

Review of TC-2016-202 paper by Lamsal et al.

The paper by Lamsal et al. presents an estimate of glacier mass balance and surface velocities of the Kangchenjunga glacier in the E. Himalaya from 1975 to 2010, using Hexagon KH-9 and ALOS imagery. The topic is relevant to the Cryosphere, and this type of study is much needed for the eastern part of the Himalaya, where glacier mass balance is poorly studied. While I appreciate the authors' efforts for creating the DEMs needed for this study, I think the paper needs much improvement before it can be published. Some of the more technical comments on the DEM generation, glacier mapping etc were already addressed by reviewer 1, and I agree with those suggestions. In addition to those technical details which I also believe should be addressed, my major concerns are:

1. **Extent of study area / representativeness: The main concern here is the focus on one glacier only seems insufficient for this study**, especially since the study is based on remote sensing data. I strongly advice extending this to a larger extent, which would allow to better examine spatial tendencies of elevation changes across a larger region. Given that the authors already have the two sets of images (ortho-rectified and referenced), this would require minimal effort. The choice of **one** glacier only would be justified if the authors had field-based mass balance observations to compare the geodetic mass balance estimates with, but this is not the case.
2. **Focus on mass balance: This is the second major concern.** I find it problematic to expand db/dz from the ablation area only to infer mass balance over the entire surface of the glacie, especially since the DEMs derived are sparse and do not cover the entire glacier surface. Also, the 4 scenarios for estimating mass balance from surface lowering are not very clear- and averaging all these scenarios lead to too much uncertainty. Furthermore, db/dz cannot be directly interpreted as mass balance changes unless the full glacier dynamics component is evaluated.
3. **Objective of the paper:** Given these concerns above, I suggest revising the objective of the paper, for example focusing on glacier thickness changes based on DEMs as in other studies (Kääb et al. 2012, Gardelle et al. 2012a) over a larger area, and compare with trends reported in these studies. Then potentially refer to mass balance changes, but with a lot of caution because of lack of validation. However, I suggest that these be a secondary (and less emphasized aspect). I believe this would provide a much more valuable contribution.
4. **Role of supra glacier lakes/ice cliffs:** This section is somewhat vague. The authors map the lakes based on the DEM (this should be revised as well to include spectral signal), and hint at the role of the lakes, but it is not clear whether the elevation changes reported over the surface of the lakes takes into account any shifting of the lakes, ie have the lakes stayed in the same place and just lowered (seems not likely so)?
5. **Figures and tables:** the paper seems quite cluttered with a lot of graphs, some of which are unnecessary (see detailed comments below). It lacks in some illustrations of the imagery, also a figure showing the extent of the images used.
6. **English language:** Some improvement in the English form is necessary- some paragraphs are hard to follow. Please see detailed comments. For example use of past tense vs. present perfect, and other nuances underlined below.

7. General structure of the paper: This also needs improvement. Most of the sections are not consistent enough on their own; also there is some mix up between study area, results and discussion. Again, comments below.

I think the paper has potential and the dataset developed seems appropriate at least for an estimate of glacier thickness changes (with perhaps hints at geodetic mass balance), but it would require major revisions. I suggest the authors extend the estimate of glacier elevation change at larger scale, given the lack of field-based measurements on Kangchejunga, and address the concern above before they can resubmit the paper.

Specific comments:

P1 I 23: “ In recent decades, glaciers in the Himalayas, once one of the most glacierized mountainous areas in the world, were widely losing mass, though they exhibited a high degree of spatial heterogeneity”
Please rephrase; past tense not appreciate here

P1 I 27: For instance, glaciers in the Khumbu region of Nepal have been frequently studied (e.g. Nakawo et al., 1999; Bolch et al., 2011; Nuimura et al., 2011, 2012; Salerno et al., 2015).
What have these studies found? What’s the gap?

P1 I 27 – 33: The authors could focus on the area of interest, and the missing gaps, rather than focusing on the Khumbu and the studies conducted there.

P1 I 33: It would be good to mention here that this study has the potential to complement the study by Racoviteanu et al. (which, in the initial version of the paper did present elevation changes but from a different dataset, with topo maps). However, the study would need to be extended to a larger area (see my previous comments).

P2 I 11: “.. insulation effect of the debris mantle (Scherler et al., 2011)”
This statement is not fully accurate; debris cover does not always have an insulating effect (only when thickness > critical thickness). Other references (Mihalcea et al. 2008, Foster et al. 2012) and others are more appropriate than Scherler et al.

