

In their paper, King and co-authors measured glacier surface elevation changes in the Everest area between Feb 2000 and 2014/2015 using remotely-sensed DEMs and studied the spatial pattern of elevation change in the ablation area of glaciers. Rate of surface elevation changes are compared between three different basins and also interpreted considering the glacier type. A special focus is drawn on the influence of proglacial lakes on glacier wastage. Sensitivity of these glaciers to the future projected warming is discussed by examining their hypsometry.

This study is not ready for publication. At several places in the manuscript (MS), there are some misconceptions, especially a problematic confusion between rate of elevation changes (dh/dt , what the authors measured) and ablation rates (i.e. surface mass balance). The two variables are different and cannot be compared as the authors do (e.g., in their comparison of their data to Benn's model). Some of the conclusions are not really supported by the data themselves (e.g., statistically significant difference between the 3 main basins? Attribution of the thinning to climate drivers). In the end, the author is also left without a real take-home message. The limited implications of the present study are partly due to the fact that the authors decided not to compute glacier-wide mass balances. This is probably a reasonable choice given the lack of knowledge of SRTM penetration depth in the upper reaches of the Everest area glacier but still it makes the interpretation of the observations very difficult because rate of elevation changes for a portion of the glacier are not equivalent to surface mass balance, they also depend on ice dynamics. In the end, the reader is left with the question: "what did we learn in this study that we did not before?"

General comments

One major issue is that authors draw some conclusions between glaciers in three different basins or with different terminus type from **dh/dt measured in the ablation area only**. Such comparisons carry little significance because these generally small differences in dh/dt the ablation areas could easily be compensated by differences of opposite sign in the accumulation areas. Hence one cannot conclude unambiguously that the mass loss is larger for such basin compared to such basin or for this type of glacier terminus. Although the differences are often not statistically strongly different. A comparison of the different rate of elevation changes with altitude (Figure 7) is also partly misleading because the elevation range of the compared glaciers is really different (due to different climate setting). A solution could be for example to normalize the elevation range as was done in (Arendt et al., 2006), among others.

All along the text and in the tables, the authors provide many details about individual glaciers such that it is **difficult to extract the big picture**, the take-home message. A table summarizing mean dh/dt in the ablation area average by large basin and glacier type (area loss / mean dh/dt for the ablation area) should be added. See also the specific comment below where I suggest moving Table 4 and 5 in to the supplement and replace them with synthetic figures.

Errors on dh/dt . One problem with the metric which is used currently is that it does not take into account the size of the averaging area, i.e., the error on the rate of elevation change is the same for a 0.1 km² and a 80 km² ablation area. This is obviously not realistic.

The discussion of the **climate drivers of this glacier thinning in the ablation area is currently very weak.** For example (13.18), the authors make a weak statement about climate trend during 2000-2015, also the period of the dh/dt measurements. Even if T,P were stable (no trend) during the study period, a strong thinning rate could still be observed between 2000-2015 if, for example, a step-like warming (or change in precipitation) occurred in the years preceding the study period. In other words, the glacier disequilibrium to the climate depend a lot on what happened before the study period and not only on the climate trend during the study period.

Figure 7 and the related text. **It is not acceptable to compare dh/dt and mass balance.** They are simply not glaciologically comparable. The statement 17.7 that " The ablation gradients shown by lacustrine terminating glaciers are also very similar to regime 3 of Benn et al. (2012)" is a clear illustration of this confusion. Authors seem to believe that they measure ablation gradient when they measured gradient in dh/dt in the ablation area. They entirely neglect the role of emergence velocity which is not physically realistic.

More specific comments (some still substantial)

Title needs to include "ablation areas"

1.17. not all these glaciers are flowing southward (the basins are located southward of the main ridge)

1.18. a negative lowering rate suggest a thickening of the glacier (double negative). Either authors should change the sign or used "rate of surface elevation changes".

