Reply to Reviewer #2:

We thank the reviewer for the time and efforts she/he spent reading our manuscript and providing constructive suggestions and advices. We apologize that we could not submit revised manuscript for a long time. We have revised our manuscript and please find below the referee's comments in black font and the author's responses in blue font.

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.

Reply: Thank you very much for your valuable suggestions. And this article has been further modified by your guidance. We hope this revised manuscript will meet the case. The following are a few answers to some questions.

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.

Reply: Thank you very much for your valuable suggestions. We reworte the introduction to set a scientific goal. We haver revised more in our manuscript, and deleted some contents in section 5.3.

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 pape.

Reply: We agree with you and we removed these in our revised manuscript.

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.

Reply: Thank you very much for your valuable suggestions. We tried to give more detail analysis using more data in our revised manuscript. We downloaded 2000-2018 ASTER DEMs (AST14DMO product, see in supplement) and used these to analyze the penetration depth of C-band radar. In fact, there are little DEMs which meet our need. The DEMs for 2014 and 2018 which have better relative qualities, therefore, we used those in surging glacier analysis. As you mentioned, 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). Because the DEM of 2014 deriving from ASTER due to lower contrast in snow cover. It is diffcult to estimate the elevation change through two ASTER DEMs. Brun et al (2017) provided a much more comprehensive way to processing a time series of DEMs. We also compared our result with their result. Their results showed as Fig.5d (Fig. 4d in revised manuscript) and have too many data voids in the glacier accumulation zone although their ASTER DEMs was proecessing using Ames Stereo Pipeline. In our revised paper, we used Dehecq et al.'s dataset (ITS_LIVE), we also supplented some velocity data deriving from Landsat images in our revised paper. You are right. GoLIVE data are generated from 16-64 days image pairs. We extracted from 16 days and 32 days image pairs in whole year and calculated the average amount. In our revised paper, we used ITS_LIVE data. We have revised in our manuscript.

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).

Reply: 1)Thank you very much for your valuable suggestions. We added the other two method for estimating the C-band penetration (see Section 3.4 and Reply to Reviewer #1), and we also added dicussion for this (see Section 5.1). Seasonality and seasonality correction were also seen in Reply to Reviewer #1. We agree with you, it is not negligible for X-band radar penetration over a period

of 2-3 years as fig. 8c and 9c. However, the elevation difference at surging glacier was often dozens of meters, the uncertainty can be neglected.

2) As you mentioned, 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). Because the DEM of 2014 deriving from ASTER due to lower contrast in snow cover. There have no more data of good quality. The ASTER DEMs for 2014 and 2018 which have better relative qualities.

3) more quantitative measurements of the suitability of these data for elevation change analysis were seen the Table 3 and the supplement.

Minor comments:

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

Reply: Done.

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

Reply: Thanks very much for pointing up this error. We revised it in our manuscript.

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

Reply: We agree with you. We revised it in our manuscript.

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

Reply: Yes, thanks. We revised it in our manuscript.

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

Reply: Thanks. We revised it in our manuscript.

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

Reply: Done.

- p8-L19-23: Same here.

Reply: Done.

- 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.

Reply: We agree with you. We deleted it in our manuscript.

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

Reply: We deleted it in our manuscript.

- 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.

Reply: We give more evidence in our revised paper.

- 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.

Reply: You are right. We extracted from 16 days and 32 days image pairs in whole year and calculated the average amount. In our revised paper, we used ITS_LIVE data. We have revised in

our manuscript.

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

Reply: Done.

- 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.

Reply: Done.

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

Reply: Glacier area, but we forgot the glacier number. We have deleted this Figure.

- 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.

Reply: Done.

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

Reply: Done.