

Interactive comment on “Reduced melt on debris-covered glaciers: investigations from Changri Nup Glacier, Nepal” by C. Vincent et al.

C. Vincent et al.

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Response to Reviewers

We thank the reviewers for their critical comments and suggestions, and feel that the manuscript is substantially improved. We address their comments below. Reviewer comments are in italics, and our responses are in normal font below. Changes to the text have been highlighted in the revised manuscript.

Anonymous Referee #1

General remarks

Debris covered glaciers are widespread in the high mountains of Asia. The mass balance of these glaciers is difficult to observe and glacier area changes do not reveal

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useful information about their balance status. Elevation change measurements are the only way to reveal mass changes of debris covered glaciers. However, the complexity of the surface conditions (debris thickness, ice cliffs, supraglacial streams and ponds), as well as unknown density distributions in the accumulation zone prevent reasonable estimates of mass exchange in many cases. On the other hand, mass balance estimates from debris covered glaciers are urgently required to analyze the general evolution of the cryosphere in Asia. Vincent et al. present a very detailed mass balance study for a rather small glacier in the Khumbu region of Nepal. A large number of measurements have been carried out on this glacier over a period of several years. An effort, which cannot be translated to large debris covered glaciers, typical for many regions in the Himalaya. However, the manuscript also shows ways to exploit remote sensing data for estimating the mass balance conditions of the debris covered part of the ablation zone. These methods and the conclusions for a more general assessment of debris covered glacier tongues provide a valuable contribution to the ongoing efforts of improving the understanding of debris covered glaciers. It especially highlights the problems involved in area wide comparison of clean and debris-covered glaciers. The manuscript is well written, even though several typos can be found in the text. There are also some minor misconceptions, which can easily be corrected (see the following comments). Several of the sections are a bit misleading or unclear. They require some reconsideration for a logical argumentation.

Specific comments

L. 19: Does that mean 25% of the glacier area in the Khumbu is debris covered, or 25% of the glaciers in the Khumbu region are debris covered glaciers?

The first is correct, and the sentence has been changed: “Approximately 25% of the glacierized area in the Everest region is covered by debris, yet the surface mass balance of debris-covered portions of these glaciers has not been measured directly.”

L. 22/23: Note the mass balance of Changri Nup is calculated, but the mass balance

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of the debris covered tongue of the glacier. This is an important difference.

Agree. It has been changed: "...to derive the surface mass balance of the debris-covered tongue of Changri Nup Glacier..."

L. 25/26 and further on: This is not the emergence velocity, but the mean vertical velocity due to mass conservation. Emergence velocity, according to LeB Hooke, 2005, Principles of Glacier Mechanics, p.91, is defined as the sum of the vertical ice velocity and the vertical expression of the horizontal surface velocity due to surface slope. But there only the net uplifting due to mass input is considered.

We do not agree with this comment. Emergence velocity, according to LeB Hooke (2005, p.91) or to Cuffey and Paterson (2010, Equation 8.65, p.332 and section 8.5.5.8 p. 337) refers to the upward or downward flow of ice relative to the glacier surface. As shown in Equation 5.26 in page 91, (LeB Hooke, 2005), considering a glacier in a steady state, the emergence/submergence velocity is given by:

$$b_n = -w_s + u_s \tan \alpha$$

α is the slope, w_s is the vertical velocity at the surface, u_s is the horizontal velocity. It means that the emergence/submergence velocity corresponds to the surface mass balance b_n for a steady state glacier. As shown in the diagrams of Figure 5.10 (p. 91, LeB Hooke, 2005), the vertical velocity is the vertical motion of a marker, not the net uplift. Here, in Abstract, the emergence velocity refers to $\Delta \Phi_{FG} / \Delta t$ (yr⁻¹) in Equation 2 (line 256) of our manuscript. It is calculated from the ice flux through the cross section M. It would correspond to the average surface mass balance of the area below cross section M for a steady state glacier.

L. 30/31: This sentence is a bit misleading. You probably mean that the insulating effect of the debris cover has a much larger effect on the total mass loss than the enhanced melt in ponds and at ice cliffs.