1.19. "small lakes". Are these supraglacial? Proglacial?

1.24. Providing the present AAR and how it will potentially change in the future due to the rise of the ELA is probably a more useful and conventional metric to illustrate this hypsometric sensitivity of the different basins.

1.28. I miss a sentence at the end of the abstract indicating the implications of this study. A sort of take-home message for the readers. To answer this question: What did we learn here that we did not before? A statement well-supported by the data that will make other researchers cite the present paper.

2.13. "ice melt from the region may contribute 8.7–17.6 mm of sea level rise". Glaciers melt seasonally even if they are in balance and even if they do not contribute to sea level rise.... Replace by "glacier imbalance". Melt is not synonym of mass loss.

2.14. Authors need to stress that these estimates are for the first decade of the 21st century only.

2.16. The study by (Kääb et al., 2015) suggest strongly negative mass balance in the southeast Tibetan plateau. Update.

2.18. Kapnick et al. 2015 was a welcome modelling effort to understand the cause of the anomaly, but this is not among the studies that documented the Karakoram anomaly. See rather (Bolch et al., 2012; Gardelle et al., 2012; Hewitt, 2005; Rankl and Braun, 2016)

2.18. Future hydrology. Is the debate relate settled? This need explanation or should be deleted. Because at least in the next decades, more negative glacier mass balance means more water in the rivers...

2.26. Description of Benn et al. 2012 conceptual model. Why is this included in the paragraph about measuring glacier mass loss. Separate paragraph needed.

3.6. Already here the reader starts to wonder why only mass loss in the ablation area is observed. This should be better explained/justified right away.

3.14. is it really the majority? I guess in term of area yes but in terms of numbers I am not so sure (there are many small glaciers...)

3.18. do the authors mean "beneath steep cliffs"? Improve terminology. Khumbu glacier also sit beneath the Everest "massif" and has a wide and flat accumulation area of several km²....

3.23 there are not so many studies measuring acceleration in the rate of surface lowering so authors could probably list them. Nuimura et al. 2012 is the other one I can think of.

4.4 Table 2 in Gardelle et al., 2013 list some ELA values from three different studies. So there is more information about ELA than what the present text suggests.

4.21 if the authors mention the two SAR systems, then they need to tell which one of the two was used to generate the version 3.0 DEM they are using. Readers are confused otherwise.

4.27. images are listed in Table 1, not Table 2. Further, these images are acquired at very different time of the year which raise the issue of how seasonal variation in height have been accounted for in the study. If not correction was applied, this needs to be well-justified and the uncertainties quantified.

6.1. Can the authors better justified the need to work on a selection of glaciers and not work on each individual glacier? Rational for that?

6.26. Although the spatial variability of the geoid height must be rather small at the scale of the DEMs processed here, it is not acceptable to compare DEMs defined above different datum. There are gridded versions of the EGM96 geoid that can easily be used to correct for the elevation difference. Conversion from geoid to ellipsoid (and vice versa) is also a built-in tool in many GIS software (including in the open source gdal libraries).

6.28. "first order trends". More details needed. Are these corrections estimated using all ice free pixels? How do the authors take into account large outliers that always occur in DEMs from satellite stereo imagery and that may contaminate their corrections?

7.7. "penetration corrections are rarely applied". Is this a good justification? Not really. Strongly biased estimates of geodetic mass balances have been published in the past due to the lack of correction of this systematic effect. See for example (Fischer et al., 2015) that demonstrated that the geodetic mass balances from (Paul and Haeberli, 2008) were strongly biased negatively and (Kääb et al., 2015) & (Barundun et al., 2015) that have shown that (Gardelle et al., 2013) Pamir mass balance estimates are likely biased toward positive values for the same reasons. This is a systematic source of errors and as such it cannot be treated by simply adding it to the error bars. The poor knowledge of the SRTM penetration depth is maybe the reason why the authors have limited their analysis to the ablation area. If this is the case, this needs to be explained/justified. But as said in my general comments, this is really limit the implications of the study.

