Response to 'Comment on esurf-2021-18' from Samuel McColl

We thank Dr. McColl for his very constructive and detailed review which will clearly help to improve this paper by pointing out weaknesses, but also suggesting the respective improvements which will be necessary to be included in the next version of the manuscript. We try our best to provide as many details as possible to better reply to the reviewer. Please find all the details below.

This paper documents changes to the unstable hillslopes adjacent to a retreating glacier using observations from satellite and UAV imagery and field visits. Three main types of hillslope response are documented – rockfall, debris sliding, and gullying – and the authors attempt to explain the occurrence of these with reference to observed changes in the climate and glacier. The paper presents data that is of interest for several reasons; 1) the research context - a relatively understudied type of glacier environment [monsoon temperate, within Southeast Tibet]; 2) several types of hillslope failure mechanisms are documented, adding to the growing knowledge of the complex response of hillslopes to glacier retreat; 3) the authors use these data to make observations on the interactions between hillslope and glacier processes, topical for research on alpine hazards, climate science, and paraglacial geomorphology. While the observations are of interest, the overall purpose and aim of the manuscript is a little unclear from the introduction, without the identification of key research questions tied to the objectives or without a set of hypotheses being developed and then tested. It rather feels like the data was collected for the sake of collecting it in the hope that it might yield some interesting observations (which it does, but the lack of a clear study setup/purpose detracts from the paper's impact). The manuscript falls short of delivering any major discoveries that change the current understanding of landscape response to deglaciation, instead providing a more incremental increase in data documenting the range of responses (which in itself can be useful, but is currently not sufficiently capitalised upon). Other than further confirming the significant role of glacier down-wasting (which is already a very well-established process of hillslope destabilisation), the manuscript is unable to provide much more than speculation on other factors responsible for the hillslope response patterns observed (e.g. long-term strength reduction or frost weathering responsible for the rockfalls observed). On the interactions between the hillslopes and the glacier, these are also rather descriptive without detailed analysis. In my 'specific comments' below I offer some suggestions for how you might address some of these short comings and better utilise your (nice) data to give your manuscript more impact. I hope that the specific comments and technical corrections below will be useful in reshaping this manuscript and enhancing its clarity, focus, and overall contribution.

While a growing body of research focusing on process of paraglacial hillslopes destabilization of the European Alpine, Southern Alps, and Cordillera Blanca (Andes), the representation of evidential findings in the southeastern Tibetan Plateau (low latitude region) is still unknown. We speculated that the recent high frequency of glacial debris flow in SE Tibet may relate to the numerous paraglacial slope failures, which act as debris source of the strongly deglaciated catchments during precipitation rich seasons in this region. To test this hypothesis, we decide to select a temperate glacier for a detailed study of glacier-slope interactions in SE Tibet. However, due to the topographical restrictive, sparsely populated, and cloud-cover, most of the glaciers in SE Tibet are difficult to be observed on the ground and by satellites for the long-term.

Hailuogou Glacier (HLG) is one of the largest temperate glaciers in Mt. Gongga which has the lowest elevation in its ice tongue area. Due to its high visibility and accessibility, it has been well documented and observed since the early 20th century. Based on these data, we could possibly relate the historical and recent glacier changes with the paraglacial geomorphology responds observed during the satellite era as well as in the resent UAV epoch. Studying this process is also of great significance for understanding the causes of frequent glacier-related hazards in SE Tibet. Meanwhile, the HLG is an important freshwater and tourism resource in the east Mt. Gongga. Various geological hazards caused by the hillslope instability have an important impact on the socio-economic development of the downstream. Analysis of existing data is therefore particularly urgently needed.

We agree with your comment that our current study is still descriptive since we had not monitored or modelled the physical mechanics processes of the slope, rock and ice, thus the long-term strength reduction or frost weathering responsible for the rockfalls observed yet could not be quantitatively examine. Thank you for listing suggestions below based on which we hope we could reshape the manuscript by analysing the interactions between the hillslopes and the glacier in more detail and strengthening our contribution by making better use of observed data.

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[1] The introduction needs to do a better job of setting up the objectives of this work. For example, what key research gaps or questions (e.g. in paraglacial hillslope response) is the manuscript addressing? How will your 3 objectives stated on L82-87 help to address these? Why is the HLG study site important/useful for addressing these questions? (I note that on L113-117 there is clearly a hazard motivation for the selection of the HLG site, so perhaps this could be presented better in the introduction as one of the justifications for addressing the key research questions and for the selection of the HLG site).

We will add paraglacial hillslope response in SE Tibetan as the key research gap and other information (e.g., why we choose HLG as the study site?) in the introduction.

