Response to Joseph Shea and reviewer 2 (anonymous) by King et al.

We are grateful for the thorough and constructive comments of Joseph Shea and an anonymous reviewer regarding our recent submission to The Cryosphere Discussions. The reviewers have highlighted a number of issues with the manuscript in its current form, as well as with our approach to data analysis. We agree with many of their points, and are pleased to be able to incorporate them into a revised version of the manuscript. Notably, with some relatively minor additional data processing, we have now calculated glacier mass balance estimates; a change that addresses many of the points that the reviewers raised.

Here, we outline our approach to amending the manuscript in line with the comments of the reviewers. Figures that have changed or new figures that have been added can be found at the end of this document.

Reviewer 1- Joseph Shea

GENERAL COMMENTS

1)   The title of the paper and section 3.6 suggest that glacier "mass change" is examined. However mass change estimates cannot be calculated for only part of a glacier (e.g. the ablation zone) using the geodetic approach! Fortunately, the authors have also neglected to show or discuss mass changes, so I would suggest that section 3.6 be removed and the title changed.

The title of the work has been changed slightly to 'Spatial variability in mass loss of glaciers in the Everest region, central Himalaya, between 2000 and 2015' because we do now calculate mass balance estimates for our sample of glaciers. We have kept section 3.6 as a result because details on mass loss calculations and associated uncertainties are required.

2)   As a short follow-up to point 1, the authors should provide the caveat that emergence velocities will affect the observed surface lowering rates, though this cannot be quantified (or can it?).

We have acknowledged (P9, L15 in the new manuscript) that without up-to-date glacier surface velocity data and ice thickness measurements we cannot specifically quantify emergence and its contribution to our surface lowering data. Previous work (e.g. Quincey et al., 2009) has identified active vs inactive ice boundaries for a number of the glaciers we include in our analyses, thus compressive flow and emergence is likely to occur, but we see no obvious evidence in our surface lowering data; unlike Immerzeel et al. (2015), who use DEMs of much higher spatial and temporal resolution.

3)   The authors focus on the largest glaciers within the respective catchments, and justify this decision by saying that these glaciers 'provide the greatest volume of meltwater to downstream areas'. I see two problems with this: first, this justification is unreferenced, and possibly incorrect as the largest glaciers will likely also be debris-covered and have the lowest melt rates. Second, the melt rates of smaller glaciers are also of significant interest, and excluding them may result in biased average lowering rates.

We have amended the text to say that these glaciers 'provide the greatest *potential* volume of meltwater to downstream areas' (P3, L28). Another justification for focusing on the large glaciers is that these are likely to be more negatively out of balance with climate, particularly the debris-covered tongues and lake-terminal glaciers, whereas small glaciers at high altitude would need a greater rise in air T (and ELA) before they lose mass.

Unfortunately, as the smallest glaciers in the area are typically found at higher altitudes and in complex topography, DEM coverage, particularly in the SRTM dataset, is lacking and we are not able to provide data for these areas.

4)   Equilibrium line altitudes are not discussed until the Results section (4.2.3) but this is a very important part of the overall approach used (i.e. surface lowering is only considered for areas below the ELA). A section needs to be added to the methods describing how the ELA is determined, and how are future ELAs calculated. Section 5.4.1 is rather slim on details.

Agreed. We have added a portion of text (P9, L18-25) to the methods section that specifically addresses how we estimate ELAs from surface lowering data, and how we calculate future prospective ELAs.

5)   The range of temperature projections (+0.9 to +2.3C) appears to be a global mean, though this is not defined or justified, and higher emission scenarios show higher increases (+3.7C by 2100 for RCP8.5;

Collins et al., 2013). Also, temperatures in the Tibetan Plateau and Himalayas are expected to increase at a higher rate (e.g. Rangwala et al., 2013). Potential increases in freezing level have been examined by other authors (Shea et al., 2015; Viste and Sorteberg, 2015).

The range of temperature projections are taken from Collins et al. (2013) for the tropics (including the monsoon influenced Himalaya) for CMPI5 RCP 4.5. Collins et al. (2013) estimate a minimum, mean and maximum dT of 0.9, 1.6 and 2.3 °C by 2100 for this region under this scenario. We have calculated ELA rise and associated AARs for all RCP scenarios in the latest IPCC Working Group report (AR5), but only showed RCP 4.5 to allow for comparison with other studies that have used the same scenario (e.g. Shea et al., 2015; Rowan et al., 2015). In light of the reviewer's comment we have now included an additional figure showing estimates of catchment averaged AARs for all temperature rise scenarios (RCP 2.6, 4.5, 6.0 & 8.5; see Figure 7 at end of this document).

6)   I am confused by the 'highlighted glaciers' and the presentation of the results, because the classes can be mixed. For example: Figure 5 shows all land terminating glaciers. Figure 6 shows clean Tibetan Plateau glaciers (top) and lacustrine terminating glaciers (bottom). Are some clean TP glaciers land-terminating? Are they counted in both graphs? Perhaps a more rigorous classification by basin would be useful to highlight the differences by glacier type and by region (e.g. Dudh Koshi -> land-terminating -> clean ice).