7.15. Such an elevation dependent correction cannot be applied to one DEM alone but to the elevation difference between two DEMs.

7.20. unclear what the authors mean by "real topographic change on the stable terrain".

8.7. what matters is not the spatial autocorrelation in each DEM but the autocorrelation in the map of elevation difference. So only one auto-correlation distance should be reported.

8.9. can the authors explain why a MED remain after all the adjustments? I would have expect the mean difference to be 0 "by construction". Did the authors examined the overlapping areas of the WV DEMs as a verification of the DEM adjustment?

8.11. "independent" of what?

8.17. Can the authors confirm that in table 3, the standard error (and not "e", the elevation change uncertainty) is listed. I find it extremely strange that the last column of Table 3 (labelled "st error") is always so close to the value of the remaining mean elevation difference as listed in the "post correction" column of the same table (Table 3). The similarity is unexpected because one column is in m and the other in m/yr. I think authors need to double check this and clarify their terminology.

8.22. The Landsat images are used to refine the outlines not to extract the hypsometry, as the authors explained earlier in the MS. Be brief here and just tell that the 100-m hypsometry was extracted from the SRTM (?) DEM and the glacier outlines. Void filled DEM or not?

8.26. "glacier area change". Not relevant in the hypsometry section.

9.9. "we did not generate mass balance estimates". Do the authors mean glacier-wide mass balance estimates? The lack of knowledge of the SRTM penetration depth is another good reason to avoid this. Still I find this disappointing, It would have allowed a direct comparison to other studies and better comparison of individual glaciers/basins.

9.18. Here I am not sure I understood what the authors exactly did. Do they mean that they only summed mass loss occurring upstream of the 2014/2015 calving front? Why not taking into account at least aerial mass loss (i.e. above the lake level) for the area between the 2000 and 2015 calving front?

10.1. to draw such a conclusion "The presence of a glacial lake altered the gradient of surface lowering over glacier surfaces" authors need to compute the dh/dt gradient and compare them to support their statement. Is it the gradient with altitude? With distance to the terminus? Statement not demonstrated in the paper.

10.23. "mean" over what? A 100-m altitude band centred around 5300 m asl? Clarify.

10.6. The mean value of 2.04 m/yr is for which catchment? All merged?

11.13. What are these two scenarios? Unclear. Also what is the meaning of "scenario" in this context?

11.17. Why not providing the same % for lake-terminating glacier.

12.1. the basin-wide hypsometries should be added to Figure 7 to be compared easily to dh/dt also averaged by basin. And figure 5-6 would keep only individual glaciers (no basin-wide average).

12.13. "The altitude at which surface lowering curves approach zero is a good indicator of the ELA of glaciers". This statement is surprising. I checked the Nuth et al., 2007 reference and indeed found the following sentence : "The hypsometric (area–altitude) distribution for Brøggerhalvøya/Oscar II Land is greatest between 250 and 550 m a.s.l., with the 54 year average ELA (position where the elevation change curve approaches zero) at 350 m (Fig. 5a)." So there is no reference or data to support this statement in Nuth et al. This is a strong approximation that suggest similarities between null dh/dt and null mass balance. Rate of elevation change and mass balance are not the same quantities, I do not see how you can do such an hypothesis.

12.16-20. Complicate wording! Do they authors mean that the AAR is 37%, 36% and 40% in the different catchments?

12.23. The sensitivity of these results to the uncertainties in the ELA need to be quantified.

13.4. Regarding sensitivity to temperature (and contrast between different regions), the studies by Fujita and Sakai (Fujita, 2008; Sakai et al., 2015) are better references. (Rupper et al., 2012) is based on very thin data and only examined Bhutanese glaciers so it is not the right reference to claim that the sensitivity is high in Nepal; By the way, high compared to what/where?

