Response to RC2

Dear Simon Pendleton,

Thank you for your constructive comments, which we address in detail below. First we respond to the specific issues raised, afterwards the detailed comments are addressed individually.

Overall Comments: I applaud the authors for tackling a complex problem that is nonetheless very pertinent for understanding the dynamics of debris covered glaciers. Using higher resolution DEMs to investigate many aspects of moraine evolution through space and time is an important task. However, I have a few concerns that I believe should be addressed before this manuscript moves forward towards publication. First, the assumption of no ice-cored moraines is substantial, and one that should be backed-up with field observations or other evidence. Debris covered glaciers are notorious for producing ice cored moraines, and elevation changes triggered by meltout could be an important component.

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 (McCarthy et al., 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.

Also, the large variability of reported elevation change measurements and the overlap between measurements makes delineating significant differences between measurements difficult (see Fig. 1 attached). A more detailed treatment of variability in reported elevation changes would greatly benefit the manuscript.

We have addressed your concerns relating to the variability below.

Specific Comments: The Authors allude (in the abstract in on pg. 3, ln 2) to the fact that debris input from lateral moraines is more important for retreating glaciers or for glaciers with stagnant tongues. What is not explicitly stated is that contribution from lateral moraines only occurs when the surface of the glacier is below the crest of the lateral moraines; though this is partly common sense, I think that explicitly stating this in the introduction is worthwhile. It may also be pertinent to expand upon the temporal variability with regards to debris input; an advancing glaciers will have a higher proportion of debris sourced from the headwall, while a retreating glacier can accept debris input from lateral moraines. The authors speak to a this later in the discussion, but setting the stage in the introduction may better lead into the later discussion.

The introduction now includes a broader description of the study site at P3 (L2-8), including a description of those situations in which lateral moraines can be an important source of debris. We also explicitly state this to be of importance when the glacier downwastes below the crest at P3 /L2-3). The new Figure 3 also illustrates our arguments better. This greatly improves the discussion of debris distribution onto the glacier. We added to the conclusions that the changing contribution of lateral moraine debris to a debris covered glacier in relation to different glacier recession scenarios should be a future research focus.

Section 2 states that the moraines are 'disconnected' from upper slopes. I take this to mean that the outboard face of the lateral moraines have an opposite aspect as the valley wall (i.e., debris falling from the valley walls will likely not travel over the moraine onto the glacier). Perhaps obvious to persons on the ground observing the glacier system, but explicitly stating this is important for justifying no debris input from valley walls. Also, comparing Fig. 1 to current google earth imagery, it is not clear if the upper part of the mapped moraines are actually disconnected from the valley wall.

We added additional explanation to the introduction to clarify this. Furthermore cross sections from the glacier are added to a new figure (Figure 3) to make the disconnect more clear.

After referring to the potential for ice cored moraines potentially contributing mass as they degrade over time in the introduction, the Authors assume that no melting ice core exists in the moraine (such that all elevation changes are due to sediment transport off the moraines; pg. 3, ln 10). In a seemingly contradictory statement, the authors discuss a '. . .hillshade with a hummocky appearance is an indicator for subdebris ice' (pg. 5, ln 4) when discussing the lateral moraines. Ice cored moraines are generally quite commonly associated with debris covered glaciers (Clark et al., 1994), and this catchment is no exception. The authors provide no observations or other evidence to support their assumption that the moraines are not ice cored. Ice cores within moraines are known to persist for thousands of years and can help maintain steeper moraine slopes (Crump et al., 2017), which are present in the Lirung glacier. Visual observations from current Google Earth imagery shows that appears to be a consistent debris cover across most of the glacier and pro-

glacial area, which suggests that the glacier very likely could have formed ice cored moraines in the past. Without concrete evidence that the existing lateral moraines are not significantly ice cored, I am hesitate to agree with the assumption that all elevation changes on the moraines are solely due to sediment transport.

In this case, P5 L13-14, the hummocky appearance is indeed used as an indicator for subdebris ice. However, we use this to construct the lower boundary of the moraine, i.e. when the surface is hummocky that part is not classified as moraine and excluded from our elevation difference calculation.

As indicated above, we have taken into consideration the ice cored moraine in the revised manuscript. We have now included this throughout the manuscript.

