Response to RC3

Dear Martin Kirkbride,

Thank you for your substantial comments, which prompted us to rethink certain parts of the analysis and which have improved the manuscript considerably to our opinion. First we respond to the general issues raised, afterwards the detailed comments are addressed point by point.

Teun van Woerkom's paper is about a specific focus of geomorphological activity in the deglaciating landscape, and as such is of significant interest. The quality of the data is very high and demonstrates very well patterns of surface change in localities bordering a wasting debris-covered glacier tongue, where interactions between glacier thinning erosion of steep proximal moraine faces, and transfer of debris from moraine to glacier are very active. Unfortunately, from a geomorphological perspective I find the explanations of the patterns identified to be confusingly written. It's hard for a first-time reader to figure out what the authors' understanding of process action actually is, and ultimately I'm afraid I cannot agree with their calculations and interpretations

There seem to be two reasons for this. First, the terminology as confusing, and some basic geomorphological terms are used loosely to give a false impression of what is meant. "Erosion" is particularly used misleadingly, where measured surface lowering seems to be assumed to equal "erosion" (sensu removal of debris by a transporting medium).

We agree that our use of certain terminology may have been confusing. We have replaced erosion by surface lowering and mass transport, depending on the meaning. Other terminology that we changed systematically includes 'lower loose moraine' to '(lower) debris apron', 'upper firm moraine' to 'upper gullied moraine' and 'tumbling rocks' to 'rockfall'.

Second, I think there is a problem with the research design. It seems the authors have collected their high-resolution topographic data, calculated the DEM difference maps, and then attempted to interpret them on the basis of elevation change. There is no geomorphological assessment of the sites on the ground. This results in some strange and probably incorrect interpretations which even quite short field visits would have corrected. Better to observe details in the field, map the features, then use that as a basis for carrying out the topographic analysis which then quantifies processes which have already been identified and interpreted empirically, and not by inference from remotely-sensed data. My detailed comments below give examples of this problem.

We believe this is a bit a matter of taste. We have access to this unique UAV dataset that we primarily collected to understand glacier melt. However our field observations of the moraines in combination with the spectacular dataset motivated the design of this research.

We agree that field-based geomorphological assessments of a glacier surface and adjacent moraines are essential to determine processes that drive the sourcing and transport of debris on debris-covered glaciers. For that reason we have referred to these studies in our approach (e.g. Benn & Owen, 2002; Boulton, 1978; Kirkbride & Deline, 2013). We however believe that a spatial assessment of elevation using repeat DEMs allows us to add insights to such an approach.

Furthermore, we have of course carried out field visits on site, accompanying each of the UAV flights, which have resulted in several publications (Brun et al., 2016; Miles, Steiner, & Brun, 2017; Steiner et al., 2015). We agree though that there was room for improvement and we are grateful for your feedback as established expert in this field. We have included further explanations retrieved from the field to back up our observations made from the DEM data, e.g. at P9 (L1-3) and P10 (L18-24).

Page/line

1/3. Replace “or” with “and”

OK.

2/3. Use the standard term for this thickness: “critical thickness”. Perhaps acknowledge that this value is actually a variable, and what Østrem found in Sweden may not apply to a Nepalese glacier.

We have changed the thickness term to critical thickness. The critical thickness is variable and depends on the local climate, however it is in the range of maximum a few centimeter.

2/16. Surely the headwall extent is a key control as well.

We have added this to main drivers at P2 (L18).

2/32. Here and elsewhere, frost action is hardly mentioned as a contributor to detachment of particles from moraine slopes. Lateral moraines are typically silty and frost-susceptible, and in winter and spring ice crystal growth detaches a large amount of debris which wind and wash then remove.

