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## **Comment on esurf-2021-18: Intensified paraglacial slope failures due to accelerating downwasting of a temperate glacier in Mt. Gongga, Southeastern Tibet Plateau**

Jan Henrik Blöthe (Referee)

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Referee comment on "Intensified paraglacial slope failures due to accelerating downwasting of a temperate glacier in Mt. Gongga, southeastern Tibetan Plateau" by Yan Zhong et al., Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2021-18-RC2>, 2021

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In their manuscript, Zhong et al. map ten medium to large scale paraglacial slope failures ( $\sim 24$  to  $\sim 160 \times 10^3 \text{ m}^2$ ) above the retreating and thinning Hailuoguo Glacier (HLG) that descends from the Mount Gongga massif in eastern Tibet using repeated UAV and satellite imagery. Comparing the mapped failures to data on glacier dynamics, as well as temperature and precipitation trends from a nearby station, the authors contribute to the growing data on paraglacial landscape adjustment. While the body of literature on the topic of paraglacial geomorphology has been growing in the past years, Eastern Tibet, where the present study is located, has not received much attention in this respect. Despite the interesting data set the study offers, the manuscript lacks a clear formulation of a relevant research question, does not explain the applied methodology in sufficient detail, and offers conclusions that are not fully backed by the findings presented. Below, I will elaborate this in a number of general comments, before offering specific comments and technical corrections that hopefully will help the authors to improve their work.

Kind regards

Jan Blöthe

**General comments:**

Overall, the manuscript is well written, but sadly lacks rigorous identification of previous work and the work conducted here. Especially in chapter 3 (Methods and data), the authors need to put more effort into making it very clear, which information has been obtained from earlier studies and how the data produced in the framework of this manuscript was produced.

A very basic and important point that the authors fail to include in their work is a thorough error assessment of all measurements they conducted. This is neither addressed in the methods section, where it remains rather unclear how exactly mapping has been conducted, nor are the errors associated with mapping (based on visual interpretation?) in remotely sensed imagery discussed later in the text. For

Chapter 3.2: Here the authors mainly present details of earlier studies that quantified glacier mass balances changes of the HLG. I would recommend to include these background information into the description of the study area (Chapter 2) and shift the focus of Chapter 3.2 to the method applied here. From the technical description in L158-164, I take that the authors used three digital elevation models (DEMs), namely "*the TopoDEM (1966), the Shuttle Radar Topography Mission (SRTM) 30m DEM (2000) and the ASTER-DEM difference between 2000 and 2016*". While I am familiar with the latter two (the ASTER DEM data is from Brun et al. 2017, include reference here), I am not aware of the TopoDEM (1966). If this refers to the DEMs calculated in Cao et al. (2019), this needs to be indicated here. Moreover, from the technical details outlined here, it remains unclear to me whether the authors only analyzed five profile lines, or calculated full DEMs of difference and used five profile lines to visualize the data. Please elaborate this in more detail and outline, in the case data was only analyzed along five profile lines, how these are encompassing the full variability of surface changes on the glacier tongue.

Furthermore, in L165-174 (still Chapter 3.2), the results of earlier studies that quantified ice flow dynamics are presented. In the final sentence the authors describe a comparison of "*long-term ice flow velocity changes [...], based on three-periods results of 1982-1983 (in situ observed), 2007-2011 and 2014-2018 (SAR satellite derived)*." Where does this data come from? Is this an analysis done by the authors, or does it refer to the studies presented before? As the dates mentioned here do not match the time spans given for the studies cited above, I would ask the authors to either clearly indicate the provenance of these data.

Chapter 3.3: This is a very brief description of how slope movement was quantified, given that large parts of the results and discussion build upon this data. While manual tracking of tie points in repeated imagery can be considered a fairly robust technique, I would recommend that the authors at least try to quantify the error associated with this tracking of tie points. This can be easily achieved by tracking stable surfaces in the vicinity of the slope failures. Furthermore, may I suggest to include the individual vectors for manually tracked tie points in Fig. S2? Last but not least, let me point out that there are multiple software solutions that allow for automated tracking in consecutive imagery, which would result in full 2D velocity fields that might enable a much deeper insight into the mechanisms of the failures (and reveal local variability).