13.9. In addition to the quoted studies, (Wagnon et al., 2013) have described in detail the precipitation gradient with the Khumbu basin, from Lukla to the Pyramid station.

13.16. This statement is in contradiction to the general belief that glaciers in maritime climate (more humid) are more sensitive to temperature change than glaciers in a more continental climate. See for example (Hock et al., 2009). Without a full sensitivity analysis and without some glacier-wide mass balance measurements, I do not see how the authors can conclude to such statement. Unsupported by the data.

13.28. Again (like in 13.18.) a weak reasoning. Why would the rise in the snowline altitude be a proof of accumulation decrease? How can the authors separate this way the respective role of temperature and precipitation trends? (this is even more complex in Nepal than in other mountain ranges because accumulation and ablation season are simultaneous)

13.29. "since the 1970s". Authors need to give the exact time period over which the decrease has been observed (i.e. provide the end year).

14.3. Again a poor reasoning. A rise in temperature is sufficient to explain a decline in snow cover (and the time period of 9 years is really short to draw conclusions). How can the authors draw conclusions about accumulation rates just based on this proxy?

14.10 Authors quote a lengthy time series of DEMs but provide the result for only a five time period... no need for "lengthy" or then authors should provide the results over the long time span.

14.11. "0.79 m/yr and 0.84 m/yr" can only be compared if error bars are provided. I doubt the authors can conclude here to a significant difference between these two highly similar values.

14.12. Comparison to the thinning rate of (Gardelle et al., 2013). Does this bring something to the discussion? Is it for exactly the same area and the same altitude range?

14.18. "given" missing I think. The entire sentence needs improvement in fact.

15.9-11. Understatement. I do not understand how these statements are related to the rest of the paragraph. What do the authors want to conclude here? Do they want to explain why the dh/dt is not as negative for Imja? Make the logics easier to capture by the reader.

15.18. Can the authors explain what is this "similar surface lowering pattern". It has not been presented in the result section. How can they be certain that this is due to enhanced ablation at cliffs/ponds rather than advection by ice flow of an heterogeneous surface topography?

16.4. "earlier epochs". Provide year of estimates.

16.15. Unclear wording. Why not simply mentioning the reduction in the AAR due to the ELA rise (this would be the theoretical reduction of course because this would be based on the present-day hypsometry not considering the future area loss, mainly at low elevations)

16.24. Again a very strange structure for this sentence: change are described for Dudh Koshi and TP glaciers and the sentence finishes with a conclusion for ... Tama Koshi basin. Improve logics.

17.21. The statement in the conclusion that there is decreased ice influx from accumulation zone comes from nowhere. Was never discussed earlier in the MS, never shown by the data.

17.24. Here and before. How do the authors calculate the uncertainty for their basin-wide average? Must not be simply the mean of the individual glacier uncertainties.

17.27. "We suggest that the across-range contrast in annual precipitation total may have caused greater ice loss on the north flowing glaciers ". Are they different enough statistically (compare 0.80 and 0.95 m/yr) to deserve an explanation? See also my general comment about the weak attribution to climate drivers.

18.1. Add "in their ablation area"

18.13. Again, same as above (see general comments). Authors did not measure ablation gradients!!! They maybe measure dh/dt gradient (with altitude? distance?). But no plot show these dh/dt gradient data.

Table 2. Authors could draw an horizontal bar to clearly separate the different catchment.

Table 4 (like Table 2 and Table 5) are not a really useful way to present the data. If the authors think that the list of glaciers is really important (I am not sure it is) then these tables should me moved as appendix or supplement. A much more concise way to present these numbers (in a figure rather than a table) should be preferred. For example a whisker plot showing the mean/median, range of values etc... for each catchment and each glacier type would condense the info and then, the corresponding text could be shorten also.

Figure 1: Authors needs to indicate in the caption what is the background image and the source of the inventory.

Figure 2: it would be good to show the off glacier dh/dt at least in a figure in the Supplement.

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