

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

Reviewer #2 J. Steiner (Referee)

General Comments:

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

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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 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

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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.

Minor Comments/Technical Corrections:

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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 profiles’.

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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’

Done

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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

Bauder A., M. Funk, G. Hilmar Gudmundsson. The ice-thickness distribution of Unteraargletscher, Switzerland. *Annals of Glaciology* 37, 331-336, 2003

Gabbi, J. (2015). ON THE UNCERTAINTIES IN GLACIER MASS BALANCE MODELLING, Doctoral Dissertation, ETH Zurich, VAW J. Gabbi, D. Farinotti, A. Bauder, and H. Maurer. 2012..Ice volume distribution and implications on runoff projections in a glacierized catchment. *Hydrol. Earth Syst. Sci.*, 16, 4543–4556, 2012. doi:10.5194/hess-16-4543-2012

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