Agreed in regard to the 'Highlighted glacier' section. This is slightly repetitive and confusing. We have removed this section and added a small amount of text to the study area section to point out the glaciers we treat as 'lacustrine terminating'.

Figure 5 shows surface lowering curves for land terminating, debris covered glaciers exclusively. The top panel of Figure 6 shows surface lowering curves for only land terminating, clean ice glaciers on the Tibetan Plateau. Likewise, the lower panel of Figure 6 is reserved for only lacustrine terminating glaciers (but from all three catchments, regardless of debris cover). Glaciers do not appear in multiple groups. We have amended figure captions to make sure that this is clear. We have not divided groups further as they can't be separated based on debris cover or terminus type.

7)   Formulas in the error analysis need to be presented correctly, and suitable symbols applied, see specific comments below.

Formulae have been formatted according to your suggestions. Thanks!

SPECIFIC COMMENTS

JS comment: Abstract: somewhere in here the time period for the analysis should be defined.

Agreed. Sentence in the abstract modified to 'We quantify mass loss rates over the period 2000-2015 for 32 glaciers…'

JS comment: P1 L13: what kind of glacial lakes? Be specific.

We have updated the text to describe the types of glacial lakes we cover with our analysis:

'…and specifically examine the role of 7 proglacial, and 2 supraglacial lakes in glacier mass change.'

JS comment: P1 L18: 'Average surface lowering rates...'

This part of the abstract has been largely re-written and the original text has been removed.

JS comment: P1 L21: what is deep water calving?

We did not intend to infer that 'deep water calving' is a specific phenomenon. Rather, we were discussing that as lake depth increases, calving rates are likely to increase, as shown by Benn et al. (2007- Earth-Science Reviews). We have amended the text to make this clearer:

'...and that rates of mass loss are likely to increase as glacial lakes expand and calving can occur in deeper water.'

JS comment: P1 L23: 'area', not volume...

Agreed. Text amended.

JS comment: P1 L26: 'respectively' is missing somewhere

Agreed. Text amended.

JS comment: P2L2: The area and number of glaciers refers to the Himalayas, the Hindu Kush, and the Karakoram (not just the Himalayas).

Agreed. Text amended to:

'Estimates of ice volume range from 2,300 km^3 to 7,200 km^3 (Frey et al., 2014 and references within) distributed amongst more than 54,000 glaciers across the Hindu Kush Himalaya (HKH) and the Karakoram.'

JS comment: P2L17: 'more stable' in the eastern Himalayas is incorrect. The greatest rates of surface lowering are observed in eastern Himalayas (Kääb et al., 2015).

Agreed. Text amended to:

'Recent studies have identified spatial heterogeneity in mass loss across the Himalaya (Kääb et al., 2012; Gardelle et al., 2013; Kääb et al., 2015). Glaciers in the Eastern Nyainqêntanglha, in the eastern Himalaya, are losing mass most quickly (Kääb et al., 2015), as are glaciers in the Spiti Lahaul and Hindu Kush (Kääb et al., 2015). Glaciers in the central Himalaya appear to be more stable (Gardelle et al., 2013). The anomalous balanced, or even slightly positive, glacier mass budget in the Karakoram is well documented (Bolch et al., 2012; Gardelle et al., 2012).'

JS comment: P3L2-3: 'and thus remains to be tested' is superfluous.

Agreed. Text removed.

JS comment: P3L20: clarify are these 40 largest glaciers *partially* debris-covered?

Agreed. Text amended.

JS comment: P4L4-7. There are observed and modelled ELA data from Wagnon et al., (2013) and Shea et al., (2015), respectively.

The manuscript has been amended to include the ELA estimates of Wagnon et al. (2013) and Shea et al. (2015) (P4, L7-9).

JS comment: P4L19: what is 'non-void filled'

The non-void filled SRTM dataset contains 'no data' in gaps rather than being filled with data from other global elevation datasets, such as the ASTER GDEM. A void filled version (known as the SRTM Plus or SRTM NASA V3) is available, but is filled with ASTER GDEM data- this dataset is multi-temporal and thus cannot be used in DEM differencing. We have not amended the manuscript in response to this question.

JS comment: P5L10-15: do you do any comparison between ASTER and SETSM DEMs on stable ground.

Yes: off-glacier differences between SETSM and ASTER DEMs are low (mean -0.16, StDev of 10.42). We therefore consider the ASTER DEMs to be a robust replacement for the missing SETSM data despite their coarser resolution.

JS comment: P5L10-15. Just to clarify, you take the GLIMS glacier extents, and modify them for 2000 and 2014 extents based on Landsat imagery. And it should be mentioned here that you use the 2000 and 2014 extents to calculate area changes.

We have amended the text here to clarify our approach for documenting glacier area change, using the suggestion above:

'Glacier outlines were downloaded from the Global Land Ice Measurement form Space (GLIMS) Randolph Glacier Inventory (RGI) Version 5.0 (Liu and Guo, 2014; Bajracharya et al., 2014; Racoviteanu and Bajracharya, 2008) and modified for 2000 and 2014 glacier extents based on Landsat scenes closely coinciding in acquisition with the DEM data. Glacier extents from these two epochs were used to calculate area changes. The 2000 Landsat…'

JS comment: P7L14. The graph shown in the Supplementary Information could be places in the main text, but the caption needs to be improved as it is not clear what is being presented.