Good suggestion, we have adjusted the sentence: "The insulating effect of the debris

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cover has a larger effect on total mass loss than the enhanced ice ablation due to the supra-glacial ponds and exposed ice cliffs."

L. 41.. surface from the atmosphere. ...

Agree. It has been changed.

L. 44 .. the surface is covered by a very thin layer. ...

Agree. It has been changed.

L. 54 and further on: Referencing authors directly in the sentence requires another style: Ragletti et al. (2015)

Agree. It has been changed.

L. 56/57: This sentence is a somewhat unclear. Do you mean: This question of differences in area-averaged melt rates between debris-covered and clean glacier areas remains unanswered.

Agree. It has been changed.

L. 74: Maybe it is good to add, that the spatial pattern can only be resolved by including additional information, like ice surface velocity, local point mass balance etc.; as you also did in your analysis.

Even with additional information, it is very difficult to obtain the spatial pattern of surface mass balance from geodetic measurements using satellite or aerial imagery. Some studies attempted to do it but the results are not convincing. For instance, Kääb and Funk (1999) attempted to infer surface mass balance from surface elevation, horizontal and vertical velocities, using photogrammetric data and GPR surveys. They obtained an accuracy of ± 0.7 m a⁻¹ on the calculated mass balance and they show that the method is most suitable for planar glacier sections. In consequence, although this method requires numerous additional information, the spatial pattern of surface mass balance cannot be solved properly.

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L. 77: DEMs constructed from terrestrial photogrammetry surveys. . .

Agree. The change has been done.

L. 78. . . and DEMs based on two satellite stereo pairs. . .

Agree. Done

L. 90: remove “during”

Agree. Done

L. 95: remove “and”

Agree. Done

L. 98: exchange “stands” by “exists”

Agree. Done

L. 116: “fixed focus lenses” Did you use more than one lense in the system?

One lens only. “es” of “lenses” has been removed.

L. 128: In order to obtain 1 cm accuracy in elevation, some effort is required. Please explain how you obtain such good accuracies (baseline length, observation times, GNSS coverage).

In the new version of the manuscript, we have added a subsection on the DGPS measurements (section 3.1), and additional details with respect to the individual measurements: Baselines length for GCP measurements do not exceed 2500 m”, “. . .the observation time is generally one minute with 1-second sampling and the number of visible satellites is greater than 6.”

L. 132: How do you reach such low uncertainties of 6 cm, which are considerably less than the pixel size?

6 cm is the RMSE at control points used for photogrammetric restitutions. This value is

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not representative of the final accuracy on XYZ given that it depends on the number of control points used for photogrammetric restitutions. The real assessment of accuracy comes from the comparison between DGPS and photogrammetric elevations for independent GCPs. These differences have been calculated from 25 independent GCPs and have a root mean squared error of 0.63 m (see Section 4.3.3). For the sake of clarity, we add some information in the new version of the manuscript.

L. 145: What observation times were used for the position calculation? Was this done by kinematic or static GNSS measurements?

The observation time is one minute, with one-second sampling. It has been done using static measurements. These information have been added in the manuscript.

L: 148-152: This description of identifying the correct bed reflections is a bit vague. Please explain this in more detail. In addition, this section is only “cut and paste” from Azam et al., 2012.

In response to this comment, some explanations have been added : “Field measurements were performed so that reflections from the glacier bed were more or less vertically aligned with measurement points at the glacier surface, which allowed the glacier bed to be determined in two dimensions. Radargrams were constructed by plotting individual radar traces side-by-side using the DGPS-measured distance along the transverse profiles. Strong reflectors close to surface were interpreted as either air within the debris layer or the ice surface. At depth, strong reflections were interpreted as the ice/bedrock interface. ”

L. 152/153: I do not understand why you apply a second migration? The envelope method (commonly used in single shot seismics) is kind of a “manual” migration which already gives the nearest reflection locations in itself.

Agree. The explanations have been changed in the new version of the manuscript.

