Reply to Reviewer #1:

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 and colleagues document glacier changes of Xinqingfeng and Malan ice caps for the period 1970-2000 and 2000-2011/12. Using a combination of radar and optical sensors, they investigate glacier mass and area changes of approximately 640 km2 of glaciers. The novelty of this study relies on: 1- to document glacier mass changes for the period 1970-2000 for the Malan ice caps and 2- to report seven previously undocumented glacier surges. The authors investigate the influence of the glacier aspect on the pattern of mass change and discuss the potential mechanisms triggering the surges.

While the data presented in this article are of interest, they cover a limited area (just as a reference, Zhou et al. (2018) reported glacier mass changes for more than 8 800 km^2 of ice for the same period 1970-2000 in a single article). Additionally, there are numerous imprecisions and paragraphs with limited scientific interest in this article, which weaken the article's potential to attract a certain audience. The text is sometimes hard to understand due to severe grammatical issues. The article has also methodological issues, which need to be clarified. The level of precision is often not well chosen: for example, very precise numbers are quoted in the introduction, where the general context is expected, and very few details are provided in the data and methods sections, where they are expected. In the results and discussion sections, I suspect that the authors discuss differences that are not significant because of overlapping error bars. Overall, this paper is far from publication, it needs to be completely rewritten in a more concise and precise way. These issues need to be carefully addressed before resubmission.

Reply: Thank you so much for your valuable suggestions. We are really sorry for severe grammatical issues in our original draft. This revised manuscript was edited for proper English language, grammar, punctuation, spelling, and overall style by one or more of the native English speaking editors at NativeEE. And this article has been further modified by your guide. We hope this revised manuscript will meet the case. The following is a few answers to some questions.

Major comments:

1- In my opinion, this article needs to find a real scientific question to be answered, instead of reporting scattered observations. What is the goal of this study? Is it to document long time series of surge activity? Is it to document mass balance changes from 1970-2000 to 2000-2012 in a previously undocumented region? Without a clear focus, the paper reads as a fact report and not as a scientific paper. The introduction has to be completely rewritten, in order to lead the reader to the scientific question addressed by the article.

Reply: Thanks, we agree that this article needs a real scientific question. The principal aim of this study is to fill a knowledge gap of detailed studies for glacier changes in this region are scarce. A further aim is to improve knowledge of glacier change anomaly, such as advising and surging. We have rewritten our introduction in an attempt to lead the reader to the scientific question addressed.2- The authors should better justify the choice of the remote sensing data they used in this study. For instance, what is the interest of the GoLive data, which cover only the period 2013-2018, when the

authors investigate longer term changes? Is there a way to extend this time series by adding Landsat imagery?

Reply: We are sorry for this. In our revised article, we supplemented some data for a longer period, such as ITS_LIVE data (1985-2018, but we cannot open the data of 1985 right, we used this dataset for 1986-2018) which extend the time series of GoLive data.

3- Serious methodological concerns: a- About the topographic maps: these specific maps are not commonly for geodetic mass balance, and they are also not publicly available. Consequently, the authors need to demonstrate in a quantitative way the suitability of these data to investigate glacier area and elevation changes. The minimum expected details are: off-glacier map of elevation changes, percentage of voids, parameters used for the transformation methods (and how many GCPs where used? Which residuals?). I am very surprised to see almost complete maps over Malang ice cap, when ASTER data on fig. 5d show large voids, pointing towards low contrast surfaces that were likely challenging to map for the aerial survey of 1970 as well. The authors need to discuss this issue.