We do not include this graph in the main text as it would mean a number of additional graphs would need to be produced and included in the manuscript to illustrate the effect of the DEM correction process on the stable ground difference statistics. We consider this to be unnecessary as the products of the correction process are much better demonstrated in other work (Nuth and Kääb, 2011). Table 3 gives a summary of the effects of the correction process which, in our opinion, is adequate evidence of its success. The caption for this supplementary figure has been rewritten to better explain what the graph shows.

JS comment: P8L1-10: Fix terms in the text: subscripts and italics are missing or inconsistent.

Done.

JS comment: Eq.2: italize n_{diff}, n_{tot}, PS

Done.

JS comment: P8L7-8: this sentence is unclear. What value of d was used in this study?

We now take an alternative approach to quantifying the uncertainty associated with DEM difference data, so the comment above no longer applies.

JS comment: Eq.3 root symbol needs to be over the whole expression: p SE2 +MED2, and I would suggest using dZstable for mean elevation differences (MED). MED looks a lot like median...

This comment is no longer applicable, but the reviewer's comment on the presentation of formulae has been considered in the amended manuscript.

JS comment: Section 3.6: suggest removing completely.

See response to general comment 1 regarding section 3.6.

JS comment: P9L25: report lowering rates with negative sign (e.g. -0.80 +/- 0.35) to be consistent with Table 4.

We now report mass balance estimates and they all have the appropriate sign in text.

JS comment: P10L1-3: 'Mass loss' should be 'surface lowering' here, and don't rates increase downglacier in all cases (not only lake-terminating glaciers)?

We are now able to show ablation gradients for lacustrine terminating glaciers which clearly show a linear trend with elevation. Debris covered glaciers have distinctly non-linear ablation gradients.

JS comment: P10L24: Refer to Figure 5 here.

Text amended to refer to figure 5:

'Mass loss rates over glaciers on the Tibetan Plateau were higher than those in the Tama and Dudh Koshi catchments (Figure 5) up to 5800 m a.s.l.'

JS comment: P11L13: 'patterns', not 'scenarios'

Agreed. Text amended.

JS comment: P11L13-25: provide rates of area change per year for comparison with other studies?

We have calculated annual area change rates and made a brief comparison with values given in previous work (P11, L17). Our annual change rate, albeit not for exactly the same group of glaciers in the study area, is very similar to that of Bolch et al. (2008) (0.12% a^{-1}). Our annual area loss values are lower than those of Thakuri et al. (2014) (and references within); this can probably be explained by the type of glaciers each set of work includes in their analysis. Thakuri et al. (2014) document area change over a number of glaciers that are free of debris cover, and therefore readily shrink in response to climatic change, whereas our sample of glaciers is made up of the largest, most debris mantled glaciers in the region (that do not lose glacier area as rapidly).

We refer to other studies of a greater temporal and spatial resolution in the manuscript, to provide more information on glacier area change in the region.

JS comment: P11L19: This is where the more rigorous classification would be useful. E.g.: Land-terminating clean vs. land-terminating debris-covered?

Fortunately, all of the land-terminating glaciers in our three main groups (Tama Koshi, Dudh Koshi and Tibetan Plateau) are covered by a substantial amount of debris, so cannot be sub-divided based on debris-cover. The glaciers we highlight as 'TP clean' are all land-terminating and debris free, so provide a direct comparison to the glaciers in the three main groups.

JS comment: P12L13-20: Suggest moving this section to methods and adding more details on how current and future ELAs are determined.

Agreed. We have added a short section to the methods to describe how we estimate ELAs from our DEM differencing and how we estimate future ELAs using lapse rates for different catchments (P9, L17). We have kept section 4.2.3 as a short summary of ELA estimates based on surface lowering curves.

JS comment: P12L16-17: This phrasing is a bit awkward. It seems like you are trying to say that the approximated ELAs give an AAR of 0.37 in the Dudh Kosi catchment. (AAR = Accumulation Area/Total Area).

We have modified the text to describe AARs in a more conventional way, as suggested.

JS comment: P13L3: Though around 80

Text amended to give estimate of the percentage of total annual rainfall delivered by the monsoon in the study area.

JS comment: P13L18-25: Some skepticism might be warranted when referencing the snow line altitude shifts given by Thakuri et al., (2014): these are based on single-image delineations of transient snowlines, and in the Himalayas these do not remain constant at the end of the summer season.

Agreed. We have amended the text as follows:

'Thakuri et al. (2014) showed a rapid ascent of the snow-line altitude in the Dudh Koshi between 1962 and 2011 (albeit through documenting transient snowlines from single scenes acquired at each epoch), and Kaspari et al. (2008)…'

JS comment: P14 Section 5.2 title. 'surface lowering' not 'mass loss'

As we are now able to provide mass balance estimates we have kept the title of this section the same.

JS comment: P14L26-27: For lake terminating glaciers its complicated, but for land-terminating glaciers thinning should reduce the driving stresses and lead to decreased glacier velocities (e.g. Berthier and Vincent, 2012; Haritashya et al., 2015)

As this section of the manuscript focuses on the potential future evolution of lacustrine terminating glaciers we have chosen not to modify it to discuss the dynamic of land-terminating glaciers too.