[2] The classification of the types of hillslope response could be better justified and used consistently throughout the manuscript: e.g., the description of the three types (A, B, C) are described differently in the abstract than in the conclusion (in which they are also labelled differently, as i, ii, iii). On lines 35-40 five main classes/modes of hillslope response are described from the literature, but it is not clear how the three types (Type A, B, C) described in the manuscript relate to these 5 modes. Also note that Mode (5) paraglacial debris cones and valley fills seems to describe a product of hillslope erosion, not a type/process/mechanism of hillslope response. Debris cones and valley fills presumably can be produced by a variety of mass movement processes. So there is some inconsistency in the 5 modes presented.

We thank you for pointing out this error and we will correct it in the next version. We have changed the way of expression in the conclusion section. If this affects the readers' understanding, we will consider correcting it.

[3] The term Paraglacial Slope Failures (PSFs) is used as a catch-all for the three main hillslope responses that are documented. However, given that the Type C response seems to be focused on headward gully erosion and said to involve fluvial processes (L411), it casts doubt on the suitability

of the term ‘Paraglacial Slope Failure’ to represent this, as fluvial processes are not traditionally considered to be a slope failure process. Perhaps a better term is needed?

[4] Related to the previous comment, I feel that the processes involved in the Type C response are somewhat unclear. Does this response type involve mostly debris flow processes (in which case the term PSF may be OK), or is it mostly fluvial erosion (rilling and gullying)? While I suspect there are not sufficient (temporal) data to confidently identify the processes causing expansion of these gully areas, perhaps there are clues from the deposits they produce (i.e. are the fans below these gullies more typical of fluvial or debris flow process?).

Thank you for your suggestion. As you have mentioned, Type C is an incredibly unique process in HLG. Most of them are based on Type B, and their shapes are different from that of Type B’s (from arc to triangle) because of the strong gully erosion in upstream. In fact, gully erosion also exists in B1-4 (e.g., L303-304), but they are not obvious enough to change the overall shape of the unstable slope and are not classified as Type C. So essentially, Type C is also one of the PSF landforms.

[5] For each section of the Data and Methods (i.e. sections 3.1, 3.2, 3.3, 3.4) I suggest starting with a brief explanation of the purpose of the method, trying to link back to the objectives where relevant. This will help readers to understand why you are doing each part.

We agree that and will add more details on the section of the Data and Methods to help readers to understand this part clearer.

[6] I would like to see some more explanation of how the three response types were identified, i.e. what key criteria were considered (i.e. elaborating on the comment on L228-230). As I understand it, they key criterion for assessing the presence of a hillslope response was the appearance of bare ground (especially sediment?). I have two potential issues/queries with this:

This suggestion is very pertinent. To help readers better understand how these three types of slopes are identified and classified, we draw up a table (Table 1) to show the classification reasons and results of each slope (A, B1-4, C1-3).

Table 1

Types		Sub-types		Classification Standard	Classification Result
ID	Name	ID	Name		
1.0	Rock slope failures	1.1	Rock avalanche	Large-scale, catastrophic rock slope failure	-
		1.2	Rockfall	Local-scale, high-frequency, and discrete rockfalls	A
		1.3	Deep-seated gravitational slope deformations	Extremely slow flows or displacements of bedrock	-
2.0	Sediment slope failures	2.1	Sediment-mantled slopes slide and collapse	Rock mass slides along the shear plane Shape: arc or strip	B1-B4
		2.2	Gulley headward	Strong headward erosion is added	C1-C3

	erosion	based on Type 2.1 Shape: triangle

a) not all bare ground exposed by the glacier will be unstable, so how do you differentiate unstable ground from stable ground (e.g. using signs of disturbance, or a slope angle threshold?), and can you quantify the abundance of stable vs unstable ground?

Indeed, not all bare ground is unstable, and the slope angle difference between the stable and unstable slopes on both sides of the glacier is different to some extent, but they cannot be used to identify them all. In the process of bare ground extraction by NDVI, we found that some areas showed an interannual change in shape (e.g. Type C), while some areas showed a sudden increase in exposure area for a short time (e.g. Type A), and vegetation developed in some areas with the obvious movement of vegetation patches. (e.g. Type B2-3). These become the important basis for us to identify the unstable slope. The temporal and spatial resolution of the data is both higher than the existing DEM data, so it is the key to obtain an unstable slope boundary under current conditions. Finally, we try to quantify the abundance of stable vs unstable ground of three response types in nine study periods between 1990-2020, please see the table below (Table 2).

Table 2

Years	Abundance	
	Unstable ground	Stable ground
1990	83%	17%
2000	64%	36%
2011	54%	46%
2013	51%	49%
2016	59%	41%
2017	57%	43%
2018	58%	42%
2019	55%	45%
2020	56%	44%

b) You state that due to the climate conditions vegetation colonisation is extremely fast in this environment. This presumably means that much of the bare ground exposed in the early part of your study becomes colonised by vegetation by the end of your study, especially for Type B responses which do not necessarily prevent vegetation from establishing on the main body (i.e. vegetation rafting). Does this pose a challenge for the identification of bare ground through time, and if so how do you get around this or how much does it affect your results?