Reply: The topographic maps were acquired from State Bureau of Surveying and Mapping of China (SBSMC, not CMGS, we are sorry for this translation error). And the contour lines except one map (we are sorry for this data gap because that SBSMC have not given us) were also acquired from SBSMC. Coordinate was transformed by SBSMC which they using national GCPs (they cannot proivide us more detail because of national security policy) and the quality meet the need of Technical specification for coordinate conversion of geodetic control points for surveying and mapping industry of the People's Republic of China (CH/T 2014-2016). The original contour lines of the topographic maps were derived manually through analytical plotter, and the accuracy of these were strictly controlled and verified according to photogrammetric Chinese National Standard (GB/T12343.1-2008, 2008). The digital contour lines were from SBSMC, which were produced and inspected in strict accordance with photogrammetric Chinese National Standard (GB/T12343.1-2008, 2008). We converted these lines to Grid DEM by the method of Triangular Irregular Network (TIN) which produces the higher accuacy with meeting Chinese National Standard (GB/T12343.1-2008, 2008) and reflect the ture mountainous terrian much better (Wang et al., 2014). Our elevation difference results did not exceed the range of ±100, therefore, there was no data voids in the difference between TOPO and SRTM. Off-glacier map of elevation changes have been provided in our revised paper, and distribution of elevation differences on off-glacier also have been provided in our supplement. Maybe low contrast surfaces were likely challenging to map for the aerial survey of 1970 as well, therefore, we discussed this issue in our revised paper.

b- About the gap filling method/outlier filtering: the authors do not explicitly write how they filter outliers and/or how do they deal with incomplete maps of elevation changes when calculating the mean elevation changes. See McNabb et al. (2019).

Reply: Our elevation difference results did not exceed the range of ±100, therefore, there was no data voids in the difference between TOPO and SRTM. The elevation difference in the areas of layover and shadow with low coherence (<0.3) for TSX/TDX was not accurate and exclude in our study. Then there are only a little data voids (about 1%) for elevation difference between TOPO or SRTM and TSX/TDX. In our study, we calculated the mean elevation difference of the nonvoid pixels without considering the data voids. We also compared different method for interpolation followed by McNabb et al. (2019), please see section 5.1.

c- The glacier area changes are not taken into account into the glacier mass balance calculation. As the area changes are limited, I do not expect large differences compared with the current estimate, but it is worth calculating the mass balance in the best possible way. See Fischer et al. (2015) for a methodological description.

Reply: Thanks! We agree with you. We have taken glacier area changes into account into the glacier mass balance followed by Fischer et al. (2015). And we added this method to our revised paper. However, the new results changed little (only the reuslt of one glacier has changed, i.e., -0.34—>-0.35, see Table 6).

d- Seasonality and seasonality correction: the climate regime is not described with enough details, and it is not very clear. When are the accumulation and ablation seasons? The TanDEM-X/TerraSAR-X/SRTM data were acquired in FebApril, whereas the TOPO_DEM data were acquired in Oct-Dec. The authors need to correct for this difference and/or justify why the do not do it.

Reply: Both snowfall and melt mainly occur in summer (From June to September), i.e., glaciers in XM are summer-accumulation type glaciers (Wang et al., 2003; Xie et al., 2000), no obvious glacier mass change occurs between the months of winter (Liu et al., 2019). Consequently, we assumed that no seasonal variation occurred Oct-Dec and FebApril in our study. We added this explanation in our revised paper (see section 3.1 and 3.4). We also discussed seasonality impact on glacier mass change results (see section 5.1).

e- SRTM penetration correction. The X-band penetration depth is likely not negligible (unlike it is stated on L11 P4), but the difference between the C and X band penetrations can be approximated by the method described in the article. However, this is an underestimation of the C band penetration, which likely biases the 1970-2000 estimates. This should be kept in mind when comparing the two periods.