P2 I 10- 13: this phrase is vague, please rephrase. Also, do you mean here “variability” in debris thickness, and not variation? Variation implies change.

P2 I 17 – 19: the discussion of debris covered glacier tongue, with some hints at SRTM/ASTER DEMS is vague, please revise. What is the main point here? Lack of adequate mass balance estimates of debris covered glaciers at large scale? If so, then the current study should propose to fill this gap by estimates at larger spatial scales.

P2 I 20: The study cannot “document” mass balance (there are no field based measurements)- only “estimate”. Please rephrase.

P2 I 23 satellite data are rarely referred to as “historical”. I suggest replacing with “multi-temporal satellite imagery”

P2 I 24-25: “It is also aims for a better understanding of the effects of supraglacial ponds and other factors on ongoing changes in debris-covered glaciers under the recent climatic change.”
Again this second objective is vague- what kind of other factors? What kind of changes?
The role of supra glacier lakes is not fully quantified- so, this objective is rather a speculation, or an item to include in a discussion, but it does not seem to me to be justified as an objective.

P 3 l 36: this is not clear: images were dated 2008 for lakes, 2010 for velocity...do you mean multi-temporal images were used? Why did the authors not use the same images for lake, velocity and DEM generation?

P3 l 5 – 10: the section on glacier mapping is very brief, not much detail is given here. Were there various iterations? What about problematic areas such as frozen lakes, shadows that are often encountered, how were those dealt with? Was clean ice delineated separately from debris cover? And what about glacier lakes? The statement about ASTER DEM to extract hypsometry is confusing. If the goal was to derive the surface in the 1970s, then the 1970's derived DEM should be used, not the ASTER GDEM. Please address.

P 3 l 11 “DEM generation from ALOS PRISM imagery”
The authors use both DTM and DEM. These terms are usually interchangeable and refer to the same thing. Please revise this throughout-

P 3 l 22 “The LPS Terrain Editor was used to correct and minimize errors contained in the automatically generated DTMs.”
Please explain what does this mean in LPS- what kind of algorithm was used? What kind of corrections were applied?

P3 l 26: distortions in declassified images do not come only from storage and processing, but also from the camera. Hexagon have considerably less distortions than Corona for example. Please include some references here about distortions present in Hexagon.

P3 l 27: what is meant by “reseau grid”?

P3 l 29 by “ALOS stereo model” you mean the ASTER DEM? Please use the same terms throughout.

P3 l 31 “off-the-glacier area, at which unchanged terrain is expected”
Awkward phrasing, please revise English language

P 3 l 33- 34 again, DTM and DEM terms are used

P4 l 1 please revise title of the sub-section “Unmeasured accumulation area” – what about it? This seems to me it pertains to one of the previous sub-section, not a section by itself.

P4 l 4 what do you mean by “continuous DEM”? please revise. Also l4-5 contain results- and these have not been reported yet.

P4 l 5 “Owing to” is awkward. Suggestion “due to..” but to not start the phrase with this. First state the issue and expose a potential explanation.

P4 l 8 the “average” of what?

P4 l 10: “Because the ELA of the glacier is unmeasured, it is assumed to be around 5800-5850 m, a rough estimate based on the geodetic elevation change, at which no change of glacier surface elevation is found for the studied period of 1975–2010”

This is confusing: the ELA is unmeasured, and the geodetic elevation change is not yet estimated- so how can the ELA be assumed then? This seems circular. The entire section here should be revised, there seem to be too many assumptions with no measurements to back up these statements. Furthermore, the authors state to “compute” an AAR- again, this is an estimate based on other estimates.

P4 l 14: What do you mean by “relative accuracy”? Is there an absolute accuracy?

P 4 section 2.8: Check numbering, Should be 2.9

this is better suited for a discussion item, not for the methodology, especially since no mass changes have been reported yet. It should be moved to the discussion section after reporting the results. Also, I do not see it fit to average the two scenarios of density. Please see Berthier et al 2007 for a similar approach, using some field based measurements.

P4 l 29 "...effects of certain dynamics on changes in glaciers"

This is vague. What do you mean by "certain dynamics?" again, what kind of changes? Area? Elevation? Authors need to be more precise throughout the manuscript.