L. 160: With 10 s acquisition time it is impossible to reach a 1 cm accuracy as stated

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above, especially in the vertical coordinate.

Agree. However, we did not write that measurements have been done with 1 cm accuracy. 1 cm accuracy is relative to measured tie points with longer observation time (section 3.1). Here, the accuracy of individual position is assessed to 5 cm. Consequently, the uncertainty on the displacement is assessed to $5\sqrt{2}$, i.e. 7.1 cm. For this reason, we wrote "Maximum uncertainty is ± 0.1 m for horizontal and vertical components. . ." (lines 162-163). For the sake of clarity, we mentioned "with a 5 cm accuracy for each individual position" in the revised version of the manuscript.

L. 165: Did you replace the stakes at the original location or at the location of the stake?

Yes, stakes were replaced at their original locations. This has been clarified in the new version of the manuscript

L. 179. . . . used also for the photogrammetry. . . .

Agree. The change has been done.

L. 186: DGPS

Agree. The change has been done.

L. 192/193: Can you provide some information about flight elevation above ground and mean pixel size.

Average flight elevation above the surface was 325 m (L177). The original resolution/pixel size of the orthomosaic is 10 cm, and the DEM is 20 cm, and we have included this information in the revised manuscript.

L. 196: Is there a reasonable argument for using a kriging algorithm for resampling a regular grid? Kriging was done only for the photogrammetry DEM, but the 20 cm UAV DEM was simply resampled. This has been edited in the text: "Before estimating elevation changes the photogrammetric DEM was interpolated to a regular 5 m resolution raster using a kriging interpolation method, whereas the UAV DEM was aggregated to

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the same resolution from its original 20 cm resolution."

L. 211. . . . larger than three times. . .

Done

L. 243. . . . so annual local surface mass balance. . .

Done

L. 256: Are you sure the + sign in the formula is correct. This depends on the sign of your mass balance. Please specify.

We checked the signs. The "+" sign is correct. Indeed, the surface mass balance B is negative in the ablation area (positive in the accumulation area). The average emergence velocity is positive in the ablation area. Consequently, the sign of thickness change will depend on the sum between the absolute value of mass balance (i.e. net ablation) and the emergence velocity. If the net ablation (here 1.21 m w.e. a⁻¹) exceeds the emergence velocity (here 0.37 m w.e. a⁻¹), the elevation change will be negative (here -0.84 m w.e. a⁻¹).

L. 268 ff.: I do not really get the point about the importance of accurate glacier delineation. As long as there is ice underneath the debris cover, there exists a mass balance. Even though ice flow might be negligible into almost stagnant marginal regions, the error made by including small areas of stagnant ice is negligible. Moreover, you state in line 273 that the discrimination is "challenging", but in line 285 the delineation of the terminus is "clear"?

We respectfully disagree. The error made by including small areas stagnant ice is not negligible. An error of a factor 2 in the downstream area will translate in an error of a factor 2 in the area-averaged emergence velocity. In our case, the uncertainty relative to the delineation of the surface area is assigned a value of ± 20 m (lines 412-413). It leads to an uncertainty of 109460 m² on the surface area located downstream of cross section M. This results in an uncertainty of 6.1 cm w.e. a⁻¹ on the final result (in

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term of average mass balance calculated on the debris covered area). In our study, this uncertainty remains weak because the delineation of the tongue is relatively clear and it has been done from field surveys. In other cases, it could be problematic and could be a large source of uncertainty. Regarding your second remark, we wrote in line 273 that "From remote sensing optical images, it is very challenging to delineate the margins of debris covered glaciers". This is one of the strong limitations of remote sensing optical images as mentioned in other studies (e.g. Paul et al., 2013). Our field surveys of surface velocity allowed us to accurately delineate the active ice area in most regions.

