Response to RC4

Dear reviewer,

Thank you for your constructive comments, which we address in detail below. First, we respond to the general comments, after which the detailed comments are addressed point by point.

General comments: The hypothesis stated on page 3 of the manuscript proposes that lateral moraines can play an important role in supplying the glacier tongue with debris, especially for glaciers that are stagnating and the tongue is disconnected from the headwall. The goals of the paper are to quantify erosion rates across a 5-year period by using high-resolution DEMs. The dataset provided by this paper is remarkable, very high resolution and from a remote location whose future demise will wreak havoc on downstream communities. This large, alpine glacier undoubtedly warrants current research and the negative feedback cycle created by debris insulating the glacier tongue is an understudied process.

Overall, I find that the study is both overly complex and overly simplified at the same time. To improve the manuscript, the authors could remove some datasets, such as the clast shape analysis, as the population of clasts sampled seems far too small to gain any interpretable insights. This would remove the field-based component of the manuscript, but I don't think that would weaken the paper. On the contrary, I think the paper could be strengthened by using only the remotely sensed data.

We appreciate your concern that additional data complicates our work but we strongly believe that ground validation of observations made from the UAV data is necessary and this was raised as one of the key points of reviewer 3. We examined over 3000 clasts, which is more than the site specific sample size used in Benn and Ballantyne (1994) & Lukas et al. (2013), which proposed the methods used in this paper. Therefor we assume that our sample size is large enough to draw conclusions from, and we do believe this data strengthens our message.

The authors present many erosion rate calculations for different sections of the moraine, but they never use those data to predict how long (in years, e.g.) it would take for the glacier tongue to remove a certain amount of debris. Although not part of the manuscript in its current state, I think the manuscript could be improved it the authors added a predictive component to their research, like answers questions such as: if the debris cover is adding a negative feedback to the glacier ice, will the debris allow the ice to persist past the year that the glacier ice is expected to melt? And if so, by how many years? At what time point would the lateral moraines deflate enough so that they are not contributing debris to the glacier tongue?

Although certainly interesting questions, indicating such precise relations between glacial melt and supraglacial debris cover requires extensive knowledge of other sources of debris, the distribution of lateral moraine derived debris over the glacier, and finally the feedbacks between debris cover and glacial melt. The knowledge and data required to do this accurately is unfortunately unavailable at present and beyond the scope of the our study.

With the approximation of lateral moraine sediment input to the glacier we want to contribute to the current discussion, with a first indication of the spatial and temporal variability lateral moraine sediment supply to a glacier tongue in High Mountain Asia. This will hopefully allow us in the future to include these estimates into projections of the development of such glaciers.

Apart from the technical issues I present below, I find that the authors use the term 'erosion' in a misleading way, and could replace it with 'transport' in some situations. Erosion implies that the material is carried away from the glacier system, as opposed to being transported to lower on the glacier tongue, thus leading to the well-insulated glacier snout that they discuss. If the authors have a mechanism to prove that the material is eroded than they can continue using that term, otherwise it could be replaced with transport.

We agree with the reviewer that the term erosion is not used correctly throughout the paper, and this issue was also flagged by reviewer 3. We have used it interchangeably for changes in elevation, transport process and erosion (entrainment and removal of material by an external medium). We adapted the terminology throughout the manuscript. Elevation differences are now referred to as 'surface lowering' or 'elevation change' and processes such as slumping are referred to as 'mass transport' or 'transport processes'. At some locations, for example in the gullied upper part, actual erosion takes place and we continue to use the term.

Specific comments:

As for the writing of the manuscript, I think lines 1-16 on p. 3 can be written so that a testable hypothesis is proposed. The authors list approximate erosion rates from the Alps and from Norway, which could be higher due to the contribution of headwall erosion. The authors need to articulate what they are testing, instead of stating their goals. Perhaps they could add "by quantifying erosion rates of lateral moraines from a DCG in Nepal, we will test the hypothesis that glaciers with disconnected tongues will have lower erosion rates than those with tongues connected to the headwalls". Lines 12-16 on page 3 do a good job of explaining the goals of the paper, but it is lacking a testable hypothesis.

The inclusion of a hypothesis is a good suggestion. Since the comparison of the erosion rates has changed as result of comments of other reviewers we have defined one hypothesis related the importance of debris supply from lateral moraines on debris covered glaciers in lines P3 (L15-17).

Technical corrections:

Page 2, Line 31: The phrase "gullied upper part" is vague, I would suggest changing it to more descriptive language. I don't get a sense of what part of the moraines are gullied.

