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The Cryosphere Discussions

Interactive comment on "Glacier changes and surges over Xinqingfeng and Malan Ice Caps in the inner Tibetan Plateau since 1970 derived from Remote Sensing Data" *by* Zhen Zhang et al.

Anonymous Referee #2

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In this article, Zhang et al. document glacier changes in the Xinqinfeng and Laman Ice cap, inner Tibetan plateau, from 1970 to 2018 from means of various remote sensing data. They examine glacier area changes, mass changes and velocity of the glaciers and report several glacier surges during the period of observation. While the observation have a certain value in a previously poorly documented region, the analysis and interpretation lack clarity and objectives, which makes the reading of the article extremely difficult.

Major comments:

In general, I agree with the comments provided by referee #1 and in particular:

1. The article lacks a scientific question and a logic throughout the text. Currently it is essentially a report of observations and the scientific value of these observations is lost in a lot of details. The structure of the paper should be revised and the text significantly reduced in order to provide a concise and clear message. Most numbers discussed in the text are purely informative and should be summarized in tables, if really important to answer the scientific question of the article, rather than enumerated. Section 5.3 (discussion) suddenly contains methods (around p 10-L12-17) and results (p9-L18 until the end of paragraph) that should be moved to the appropriate section and introduced.

2. The whole discussion about the dependence of the area/mass changes with topographic characteristics (essentially aspect) seem anecdotical. The sample size is relatively small (77 and 59 glaciers) and once divided into 8 aspects represents less than 10 glaciers per sample on average. All the spatial variability is basically dominated by individual glaciers behavior and in this sense the discussion does have much scientific interest. I recommend to remove this discussion along with figures 2,4, 6 and 7 to focus on other more valuable aspects of the paper.

3. The justification of the data and methods used is often unclear. The authors try to provide a picture as complete as possible of the changes affecting these glaciers but this gives a general impression of scattering and not enough exploitation of the available data and in-depth analysis. For example, why focus on two dates of ASTER data when 19 years are available. What do the generated results bring compared to the observations of Brun et al (2017), who provide at least two time period (2000-2008 and 2008-2016)? Similarly, the analysis of the glacier velocities lacks some depth. This is of course partly due to the fact that GoLIVE observation are only available since 2013, but over such a limited area, the exploitation of Landsat data to look at velocities over the entire time period would be interesting. Alternatively, regional datasets are and/or will soon be available (see for example Dehecq et al., 2019). Moreover, if I recall correctly the GoLIVE data are generated from 16-64 days image pairs. How is

the annual velocity showed in figures 8 and 9 estimated? Is this just one velocity field or an average of all pairs in a year? What about the seasonal variability? What is the uncertainty of the observations? Please explain.

4. The elevation change results raise some concern. First, while the C-band radar penetration is discussed, the X-band penetration is ruled out very quickly. The assumption that X-band penetration is negligible (made for example in Gardelle et al., 2012a referenced in the study), has since then been quite criticized and X-band penetration in snow/ice has been shown to reach several meters in high altitude and dry conditions (Leinss et al. 2015; Dehecq et al. 2016, Abdullahi et al., 2018), which is most likely the case in your study area in February (SRTM-X) or March (TanDEM-X). A penetration of several meters is not negligible over a period of 2-3 years as represented on your figures 8c and 9c and also similar to your estimated C-band penetration (Figure S1). This should be taken into account in the uncertainty estimate and the interpretation of these results. Second, the elevation changes shown on figures 8 and 9 show a very large spatial variability with an amplitude over 100 m and an elevation gain in the glacier accumulation zone (blue curve panel 8c) that are highly suspicious. The time periods of a few years discussed seem too small compared to the apparent uncertainty of the observations. Third, the elevation change maps on Figure 5 show some suspicious patterns of elevation gain/loss particularly in Malan and for the historical period. As pointed out by referee #1, more quantitative measurements of the suitability of these data for elevation change analysis (off ice statistics, distribution with altitude/slope etc, see for example Gardelle et al 2012 a/b or Girod et al 2017) must be provided and the uncertainty related to the seasonality of the observation must be discussed (Gardelle et al 2013).

Minor comments:

- p1-L14: "with heterogenous variations" -> be more specific

- p1-L19: I think a "per year" is missing in the area change values

- p1-L31: "Bhutan, Nepal and Spiti-Lahaul" are part of "the Himalayas", so both groups should not be considered as separate.

- p2-L16: "glacier balance heterogeneous sub-regions" -> This part does not make sense. Maybe "aggregation of sub-regions with heterogeneous glacier balance conditions"?

- p6-L20-21 : The reference to Nuth & Kaab should probably be after "altimetric shift" rather than "differences between the DEMs".

- p7-L27: Until the end of paragraph. This section should probably be removed (see major comment 2)

- p8-L19-23: Same here.

- p8-L24-27: What is the interest of a mountain range average velocity? You bring up the difference in slope to explain the difference in average velocity, but what about the ice thickness? This paragraph says either too much or too little.

- p8-L30: the time periods mentioned in the text don't match the time periods on the figures. Please explain or rephrase.

- p9-L3-9: The variability and uncertainty of the velocities are huge compared to the signal. Similarly, for the elevation change (figure 9). I really don't find that the results provide sufficient evidence for a surge. Please provide better evidence of it or remove this paragraph.

- Figure 8 and 9: If I recall correctly, GoLIVE velocities are derived from 16-64 days image pairs. How do you estimate your annual velocity? Is each curve extracted from just one pair or an average of several pairs? If so, which ones? Please explain.

- p9-L12-20: this whole paragraph is redundant with the Table and could be much more concise.

- Table 2: Remove column 1970-2013 as it is essentially redundant with the other

results. In general, the tables 2-4 could probably be reduced to the most important information.

- Fig 2: What do the numbers on the radial axis mean?

- Fig 5: The legend font should be increased a lot! Maybe by combining all legends into one. The color scale should be improved, maybe by using a logarithmic scale, as a change of +/-4 m/yr is huge and the scale masks probably large noise in the accumulation areas.

- throughout the text: surged glacier -> surging glacier; advanced glacier-> advancing glacier

Additional references:

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Dehecq, A., Gourmelen, N., Gardner, A.S., Brun, F., Goldberg, D., Nienow, P.W., Berthier, E., Vincent, C., Wagnon, P., Trouvé, E., 2019. Twenty-first century glacier slowdown driven by mass loss in High Mountain Asia. Nature Geoscience 12, 22. https://doi.org/10.1038/s41561-018-0271-9

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https://doi.org/10.3390/rs9070704

Leinss, S., Wiesmann, A., Lemmetyinen, J., Hajnsek, I., 2015. Snow Water Equivalent of Dry Snow Measured by Differential Interferometry. IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing 8, 3773–3790. https://doi.org/10.1109/JSTARS.2015.2432031

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