P 5 l 7 "The velocity distribution of Kanchenjunga Glacier is consistent with that of the produced DEMs.." I am not sure what you mean here by velocity being consistent with a DEM

P 5 section 2.8: again check numbering, should be 2.10 here

This section should be merged with glacier mapping- it belongs to the mapping process.

P5 l10 "glacier degradation"? please check term, this is not standardly used. In this study the authors talk about glacier surface change, not degradation (= disintegration?)

P6 l16 this is confusing- why delineate ponds in Khumbu? Aren't there already estimates of % cover lakes on Khumbu in the literature? And why a comparison with Khumbu? It would be better to stay focused on the study area and extend the spatial coverage. The authors do not study Khumbu, so I do not see necessary to process images from Khumbu-

P5 Results: sections 3.1 and 3.2 should be combined into glacier area and elevation change. There is not enough material in 3.1

Also, p5 l 22 why is the average of the change used to derive elevation changes?

P5 l 24 "less significant area loss of the glacier"

The area change to start with does not appear to be significant (it is very small). In any case, the authors did not do any statistical tests, so the term "significant" should not be used.

P5 l 24 "Considering the uncertainty in area delineation.."

Should mention here again the uncertainty, this was estimated at 1 pixel

P5 l 26: "changes in glacier mass" – I suggest the authors report first glacier elevation changes before over interpreting this as mass changes. I suggest removing "mass"

P5 l 28 "minus and plus signs denote glacier surface lowering (down-wasting) and thickening (Fig.2a)"

Not needed here, this is evident and otherwise can be included in a caption.

P5 l 1 31 "extensive debris cover"

The authors seem to confound extensive (ie area) with thickness (related to insulating effect). No data are available here to support the statement on insulating effect. Please revise, the phrase is vague..

P 5 33-34: this can be summarized in a caption, along with the figure, the pattern here is hard to follow.

P 6 l 7: see my general comments. Can the geodetic mass balance be estimated given a) incomplete DEM coverage b) lack of bn measurements and c) uncertainties in type of material used? I suggest the authors report only the surface lowering/thickening without further extrapolations.

P6 l 8: section 3.3 on surface velocity does not seem consistent enough. Perhaps can be included in a section called "glacier area, elevation and velocity changes"

P6 l 10 "surface velocities show a general pattern"

A "general pattern" does not say much- why would it be a general pattern? Do all glaciers of this type show the same pattern? Please revise.

P 6 l 12 : the authors speculate on "remarkable" differences in velocity which may be due to slope etc..

First of all what does "remarkable" mean? need to quantify this. \

Second: the authors have derived DEMs for this glacier- with extended analysis they can explore these hypothesis (ie correlate velocity variations with slope, terrain etc..), but this has not been done in the paper.

This is another area where the paper needs improvement.

P6 section 4.4: again: consider merging all these into a single results section (to my comment above authors could add “Changes in glacier area, elevation, velocity and supra-glacial lakes”, for ex... The paper is too fragmented, and each sub sections does not have enough material by itself.

P6 l 18: there is a contradiction here. The supra glacier ponds are estimate to last a few years, so it is not a surprise that the same ponds will not be found in 4 decades’ time. Please revise.

P6 l19 it is not the number of the lakes that necessarily leads to the increase in the average size of a lake. There can be more lakes, with small surface hence no change in average size. This is a speculative statement.

P6 l 24. I do not agree with the statement here. Topographic profiles only show tendencies, they do not explain statistically the influence of topographic variables on elevation changes. The authors should mention the technique used – correlations? Regression analysis?

P6 l 31- 32 this belongs to methodology- and the calculation of these variables is not explained in methodology.

P6 l 30 – 33 this is a circular statament, please rephrase. Also l 33- 34: this should be included in the methodology, and also in the abjectives if it is an important focus of the paper.

p6 l 33 please replace “unobtainable”
also references needed here for studies which did attempt to estimate thickness of debris from RS (Mihalcea et al. 2008, Mihalcea et al. 2008, Foster et al. 2012)

p7 l 2: this is an extrapolation, and the pattern is not valid for all glaciers. Please remove/revise.

P7 l 3 -6 : this is background information, and it is quite vague (no critical thickness values given, no melt rates available yet). Remove/ move later, i.e .focus on interpreting the results (l 8-9) and only then presenting general patters of debris cover behavior.

P7 l 8: “relationships with elevation look more straightforward”
This is qualitative. Is this assesses statistically / what is meant by “straightforward”?