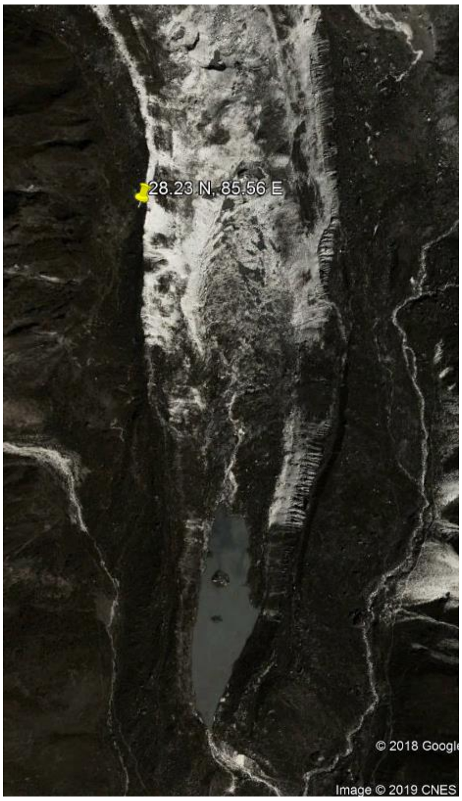
The possible importance is mentioned in the Introduction at P2 (L35). A further explanation is given in section 4.4 P9 (L23-25).

3/5. Make it clear from the outset that all rates are rates of vertical lowering, and not of horizontal retreat, both in this study and others cited (notably Curry et al. 2006).

This is indeed the case. Due to the mis-use of ‘erosion’ this was confusing throughout the paper. We now use ‘(vertical) surface lowering’ or ‘elevation change’ consistently to make this clear.

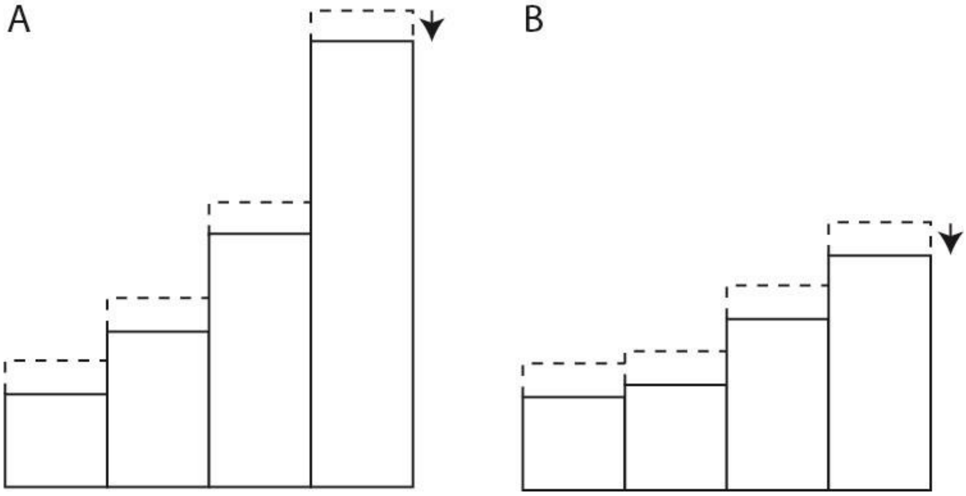
3/19. Just an observation, but this latitude/longitude location does not plot anywhere near Lirung Glacier in Google Earth. I am not saying this location is incorrect, but I wonder where the error lies?

The coordinates seem to be the correct ones. I’m not able to reproduce the error, this might have something to do with standard coordinate types used by the software? How it shows on my computer:



Generally: there are no data on the gradients of the eroding proximal faces of the lateral moraines. This is important data, because for the same rate of vertical lowering, a steeper moraine face will be releasing less debris than a gentler moraine face. A quick calculation shows that for a vertical lowering of 0.31 m/yr (p.1 l.8), a 55 degree face will retreat horizontally by 0.22 m/yr but a 65 degree face by only 0.14 m/yr. If the two faces are similar height, the gentler one will release much more debris in a year. The values of vertical lowering are not therefore very comparable between different locations without some gradient data: a conversion to horizontal retreat rates of the steep moraine faces would be useful.

