12: true, but the cited studies are rather old and there are more recent studies dealing with this topic, focusing explicitly on TanDEM-X and SRTM data (see for example Vijay, S. et al., 2016 and Neelmeijer, J. et al., 2017).

Page 7 line 14-18: this is probably a reasonable assumption, however surface properties could still have been different in both years.

Page 7 line 23-24: was this mean value used for a correction? Not clear. I strongly suggest to read the above studies (Vijay, S. et al., 2016 and Neelmeijer, J. et al., 2017) as radar penetration is clearly a function of altitude and surface properties. The authors need to account for this.

Page 7 line 32-34: I think this sentence can be removed.

Page 7 line 34-36: so how is it calculated then? Is this sentence related to the following section?

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Page 8 line 1-4: I have the feeling that this sentence is rather related to DEM processing than to accuracy assessment. How did the authors find that the accuracy of both DEMs is similar? Not clear.

Page 8 line 10: it might also be interesting to see the sinusoidal relationship between vertical bias and aspect (Page 6 line 36-Page 7 line 3, before and after the correction). Further it would be interesting if any systematic bias is remaining in the off-glacier regions. See for example Neelmeijer, J. et al., 2017 on this issue.

Page 8 line 5-9: true, the elevation difference in off-glacier regions is an important quantity and gives an important insight into the quality of the dataset. I therefore suggest to show the elevation differences in off-glacier regions and do not clip the difference maps (e.g. Fig. 5).

Page 8 line 24-25: is mean glacier size an important quantity (at least the way it is calculated here)? Please explain or remove.

Page 8 line 32: which area? Not clear.

Page 9 line 12-15: language.

Page 9 line 30: not really if error bars are considered.

Page 9 line 31-33: wording.

Page 10 line 29-38: see also my above comments on this issue. There is also an interesting study of Rignot et al., 2001 who discusses differences in penetration depth.

Page 11 line 26-28: and what did they find? Please explain or remove. There is also a recent study of Ji et al., 2018 which might be of interest in this context. I am not sure if Table 7 is really necessary. I have the feeling that it makes things more confusing.

Page 11 line 44: please reconsider the term “mass thinning” and the associated unit.

Page 12 line 10-13: please see my comments above regarding radar penetration depth.

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Page 12 line 18-24: a third reason could be a different solution of filling no-data voids in the accumulation areas. While Zhou et al., 2018 use reasonable assumptions to fill no-data gaps, in this study interpolation artifacts might arise from the interpolation of the TOPO DEM.

Figure 3: I assume that the colored dots represent the median elevation of each glacier, but how did the authors calculate the relative amount of debris cover? E.g. has every glacier with a yellow dot a relative debris cover of 10%? Please explain.

Page 12 line 44-Page 13 line 3: did the authors find such a correlation?

Page 13 line 13-41: I am not a climate modeler, however when looking at Figure 8 I am wondering how well this dataset is resolved in mountainous areas such as the study area and how well this dataset fits to other estimates in the region. I think such points need to be discussed in more detail. Are there any other glacier related studies relying on this dataset?

Page 14 line 5-6: I am wondering on which data basis the authors draw this conclusion, having only two time steps of glacier area estimates (1968 and 2016).

Figure 5: I think that at this scale the reader is not able to detect any details in the elevation change maps. I therefore suggest to additionally zoom in into several subsections which could be shown in a supplement. As mentioned before it would be of great value to also see the elevation changes in the off-glacier regions. I also found the limits of the color bar rather strange. Is 7.69 m really the maximum value? Furthermore I have the feeling that the TOPO DEM has several regions with unreliable interpolation artifacts. As mentioned above, generating DEMs from the original aerial images could possibly improve the results.

Table 5 and 6: here the reader is not able to locate the glaciers in the Figures. There are numbers in Figure 1 which also could be used here to link the tables with the figures. These numbers could further be shown in Figure 5.

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Additional References:

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Rignot E, Echelmeyer K and Krabill W (2001): Penetration depth of interferometric synthetic-aperture radar signals in snow and ice. *Geophys. Res. Lett.*, 28(18), 3501–3504. doi: 10.1029/2000GL012484.

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