Indeed, vegetation colonisation is extremely fast in this Mt. Gongga. This also makes the area of bare ground in this environment much smaller than the paraglacial environment in the central Himalayas. However, vegetation colonisation is limited by slope, elevation, and time. In four years of UAV images, we observed that the rapid vegetation colonisation mainly affected the bare ground areas in glacier forland after the ice tongue has completely retreated. Therefore, we eliminated this part of the area from the automatically extracted PSFs during classification and their geometry calculation. We will describe this issue clearly in the next version of the manuscript.

[7] L148-149: Please elaborate upon how you calculated the 'mean quality of 0.15 m in XY'. Did you calculate this by withholding a sub-set of your ground control points not used in the georegistration, in order to provide an independent check of georegistration error? It would be preferable to also provide the maximum error or at least a measure of dispersion (e.g. standard deviation), not just the mean. Please explain what you mean by 'successfully occupied positions' or revise this wording to make it clearer what you are referring to.

We used all ground control points in four study periods to calculate the georegistration error, and we will provide the maximum error or at least a measure of dispersion in this section. We will revise this wording of 'successfully occupied positions' to make it clearer.

[8] The five profile lines A-E and their data (ice surface elevation, thinning rates, flow velocity) provide some interesting data on changes to the glacier, but it is hard to see how these data are actually used to help understand the hillslope response processes. It is unfortunate that these data are not more thoroughly used to explore the relationships in space and time between glacier changes and hillslope changes. Although on L452-454 you state 'our analyses show a temporal and spatial component to PSF development' there is no real attempt to combine the data of Figure 3 with the data of Figures 4-7 in any quantitative way. It might have been worth setting up a hypothesis, for example that the rate of Type B movement will correlate with the rate of ice thinning, or that the magnitude of ice thinning would correlate with the growth in size of Type B and Type C responses, and then quantitatively/systematically test these. Such relationships are only somewhat qualitatively/subjectively commented on in the manuscript. Likewise, why were four (and not some other number of) transverse profiles chosen and what was the rationale for their placement – was the hypothesis that thinning and hillslope response will differ depending on distance up-valley of the terminus, or were there differences in terrain type, geology, or some other environmental variable that were being captured in these profiles?

Thank you for your advice, we will add this kind of hypothesis/test to maximize the use of our data.

This is because we want to cover every type of slope and keep a certain distance between the transverse profiles so that the extracted results have obvious differences for comparison, to select the number and location of the transverse profiles.

[9] The glacier velocity data are interesting in themselves but do not seem to be well utilised or particularly relevant to the objectives. What was the hypothesis that was being explored with this, or why would changes in glacier velocity be expected to cause (or respond to?) hillslope processes? On L430-438 we get a sense that the velocity data is being used to infer that the high flow rates have been responsible for a high erosion rate and steepening of the valley flanks. But this is not supported with any data or further context – there are no data to show that the valley walls are steeper than other glacial valleys with lower flow velocities, and there is no comparison made between the rates of hillslope response in the HLG to other locations to explore whether the rates are unusually high and therefore are correlated with a high flow rate. I suggest that unless there is a good case for retaining the glacier velocity data, then it is removed from the manuscript because currently it adds little insight into the hillslope processes observed.

Thank you for your suggestion! We decided to add more detail to make the link between glacier velocity and hillslope processes clearer.

The slowing of the glacier velocity and the glacier thinning corroborate each other. On the one hand, the thinning of the glacier slows down the glacier velocity; on the other hand, when the glacier velocity slows down, the ice flux transported from upstream to downstream is reduced, which accelerates the glacier thinning and ultimately leads to the slope sliding.

[10] There could be more information provided on the rockfalls. At present it seems that only the largest is described in any detail, with the other failures described only in general terms (e.g L253 'We also observe other major rockfalls…suggesting that numerous smaller scale rock falls have occurred in this locality' or L258 'small magnitude rockfalls occur more frequently' Can you present the data for these events – e.g. a freq/mag histogram or table showing their source elevations, when they occurred, and their magnitude, and a map showing location? Presenting these data may help to tease out relationships between the failure patterns and the factors governing them.

Yes, we agree that our description of small rockfalls is slightly lacking. Because of their small size, they can only be identified by comparing annual UAV imagery, but we can't determine exactly when they happened because unfortunately, we have not fixed auto-camera to monitor these slopes during the past years. But we will insert a table in the modified manuscript to present some information we could achieved now such as their aspects, slopes, source elevations, heights, estimated magnitude et al.

The locations of the small rockfalls were noted in two images (Fig 5a and Fig S3) in the current manuscript.

