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Interactive comment on "Estimating lateral moraine sediment supply to a debris-covered glacier in the Himalaya" *by* Teun van Woerkom et al.

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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

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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).

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.

Detailed comments

Page/line

1/3. Replace "or" with "and" 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.

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

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.

3/5. Make it clear from the outset that all rates are rates of vertical lowering, and not of

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horizontal retreat, both in this study and others cited (notably Curry et al. 2006).

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?

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.

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?)

6/13. "decrease", not "decreases".

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.

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

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

6/28. Delete “m yr-1” after “0.02”.

7/3. “slumps and rockfall” ?

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 debris-covered 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).

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. 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.

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

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.

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.

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.

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.

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.

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 processes is presented: it’s all based on the lowering rates. Lowering will almost certainly be controlled by melting buried ice: I would venture that there is little or no erosion generally on this slope segment.

8/17 et seq. At last there’s a more realistic explanation being presented related to ice surface lowering letting down the debris which has accumulated on its surface. This section could be written with more conciseness and clarity.

8/31. Why “flow velocity”. Flow of what?

8/33. Again, “erosion in the lower part” is mentioned. Lowering does not mean erosion at this site. Geomorphologically, it is a depositional slope unit.

Section 4.5. A much better discussion. The key finding is that erosion of lateral moraine faces adds supraglacial material to marginal strips of the thinning glacier surface, but that this material doesn’t reach the central zone. Thus, it cannot be the rate-controlling process influencing the overall rate of thinning of the glacier tongue because ice in the centre is able to melt at a higher rate (though retarded by debris advected from much further upstream). The significance of lateral moraine supply is that it generates lateral ice-cored moraines during the late stage of glacier decay. Therefore the wider implications in Section 4.7 (p.11 l.1-6) are an overstatement of the importance of the

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.

9/15 “approximately”

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?

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.

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.

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

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

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.

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