Reply: We agree with you. However, it is difficult to determine the X-band penetration depth in our study. We added the other two method for estimating the C-band penetration. One method is followed Wang and Kääb (2015) (c.f. Berthier et al., 2016), we linearly extrapolated the time series of the elevation from ASTER DEMs (AST14DMO products, Table 1S), which corrected by three-dimensional (3-D) coregistration (Nuth and Kääb, 2011) using SRTM DEM as the reference DEM off-glacier, to reconstruct the glacier topography in mid-February 2000. Then, the penetration depth of C-band radar signal over XM glaciers was generated by subtracting the SRTM DEM from this reconstructed DEM. The other method is is comparison bewteen ICESat GLA 14 footprints during 2003-2004 and SRTM DEM as described by Kääb et al. (2012). We used the elevation change rate between the footprints acquired in 2003 and 2004 to linearly extrapolated to mid-February 2000. We compared these results with Zhou et al. (2019) and found that these results have a little difference (see Table 2). The result of Wang and Kääb (2015)'s method was slight larger than ours, which might be resulted from the X band radar penetration. In our study, the SRTM elevation was roughly refers to the state of glaciers at the end of the ablation period for 1999, the slight X band radar penetration could be offset by the slight seasonal change. And there are too many data voids (~40%) in the result of Wang and Kääb (2015)'s method and the insufficient spatial sampling of the ICESat measurements in the other method. Therefore, we used the first method to estimating the C-band radar penetration. See section 3.4. We also discussed SRTM penetration correction impact on glacier mass change results (see section 5.1).

4- Uncertainty analysis. Eq 3 is not reallyclear to me. If the corrections of Nuth and Kääb (2011)

and Gardelle et al. (2012) are applied E_{med} should be zero by construction (otherwise it would mean that there is an offset between the two DEMs). Additionally, on P6L29, the authors wrote "of the stable region for each altitude band (50 m)". If they calculate Emed and σ for each 50 m elevation band, which value is retained in eq. 3? Similarly, the way Neff is calculated is not very clear. As the glaciers have very different size in their study area, I expect a much wider range of values for the uncertainties as the later depend strongly on Neff (which is proportional to the glacier area) than the values reported in Table 4. These values need to be carefully checked and the authors should provide a range for Neff. P7L21-23: why is the uncertainty in area changes always ±0.03% a^{-1}? Please check.

Reply: Yes, E_{med} should be zero under ideal circumstances. However, the residual errors still existed in our results and caused uncertainty for the elevation change of the glacial region. Thus, we estimated the uncertainty of the glacier elevation change through that of offglacier region. I am very sorry for no making eq.3 clear and N_{eff}. The parameters of Eq.3 were calculated for each 50 m altitude band. N_{effi} is the effective number of observations at off-glacier region, i.e. the number of included noglacier pixels for each altitude band. These contents have been changed in our revised manuscript. We are very sorry for some error in calculating the uncertainty in area changes at different period (we divided the same time period), we revised it in our paper. However, the uncertainty at Malan and Xinqingfeng were the same due to rounding off (Malan: 0.034~0.03; Xinqingfeng: 0.027~0.03).

5- Comparison of changes between glaciers with different aspects and between the different periods. In the results and discussion, the authors report differences between the different categories of glaciers, but they should make sure that the error bars do not overlap. They should keep in mind that they investigate only a small sample of glaciers.

Reply: We agree with you. We removed comparison of changes between glaciers with different aspects in the results and discussion.

Specific comments: P1L14: "heterogeneous variations" -> meaning not clear to me

Reply: We are very sorry for unclear words. We want to express the meaning is glacier changes heterogeneous. We corrected it in our revised paper.

P1L15: "there are limited processes available to understand" -> not clear to me. Do you mean that there are no explanation about the heterogeneity?

Reply: Yes. We are very sorry for unclear words. We removed these words in our revised paper because our paper did not explain this heterogeneity in detail.

P1L20: "there was" -> "had"

Reply: Done.

P1L18-21: this sentence should be split into two sentences.

Reply: Done.

P1L21-22: is this sentence useful in the abstract?

Reply: No, we removed this sentence in revised manuscript.

P2L3: I would say that the presence of a "Karakoram anomaly" is not much debated. However, you can insist on the fact that its extent is not completely clear, in particular in the inner Tibetan Plateau.

Reply: We agree with you, and we removed "although these results have been debated" in this sentence.