JS comment: P15L25: the sensitivity of Dudh Kosi glaciers to future ELA changes based on its hypsometry was noted previously by Shea et al. (2015).

We have altered the text to include acknowledgement of the hypsometric analysis conducted by Shea et al. (2015):

'The coincidence of maximum surface lowering rates with the altitude of maximum hypsometry in the Dudh Koshi catchment (Figure 5) means a large amount of ice is readily available to sustain mass loss rates here. Sustained and prolonged mass loss may lead to a bi-modal hypsometry here, with the separation of debris covered glacier tongues and their high-elevation accumulation zones a possibility (Rowan et al., 2015; Shea et al., 2015).

JS comment: Table 3: separate columns for means and standard deviations

Table modified according to the above suggestion.

JS comment: Figure 1: Add the imagery extents here.

The large footprint of the Landsat scenes used in this figure means that only one image is needed to cover all of the glaciers in our sample. The Figure has therefore not been modified.

JS comment: Figure 2: text labels with glacier names are impossible to read. Also, is it possible to show the data voids in DEM differencing?

The size of the text labels for the glaciers we highlight has been increased and the labels moved where possible to make them more obvious. Data voids have been set to transparent to maintain the clarity of the figure and to avoid distraction from the surface lowering data. We haven't changed the figure in this regard.

JS comment: Figures 3 and 4: Why are glacier extents shown in 2014 (left panel) not also present in 2000 extents?

2000 and 2014/15 extents are both shown on the right hand panel. Only the 2000 extents are shown on the left panel to show the DEM differencing data as clearly as possible.

JS comment: Figure 5 and 6: Larger fonts required! Caption should point out that surface lowering curves are on the right and hypsometry on the left. Maybe show hypsometry as relative (% of total area) as opposed to absolute? and show surface lowering rates as boxplots by elevation band?

Font size has been increased and the caption amended as suggested. We have kept the mass balance curves the same to allow for easy comparison with the conceptual mass balance curves of Benn et al. (2012).

JS comment: Figure 6: Why is approximate ELA only shown on top panel? What about projected future ELAs?

We have calculated and added future prospective AARs for the clean glacier sample and included them in Figure 7 (see below). As we have normalised the elevation range of glaciers in Figures 5 and 6 the ELAs cannot now be plotted here. We have marked the approximate ELA (where the mean mass balance curve of all the glaciers in the sample first approaches 0 on the x-axis) for all glacier types in our sample to figure 8. We have not calculated projected AARs for lacustrine terminating glaciers as this group contains glaciers from either side of the orographic divide (thus different lapse rates must be considered) and glaciers of contrasting hypsometry.

Review of King et al., The Cryosphere, July 2016

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.

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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?"

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General comments

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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 signed 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 has 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.

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Several of the reviewer's comments relate to the fact that we present surface lowering data rather than estimates of glacier mass balance. The reason that we did not initially calculate mass balance is partly because of the lack of a thorough understanding of C-band radar penetration depth into snow, firn and clean ice in the Himalaya and its influence on the quality of the SRTM dataset over glacier accumulation areas. However, the recent work by Kääb et al. (2012, 2015) has considerably refined our knowledge of this problem, and they demonstrate how their corrections reconcile previously divergent estimates of glacier mass balance in the Eastern Himalaya (Gardelle et al., 2013 Vs Kääb et al., 2012). With a small amount of additional processing we have thus corrected our data to account for C-band radar penetration following the method and values presented in Kääb et al. (2015). As our baseline dataset (the SRTM DEM) is the same as Gardelle et al. (2013), this yields the same spatial coverage and we are thus able to directly compare our results.

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To allow for direct inter and intra catchment comparison of surface lowering curves (i.e. what has become mass balance data in the revised manuscript), we have followed the approach of Arendt et al. (2006) and normalize these data by the each glaciers elevation range. We thank the reviewer for this suggestion.

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We have simplified our discussion and presentation of results to avoid unnecessary mention of individual glaciers, and focused solely on the behavior of glaciers depending on terminus type and the variability of mass loss between catchments. We have amended text in the conclusion to give the key findings greater emphasis. Additionally, as suggested by the reviewer, Tables 4 and 5 are now presented as supplementary information should a reader require information on individual glaciers.

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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.

Thanks for pointing this out. Choosing the most appropriate error metric was something that we deliberated over for some time. In light of the reviewer's comment we have reassessed the uncertainty estimates associated with our elevation change data using a root of sum of squares approach, similar to that of Wang and Kääb (2015) & Melkonian et al. (2013, 2014). We acknowledge that the total error budget should contain assessments of not only the standard error associated with pixel scale differences, but also uncertainty estimates of elevation differences/ volume change averaged over a larger area. We have taken an area-weighted approach to calculate catchment wide error budgets in the updated version of the manuscript.

We also acknowledge a later comment by the reviewer that the contrasting acquisition dates of the WorldView imagery used in SETSM DEM extraction may introduce a seasonal variation in glacier surface heights. See our response to comment 4.27 in regard to this problem.