L. 287/288: Is there a threshold for ice velocity when applying the delineation criterion?

We typically use a threshold ice velocity of zero, interpolated from stake measurements, to delineate the active ice area when it is unclear. In the upper part of the area, the ice velocity close to the margin is far from 0 and can reach 2 m a⁻¹. However, the delineation of margins in the upper part is not problematic given that the margin is obvious in the field. Near the terminus we have also used field-based observations (L295-L297 of the revised manuscript) to delineate the active ice area and the delineation does not correspond exactly with the zero ice flow velocity contour (Figure 3). At other locations for which the glacier margin was unclear, we used the interpolated ice flow velocities and delineated the margin at locations where the ice flow velocity is zero.

L. 296:...assessed as 79300 m²...

Done

L. 314: Is there any information about annual variations in surface velocity? This might support your arguments.

Unfortunately not. The field measurements needed to derive ice flow velocities have always been carried at the same period of the year (November or December).

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L. 319/320: Did you use the mean cross sectional area as well? What error is introduced by this mean value?

We do not understand what "mean cross sectional area" means. The cross-sectional area was determined from the GPR ice depth measurements at profile M. The uncertainty on our calculated cross sectional area (L419-422) is 7000 m² i.e., or about 9% of the cross sectional area. It results in an uncertainty of 3.6 cm w.e. a⁻¹ on the final result (in term of average mass balance calculated on the debris covered area).

L. 330...along profiles...

Corrected.

L. 331: What do you mean with "homogeneous elevation changes"?

In clean-ice areas, interannual elevation changes are expected to have low spatial variability across the glacier. Revised text reads: "In general, elevation changes in clean-ice areas are expected to have low cross-glacier variability (Berthier and Vincent, 2012; Fischer et al., 2005; Vincent et al., 2009). At profile M, this is confirmed by the similarity in elevation profiles between years, and the mean rate of elevation change is -0.8 m a⁻¹ between 2011 and 2015 at this location (Fig. 5)."

L. 335...the mean rate...

Done

L. 337/338: This is only because the region between profiles M and N is rather small, with a steady surface slope.

Yes, we have clarified this in the text.

L. 346...were obtained...

Done

L. 338/347: The results indicate that the mean surface change of the clean ice section

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below line M is considerably less negative than for the debris covered part. Can you comment on that specifically (in the discussion part)?

The mean elevation change between profiles M and N is -0.65 m a^{-1} . On the other hand, we obtained a mean elevation changes of -0.95 m a^{-1} downstream profile N. However, a direct comparison is not possible here given that the considered areas are located at very different elevations and with different emergence velocities. The only relevant comparison is about the difference between the mean surface mass balance calculated over the debris-covered area and the mean surface mass balance calculated on a similar clean ice surface area as it has been done in Discussion section.

L. 356-368: This section is rather unclear to me. This requires a clearer structure, which data have been compared with which.

Thank you for the suggestion. We have moved the validation section up above the short section on area-weighted elevation and mass changes, and revised the text to have a clearer structure:

“Elevation changes obtained from photogrammetry and UAV data have been validated using DGPS measurements and high-resolution stereopair satellite imagery. First, we directly compare point elevation data from photogrammetric and UAV DEMs with DGPS elevations observed at independent GCPs (i.e. those not used in DEM generation). Differences between DGPS and photogrammetric elevations for 25 independent GCPs near the terminus and profile R have a root mean squared error (RMSE) of 0.63 m. A similar comparison between DGPS spot heights and UAV-derived elevations at 10 independent points gives a RMSE of 0.25 m. A direct comparison between photogrammetric and DGPS elevations on cross-glacier profiles (Fig. 5) shows that differences are generally less than 1 m. We also compare surface elevation changes obtained from photogrammetric DEM differencing and repeat DGPS measurements (Table 1). As photogrammetric measurements are incomplete along the transverse profiles due to terrain obstruction, we only consider the sections of the profiles where both DGPS

