

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 #2

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General comments

This work entitled “Remote-sensing estimate of glacier mass balance over the central Nyainqentanglha Range during 1968 – ~2013” by Wu et al. estimates glacier mass balance over the central Nyainqentanglha Range during 1968-2013, using elevation data obtained from topographic maps (1968), SRTM (2000), and TerraSAR-X/TanDEM-X (2013). By measuring two separate intervals, the authors quantify an acceleration of ice loss rates in the region. They also analyze debris-covered glacier thinning relative to clean ice glaciers, and highlight temperature and precipitation trends (at a 0.5 x 0.5 degree resolution) from the Grid-Based temperature precipitation in China V 2.0

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Dataset. Based on these trends, the authors suggest that overall temperature changes are consistent with the changing glaciers.

The geodetic mass balance portion of the study is done reasonably well, although some issues should be addressed, particularly regarding how well the given uncertainty ranges capture the true uncertainty involved with using the historical topographic maps.

The climate analysis provided at the end of the paper is very brief, based on a single climate dataset of course spatial resolution, and thus its robustness is somewhat questionable (see specific comments below).

By quantifying the rates of ice loss in the region over several decades, this study has potential to improve our understanding of multi-decade glacier changes and water resources in an important region. However, a primary concern with this paper is the large degree of overlap with a previously published work covering the same topic in a nearby area (<https://doi.org/10.5194/tc-12-103-2018>). Many aspects of the methodology are nearly identical, except that a 1968 topographic map is used instead of a 1980 topographic map. I recommend the authors summarize all similar aspects, then simply cite their previous work. This will allow the new manuscript to focus on the unique portions, such as the different time intervals and the quantification of the acceleration of ice loss. This will require a significant revision by the authors. However, in its current form I feel the manuscript is too similar to the previous work for publication in TC.

Specific comments:

Page 1 line 26: The uncertainties for the 1968-2000 interval (± 0.05) seem rather small, especially when viewing the vertical error statistics in Table 3, where the standard deviation of vertical error between TOPO and SRTM ranges from 20 to 27 meters. Figure 5a (elevation difference between 1968 and 2000) also shows large areas with significant vertical error over both ice and ice-free terrain, which may be due to interpolation procedures used when the topographic map was originally created. The limits

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of the color scaling (-7.69 to 7.69 meters) also seem too narrow - a wider elevation range should be used in this figure so that larger elevation changes are not saturated at the endpoints of the color bar. Based on these, I would expect uncertainties larger than ± 0.05 . I recommend careful revisit of the uncertainty estimation procedure, to ensure that the results are representative of the vertical error associated with using the historical topographic maps.

Page 2 line 1: The latest studies by Brun et al. (<https://doi.org/10.1038/ngeo2999>) and Zhou et al. (<https://doi.org/10.1016/j.rse.2018.03.020>) are cited later in the paper, but should probably be included here as well.

Page 2 line 40: I am wondering if the authors drew the topographic maps themselves? The wording is unclear here.

Page 3 line 3: Most readers will not be as familiar with the region, thus using county names may not be the best way to describe the location.

Page 4 line 25: Not sure what is meant here by “higher quality images could not be acquired in 2000-2010”. There are several Landsat 7 scenes obtained in the early 2000’s which are cloud-free and available over the region of study (Landsat scene LE71350392001335BJC00 acquired on Dec 01 2001 for example). What is this referring to?

Page 4 line 29: “No horizontal shift was observed”. What is the horizontal shift being measured relative to?

Page 4 line 33: Do the authors have access to the original aerial photographs? If so, it would be useful and interesting to create a figure showing a sample aerial image for some of the glaciers. Also, later in in the paper, it states that Corona satellite images were used to estimate the uncertainty of glacier outlines (Page 10 line 5). Were the Corona images used in the creation of glacier outlines as well? Please clarify exactly how the aerial photographs, topographic maps, and Corona images were used

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to derive the glacier outlines.

Page 5 lines 5-9: The logic in this statement is unclear to me. How are the $\pm 0.8\%$ and $\pm 3.0\%$ values derived?

Page 5 lines 25-34: A more detailed and clear description is required to understand how the glacier centerlines were derived. In what way are the glaciers divided into two polygons? Also I do not understand how and for what purpose the derived centerlines are compared with high-resolution aerial imagery. How are the uncertainties of 6 and 7.5 meters obtained? Perhaps a figure helping to illustrate this process would be helpful.

Page 8 line 9: Regarding the statement “probably overestimate the uncertainty of the larger sample”. What “larger sample” is being referred to here?

Page 8 line 13: How do the decorrelation lengths factor into the uncertainty estimates? An additional equation showing exactly how they are used would be helpful.

Table 3: Using MED as an abbreviation for mean may be confusing, as MED is commonly used to abbreviate median.

Page 8 Line 33: How were debris-covered portions of the glaciers delineated?

Page 9 line 14: The magnitude of this length change seems extremely large. Is this a lake-terminating glacier? Can the authors show images of the glacier in 1968 and 2016 to confirm this? On another note, it is difficult in this paper to determine which glacier ID corresponds to which glacier in the figures. For example, the numbers in Figure 1 should correspond to the numbers in Table 6, as currently this does not seem to be the case.

Page 13 line 13: It is interesting to see the 0.5×0.5 degree temperature and precipitation trends derived from the Grid-based China v 2.0 dataset. However, it seems rather tenuous to base conclusions on a single climate dataset at very coarse spatial resolution over a region of extreme mountain topography. As the authors summarize in the

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paragraph starting on page 13 line 14, different climate datasets give widely varying results. The authors have chosen this particular climate dataset to base their conclusions on - is there sufficient reasoning for why this dataset is more accurate than any of the other cited ones? Some justification is required for using this climate dataset instead of others. In turn, more details regarding how the actual trends were computed would be helpful. Did the authors calculate the trends, or are they from prior studies? Overall, while this section provides a good overview of a particular climate dataset, it may not be robust enough to attribute glacier changes.

Page 13 line 27: Where are the weather stations located to derive this climate dataset? Were any high altitude stations used which are located nearby the glaciers?

Page 13 line 28: How were the temperature and precipitation trends derived?

Page 13 line 37: Figure 8 does not show the climate changes separated into the 1961-2000 and 2000-2013 intervals. Do you have any data suggesting that the warming rate has increased after 2000?

Figure 7: It would be nice to see the locations where each photo was taken on the map image of the glacier (in panel a).

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2018-90>, 2018.

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