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.

We agree that the response time of these glaciers to climatic change is likely to be greater than the length of our study period. Here we were simply trying to suggest that the contemporary climate records taken at the Pyramid research station and at Dingri on the Northern side of Everest are evidence that glacier mass loss will continue into the future. We have clarified this in the revised manuscript.

We have also ensured that studies such as Yang et al. (2011), who present temperature data for the period 1959-2007 at Dingri, are clearly acknowledged in the revised manuscript, to give the importance of long-term records more prominence.

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.

As detailed above we have now estimated mass balance from our data. These are now directly comparable with the conceptual mass balance curves of Benn et al. (2012).

We have acknowledged (P9, L15) that, without up-to-date glacier surface velocity data and ice thickness measurements, we cannot specifically quantify emergence and its contribution to our surface lowering data. Previous work (Quincey et al., 2009) has identified active vs inactive ice boundaries for a number of the glaciers we include in our analyses so emergence is likely to occur, but we see no obvious evidence in our surface lowering data; unlike Immerzeel et al. (2015), who use DEMs of much higher spatial and temporal resolution. A clear explanation of this caveat will be included in the amended manuscript (also suggested by reviewer Shea).

More specific comments (some still substantial)

Title needs to include "ablation areas"

Now that we have generated mass balance estimates we have kept the title the same.

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

Text amended.

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".

We now give glacier mass balance estimates throughout so the comment no longer applies.

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

The text has been amended to give more detail on lake type.

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.

Text amended here and throughout the manuscript to report AAR in a more conventional manner.

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.

We have added a couple of sentences at the end of the abstract to emphasise the importance of our results:

'Our results are significant because they suggest that documented glacial lake growth and/or expansion across the Himalaya is likely to be accompanied by increased ice mass loss in the near future. Further, the influence of temperature increases may be highly variable across different catchments, complicating the prediction of the future contribution of glacial meltwater to river flow.'

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.

Text amended according to reviewer suggestion.

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

Text amended.

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

Text amended to give more detail on the results of Kääb et al. (2015).

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).

Text amended and we now cite several of the studies suggested by the reviewer.

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...

We agree that the long-term impact of negative glacier mass balance in the region is uncertain, but it is an implication that should be acknowledged. We have slightly reformatted this part of the introduction so that this matter is not mentioned in the middle of the discussion of mass loss heterogeneity.

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.

Now in a paragraph on its own.

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.

We now quantify glacier mass balance, thus this sentence has been removed and the comment no longer applies.

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...)

Text amended slightly later in the paragraph to emphasise that most glacier area is debris covered.

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.....

We are happy with our current description of glacier types in the study area.

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.

Nuimura et al. (2012) now cited in text.

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.

Text updated to include ELA estimates of Gardelle et al. (2013), among others suggested by Joseph Shea (other reviewer).

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.

Text amended to give more information on the C-band SAR system used by the SRTM.

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.

Text amended to refer to the correct table. Two overlapping SETSM DEMs (ending FA100 and 3C00 in Table 1) have been generated from Worldview imagery acquired before and after the summer monsoon (when glaciers receive most accumulation) of 2014, thus any spatially consistent off-glacier differences may show a remnant snow pack that would cause an elevation bias. The difference between these two SETSM DEMs is slight (mean -0.17 m, σ 2.84 m), but we cannot be sure that these differences represent a region-wide average. We incorporate an assessment of the standard error (σ_{season}) of these seasonal differences into our overall uncertainty budget.

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?

We have selected the glaciers containing the largest volumes of ice and therefore the greatest potential contribution of meltwater. Our ability to include many other smaller glaciers is hindered by the lack of suitable data coverage over steeper, higher topography where many smaller glaciers in the study region are located. We have clarified our rationale in the updated manuscript.

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).

We have now incorporated a geoid correction into our data processing and incorporated the details in the methods section of the paper.

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?

We mention later in the manuscript that outliers of $>\pm 60$ m over stable, off-glacier terrain were filtered from the difference data. We have now included this statement in this earlier section. Only ice-free pixels were used to inform on the shifts applied to DEMs. 'First order trends' refer to linear trends fitted through difference data showing clear along or cross track biases. We have amended the text to make this clearer.

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.

We have now corrected for SRTM radar penetration following the approach of Kääb et al. (2015). We were reluctant to attempt to correct the SRTM for C-band radar penetration using the estimates of penetration depth given in studies such as Gardelle et al. (2013) as no thorough comparison of the contrast in X Vs C band radar penetration had been carried out. The success of Kääb et al. (2015) in reconciling previously divergent mass balance estimates using a different C-band penetration correction approach means we can now be confident in the correction we have applied.

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

Agreed, text amended.

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

The section of text describing the calculation of uncertainties associated with mass loss data has now been re-written in the updated manuscript (P7, L16 onwards), therefore this comment no longer applies.

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.

As the DEM difference grid has a pixel size of 30 m, the autocorrelation distance would be 600 m following Bolch et al. (2011). This has been specified in the updated manuscript.

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?

