

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

Anonymous Referee #1

Received and published: 1 June 2016

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 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

C1

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?

L. 22/23: Not the mass balance of Changri Nup is calculated, but the mass balance of the debris covered tongue of the glacier. This is an important difference.

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 LeBlond and 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 here only the net uplifting due to mass input is considered.

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.

C2

- L. 41: ...surface from the atmosphere...
- L. 44: ... the surface is covered by a very thin layer...
- L. 54 and further on: Referencing authors directly in the sentence requires another style: Ragletti et al. (2015)
- 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.
- 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.
- L. 77: DEMs constructed from terrestrial photogrammetry surveys...
- L. 78: ... and DEMs based on two satellite stereo pairs...
- L. 90: remove "during"
- L. 95: remove "and"
- L. 98: exchange "stands" by "exists"
- L. 116: "fixed focus lenses" Did you use more than one lense in the system?
- 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).
- L. 132: How do you reach such low uncertainties of 6 cm, which are considerably less than the pixel size?
- L. 145: What observation times were used for the position calculation? Was this done by kinematic or static GNSS measurements?

C3

- 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.
- 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.
- L. 160: With 10 s acquisition time it is impossible to reach a 1cm accuracy as stated above, especially in the vertical coordinate.
- L. 165: Did you replace the stakes at the original location or at the location of the stake?
- L. 179: ...used also for the photogrammetry...
- L. 186: DGPS
- L. 192/193: Can you provide some information about flight elevation above ground and mean pixel size.
- L. 196: Is there a reasonable argument for using a kriging algorithm for resampling a regular grid?
- L. 211: ...larger than three times...
- L. 243: ...so annual local surface mass balance...
- L. 256: Are you sure the the + sign in the formula is correct. This depends on the sign of your mass balance. Please specify.
- 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

C4

of the terminus is “clear”?

L. 2897/288: Is there a threshold for ice velocity when applying the delineation criterion?

L. 296: ...assessed as 79300 m² ...

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

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

L. 330: ...along profiles ...

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

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

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

L. 346: ...were obtained ...

L. 338/347: The results indicate that the mean surface change of the clean ice section below line M is considerably less negative than for the debris covered part. Can you comment on that specifically (in the discussion part)?

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

L. 369-373: I do not share this conclusion just from the details given. There should be a specific example for this conclusion.

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

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

C5

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

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.

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

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

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.

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

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

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.

L. 480: mean observed SMB at profile M should be negative

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

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

L. 509: remove the closing bracket.

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

L. 517/518: This statement is not backed by any data or arguments in the manuscript.

C6

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.

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

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

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

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

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?

L. 774: remove the first "only".

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-75, 2016.