In section 4.1 a vertical error of 0.02 ± 0.33 m y-1 is quoted, citing (Immerzeel et al., 2014). Upon further reading of (Immerzeel et al., 2014), it is difficult to tell, but it appears that the off-glacier area used to compute this error is actually the lateral moraines themselves, which are likely to change in elevation between May and Oct 2013); perhaps I am interpreting this incorrectly, but moraines don't seem like an ideal location to compare the accuracy of DEMs due to their changing elevation.

The chosen areas are valid locations to assess the accuracy, as dGPS measurements through time show that there is no trend in elevation difference on the off-moraine terrain. Most of our dGPS points are located well away from the moraine (**Error! Reference source not found.**; top 8 locations on the Western side and top 5 locations on the Eastern side). All others are located on or close to the distal slope and one at the outlet of the terminus lake.

Elevation changes on the distal slope of the moraines, as evidenced by comparing areas on all DEMs as well as comparing the DEMs to the dGPS points in 2015, show differences in the range of approximately -1.0 m to 0.7 m. There is no apparent trend and we attribute this to the DEM error than any actual elevation change (Table 1). Furthermore there are paths as well as sensor setups positioned on this distal slope, that have not changed since we visit the field site in 2012.

Similarly, referring to Figure 6 in the original manuscript, the DEMs show positive as well as negative mean annual change in what we referred to as the 'upper gullied section' in the manuscript. On the other hand, there is a clear line separating it from the lower part, where elevation change is distinctly negative, likely also due to dead ice underneath (as visualized for example in Fig. 11f in (Lukas, Graf, Coray, & Schlüchter, 2012)). This difference indicates the probable lack of ice underneath the distal moraine, and supports the dGPS data in showing the stability of the off-glacier terrain used for error computation.

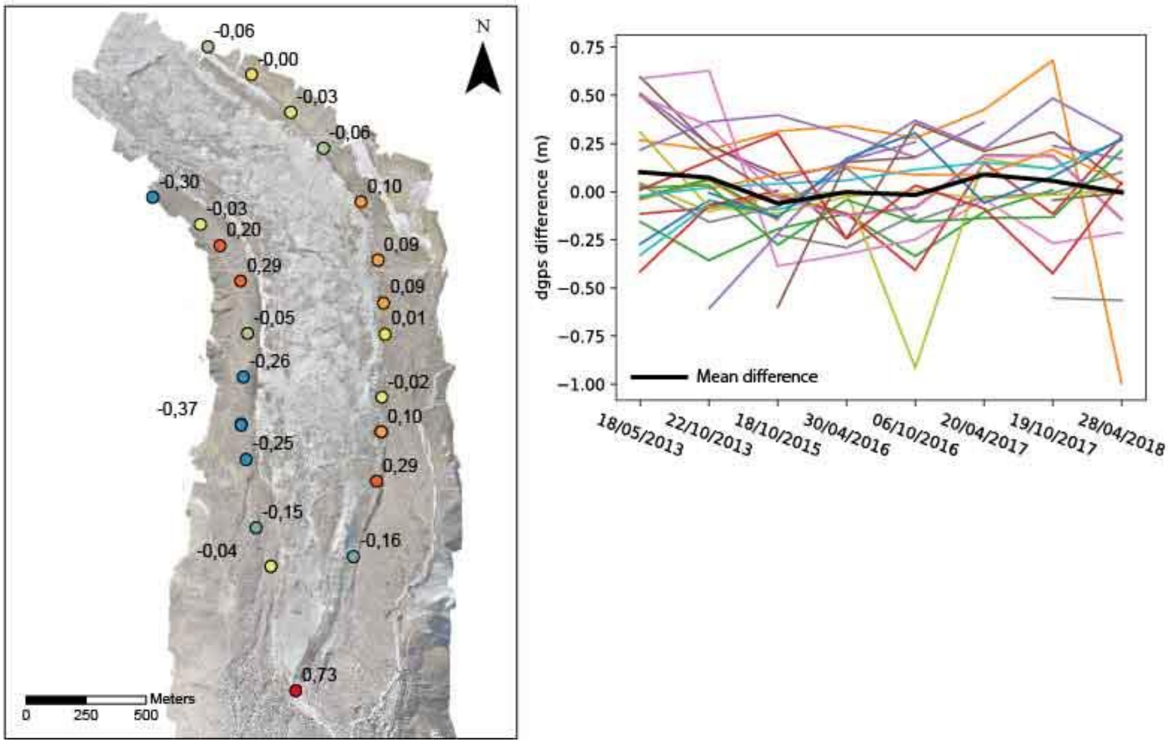

Figure 1; Locations of dGPS markers and mean differences (m) between dGPS measurement and all DEMs used in this study. Also all differences and their mean (corresponding to Table 1) are plotted, which do not show a distinct trend.