The success of the co-registration process is limited by pixel size of the DEMs involved. For example, Nuth and Kääb (2011) suggest that the co-registration solution has an internal horizontal accuracy of 1/3 of a 30 m ASTER DEM (although often 1/10 of a pixel) so there will be a residual difference that could only be eliminated if the DEMs being corrected were of a finer resolution. Our residual mean differences are all below 1/10 of the pixel size in our DEMs, thus we are confident that our co-registration is optimal.

See our response to comment 4.27 about the comparison of overlapping SETSM DEMs.

8.11. "independent" of what?

Comment no longer applies as we now take a different approach to uncertainty estimation.

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.

As above, we now use an alternative method to calculate uncertainty associated with our difference data. But to clarify the point raised by the reviewer, SE is always similar to the MED when there are a large number of elevation difference measurements and our values were correct.

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?

Text amended according to the above suggestion.

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

Agreed. Text deleted.

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.

We now give mass balance estimates following the correction of SRTM data for radar penetration. In doing so, we achieve the same data coverage as Gardelle et al. (2013) and can directly compare our results to this study, and others such as Bolch et al. (2011) and Nuimura et al. (2012).

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?

We have modified our approach to incorporate the areal mass loss from between the 2000 and 2014 calving fronts and explain that below water level ice loss cannot been included in the revised manuscript.

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.

We have now calculated ablation gradients (from the ELA to the termini of lake terminating and clean ice glaciers, and from the ELA to the altitude of maximum mass loss for debris covered glaciers) and compare them in the revised manuscript (P10, L29). The ablation gradient of lacustrine terminating and clean ice glaciers is linear from ELA to terminus, whereas the ablation gradient is clearly non-linear for debris covered glaciers, something which has also been identified in previous studies.

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

Text removed so comment now no longer applies.

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

Comment no longer applies as this part of the text has been re-written.

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

Text amended and we now describe 'patterns' of ice loss that occurred. Specifically, there we refer to the loss of glacier area around the termini of lake terminating glaciers and clean ice glaciers, and the loss of glacier area as glacier surfaces lowered and narrowed, mostly in the middle portions of debris covered glaciers.

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

This section has been mostly rewritten to give ice area loss totals and as percentages of total glacier area for all groups of glaciers in our study.

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).

We have not changed the format of Figures 5, 6 and 8 as the mass balance curves and glacier hysometry curves are easily compared in their current format, especially now that glacier elevation ranges have been normalised.

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.

Now that we are able to show mass balance curves we can use the approach of Nuth et al. (2007) with more confidence, as the point at which the mass balance curves approaches or crosses zero is the point of null mass balance over the study period. We prefer this method of ELA calculation to, for example, the mapping of the maximum snowline altitude at the end of the ablation season as the snowline is transient, and thus a long time series of snow and cloud free imagery would be needed to delineate an accurate, average snowline altitude. The mass balance data also incorporate the mass contribution of avalanches to the glaciers, which is an important influence on glacier mass balance in the study area (Benn and Lehmkuhl, 2000).

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

We have amended the text to summarise AARs in a more conventional manner.

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

Again, we now generate glacier wide mass balance estimates rather than limiting our data to below the ELA, so this comment no longer applies.

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?

We thank the reviewer for pointing these studies out. We have amended the text to cite these two. Sakai et al. (2015) give a thorough description of the sensitivity of summer accumulation type glaciers to temperature and also give a comparison with the sensitivity of winter accumulation type glaciers to temperature. They conclude that summer accumulation type glaciers are more sensitive to temperature variations than winter accumulation types.

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.

We have now cited Wagnon et al. (2013) at this point in the manuscript.

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.

We agree with the reviewer that glaciers in wetter climates are more sensitive to temperature change, and we were not trying to state the contrary at the point in the manuscript to which the reviewer refers. To avoid such confusion,

we have altered the structure of this section (P13, L14) slightly to separate the description of published temperature and precipitation data and our inferences about their effect on glacier mass loss from the study area.

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)

We have amended the text in the updated manuscript (P13, L28) to avoid the direct inference of decreasing accumulation on the southern flank of the Himalaya caused by a rising snowline altitude. The data of Kaspari et al. (2008) allow the more confident suggestion that accumulation has been decreasing on the northern flank of the mountain range.

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).

We have added this detail to the text.

14.3. Again a poor reasoning. A rise in temperature is sufficient to explain a decline is 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?

This section compiles evidence that temperature is rising and solid precipitation is decreasing in the region; trends that are likely to adversely impact accumulation rates if they continue. We feel this link between climate and accumulation is reasonable to make, and have therefore not altered the text.

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 spam.

Text amended- deleted the word 'lengthy'.

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.

Now that we give estimates of mass balance the previous comparison has been removed from the manuscript.

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?

We are now able to make a comparison of our mass balance estimates to those of a number of other studies (Bolch et al., 2011; Nuimura et al., 2012; Gardelle et al., 2013; Kääb et al., 2012) who generated mass balance data for different time periods over a similar selection of glaciers and over a time period stretching back to 1970. This new section starts at line 9 on page 14 of the updated manuscript. Due to differences in identifying the sources of data used in each study we are not able to compare the same area and altitudinal range of the same glaciers. Apart from the data published in Gardelle et al. (2013), which the reviewer suggests is biased towards the positive because of their SRTM correction, there appears to have been a steady increase in mass loss rates in the study area since the 1970s. We include this discussion in the updated version of the manuscript.