The term 'gully' or 'gullied' is generally used in moraine geomorphology for the distinct features of the upper part of moraines, see for example (Draebing and Eichel 2018; Lukas et al. 2012). This was not very well visible in the otherwise too small Figure 3, which has now been enlarged to make this clear. We have added this in the respective sentence.

Page 2, Lines 34-35: The sentence: "The transport processes on the moraines are most active directly after deglaciation" makes intuitive sense but is a large claim. Does this imply that your modern study falls within the period of most active transport? I would either add another sentence or two linking this claim to your study, or remove it all together.

We decided to remove the sentence, as we do not further elaborate on the stage the moraine is in.

Page 4 line 10, that is a big assumption that no melting core exists in the moraine. Do they have any data that would substantiate this claim?

As noted by several reviewers, we have failed to address the presence of an ice-core within the moraine in our original manuscript. While there is no actual evidence of an ice-core, the formation process of lateral moraines suggests they are likely present and field evidence from other debris-covered glaciers in HMA exists (e.g. (Hambrey et al. 2009)). However at least for the case of Lirung and other debris-covered tongues in the Langtang catchment, field observations suggests that these ice cores are covered in a very thick mantle of debris, very likely larger than 2 m, contrary to more thinly covered moraines elsewhere (e.g. (Lukas et al. 2005)). During many walks in multiple field seasons on the glacier tongue, including the flanks, nowhere along or within 50 m of the foot of the moraine have we observed any ice, neither as ice cliffs nor as ice covered under thin moraine material. Furthermore, none of the debris thickness measurements ever taken in the field close to the moraine on the glacier surface where thinner than 50 cm (see e.g. (Ragettli et al. 2015)), and since the debris progressively thickens (see e.g. (Nicholson et al. 2018) towards the moraine it is likely much thicker there (Nicholson et al. 2018). We estimate the maximum downwasting rate under the moraine by quantifying the elevation change in a relative flat zone of 20 meter wide close to the moraine. We removed sections which showed clear depositional features, to limit the noise caused by debris deposition. The maximum downwasting rate is approximately 0.6 m yr^{-1}. The top melt of buried ice declines exponentially with increasing debris-cover thickness (Östrem 1959; Schomacker 2008), but as debris thickness on the glacier is generally > 50 cm (Ragettli et al. 2015; McCarthy et al. 2017), the decline of melt rates underneath the moraine due to the additional debris thickness is relatively small.

Assuming a maximum downwasting rate of 0.6 m yr^{-1} due to the ice core, the remaining elevation change due to mass transport in this part is +0.19 m yr^{-1}. This results in a significantly reduced rate of material reaching the glacier surface and hence our interpretations of the results. As a consequence, we removed the explanation on page 10 (line 20-27) as suggested.

While we acknowledge that further detailed analysis could be carried out to ascertain presence of a potential ice core (i.e. GPR) or to understand the processes in recent decades and centuries, our aim here was to determine the volume of debris that moved onto the glacier surface in recent years using an UAV. We believe that the use of high-resolution DEMs in combination with geomorphological analysis has great potential to understand the dynamics of debris-covered glacier tongues.

Page 6, Lines 12-15: This sentence reads as a hypothesis since the authors are predicting what the . It reads as more of a topic sentence and could be moved to the beginning of the paragraph.

This section is rewritten to first mention the goals and hypothesis, and afterwards the methods used to test this.

Page 6, Lines 19-20: Here the terms “lower loose” and “upper firm” start getting usage as descriptors for parts of the moraine. I understand that terms loose and firm correlate to the different erosion rates, but perhaps more descriptive terms could be used, such as “low erosion area” and “higher erosion area”.

The terminology of these parts is changed throughout the manuscript, and is more descriptive in terms of the dominant active processes and geomorphological type. The upper firm part is referred to as upper gullied area, the lower part is currently a debris apron or a slope with coalescing debris cones. The terms used in the paper are indicated in Figure 3.

Page 7, Lines 19-20: Solifluction is mentioned as a main transport mechanism, yet little discussion in the text is allocated to this process. Is there a way the authors can interpret solifluction processes from their DEM data?

As indeed the signs for solifluction were unclear, and the speedup of horizontal displacement in summer months does not support solifluction as the main transport mechanism, we only refer to the transport mechanism of ‘slow slumping’ in this regard and have adapted this in the manuscript.

Page 11, Line 13: Change mechanisms to mechanism

Changed

Page 11, Line 22: Change are to is

Changed

Figures 4 and 5: It would be useful to have an index map of where these images/data are located on the glacier.

The location of these insets is added to Figure 2.

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