P7 l9 “rates of elevation change increase negatively in a similar trend from higher to lower elevations”
Awkward phrasing throughout manuscript, simply rephrase to something like “glacier thickness decreases towards the glacier terminus, at a higher rate than in the middle of the glacier” for example.

P7 l 10 “Along the main tributary (T3) below 5400 m a.s.l. or down-glacier from 18 km”
Please interpret these separately- as the controls may be different

P 7 l 12: not clear, please restate what the statistical test shows (and what test as performed)

P 7 l 18: awkward prhasing (**less gradient, more negative lowering**).
This seems odd- If I read this correctly, would expect less slope → lower flow (glacier more stagnant) and therefore glacier thickening. Lowering already implies a negative dh/dz – do you mean that less slope implies higher lowering rates? Please explain/justify here.

P 7 l 19 -22 statement is vague, and circular. Please rephrase. What is the conclusion here?

P 7 section 4.2- this should be presented before 4.1 and merged with that section. Not enough material for a stand alone section.

P7 l 23 -25 remove “where two surfaces co-exist” and quantify the statement – what does “similar” mean? Or “slightly stronger”? please quantify.
Gardelle et al 2013, should also be mentioned here.

P7 l 25 “exposed ice surface” please revise to “clean glacier tongues ”

p 7 l 27 – 28 “These findings support the proposition that the role of debris mantles in glacier mass balance and glacio-hydrological models needs to be reassessed or treated like debris-free glaciers (e.g., Käab et al., 2012).

I do not agree with this statement, ie the debris covered glaciers cannot be treated in the same way as clean glaciers in the hydrologic model- the parametrization (for ex ablation gradients etc. need to be different than clean ice, for example non-linear vs linear). Please revise/remove. This does not improve the discussion here.

P7 l 30: use of passive voice, please revise. Mention what test was used.

P7 section 4.3 there is a missing word, perhaps “Role of supra glacier lakes...”?

P7 l 30 -33 methodology not clear, was elevation change extracted pixel by pixel and then a correlation was performed? Or glacier-wide? I do not see how the lake area was treated here while these surfaces are highly dynamic, and the lake in 1970s will not be in the same place in 2010. Please revise the methodology here. I suggest assessing the rate of elevation change as a function of % lake cover- but this again will yield more results when a larger area will be taken into account.

P7 l 34 “the rate of elevation change for supraglacial ponds ($-1.25 \pm 0.34 \text{ m a}^{-1}$) is significantly more negative” again rephrase, easier to interpret “supra glacier ponds show more lowering..”

P7 l 35 ‘ [lakes and cliffs] are the principal spots of extensive heat absorption’

This is not fully argued here and it needs to be developed. May be true for lakes (but what about edges of lakes vs lake surface. And what about clean ice cliffs, which do not have a low albedo?” Statement is speculative.

P8 l 2 “The distribution of significant lowering of supraglacial ponds..”

Awkward, rephrase

P8 l 6 – 15 this whole paragraph on the role of supra glacier lakes reads quite speculative and should be marked so. Also the discussion is not very concluding, ie why would a gentler slope be expected with the lowering of the surface? Regarding Ragletti et al 2016 it is not clear whether the pond density correlated positively or negatively with a stagnant glacier tongue. Please completely revise and clarify this section.

While I do agree that supra glacial ponds play a role in surface evolution, these statements are not well supported with the current data, and they should be marked as speculative.

P8 section 4.4 This is a different study area, and it does not fit easily in the discussion, If the authors want to compare and contrast 2 areas, then the same analysis should be applied to both.

This entire paragraph can be condensed into one concise phrase.

P8 l 22-24: this is beyond the scope of the paper to understand the mass balance of Khumbu glacier. Revise/remove. Also, what is gELA? gAAR? This is too much focus here for a comparison with the main study area.

P8 l 35-37 “This high pond density should have caused slightly negative rate of elevation change around 5100 m a.s.l. (Fig. 9a) and thus likely contributed to the more negative geodetic mass balance of Khumbu Glacier.”

Statement not conclusive ..”should have..” what does this mean? Are the authors implying that the high % of supra glacier ponds explain the higher rates of lowering of Khumbu compared to Kangchenjunga?

Please revise/clarify- the reader is left unclear here.