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

We have changed the wording of this sentence slightly to improve its clarity.

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.

Now that we have calculated mass balance estimates for Imja and other nearby, land-terminating glaciers, we see that Imja has lost much more ice over the study period. As a result, we do not have sufficient evidence to suggest that ice loss from this glacier is being slowed by the presence of an ice foot in the lake. This section of text has been removed.

15.18. Can the authors explains 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?

This section has been re-written (see P15, L10) to more clearly explain the surface lowering pattern common in areas of stagnant ice (*cf.* Quincey et al. 2009) with well-developed supraglacial pond networks (e.g. Watson et al. 2016). A similar pattern of surface lowering is evident over the long, debris covered tongues of the larger glaciers in the Tama Koshi catchment and on the Tibetan Plateau, and it is not unreasonable to suggest that large parts of these glaciers are now stagnant (indeed we have unpublished data that confirm this).

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

We provide the years associated with ELA estimates in section 2, so now refer back to this section at this point in the text.

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.)

Again, we have amended the text to refer to ELA rise induced AAR change in a more conventional way.

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.

We have altered the structure of this part of the manuscript slightly.

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.

With hindsight we agree that this statement cannot be shown to be true from our results alone and we thank the reviewer for picking this up. We have removed it from the manuscript.

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.

We have now calculated an area weighted average uncertainty for each catchment and these are quoted in the manuscript where average mass balances for each catchment area described.

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.

Given that the mean mass balance estimates that we have now produced for each catchment are not markedly different across the orographic divide, we have toned down what was previously written about potential drivers of the differences in mass loss between Tibetan and Nepalese glaciers.

18.1. Add "in their ablation area"

Comment no longer applies now that we have calculated mass balance estimates.

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.

We now do. See comment 10.1.

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

Table amended according to suggestion.

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.

We have moved the three large tables containing information on each individual glacier to the supplementary information. The results section of the manuscript has also been largely rewritten to give a more concise summary of the key statistics associated with glacier groups of different location and terminus type. We have not added any additional figures to summarise these statistics.

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

Figure caption amended according to suggestion.

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

An additional figure has been added to the supplementary information to show off-glacier difference data. See Figure S2 below.