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and photogrammetric elevation data are available. At profiles R, P, and Z the differences in rates of surface elevation change measured with the two approaches are approximately 0.1 m a^{-1} . However, repeat DGPS measurements obtained from transverse profiles are not sufficient to obtain a representative mean elevation change of the tongue despite the numerous profiles. This is a direct result of the high spatial variability of elevation changes in the debris-covered area of the glacier. From repeat DGPS profiles, we found a mean elevation change (2011-2014) of -0.6 m a^{-1} (Table 1). In comparison, we observed a mean elevation change of -0.95 m a^{-1} over the debris-covered area from photogrammetry and UAV data. Consequently, there is no agreement between the mean elevation changes obtained from repeated profiles along the debris-covered tongue and the area-averaged elevation change. Given the large spatial variability in surface height changes over the debris covered tongue (Figure 6) and the lack of any clear relation between surface height change and elevation (Table 2; Figure 6), DGPS profiles cannot be used assess mean elevation change over debris covered glaciers. As a final test, elevation changes downvalley of the delineated glacier terminus were calculated from photogrammetry and UAV data. In this small (0.014 km^2) region, average thickness changes of -0.07 m and -0.18 m were observed over the periods 2011-2014 and 2011-2015, respectively. These are not significantly different from zero, when the margin of error is considered. However, the unconfirmed presence of stagnant ice in the test area may lead to the slightly negative surface height changes (e.g. Figure 6c). Finally, photogrammetric and UAV-derived elevation changes (2011-2015) can be compared to elevation changes measured from the stereopair DEMs (2009-2014), though the periods of measurement are slightly different. From 2009 to 2014, we find a mean elevation change of -0.88 m a^{-1} on the debris-covered tongue downstream of profile M (Table 1; Fig. 6c). If we consider only the areas where the UAV-photogrammetric elevation changes are calculated, the mean elevation change is -0.95 m a^{-1} (Fig. 6c). This compares well with the mean elevation change of -0.96 m a^{-1} obtained from photogrammetry and UAV data. Given the uncertainty in the ground-based measurements, elevation changes derived from satel-

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lite imagery support the assumption that elevation changes measured on 60% of the tongue are representative of the whole area. " L. 369-373: I do not share this conclusion just from the details given. There should be a specific example for this conclusion.

These conclusions result directly from results shown in Table 1, and have been rewritten slightly as above. The first conclusion "the photogrammetric results are consistent with DGPS measurements" comes from the comparison between DGPS and photogrammetric results shown in Columns R, P, V and Z of Table 1. The second conclusion "repeated DGPS measurements obtained from transverse profiles are not sufficient to obtain a representative mean elevation change of the tongue..." results from the comparison of elevation changes between R, P, V and Z (lines 3 and 5 in Table 1) which shows large spatial differences. We added some explanations in the revised version: "In our case, the mean 2011-2014 elevation change from DGPS profiles is equal to -0.6 m a-1 (mean of profiles R, P and Z or M, R, P and Z) compared to -0.95 m a-1 over the debris-covered area from photogrammetry. Consequently, there is no agreement between the mean elevation changes obtained from repeated profiles along the debris-covered tongue and the area-averaged elevation change. But more importantly, even though there had been more profiles and better distributed along the tongue, the large spatial variability of elevation change observed over the debris covered tongue and not related to elevation (-1.3 m a-1 at profile P, compared to -0.2 or -0.3 m a-1 at profiles R and Z respectively, although located higher and lower than profile P) prevent from assessing a reliable mean elevation change from profiles."

L. 374: ...outside the delineated glacier terminus...

Done

L. 378: ...in the test area...

Done

L. 391: Which mean elevation changes are considered here?

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It has been specified in the new version of the manuscript.

L. 398: Are you sure about the signs? Usually this formula is given as input – output. Then the sign of the mass balance is correct.

It is correct. See the Reply to comment above (l. 256).

L. 399: Do you mean "B" instead of "b"?

Agree. It has been changed.

L. 416-418: This part is again unclear. How do you derive this uncertainty of 0.1 m from the values given above?

We derive an uncertainty of 0.1 m a-1 from the following comparisons. The elevation change has been assessed as -0.95 m a-1 between 2011 and 2014 (from photogrammetry), -0.96 m a-1 between 2011 and 2015 (UAV data and photogrammetry) and -0.95 m a-1 between 2009 and 2014 (Pleiades DEMs). These figures are very consistent. We assume that the main uncertainty on these figures result from the fact that these results have been obtained over 60% of the ablation area due to terrain obstruction. The satellite measurements show that the mean elevation change obtained on 60% of the surface differs by 0.07 m from the mean elevation change calculated on the whole surface area. Due to this difference, one can assume that the uncertainty on elevation change does not exceed 0.1 m a-1.