We agree that the same amount of surface lowering results in a different amount of horizontal retreat under different slope angles. However, we do not agree that a conversion to horizontal retreat rates would change the results. As our results are derived from gridded DEM's, we analyze vertical elevation changes (Figure below). The figure shows two situations with a different slope but the same magnitude of surface lowering. The total amount of debris released from both slopes is therefore the same, as their horizontal surface area is also equal. An advantage of using vertical lowering rates over horizontal retreat rates, is that they can be easily used for volumetric calculations, and incorporated in glacier flow or melt models. As we focus primarily on quantifying overall volumes of debris transport this explains the choice for our approach.



5/9. “Washout” zone is introducing a new term, and I question whether this is necessary because standard geomorphological terms exist already for debris slopes below retreating faces. If the dominant process is rockfall, it’s a talus. If debris flow, it’s a colluvial apron or cone or simply slope. If dominant process is uncertain of complex, use a simple morphological term such as debris apron. (Also p.8 l.1 and other places where “loose part of the moraine” is used: is this the same zone?)

The terminology is changed throughout the manuscript. The previously mentioned ‘washout’ zone is part of the debris apron, but has some different characteristics which are related to different depositing processes. The difference and interpretation is specified in Figure 3 and P5 (L15-18).

6/13. “decrease”, not “decreases”.

Changed

6/19 “Most elevation change occurs in the lower loose part of the moraine. . .”. The terminology and interpretation of this “lower loose part” of the moraine is where I think there are the main problems with the paper. What follows is full of confusing explanation and misleading interpretations. Having said this, all could be easily corrected with some thought and circumspection about the wider setting of the study sites. What I mean will become clear in the comments below

6/19 – 6/20 et seq. A key finding is that the “lower loose part of the moraine” (presumably equivalent to the “washout zone”) has lowered more quickly than the upper eroding face. This lowering is variously described as “erosion” of the slope (p.7 l.24) or “slumping” (p.7 l.16). The first of these is strange given that this is a depositional slope, and it should be aggrading. The second will be discussed in due course. A further curious observation is “a patch of higher erosion. . . below the firm zone” which the authors explain as wash “plunging” off the steep slope above (see below). This whole set of interpretations is riddled with problems which I will discuss in the line-by-line points below.

We have taken great care to clarify our terminology throughout the manuscript.

Furthermore, 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, Nicholson, Ross, & Humlum, 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, McCarthy, Pritchard, & Willis, 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 (McCarthy, Pritchard, Willis, & King, 2017; Ragettli et al., 2015), 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.

6/20-22. I find this comparison of lowering rates confusing. It needs to be more clearly stated what is being compared with what.

The comparison is indeed confusing, as the rates found by Watanabe et al. (1998) were reported from a different field situation including the valley rockwall. We have therefore removed this comparison from the manuscript.

6/23-24. These differences could be the result of slope angle differences (see above).

See response to concern above.

6/28. Delete "m yr-1" after "0.02".

Deleted

7/3. "slumps and rockfall" ?

Changed. The word 'or' indicates an indistinguishable process, which is certainly not the case here.

7/6 – 7. If I am correct in what this sentence is referring to (and I'm uncertain about this after looking at Figure 5A), I wonder whether this refers to a short steep step of fresh-looking till which commonly forms the very base of the eroding till cliff, and right at the very top of the debris apron below. This is a very common feature of lateral moraines above thinning debris-covered ice generally. Though I haven't read about it in the literature, it has always seemed to me that it forms by lowering of the debriscovered ice at the base of the steep moraine wall: nothing more complicated than that. This paper actually seems to demonstrate this, because the debris aprons are lowering and not aggrading upwards. This just creates a short slope segment marking (probably) that melt season's lowering and separation of the debris slope below from the steep moraine face (the "firm zone"?) above. It's nothing to do with greater erosion by water plunging off the wall above: it's too continuous and uniform to be caused by this. All the evidence presented strongly suggests that the debris apron isn't eroding, but being gently let down by ablation of glacier ice underneath. Whether this ice is still connected to the glacier or not is secondary: the important point is that its surface is lowering. (This, after all, is what exposes the proximal moraine faces in the first place).