	Mean (m)	Std (m)
18/5/2013	0.101	0.297
22/10/2013	0.072	0.277
18/10/2015	-0.061	0.210
30/04/2016	-0.003	0.184
6/10/2016	-0.017	0.302
20/04/2017	0.089	0.160
19/10/2017	0.056	0.284
28/4/2018	-0.005	0.312

*Table 1: Mean deviations between all dGPS measurements taken at 18-10-2015 and the respective DEMs. The locations are shown in **Error! Reference source not found.**, as well as the entire dataset variability.*

Maybe the vegetated areas are more stable, so are appropriate to use to constrain DEM accuracy at different timesteps? Also the quoted accuracy of the DEMs is only calculated for DEMs produced in May and Oct of 2013; what about the accuracy of DEMs produced up through 2018? Further reading of the more recent Kraaijenbrink et al. (2016) indicates that "The bulk of the vertical errors at the tie points are within 50cm, and 75% are even within ~25cm." I am by no means an expert in DEM creation, let alone from high resolution UAV imagery, but if this is the actual uncertainty in elevations between DEMs, then the reported elevations changes in this manuscript (e.g., Table 3) are well within this uncertainty. Therefore, how can you be certain that your extracted measurements are above the vertical uncertainty and noise between your DEMs from different

times? Perhaps I am not reading Kraaijenbrink et al. (2016) correctly, but at minimum, it would be appropriate to add more detail regarding the vertical errors associated with your DEMs into this study (in addition to citing Immerzeel et al. (2014) and Kraaijenbrink et al. (2016)), since changes in elevation are the key component of this study.

It is indeed very likely that the surface under vegetation is most stable. However, it is the surface elevation of leaves, shrubs and grasses that is observed by the UAV, which is highly variable due to vegetation growth. Therefore it is not suitable.

We agree that more information regarding the vertical accuracy should be given, and an additional table (Table 4) is listing the off-glacier offset for all time periods used. More information regarding these uncertainties are given at P7 (L7-10). The value for 2013 is now -0.04± 0.44 m yr^{-1} instead of the previously published 0.02 ± 0.33 m yr^{-1}. This difference is caused by the Gorka 2015 Earthquake, which caused a large lateral replacement. After the event (and thus after Immerzeel et al. (2014)) the pre-earthquake data were reprocessed to take account of this offset and to still be able to compare with the full time series.

From these values it is clear that f the signal that we observed on the moraines our data is much larger than the possible error. Locally errors may be larger, but therefore our paper focusses on mean elevation changes and general patterns.

The Authors mask out vegetation areas from the DEMs to ensure that the moraine elevations changes represent real change. I am confused by this. Mass wasting events and sediment transport within vegetated areas aren't real change? Perhaps I am not interpreting this correctly.

Yes, sediment transport in vegetated areas on the proximal moraine slopes certainly happens, but very little is observed during our field visits. In addition as stated above, incorporating vegetated areas would cause errors, as seasonal vegetation dynamics will result in temporally variable deviations between the UAV DEM and the actual surface elevation. This is clarified in the manuscript at P4 L15-17).

The Authors also state that they correct the DEMs for off-glacier and off-moraines elevation changes with the assumption that those landscapes should be stable. Again it is not entirely clear why the DEMs must be corrected for these large changes; aren't large sediment transport events possible on 'stable' landscapes (though less common)?

Yes, sediment transport is surely also possible on the landscapes that are assumed to be stable. After both field visits and careful examination of the drone images no sign of significant sediment transport was observed, which leads to the conclusion that these should have a stable elevation over the investigated period of time.