L. 433/434: If the method does not work on a single glacier (stated above), it cannot work for a comparison of different glaciers. I agree that the direct determination of the surface mass balance from stakes is not possible on debris covered glaciers. But in combination with debris thickness distribution from other methods, the stake values can be used to model the surface mass balance. There are several published examples.

The purpose of our study is to obtain the mass balance of a debris covered glacier from measurements. Here, we showed that the very strong spatial variability in height changes (Fig. 6) does not result from the spatial variability of emergence velocity. It

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comes from the spatial variability of surface mass balance. Consequently, the surface mass balance obtained at individual stakes is not relevant to assess the surface mass balance of the debris-covered area. This is the only message of this section 5.1. The calculation of surface mass balance of the debris-covered area using numerical modeling is beyond the scope of our purpose.

L. 464-466: It is representative within certain bounds, given by the difference of the mean elevation change for the two areas.

We are not clear what this comment refers to.

L. 477: Does “lower” mean “more negative” in this context?

Right. It is more negative. The sentence has been changed for the sake of clarity.

L. 480-486: Again, this section is a bit unclear, because different areas, SMB values and gradients are mixed. Please try and restructure this section.

New text reads: “The average SMB assessed over the debris-covered Changri Nup Glacier tongue (-1.21 ± 0.2 m w.e. a⁻¹) is similar to directly observed SMBs at profile M (-1.50 and -0.85 m w.e. a⁻¹), and less negative than measurements from the stake farm (-1.35 to -1.98 m w.e. a⁻¹). This implies that (i) the average SMB of the tongue would be much more negative if it was debris-free, and that (ii) the stake farm measurements are not representative of melt rates over the rest of the debris-covered area. To estimate the effect debris cover has on the SMB, we estimate the average SMB for Changri Nup with the vertical gradient of SMB (-1.4 ± 0.5 m w.e. (100 m)⁻¹; Fig. 7) observed at a nearby debris-free glacier (White Changri Nup). Extrapolating from the mean observed SMB at profile M (-1.16 m w.e. a⁻¹), we estimate that an area-averaged SMB of -3.0 m w.e. a⁻¹ would be found for the entire debris-covered area if it were debris-free. The difference between the debris-covered and theoretical debris-free SMB estimates (1.8 ± 0.6 m w.e. a⁻¹) represents the overall reduction in melt due to debris cover.” L. 480: mean observed SMB at profile M should be negative

C15

Agree. The sign “-“ was omitted. It has been changed in the new version.

L. 481: -3.0 m w.e. a⁻¹: Is this the areal mean or the value along the gradient?

It is the areal mean given that this theoretical SMB averaged over the tongue has been obtained by multiplying every 50 m altitudinal area by its corresponding SMB. . . as mentioned in lines 482-484. The revised text should clarify our methods here.

L. 492: I suggest another publication in which the contribution of ponds and cliffs is seen more critically: Juen et al., 2014, The Cryosphere.

Agree. It has been added.

L. 509: remove the closing bracket.

Done.

L. 514/515: This surface mass balance only is valid for the considered area.

Agree. We added “in the studied region”.

L. 517/518: This statement is not backed by any data or arguments in the manuscript. It results directly from our observations: given that the mass loss is reduced by 1.8 m w.e.a⁻¹, in the debris covered area, and that supraglacial ponds and ice cliff enhance the ablation, we can conclude that the insulating effect of the debris cover dominates the impact of supraglacial ponds and ice cliffs. L. 521: Again, I doubt that the precise delineation needs to be so precise. In addition, it will only add a small error for large glaciers.

