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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 #1

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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 km2 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.

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. 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? 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. 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). 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. 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 Feb-April, 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. 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 4- Uncertainty analysis. Eq 3 is not really clear to me. If the corrections of Nuth and Kääb (2011) and Gardelle et al. (2012) are applied Emed 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 $\pm$0.03%a-1? Please check

the calculation carefully, as this uncertainty should depend both on the glacier area change and the time period considered. 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. Specific comments: P1L14: "heterogeneous variations" -> meaning not clear to me P1L15: "there are limited processes available to understand" -> not clear to me. Do you mean that there are no explanation about the heterogeneity? P1L20: "there was" -> "had" P1L18-21: this sentence should be split into two sentences P1L21-22: is this sentence useful in the abstract? 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. P2L4-14: this paragraph is hard to follow because it mixes different results from specific areas, consider simplifying. P2L23 and everywhere else: "surged glaciers" -> "surging glaciers" P3L4: the goal of this study should be stated here in a clear way P3L9: "terminal" -> "terminus" P3L16-20: the climate setting description is too short and cannot be based only on one field campaign of two month P3L27: why is the J-46-134 different from the others P4L2: more details needed on the quality assessment of these maps P4L16: what is the average penetration value? P5L16-17: GCPs were used? P5L26: I don't understand "changing" P6L20: this is in contradiction with paragraph 3.3. How was generated the TSX/TDX-SRTM difference in the end? P6L29: give values of sigma for each DEM difference. P7L2: 30 m is inconsistent with the value of 12 m from P5L11 P7L5: I do not understand the method here P7L8-9: the radar penetration is a systematic bias. Here it is treated as a random error, which is not ok. P7L10: "km" -> kg P7L11: Bolch et al. 2017 -> Huss 2013 P7L16-17: repetition from section 2 P7L21-24: the P7L22: add "insignificantly" before "decreased" P7L24 and everywhere in the text: "advanced" -> "advancing" P7L25: carefully check that the differences are significant before interpreting P8L1-6: are these differences significant? P8L8 and everywhere: inconsistency between "decrease" and "-3.50". I

suggest to stick to “elevation changes” and “mass changes” to avoid these inconsistencies. P8L18-19: are the mass balances significantly different? P8L19: “different slopes” -> “different aspects” P8L24 and after: could you convert the velocities in m a-1? 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. P8L28: uncertainty? 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. P9L13 and elsewhere: provide a number of digit that reflects the uncertainty “278.4 $\pm$ 21.2 m” -> “280 $\pm$ 20 m” P9L26-27: the sentence is hard to understand, at the moment it reads as if the glacier retreated due to the lack of data! 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. P10L2: “We confirmed that Glacier No. 14 surged as a consequence of its advance from 2007-2012” -> I don’t follow the reasoning here P10L4: not clear. A precise reference to the classification is needed here. P10L19: this statement does not seem back up by much theoretical background. Consider either elaborating or removing. 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. P11L2: “Their estimated trend is more significant than ours” -> “Their estimated trend is significantly more negative than ours” P11L15: I don’t understand the reasoning about the turning point 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. 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). P14L2-3: this statement is extremely speculative P14L16: “decreased” is it significant? If yes, write it, if not remove. P14L19: “lower” than what? P14L27-30: I do not think that the data presented here support this conclusion 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? Figure 3: the quality of the figure is very poor, it needs to be improved. Figure 5: off-glacier elevation changes should be shown as a supplementary figure. 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.

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