P2L23 and everywhere else: "surged glaciers" -> "surging glaciers"

Reply: Done.

P3L4: the goal of this study should be stated here in a clear way.

Reply: Thanks for your suggestion. We have stated the goal of our study.

P3L9: “terminal” -> “terminus”

Reply: Done.

P3L16-20: the climate setting description is too short and cannot be based only on one field campaign of two month.

Reply: We add some climate information in our revised manuscript.

P3L27: why is the J-46-134 different from the others?

Reply: We are very sorry, but we donot know the specific reason. SBSMC have not given us the digitized contour lines of the J-46-134 that SBSMC have transformed coordinate from Beijing1954 to WGS1984. Although we have the papery topographic map, we have not GCPs in order to transform coordinate from Beijing1954 to WGS1984.

P4L2: more details needed on the quality assessment of these maps.

Reply: We give more details in our revised manuscript and supplement.

P4L16: what is the average penetration value?

Reply: 3.69 m. More details are available table 2.

P5L16-17: GCPs were used?

Reply: No.

P5L26: I don’t understand “changing”

Reply: This was changed to “the calculation of the area change”.

P6L20: this is in contradiction with paragraph 3.3. How was generated the TSX/TDX-SRTM difference in the end?

Reply: Changes in glacier elevation from 1999 to 2011/12 were calculated using the D-InSAR, changes in glacier elevation from 1970/71 to 2011/12 were calculated using DEM differencing between TOPO DEM and TSX/TDX DEM.

P6L29: give values of sigma for each DEM difference.

Reply: We give it at Table 3 in our revised paper.

P7L2: 30 m is inconsistent with the value of 12 m from P5L11.

Reply: We revised it in our revised manuscript.

P7L5: I do not understand the method here

Reply: We are sorry for making it unclear. The overall uncertainty of the DEM difference was calculated by the glacier area weighted average of $E_{\Delta Hi}$ for each altitude band. We revised it in our revised manuscript.

P7L8-9: the radar penetration is a systematic bias. Here it is treated as a random error, which is not ok.

Reply: We agree with you. The radar penetration accuracy includes the uncertainty of the differences between SRTM X and C and the possible slight penetration of the X-band radar beam. However, it was impossible to evaluate the radar penetration accuracy. It was assumed that the possible slight penetration of the X-band radar beam is within this uncertainty range.

P7L10: “km” -> kg

Reply: Done.

P7L11: Bolch et al. 2017 -> Huss 2013

Reply: Done.

P7L16-17: repetition from section 2

Reply: This is the glacier status in 1970/71, section 2 is that in 2006.

P7L21-24: the P7L22: add “insignificantly” before “decreased”

Reply: Done.

P7L24 and everywhere in the text: “advanced” -> “advancing”

Reply: Done.

P7L25: carefully check that the differences are significant before interpreting

Reply: OK, the glacier area was stable after 2013 and more area loss before 2000 according to multiphase images.

P8L1-6: are these differences significant?

Reply: We have deleted this content.

P8L8 and everywhere: inconsistency between “decrease” and “-3.50”. I suggest to stick to “elevation changes” and “mass changes” to avoid these inconsistencies.

Reply: Done.

P8L18-19: are the mass balances significantly different?

Reply: We think the mass balances are significantly different. If the depth of X-band penetration was not zero, the mass loss during 1970-1999 might be much lower. We also found faster mass loss after 1999 as showed in Fig. 5.

P8L19: “different slopes” -> “different aspects”

Reply: We have deleted this content.

P8L24 and after: could you convert the velocities in ma-1?

Reply: We have deleted this content.

P8L24-27: Here there is the same problem as for the mass balance calculation from the rate of elevation change maps. The velocity maps are likely incomplete and the authors need to explain how they can calculate the average velocity from these maps.

Reply: We have deleted this content.

P8L28: uncertainty?

Reply: We have deleted this content.