Section 5.1.1: Here the authors discuss the possible preparatory and triggering factors for the rock fall they observe during their study period. The authors might want to further elaborate how the exceedance of a precipitation threshold of 60 mm for a single day in June 2018 is connected to the triggering of a rock fall in October 2018. This is mainly referring to L362-65 and Tab. 3, where the authors argue for a precipitation intensity anomaly, which I find hard to follow given the data. Yes, 2018 has seen one day with more than 60 mm of daily rainfall, i.e. 61 mm (L364). Without giving more details on the exact precipitation values for 2016 that also saw three days with more than 40 mm per day, I do not think this can be seen as an indication for a precipitation intensity anomaly. In my view, it would be worth to look at the antecedent rainfall in the five to ten days before the failure and compare antecedent rainfall statistics between years, especially in a setting with very few days without rainfall. Furthermore, in section 5.1.1 the authors argue for a potential triggering by frost action. It is my feeling that also this remains speculative, as the cold interval the authors refer to here (01-09 October 2018; L369, Fig. S4) happened at least 5 days before the failure that the authors date to 15 October 2018. However, the temperature data for 2018 is unavailable for the days following 09 October, making this link questionable.

### **Specific comments:**

- L35-40: Here the authors gather five modes of response to slope failure, though I have the feeling that mode 5 “paraglacial debris cones and valley fills” is rather the results, i.e. deposit of the processes listed in 1-4.
- L67-68: Not really relevant at this point, as the tourism activity at the site is detailed in L109-117.
- L101-02: In L152-53, this information is from Zhang et al. 2010, please add reference here
- L124: How was the glacial area mapped exactly? As the HLG is debris-covered, a precise delineation between debris-covered ice and the debris-covered surroundings is not trivial. Please elaborate in detail, how mapping of glacier extent was conducted and what the associated uncertainty of this mapping was.
- L124-26: Also for the paraglacial slope failures (PSFs), it is not clear how exactly these were mapped. In L121-24 it is described that using the NDVI, vegetation covered areas were excluded. But how exactly was the mapping of the PSFs achieved in remote sensing imagery. What were the criteria for mapping? Was this mapping field-evidence based and if so, when was field-work conducted?
- L130-34: Again, it remains unclear at this point how PSF boundaries were extracted, what validation means in this respect and based on which criteria manual correction was done.
- L138: I would suggest to state this in more detail. At this point, it is unclear, whether the “mean quality of 0.01 m” refers to the position accuracy of the RTK UAV, or to the SFM output. Furthermore, “+1 ppm (RMS) in XY” is neither clear in this regard. In order to allow the reader to follow and to judge the quality of the data used in this study, I suggest to explain in detail, how the UAV images were processed and what the residual mismatch of their geolocation is.
- L142-43: I take it that the authors co-registered and orthorectified the UAV-derived orthomosaics with the PALSAR DEM, or is the sentence correct that individual UAV

images were used for this? May I ask the authors to elaborate this a bit more, as this is confusing? IN line XX you write that these are already orthorectified?

- L156: The authors compiled a data set here, but I fail to see where this data set is included in the manuscript? Is there a figure or table that shows this compilation?
- L174: In the very brief section following this heading, the "outline change rate" is not mentioned nor explained how this is quantified. Instead, the authors quantify the rate of headscarp erosion. Consider rephrasing the heading here.
- L178-180: May I suggest to elaborate more clearly how the outlines of failures were used to calculate a mean annual retreat rate for headscarps?
- L220-23: The time spans given here in the text are not the same as in Fig. 4. What is the rate between 2000 and 2019?
- L224: Which area are you referring to here?
- L225-26: Where is the data for the lower frequency of PSFs?
- L227: Where does the knowledge of slope material come from? Did you map out slope material distribution during field work?
- L239-44: This description of the rock fall (PSF type A) needs to be refined. In L240 it is stated that the rock fall occurred on a south-west facing slope, while in L244 it is stated that the mass detached from a steep north-facing slope? Is it that the general topography is south-facing and the nice of the detachment faces north? The picture in Fig. 5b, however, does look like the source is also facing the glacier – please clarify.
- L241-42: This is not precise enough. How can a deposit of a rock fall be "450m in height fro the glacier surface" and at the same time, "cover a height of 380 m"?
- L258-59: Please indicate what magnitude is considered small here, as to me "each with a mean area of 750 m<sup>2</sup>" is not clear.
- L262-63: Please try to be consistent in labelling the processes: "Sediment-mantled slopes slide and collapse" vs. "Sediment-mantled slope slide and collapse". In my view, both seem a bit clumsy – you might want to rephrase.
- L263-66: The numbers given here are surprisingly round and do not match the sums of the individual numbers mentioned in Tab. 2. Also, in Tab. 2, there are no errors associated with the numbers given for the area. Please include a statement on the precision of these estimates in the text. This also applies to L310.
- L271-75: This comes as a surprise here and rather belongs to the discussion.
- L276-78: How was the error of 0.04 cm d<sup>-1</sup> quantified?
- L283: How do the authors know that the detectable slope movement began around 2000? Has this been published, or did the authors run additional analysis beyond the 2016-2019 UAV surveys that have been described in the text. Same applies for L296.
- L286: "the landslide has fallen" sounds as if it was a vertical movement that was quantified? Until now, it is my understanding that the authors quantified 2D horizontal displacement.
- L400-02: This comes as a surprise as a) in L361-62 the authors argue that 2018 has seen relatively low temperature. Furthermore, Fig. 9 suggests that 2017 has a data gap in temperature readings.
- L430-38: I am not sure how this is connected to the data and topic presented in this study? Consider removing paragraph or elaborate the connection to paraglacial slope adjustment.
- L444-46: Is this supposed to be a general statement? Also, check grammar.
- L448-50: It is hard to see how the deposition of debris onto the glacier is directly affecting climate. As the authors try to outline in Figure 8, debris cover generated by slope failure might have an effect on glacier downwasting, which in turn can have a tiny effect on the climate.
- L464-70: While McColl and Davies (2013) showed that also failures of similar magnitude as B2 in the present study can deform glacier-ice at the rate of mm/yr (assuming a min. average thickness of ~1 m), the evidence presented here is limited and the discussion of this aspect remains too surficial. It might be a way forward, to present the displacement data in more detail and quantify the supposed narrowing and squeezing effect that is mentioned here to back this aspect with data.