We believe that accurate delineation of the terminus is critical in our approach. As explained above, the error made by including small areas stagnant ice is not negligible. In our case, the uncertainty relative to the delineation of the surface area is assigned a value of ± 20 m (lines 412-413). It leads to an uncertainty of 109460 m² on the surface area located downstream of cross section M. It results on an uncertainty of 6.1 cm w.e. a⁻¹ on the final result (in term of average mass balance calculated on the debris

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covered area). In our study, this uncertainty remains weak because the delineation of the tongue is relatively clear and it has been done from field surveys. In other cases, it could be problematic and could be a large source of uncertainty.

L. 524/525: The method works for every cross section flux. It does not necessarily beat the transition from debris to clean ice.

Agree. In response of this comment, we removed “located upstream the debris-covered area” in the new version of this manuscript.

Fig. 1: Is it possible to indicate the elevation range in the figure (maybe 500 m isolines)?
Done.

Fig. 2: Where are the locations of the terrestrial photogrammetry? Where is the location of the reference GPS?

The locations of the cameras for terrestrial photogrammetry are outside the figure extents. In the revised figure we have added the DGPS base station location.

Fig. 5: It is probably more instructive to show the differences instead of the absolute values.

We initially considered this approach, but showing absolute elevations allows us to see the influence of the relief on thickness changes.

Fig. 7: The geometry of the two grey boxes probably show different properties. In my interpretation the height of the dark grey box is the error of the SMB, while the height of the light grey box shows the SMB variability due to the differences in elevation. Is this correct?

Figure 7 and the caption have been changed for the sake of clarity.

L. 774: remove the first “only”. Done Reviewer #2 J. Steiner (Referee)

General Comments:

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The authors submitted a mass balance study of a previously researched debris covered glacier in the Nepalese Himalaya, based on a number of years of field data including stakes, GPR measurements, DGPS measurements, terrestrial photogrammetry as well as UAV surveys. Their measurements are distributed over the ablation area which is predominately covered by debris. They furthermore compare their results to other MB data from nearby clean ice glaciers. MB studies, especially including field data, are still rare and a very important contribution to current literature for the region. This is especially true for the ongoing discussion of the effect of debris on glacier melt. The manuscript is therefore an important contribution to current science and well suited for the Journal. The authors present their data very well and provide a clear description of the work process. They also provide important results in terms of applicability of local stake measurements for wider MB studies. The results could well be used by other field studies as a solid comparison and should be a reference publication for future remote sensing studies that make judgements about the differences between debris covered and bare ice glacier's response to climate change in the region.

Specific comments:

L31 / L526: Stating in the manuscript that it will have ‘major’ implications for future work is – personally speaking – not called for as this judgment should be left to subsequent researchers referring to this work or using its Results. If the authors however think it is necessary for their work to use this wording, at least a strong backing for it should be provided. While I agree that the study is a significant contribution and there are many arguments in the text that warrant that, I believe that after it is stated in L526 there is no actual explanation which are these specific implications (and the reader is left to judge him- or herself). I believe it should be made more clear which of the excellent results the authors believe lead to the major implications (also because ‘results’ is not equal to ‘implications for future work’).

This is a fair comment, and perhaps we were overzealous in the abstract. The final sentence of the abstract now reads: “The insulating effect of the debris cover has a

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larger effect on total mass loss than the enhanced ice ablation due to supra-glacial ponds and exposed ice cliffs. This finding contradicts earlier geodetic studies, and should be considered for modeling the future evolution of debris-covered glaciers. ”

Section 4.2 – on cross sectional velocity (Fig.4): There seems to be a reduction in velocity between 2011/2012 and 2014/2015 (Fig. 4b). Although it is only reasonable to take a mean value, perhaps a discussion of this trend would be prudent also in relation to a trend in SMB over the study period.

With 30 years of experience measuring velocities in the Alps, we think that discussing a “trend” from 3 years of ice flow velocities or SMB measurements could be risky. For this reason, we preferred to take the averaged value and to include the differences to the mean in the uncertainties.