We added this suggested interpretation to the manuscript at P7 (L18-21). This process is certainly a possible explanation, but the area of increased surface lowering continues further down the debris apron and changes gradually, which does not stroke with the formation of a steep 'step'. Also, there seems to be more surface lowering between more intensely gullied sections, and therefore we cannot exclude the possibility that these higher rates are (also) related to water flow processes.

7/13. Replace "tumbling rocks" with "rock fall". Oversteepening of the slope isn't by water flow processes: it is by downward extension of the slope base as the ice below the slope thins.

Done and it has been included.

7/14-15. "The slump toe. . ..the event": meaning unclear. What slump? What do you mean by "the moraine"? After what "event? Confusing. "The lower part of the moraine. . ..": are you including the debris apron ("washout zone/ lower loose part") as part of the moraine? If so, why? It isn't part of the moraine once the debris has been removed from the face and redeposited below.

The slump is visible in Figure 6, which is now specified. The terminology here is changed as well, which now indicates that the slump deposit can be found on the coalescing debris below the upper moraine.

7/17-18. Average velocities of what? What process is faster than creep? Creep of what? Why make this comparison? Confusing.

The debris on the talus has a lateral displacement towards the glacier. This rate is compared to the rate of several processes to indicate those processes that are related to the displacement. We have clarified that in the paper on P7-8 (L31, L1-6).

7/20. Permafrost requires seasonally-frozen ground: it doesn't require permafrost. On p8 l.2 you show it's probably not solifluction anyway, because motion is greatest in the wet summer season, so it's slow slump or creep of unfrozen saturated debris.

Thank you for the correction by email afterwards! We changed the argumentation of this part and now explicitly exclude solifluction from the likely processes. We also argue that creep of unfrozen debris is an unlikely option, as the displacement rates related to creep generally are lower than those observed here. Changes were made in P7-8 (L31, L1-6).

7/24. "erosion rates" of the "loose part of the moraine"? Is this not surface lowering due to a melting ice core, with slow downslope movement of the debris cover over the ice beneath? In other words, it's behaving like a wasting ice-cored moraine. It cannot be called "erosion" because no debris is being entrained and removed by an external medium. Use of this term is misleading and confusing. It's slow gravitational transport.

The term erosion was often incorrectly used for elevation change rates. This is improved throughout the manuscript as indicated above.

8/6. Freeze-thaw cycles are important for causing needle-ice growth and detachment of particles in moraine faces. A problem is that few scientists observe moraine faces in winter and spring, when ice crystal growth is evident and thaw-saturation of silty till gives it a very different consistency to the indurated, dry material we see in dry summer weather.

While we don't have precise measurements of any such process we definitely observed it in the field on shaded lateral moraines and the difference in consistency is apparent while probing the material. While we can acknowledge that qualitatively, it is difficult to account for that in actual estimates of debris transport. This is specified in P9 (L23-25).

8/9-12. Again your "erosion" values are slope-angle dependent. They are a lowering value, not erosion rates. As discussed above, the lower slope isn't actually eroding at all, so this comparison is spurious.

As discussed above, we do not think that the values presented are slope dependent. The term erosion is changed to surface lowering/elevation change. See our earlier comments.

8/16. Is the "lower moraine" the same as the washout zone, loose part, etc? There needs to be clarity and consistency of terminology throughout the paper. A schematic diagram showing a cross-section through the moraine and toe area with the different zones labelled would be helpful.

As pointed out above, we have now revised our terminology throughout the paper. A diagram is added to Figure 3.

8/16-17. Again, it is being assumed that lowering of the lower gentler depositional slope is by erosion, but no geomorphological evidence of erosional process. The exception is when glacier confluences form medial moraines from lateral moraines in-transport (sensu Boulton 1978), which then spread debris across the centre of the ablation zone by secondary dispersal (sensu Kirkbride & Deline 2013) to form full-width debris covers. By this mechanism complete debris covers can form. Perhaps this is worth a mention in the wider implications: but the debris needs to be introduced to the glacier centre some distance upstream.