[11] The role of mass movements for producing supraglacial debris (e.g. L45-43) seems to be a theme introduced and returned to several times in the manuscript, but at present the manuscript makes little contribution to this topic. While rockfalls were observed to deposit sediment onto the glacier (e.g. L260, L448), this manuscript is hardly the first to identify the role of rockfall in producing supraglacial material so this is not a particularly helpful finding. Moreover the actual effect that these few documented failures have had to glacier ablation is not in anyway quantified in the manuscript, so as it stands the qualitative observation of supraglacial debris accumulation is not particularly insightful. Therefore, I would suggest that either this aspect of the manuscript is removed, to improve the focus of the manuscript, or this aspect is enhanced. Enhancements could be to:

a) provide more quantitative data on the total areal contributions to supraglacial cover of the rockfalls documented in the manuscript and make comparisons with other studies (i.e. substantiate the statement on L448-450 with data and context).

b) include a more detailed description of the contributions (or not) of the other two types of hillslope response (B and C). To what extent have these processes also delivered supraglacial material to the glacier during the observation period, and if they have been delivering sediment then to what extent has supraglacial sediment delivery by these types of hillslope process previously been documented in the literature, and are your findings consistent with that?

c) there is a nice opportunity to discuss sediment delivery to glacier systems more widely than just supraglacial sediment delivery. You pick up on the fact that some of the Type B failures are deforming the glacier (sensu McColl and Davies 2013), which is a nice observation – to what extent are these failures also delivering sediment subglacially, similar to what was identified by Cody et al., 2020 in the Fox Valley, or are the slopes at your site not engaging this recently-documented sediment pathway? Further, it appears that some of the Type C processes are providing sub-glacial water supply, and therefore presumably these are also delivering sediment to the sub-glacial environment? If so, exploring to what extent these paraglacial transport pathways (i.e. from recently exposed moraines) have been previously documented in the literature would be a good point of discussion.

Thank you for your three very constructive suggestions! We will follow your above suggestions and add the analysis mentioned below in the next version of the manuscript.

We calculated the total areal rockfalls covered is 62216 m^2 (10^3~10^4 m^3); according to Liao et al. (accepted in 2021) the supraglacial debris cover of HLG is 9.747 km^2, as a result, the total areal contributions to supraglacial cover of the rockfalls is 0.62%.

As shown in figure 1 (please see below), Type B and C slopes, often have a deeper contact with glaciers during the slow slide and may deliver sediments underneath the glacier. However, there are still a limited number of materials that are delivered to the supraglacial environment through direct rolled (the sediments are more dispersed compared to rockfalls) and through debris flow (faster changes). Type C slopes can also deliver materials to sub-glacial environment through runoff. Their contribution to glacial sediments is hard to quantify, but it is clear that this process does result in a long-term and steady sediments supply to the glacier.

As you said that Type B 2 failures are deforming the glacier, and at the same time we also observed the delivery of local sediments (Figure 1, B2). In the 718 days from 2016/8/31 to 2018/8/19, two parts of the slope collapsed while sliding, and the collapsed area increased by 94% in total, with an increase rate of 8 m^2 d^{-1}.