Table 3 presents the elevation changes derived from differencing various DEMs over the study period. I am assuming that the mean the 1sigma uncertainty is presented, but this is not explicitly stated in the table (see technical corrections). When plotted, the mean and uncertainties all overlap well within each other's (see attached Fig. 1). Are these measurements statistically different from each other? Enough to back up the arguments regarding the spatial and temporal patterns presented in the manuscript? It would be nice to see the raw data plotted in a histogram to see the distribution of elevation changes within each different moraine region/time period (e.g., Immerzeel et al. (2014)

Fig. 6). This would allow the reader to get a better sense for how the calculated elevation differences vary within a region and a nice comment to Fig. 6.

The caption below Table 3 is changed to clarify the 1-sigma uncertainty. We tested the significance within the designed zones (entire moraine, upper gullied section, lower debris apron) and between all time periods. A column is added to Table 3 with the period numbers of statistically different datasets. Differences per time period over the entire moraine are also always statistically different. The results also indicate that there is a significant difference in elevation change between the upper and lower moraine over the entire period.

Table 3. Seasonal elevation change values, furthermore divided in upper and lower moraine. The zonal mean (μ) is reported, as well as the 1-sigma standard deviation (σ). Precipitation is measured at Kyanjing station in 2013 and Langshisha station in all others seasons. Elevation change values are in m yr^{-1}, precipitation values in mm and mm hr^{-1}. The significance column indicates the periods from which that specific dataset statistically differs ($p < 0.05$).

		Entire moraine			Gullied upper part			Debris apron, lower part			Precipitation	
Period	Dataset	μ	σ	sig	μ	σ	sig	μ	σ	sig	cumulative	mean intensity
1	2013 / 05 - 2013 / 10 (wet)	-0.39	0.79	all	-0.28	0.77	all	-0.41	0.83	2, 4-6	697	0.90
2	2015 / 10 - 2016 / 04 (dry)	-0.17	0.44	all	-0.07	0.43	all	-0.21	0.44	all	145	0.62
3	2016 / 04 - 2016 / 10 (wet)	-0.34	0.57	all	-0.14	0.55	all	-0.41	0.59	2, 5-6	584	0.58
4	2016 / 10 - 2017 / 04 (dry)	-0.36	0.37	all	-0.24	0.37	all	-0.41	0.36	1-2, 5-6	172	0.84
5	2017 / 04 - 2017 / 10 (wet)	-0.52	0.84	all	-0.19	0.82	all	-0.60	0.82	all	541	0.58
6	2017 / 10 - 2018 / 04 (dry)	-0.22	0.98	all	-0.12	0.98	all	-0.25	1.00	all	117	0.50
7	2013 / 05 - 2018 / 04 (total)	-0.31	0.26	all	-0.16	0.26	all	-0.41	0.21	2, 5-6	2257	0.67

The histograms of the entire time period are added to Figure 7.

Technical Corrections: Please see attached PDF for technical corrections.

Thank you for the technical corrections. We have implemented all. The remaining comments are addressed below.

P2l23: Expand on this; I think you are trying to say that the moraines are separated enough from the valley wall that the influence of rockfall/debris input from the valley wall is negligible

Indeed, this is the case for the entire study area. We added Figure 3 and an additional explanation to clarify this.

P3l30: Given that a majority of your conclusions are based on elevation differences derived from DEMs, supplying details on DEM creation and quantifying the uncertainty here is important.

More information on DEM creation is added to the manuscript at P4 (L2-7). Furthermore Table 4 and P7 (L7-10) are added with the uncertainty of each of the timesteps.

P7l27: I'm not sure just reporting the means here is entirely transparent, most of these mean elevation changes have large uncertainties, which are important to include when reporting the data here.

We included the uncertainty estimate to the reported mean values.

P8l9: Report uncertainties along with these values.

This has been included.

Figure 1: A larger photograph would help to give the reader a better idea of the overall geomorphology of the glacier system. / Figure 3: Could these be bigger maybe?

This has been included.

Figure 5: What is the dashed line? Is it the same as in Fig. 4? All parts of the figure need to be defined.

This is now explained in the legend.

Figure 6: Are the blank areas within the data where vegetated areas were masked out? if so, that should be stated in the figure caption.

The caption was changed accordingly.

Figure 10: Parts of this figure are also quite small and hard to read, could it be made bigger?

We enlarged the text in the figure.

Table 3: -units should accompany all columns

-please define μ and σ

Changed

-A histogram or frequency diagram of each of these would be useful to look at the distribution of elevation changes in space and time

The histograms of the entire studied period were added to Figure 7.

References

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