P9L3-11: Maybe I missed something, but I am not so sure that the arguments of the authors convince me that Zu Glacier is a surge type glacier: : :The 2013-2018 velocities are very stable, and the front position as well. I agree that the shape of the terminus suggests a surge, but the surge cannot be demonstrated from the data presented here. On figure 9c the authors should plot rate of elevation change instead of elevation changes as the time spans are different.

Reply: We give more evidence in our revised paper. We redrawed Fig. 9 which plot rate of elevation change.

P9L13 and elsewhere: provide a number of digit that reflects the uncertainty “278.4 ± 21.2 m” -> “280 ± 20m”

Reply: Done.

P9L26-27: the sentence is hard to understand, at the moment it reads as if the glacier retreated due to the lack of data!

Reply: We are sorry for making it unclear. We revised it in our manuscript.

P9L27-29: it is hard to know if these thinning and thickening are “normal” or “surge-type” behavior, as they do not show the typical surge signature.

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

P10L2: “We confirmed that Glacier No. 14 surged as a consequence of its advance from 2007-2012” -> I don’t follow the reasoning here

Reply: We think Glacier No.14 surged according to its advance from 2007-2012. Of course, this is just a speculation. We revised it in our manuscript.

P10L4: not clear. A precise reference to the classification is needed here.

Reply: We revised it in our manuscript.

P10L19: this statement does not seem back up by much theoretical background. Consider either elaborating or removing.

Reply: We removed it in our manuscript.

P10L29: about the comparison between the authors estimate and Gardner et al. (2013), the SRTM C band penetration cannot be the explanation of the difference. The authors' estimate should be biased towards too positive values because of the penetration (SRTM is used as a start date for the geodetic estimate), but the opposite is observed with Gardner et al.'s value being less negative.

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

P11L2: "Their estimated trend is more significant than ours" -> "Their estimated trend is significantly more negative than ours"

Reply: Done.

P11L15: I don't understand the reasoning about the turning point

Reply: Our original intention is the transition region from the west to east. In our revised paper, we deleted this.

P11L18-19 and figure 10: this figure does not demonstrate the authors' point, as almost all glaciers have steeper accumulation area than ablation area. If they want to reinforce their argumentation they need to show the profiles of retreating glaciers as well to show a difference between these glaciers and the surging and/or advancing ones.

Reply: We have deleted this content.

Section 5.3: all this section is hard to understand. The authors try to relate general statements to their local observations, but they lack precision and quantification (e.g. "These glaciers have relatively narrow width, so the mass transported from the reservoir area might be limited." -> what to conclude from this? Or "the observed small ice mass losses have not been enough to result in a strong negative mass balance"). In general, I have the feeling that the references, theories and mechanisms discussed are a bit outdated. I suggest to rely on more recent literature and approaches to completely revise this section (e.g., Gilbert et al., 2018; Sevestre et al., 2015).

Reply: Thank you very much for your valuable suggestions. We have revised it in our our manuscript.

P14L2-3: this statement is extremely speculative

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

P14L16: "decreased" is it significant? If yes, write it, if not remove.

Reply: Thank you very much for your valuable suggestions.

P14L19: "lower" than what?

Reply: "lower" than glacler mass loss from 1999 to 2012. We revised it in our manuscript.

P14L27-30: I do not think that the data presented here support this conclusion

Reply: We have revised it in our our manuscript.

Figure 2, 4, 6, 7: I do not think that rose diagrams are relevant representation of the data for such a small sample of glaciers. In particular for figures 4, 6 and 7, how are represented the glaciers with positive area or mass changes?

Reply: We deleted these figures in our revised manuscript.

Figure 3: the quality of the figure is very poor, it needs to be improved.

Reply: We redrawed Figure 3 in our revised manuscript.

Figure 5: off-glacier elevation changes should be shown as a supplementary figure.

Reply: Done.

Figure 8 and 9: elevation differences need to be converted into rates of elevation difference (panels c). The transect should be plot on the d panels.

Reply: Done.