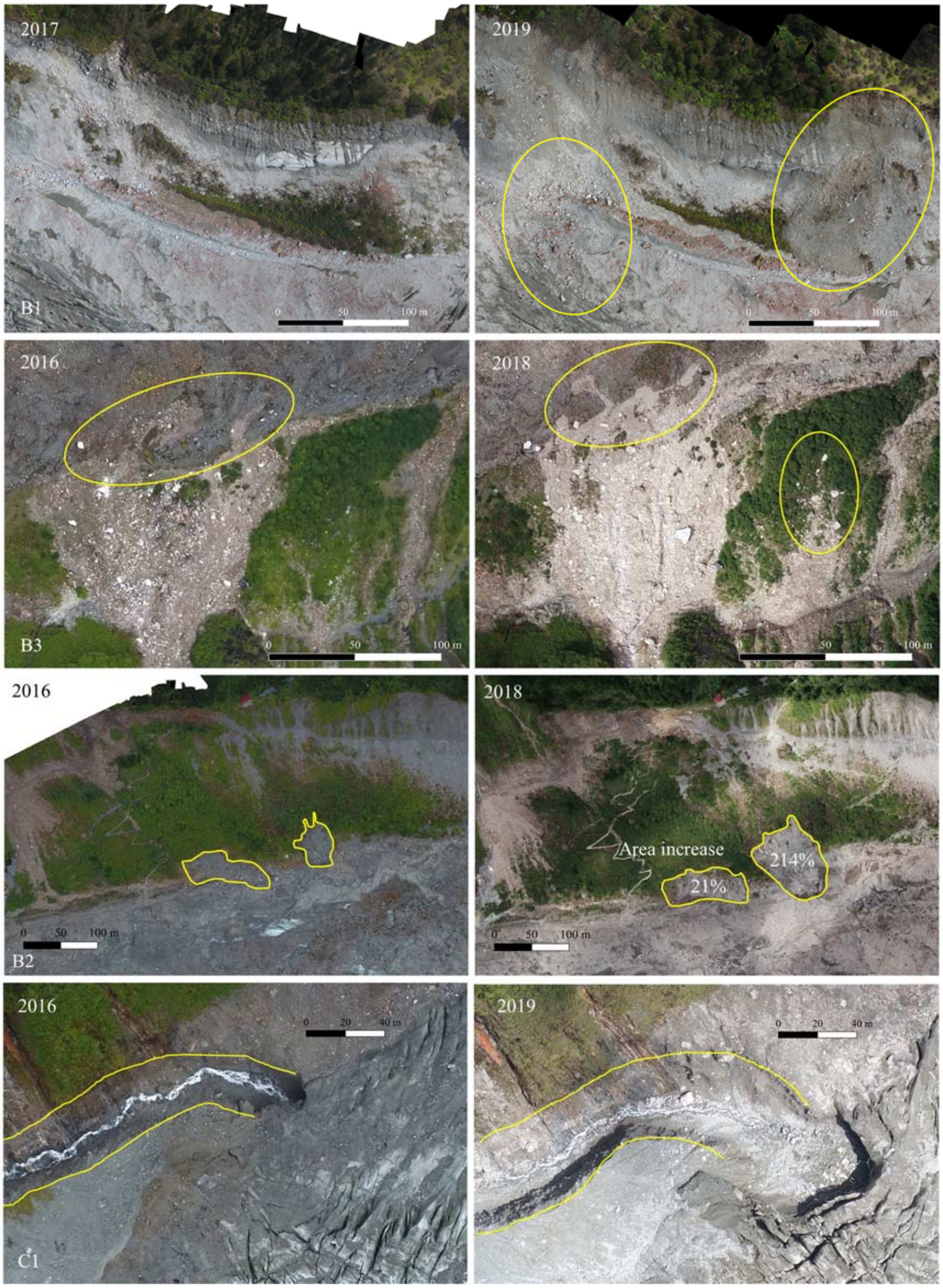

Figure 1

[12] Related to the previous point, on L45 you refer to the role of 'high-frequency, low magnitude PSFs' in delivering a 'considerable volume' of debris onto glacier surfaces. But what about the low-frequency, high-magnitude events (e.g., large rock avalanches) that are well documented in the literature for their role in dramatically changing glacier ablation? Why do you focus on the small high-frequency events here?

Indeed, rock avalanches are well documented in European Alps, Southern Alps, and others. However, we have not observed such an event at HLG (it may exist, but it has not been documented), and the 2018-event is 'rockfall' in terms of both scale of collapse and impact. We will make an explanation in the next version.

[13] L265, L270, & L287. Please describe the process(es) by which the Type B features become larger; e.g. is this through glacier downwasting exposing more of the slope, lateral expansion of the failure mass, or headward expansion from retrogressive failure or degradation of the scarp (e.g. from surface erosion processes)?

Both processes exist! We will add this in the next version.

[14] The observation of nested processes (e.g. gullies developing within Type B failures) is nice but it would have been great to see an analysis of the temporal evolution of these. For example, in Cody et al., 2020 they describe a temporal evolution in hillslope response, whereby moraines initially begin collapsing through sliding and internal deformation, and then later surficial debris flow processes (i.e. gully forming processes) takeover, and eventually both processes relax as the slope adjusts to its angle of repose. Are you able to see a similar or a different evolution in the slopes you observe? This would make another very nice comparison.

This is a useful idea. We observed that the debris flow occurred first, and then the slope slipped. We will add the below figure (figure 2) and the text to show their evolution and compare them with Cody's research.

Based on 17 years (2002-2019) RS monitoring of the B3 slope, we found that the surficial debris flow occurred before the hillslope movement. Firstly, some surficial debris flows occurred, forming 3 gullies, and no slide was evident in 2002. Secondly, the debris flow gullies gradually expanded, the number increased to 5, the slope slid slightly, and the slide cracks were started to form around in 2013. Finally, the debris flow gullies increased to 6 and expanded further (the largest gully is about 130 m wide at its widest point), the slope slide significantly (up to 2 cm d^{-1} between 2017 and 2018), and the cracks increased in response to rapid glacier downwasting in 2019, which is different from the discovery of the Fox glacier in New Zealand by Cody et al., 2020.