P 8 section 4.5 Here the authors introduce yet another area, the Langtang. Please revise this reasoning- the authors could simply stay focused on the study area and then compare and contrast where appropriate with results from other areas without having a subsection for the other study areas. Again, statements in section 4.5 are not conclusive. Please revise carefully statements such as “ On the other hand, the geodetic mass balances in the Khumbu region are similar each other..”
English language is not correct in many of these cases.

P 10 l 17 : please remove overstatements such as “.. even though it is widely expected that the insulating effect of debris suppresses the ice melting underneath.”
There is no debris cover thickness available to be able to have such a hypothesis to start with.

P 10 l 18 again instead of “largely negative elevation change” one could simply say “lowering”

P 10 l 30 : It is not the purpose of the study to demonstrate the effectiveness of DEM to derive geodetic mass balance. This was already demonstrated by Berthier et al 2007, and many others. Furthermore the authors do not have validation data to support this statement. The study simply derived elevation thickness based on multiple DEMs.
The conclusion section is quite decreative and conclusions are not very clear. Please rework this section. What are the shortcomings? Future directions? What about uncertainty? And there is no discussion to why the larger area was not considered?

References

The reference list seems incomplete. Suggested references to include below, furthermore work of (Takeuchi et al. 2000, Yukari et al. 2000, Suzuki et al. 2007, Wang et al. 2011, Foster et al. 2012) for mass balance.

Figures and tables

Table 1 “resolution” should be “spatial resolution”. Also see my previous comment, it is unclear why the ASRER GDEM was used of authors derived their own DEMs, and how compatible this is with the current DEMs.

Table 2 see previous comment about averaging vs reporting the two ranges. Also, 3 decimals seems too much given the uncertainties, 1 decimal would suffice. I suggest only 2 scenarios with 2 densities without extrapolating to unmeasured area.
“Averaged mass balance” should be “averages mass balance change”

Table 3: debris covered area is not an attribute of ponds. Either rename the table or move this variable.
Dp (km-1) – how can pond density be in km-1, and not in % of glacier area? Please address.

Figure 1: Is this the entire extent of the images (most likely not)? If not, show the entire extent. It does seem from this figure that there is enough data to extend the analysis to a larger area (see general comments).
I do not see needed to put the GCPs here. Also, I note that there are no GCPs towards the accumulation area- it may affect the quality of the orthorectification.

Fig 2 it is confusing to show on glacier and off glacier with outlines. I suggest removing the outline and only showing the glacier extent as in fig 2b. “Spatial distribution” in the caption is not really needed.

Fig 2a green crosses are not visible, it is enough just mentioning in text

Fig 2b velocity vectors not visible. The map seems to contain a continuous surface made by extrapolating the vectors? Not clear. I suggest redoing only with vectors perhaps in black, or red to blue with magnitude on the pan image.

Fig 3 caption "Red and blue denote debris-covered and debris-free surfaces, respectively" remove this, it is already in the legend. Fig caption describes the symbol but does not explain the figure. On both 3 a and 3 b, it is not clear how you can have both debris free and debris covered at the same elevation- different branches? This should be explained/shown on a figure.

Fig, 4 does not bring much, I suggest removing.

Fig 5 "Pond area" – would be clearer to say pond size (because this can be interpreted as surface area covered by ponds) Fig 5 a and b I am not sure they bring much to the discussion, it would be clearer to show this on a figure (PAN +lakes 1075 and 2010 in different colors). I suggest removing these and focusing on fig 5c.

Fig 5c "distance from terminus" – specify "up the glacier". Please comment on the variability (spikes) in the caption. Some peaks correspond with lakes location, others don't- so the hypothesis of increasing number of lakes → thinning is not fully supported with the figure as it is.

Also, while the error bars are good to have, for the lakes this is pretty noisy. I suggest doing this without error bars for the lakes and just mentioning the area uncertainty in the text.

Fig 6 the profiles should be shown on a figure

Also, the figures are hard to understand- signal seems noisy –

Fig 7 There is some interesting stuff here but it is so busy that it is hard to see any patterns, let alone match colors with the previous figures. Not sure it is useful as is.

Is it really such a steep gradient in dh/dt at elevations > 5500 ? Or is this due to error? Please comment.

Fig 8 is not needed, it is enough to talk about other studies in the text.

Fig 9 dh/dt vs elevation was already presented in previous figures in a different form. I do not see it fit to bring in the Khumbu data, since the analysis was not performed in this study; again a discussion in the text would be enough.

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