- L474-75: What data is this statement based on? Is there a study that found this increase in debris-flows and flash-floods that could be cited here?
- L482: In L195-96 the authors state that between 2016-2019 the glacier terminus retreated by >150 m with a rate of  $\sim 52 \text{ m yr}^{-1}$ . Which is true?
- L485-86: In your data, type A landslides are limited to one rock fall. Did you find evidence for debris avalanches as well?
- Figure 1: Might I suggest to add the glacier outline for the years 1982/83, as in the text the authors have quantified (or cited) the displacement values for this period (Fig. 3).
- Figure 3: Is there a reason for the black line being thinner in D-D' of the first column? Also, in the caption, replace annual thinning rate with average annual thinning rate, as these have been quantified over multiple years, right? What remains unclear is the provenance of the data shown in the right column. Has this data been calculated from remote sensing data by the authors? If not, add the references to the data to the caption.
- Figure 4: Change "(white line)" to "(blue line)" in the caption, as this is showing the glacier outline, judging from the legend.
- Figure 6: Might I suggest to label the vertical axis of the last column with "displacement velocity"? Slide velocity implies a vertical component, but these are horizontal displacement values, right? Furthermore, in the caption, what does the reference to Qiao Liu et al. imply? Has this data been published before? I cannot find this reference in the reference list.
- Figure 8: May I suggest to give this figure a more detailed caption that explains the arrows and the reciprocal effects? Also, why does Type C have a larger arrow than Type A and B, what exactly does "remarkable glacial debulking" imply? Also check grammar of text box PSF Type A.
- Figure 9: Here it should be indicated over which time window the moving average for temperature and precipitation has been calculated. Furthermore, the reason for and length of the temperature data gap in 2017 should be mentioned in the caption. Also, are Tem\_smooth and Pre\_smooth appropriate abbreviations for a running mean of these variables?

### Technical corrections:

- L130: delete "can"
- L132: "generally suffered less from" (?) instead of "are generally less suffered from"?
- L136-140: very long and in my view incomplete sentence; check grammar
- L166-67: check grammar
- L196-98: please rephrase
- L228-29: broadly coincide with?
- L239-41: "rock fall event [...] occurred around October 15, 2018"? Check grammar
- L246-249: check grammar
- L250: "can thus can be some clearly"?
- L256: delete "are"
- L259-61: please rephrase, not clear what exactly this refers to and how relevant a mean distance to the glacier is in this context.
- L301-04: check grammar

- L309: seasonal snowmelt
- L 310: Numbers in Tab.2
- L335: glaciofluvial sediment instead of glaciofluvially transported glacial sediment
- L362: You should maintain the figure order! Figure 8 has not been referenced in the text and is only done so in L450.
- L361-62: There is no temperature given for 2017 and both 2014 and 2015 seem to be below 5°C, so I don't think you can make this statement
- L373: freezing of water begins?
- L389: delete hosting
- L409-10: check grammar
- L414: check grammar
- L415: uprush? Can this be used in this context?
- L418: HLG
- L441: obvious compared to?
- L448: replace into with onto