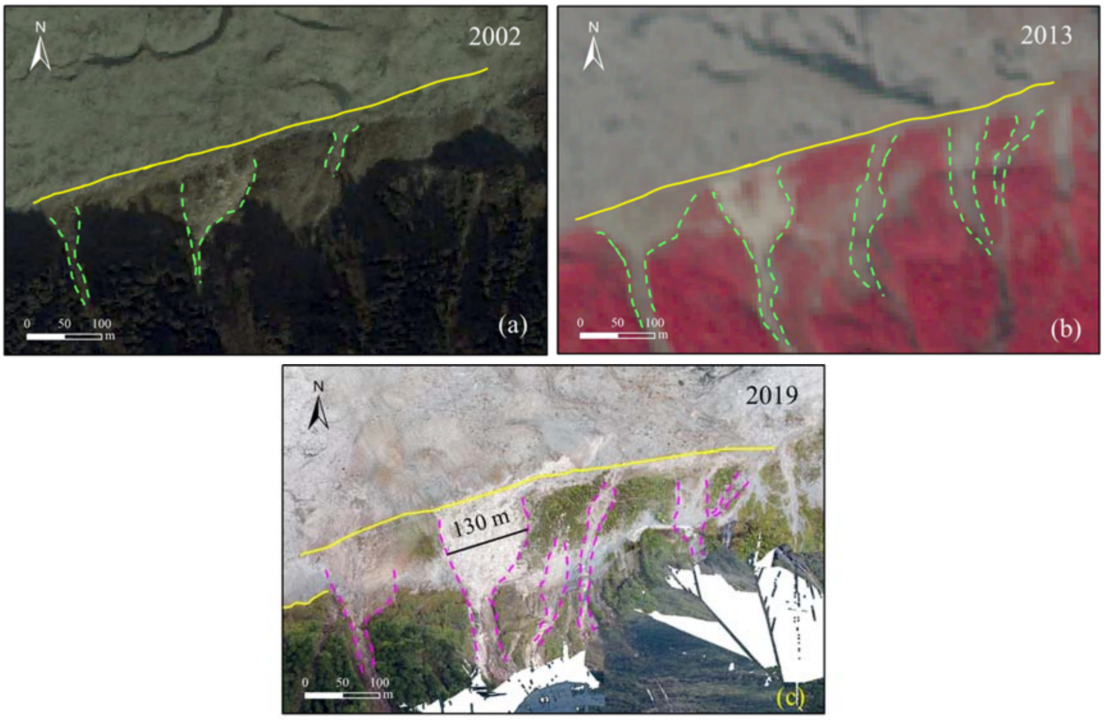

Figure 2

[15] L272-275: Comparison is drawn between the Type B failure process and the conceptual model of moraine evolution by Eichel et al (2018). However, this comparison is hard to follow. Eichel et al describe a transition from an unstable state dominated by debris flows and gully erosion through to a period of solifluction modification, through to stabilisation. It appears to me that the typical Type B hillslope responses you describe in this manuscript seems to be more dominated by debris sliding than debris flow or gullying, and therefore is not a great comparison to stage A of the Eichel model (for the sites they studied) – perhaps your Type C is a better comparison? It would be good therefore, if you could further explain why you make this comparison, and it would be really interesting if you additionally compare the evolution of the moraines at your site to the other two stages of the Eichel model – do they also transition to solifluction and then stabilisation, i.e. the older lateral moraines nearer to the LIA terminus? Perhaps you can identify solifluction features in the imagery data or from your field visits, or perhaps the climate is not suitable for this? If you do find differences then you could instead suggest that at your site you observe a different evolution pathway to what is found for the European Alps? This would be a nice contrast and provide a rich vein of discussion (in the discussion section) if in fact there are differences that can be observed.

Thanks for mention, we will correct it.

[16] L300-306: Please elaborate further on what is meant by a 'transition form', and 'landsliding behaviour' and what is different between the two zones referred to.

Thank you for this comment. B4 is a transitional type between Type B and Type C. From the geomorphological point of view, we divide it into Type B, but it is more connected to Type C than B1-B3, and the proportion of landslides is also smaller (24.9%). *'Exhibits landsliding behavior in two distinct zones'* means 'Landslides can be observed in two zones'. We will correct the wording in the new version to improve the clarity of expression.

[17] The observation that south-facing slopes were generally more unstable than northfacing slopes is a potentially interesting observation, but one that is not robustly analysed. For example, the authors might consider a wider range of (intrinsic and extrinsic) factors explaining this difference, e.g:

a) differences in the availability of material (i.e. asymmetrical deposition of glacial drift and moraine construction on either side of the valley), with more sediment on the south-facing slopes;

b) asymmetry in morphology (i.e. differences in slope angle). The latter could be easily tested using DEM analysis; the former could possibly be explored through aerial image interpretation?

Thank you for your constructive suggestion! We will add more analysis based on these two points, which is not difficult.

[18] Section 5.1. Unfortunately, this section is heavily reliant on speculation, and analysis of only the 2018 rockfall. Analysing the location and timing of a single (and a not particularly spectacular) rockfall in the valley is not sufficient for making meaningful generalisations of the causes of rockfall in the valley. Perhaps this section could be strengthened if more attention was paid to the smaller rockfalls - e. examining patterns in the timing and location of several failures and not just a single failure.