L 491ff: Although the authors conclude that for Changri Nup the insulating effects dominates likely enhancing melt factors like ice cliffs and lakes, considering the observed consistent differences in local elevation changes (Table 1), it would be interesting to see– without a detailed study which would of course exceed the extent of the manuscript with its current aim - whether for example the higher rates around cross section P and compared to others correspond to an increased occurrence of cliffs and lakes.

In response to this comment, it is true that we could compare directly the elevation changes with the presence of supraglacial ponds and ice cliffs. A quick look on the comparison between the elevation changes and the presence of ice cliffs show that the elevation changes are more negative at the location of the ice cliffs. However, we do not wish to include this comparison in this paper for the following reasons. First, this comparison would remain qualitative. Indeed, elevation changes are not only a function of surface melt, as spatially variable emergence rates complicate the picture. Second, a thorough comparison needs additional data and thorough analysis which is beyond the scope of the manuscript. It should be the purpose of a future study.

C19

Minor Comments/Technical Corrections:

L19: Since that number ($>1/4$) is often confusingly defined in many studies as it is sometimes not clear whether authors mean ‘just debris covered area’ or ‘the cumulative area of all glaciers with debris cover’ (the way it is used in the manuscript is correct) it would be prudent to refer to a publication at this place.

We also addressed this comment in our response to reviewer 1. The new text reads: “Approximately 25% of the glacierized area in the Everest region is covered by debris”, and we have included references for this statistic in the Introduction (L39-40).

L133: I believe ‘accrued out on’ is neither correct English nor is it really clear what is meant with it.

Yes, this has been removed.

L196: ‘Kriging’

Done

L211: ... the elevation was three times larger than the normalized median absolute’

Done

L335: ‘...profile M, and the mean rate of...’

Done

L346: ‘Mean elevation changes ..were (!, plural) obtained’

Done

L363: ‘a RMSE’

Done

L366: ‘changes were compared’ (tense!). L366: I do not think that ‘reduced profiles’ is the correct term. Either repeat a word and ‘incomplete profiles’ or simply ‘on these

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profiles’.

Sentence has been revised: “As photogrammetric measurements are incomplete along the transverse profiles due to terrain obstruction, we only consider the sections of the profiles where both DGPS and photogrammetric elevation data are available.”

L378: what is meant with ‘check area’?

It has been changed

L399: the variable ‘b’ is ‘B_M’ in the equation

It has been changed.

L420: I would expect a citation for the ice thickness uncertainty. Other studies even provide lower estimates (e.g. Gabbi, 2015)

Agree. We added a citation for the thickness uncertainty in the new version of this manuscript. However, we did not cite Gabbi (2015) given that the GPR measurements carried out in her study have been performed from different instruments (helicopter-based GPR surveys RST Radar Systemtechnik GmbH and ground-based GPR surveys with very large range of frequency (4.2, 50, 150 MHz). In the study of Gabbi (2015) or Gabbi et al. (2012), the authors provide an “overall uncertainty of ± 5 m” (Gabbi et al, 2015, section 3.1.2) which is the result of GPR measurements combination. Here, in our study, we used ground-based GPR surveys similar to the instruments used in Bauder et al. (2003). From a comparison between GPR soundings and boreholes, they found a mean difference of 8.4 m with a RMS of 26.7 m. Note that, in this study, the boreholes have not been performed on the GPR profiles. The differences result from the comparison between interpolated data using GPR data and depth measured in boreholes. However, an uncertainty of ± 5 m would be certainly optimistic. In the new version of the manuscript, we provide an uncertainty of ± 10 m with a reference to Bauder et al. (2003) study.

L 521: Remove ‘Indeed’

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Done

L522: ‘...ice flow velocities derived from DGPS field measurements...’

Done

Figures and Tables: Table 1: Explain what the letters M to Z refer to (i.e. ‘the letters refer to cross sections as in Fig. 2’)

Done

Figure 1/L735: ‘...delineation of...’

Done

Figure 4/L753: ‘...a second order polynomial function...’

Done

Figure 7/L774: remove first ‘only’

Done

References

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