We now acknowledge the likeliness of ice beneath the debris apron throughout the manuscript. Morphological evidence of debris flow and water flow erosion is presented in section 4.4, which would certainly be able to entrain, rework and remove sediment from the debris apron below. Therefore we do not completely remove the erosional processes on the debris apron from the manuscript. The possibility of forming a complete debris cover of lateral moraine derived debris by medial moraine formation is added in line P10 (L20-23).

9/15 "approximately"

Changed

Section 4.6 Clast Analysis. This is all fine, but how did you assess the roundness of rounded moraine clasts which have shattered on impact when falling from the moraine, to give some angular edges? Did you measure the sharpest edge or the most rounded?

Thank you for pointing this out, this was indeed a question raised during the analysis of the clasts in the field. When investigating the clasts, we looked at the rounded edges, hence clasts that were round but broken into smaller parts due to possible impact were classified as round. Unfortunately we did not however record the number of clasts that were possibly shattered upon impact, also because we found it hard to distinguish which clasts were shattered locally (frost or impact of larger boulders moving locally?) and which ones had fallen from higher up the moraine. For a more detailed analysis this may however be an issue to look into also on Lirung.

10/11-14. This calculation should be omitted because lateral moraine supply is clearly demonstrated not to cover the full glacier width, so it is a pointless calculation. Also, your average rate of 0.31 m yr-1 isn't valid because it includes the debris apron below the moraine (assuming I have read p.6 l.16-20 correctly), which I have argued earlier is a depositional slope whose lowering is not due to erosion.

We included the effect of ice melt lowering on the debris apron. Assuming a lower melt rate underneath the thick debris apron (Schomacker, 2008), an approximation of the maximum debris supply is currently made using only erosion from the upper gullied slopes. This results in a debris thickness increase of 5 cm yr^{-1} instead of 17 cm yr^{-1}. The calculation including the maximum modelled runout length is kept to give an indication for other glaciers, where runout may be greater. However, we do acknowledge in the text that this is not the case for our study area.

10/14. An annual debris thickness increase of 0.29 m yr-1 would generate a layer 10 m thick in about 35 years. So do you see debris layers this thick over the margins of the glacier? I very much doubt it. This reinforces that your rate is not based on a valid calculation. Yet (l.16) you persist in arguing that it is correct.

As indicated above, the rate is reduced to 0.05 m yr^{-1}, which is more in line with other supply rates and debris thicknesses found.

10/20. Point (b) is simply not a realistic process. Debris accumulating below most of the lateral moraines is not affected by terminus behaviour.

We agree that the process mentioned under (b) is irrelevant for most of the glacier, which is indeed not affected by processes at the glacier terminus. This section is removed from the manuscript.

10/27. Point (c) ditto. This whole set of justifications is spurious.

Point (c) is rewritten, and now suggests that the form of the glacier terminus might also be influenced by the lateral moraine supply.

11/1-6. Overstates the significance of lateral moraine near a terminus. To effectively create a complete debris cover which will affect glacier mass balance, debris needs to be introduced to the transport system much higher upstream. Debris introduced from lateral sources close to the terminus is of more geomorphological than glaciological interest, by creating ice-cored landforms along the base of lateral moraines (which are increasingly common as glaciers retreat). It is most significant in the later stages of the decay of glacier tongues, so a comparison with models of active debris-covered glaciers (p.11 l.4) is misleading. Overall, my view (reluctantly) is that the paper needs a very careful rethink and rewrite to possibly then be acceptable for publication.

We agree that moraine development and processes are of geomorphological interest, however we believe that in our attempt to understand debris-covered tongues better they are also of interest from a glaciological perspective. It is certainly true that initial source of debris are the headwalls or the bedrock. We hope we have made this clearer now. We acknowledge that the moraine material is just remobilized material. However, we observe constant transport of debris down lateral moraines in our field site. In addition thickening rates at the terminus of debris-covered tongues seem to be faster than what englacial transport could provide (Gibson et al., 2017). Therefor we hypothesized that remobilized material from the moraines also plays a role, even near the terminus. As we show, it does so primarily at the margins, and this may be a reason why ice-cliffs and lakes are primarily found towards the glacier center line.

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