As stated in [10], it is difficult to confirm the specific time of small rockfalls occurred, so we cannot do too much detailed analysis. In addition, the locations of small rockfalls are also very scattered (see Fig 5a and Fig S3). Nevertheless, we will increase the discussion and analysis of small rockfalls in the next version of the manuscript to make the research more rigorous. There are many studies (Huggel et al. 2005; Fischer et al. 2010) of single rockfall/rock avalanche events, and our analysis is more than just speculation.

[19] L389-390: ‘instability typically have a slope angle of 25’. Upon what basis is this statement made? Do you systematically measure the slope angles from the DEMs? Are you able to more robustly compare slope angles between the unstable debris-covered slopes and the stable debris-covered slopes to test whether the unstable sites tend to be oversteepened? Perhaps an examination of the slope angle of stabilised moraines closer to the LIA terminus will give some rough indication of the ‘long-term’ angle of repose of the till making up the moraines in the valley. This could provide a useful test of your hypothesis presented on L457 ‘we hypothesise that this (moraine collapse) will continue until critical angles of repose are reached which will be followed by vegetation colonization and advanced soil development’.

We did do some slope analysis, and we will combine [6][17] and this part to make better use of slope analysis from the DEM.

The slope analysis results of 2016 DEM in HLG are shown in the figure 3 which is synthesized with 2000 SRTM DEM and the High Mountain Asia Glacier mass balances from 2000 to 2016 published by Burn et al. Interestingly, we found that the Type B (with a mean slope of 29°) and C (with a mean slope of 32°) unstable slope angles were lower than that of the stable slope (40-60°), but Type A slope angles (mean slope is 54°) are similar to those of the stable slope. This result was verified in field investigation (please see figure 4a).

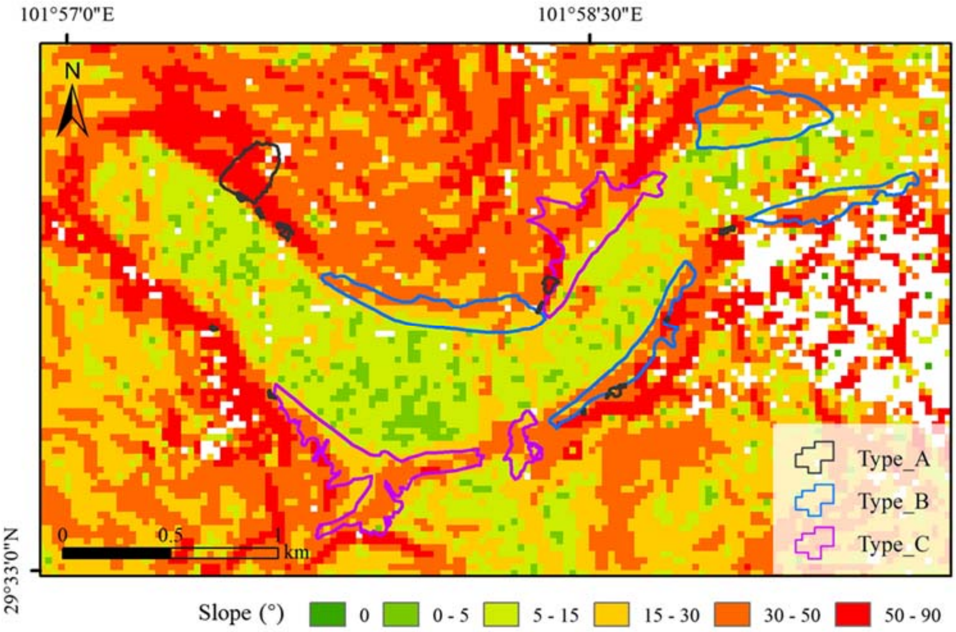

Figure 3

This result is contrary to the general conclusion of landslide study-the steeper the slope, the stronger the shear stress and the lower the Factor of safety (FoS)-(McColl, 2015). Why is that so? After carefully studying the similarities, differences and correlations of three types of unstable slopes, we proposed a conjectural paraglacial slope mass transport model of HLG (figure 4b).

After the glacier was downwasting from the initial state (stage I), the upper of the steep moraine slope quickly destabilized, making the entire slope slow, corresponding to funding in van Woerkom et al. (2019), Ballantyne (2013), and Cully et al. (2006); then the moraine slope slowly slides down (stage II). During the exposure of the moraine, vegetation may be colonized, or gullies may be formed by debris (or water) flows washing away (stage II a); when the gullies gradually expand, headward erosion may occur in the upper of the gullies (stage II b). Sediments at the base of the slope or fell onto the glacier surface are transferred twice as the glacier moves (stage III); until the slope is steepest when moraine has been removed and bedrock is completely exposed to the ground; and may collapse under other disturbances (stage IV).

We will add this part in the next version.

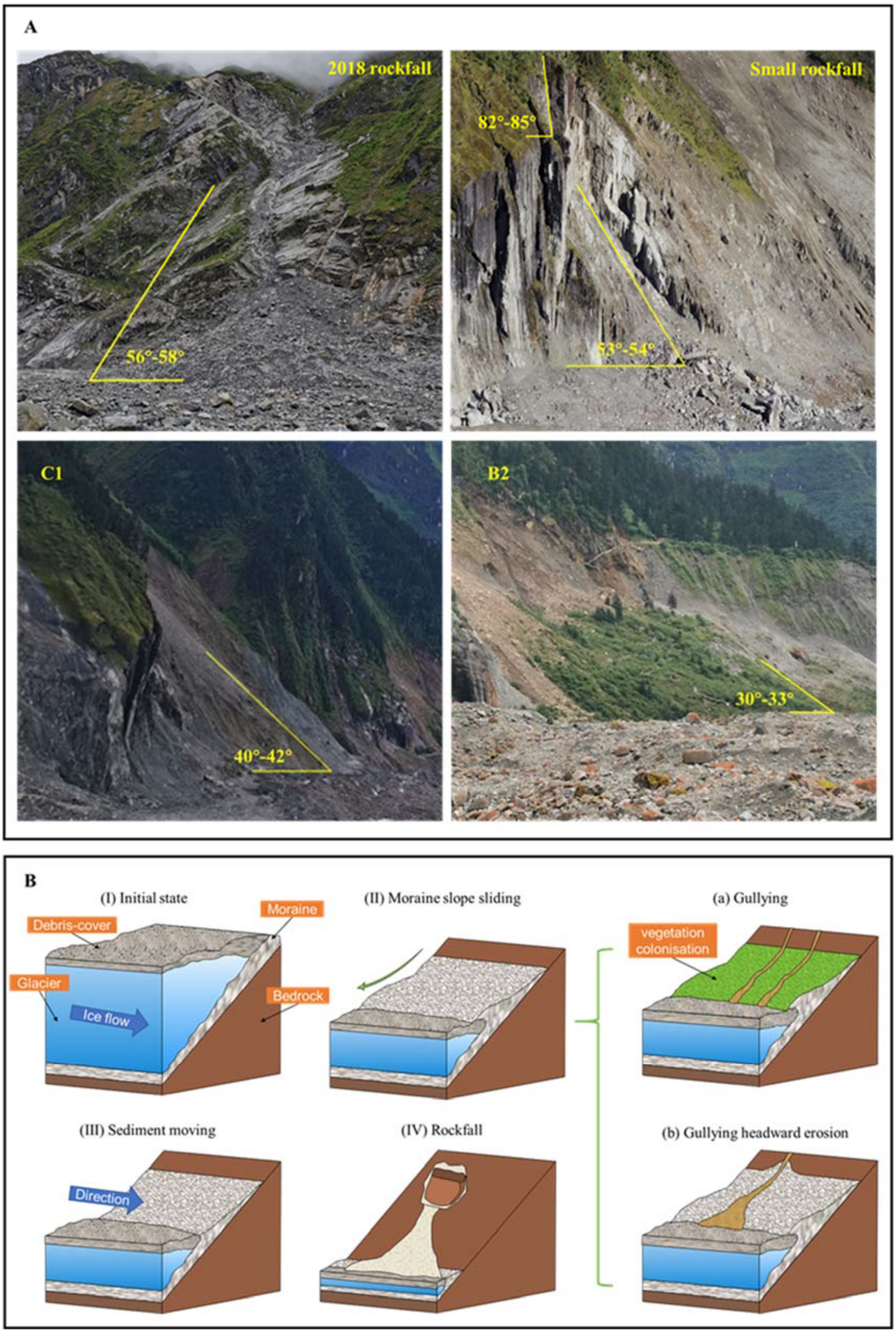

Figure 4

[20] L395-402: The role of vegetation colonisation is (reasonably) discounted for stabilising the Type B failures that involve deeper-seated sliding, but what about the role of vegetation colonisation

for stabilising other types of erosion process in the valley? Do you see a reduction in say Type C hillslope responses over time? Again, this might make for another comparison/contrast with the Eichel et al (2018) model.

We didn't see that kind of reduction in Type C, at present. But we will consider this in the next revision.

[21] L400-403: It is a shame that there was not more effort made to understand why all Type B sites appeared to increase in movement rate between 2017-2018. What further analysis could be done to explore this? Did Type C response (i.e. gullying) also increase? Can the data from Figure 3 be used to analyse this further? Did any slopes downstream of the glacier terminus show any increases in movement or erosion during this time (i.e. helping to rule out glacier thinning as a cause)? Did the upper parts of the slope failures speed up to the same extent as the lower parts (perhaps more suggestive of rainfall as a driver) or did the lower part speed up the most (perhaps suggesting removal of toe support from ice thinning)?

Thank you for your comment! We will do more further analysis to explore this part.