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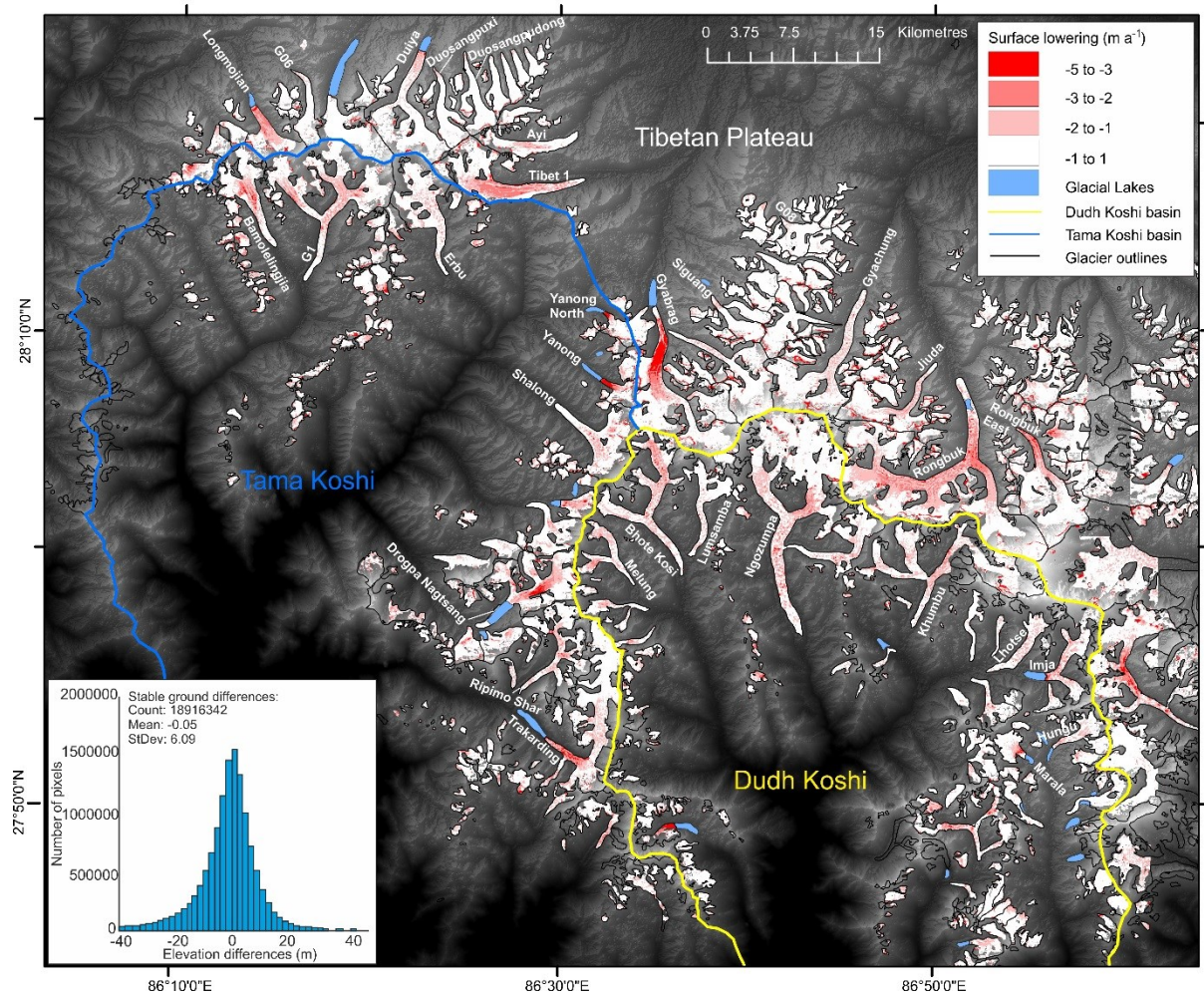
Figure 2. Glacier surface lowering over the study area between 2000 and 2014/15. Also shown is a summary of off-glacier terrain differences.
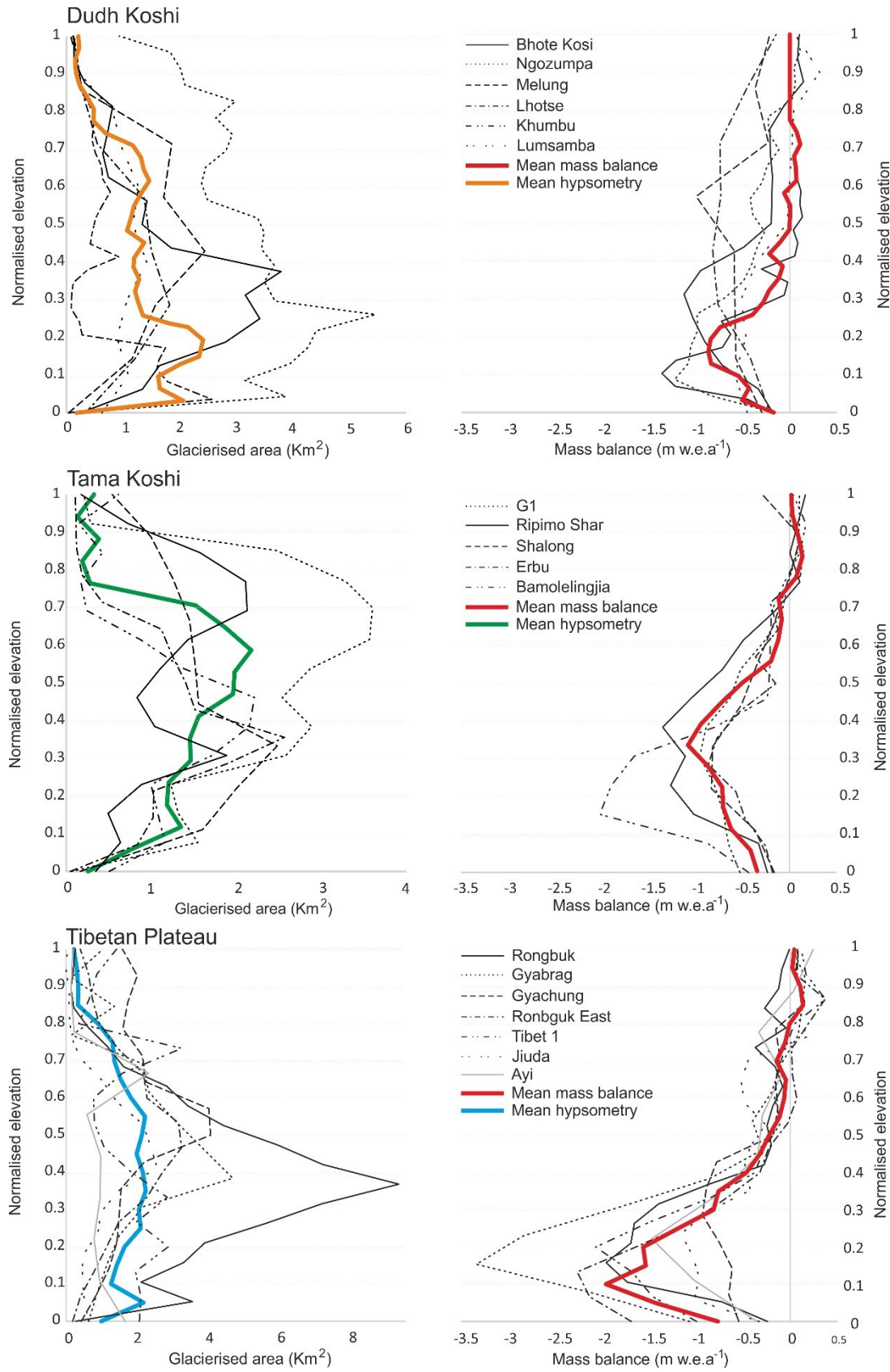
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Figure 5. Mass balance and glacier hypsometry curves for all land terminating glaciers in the three different catchments of the study area.
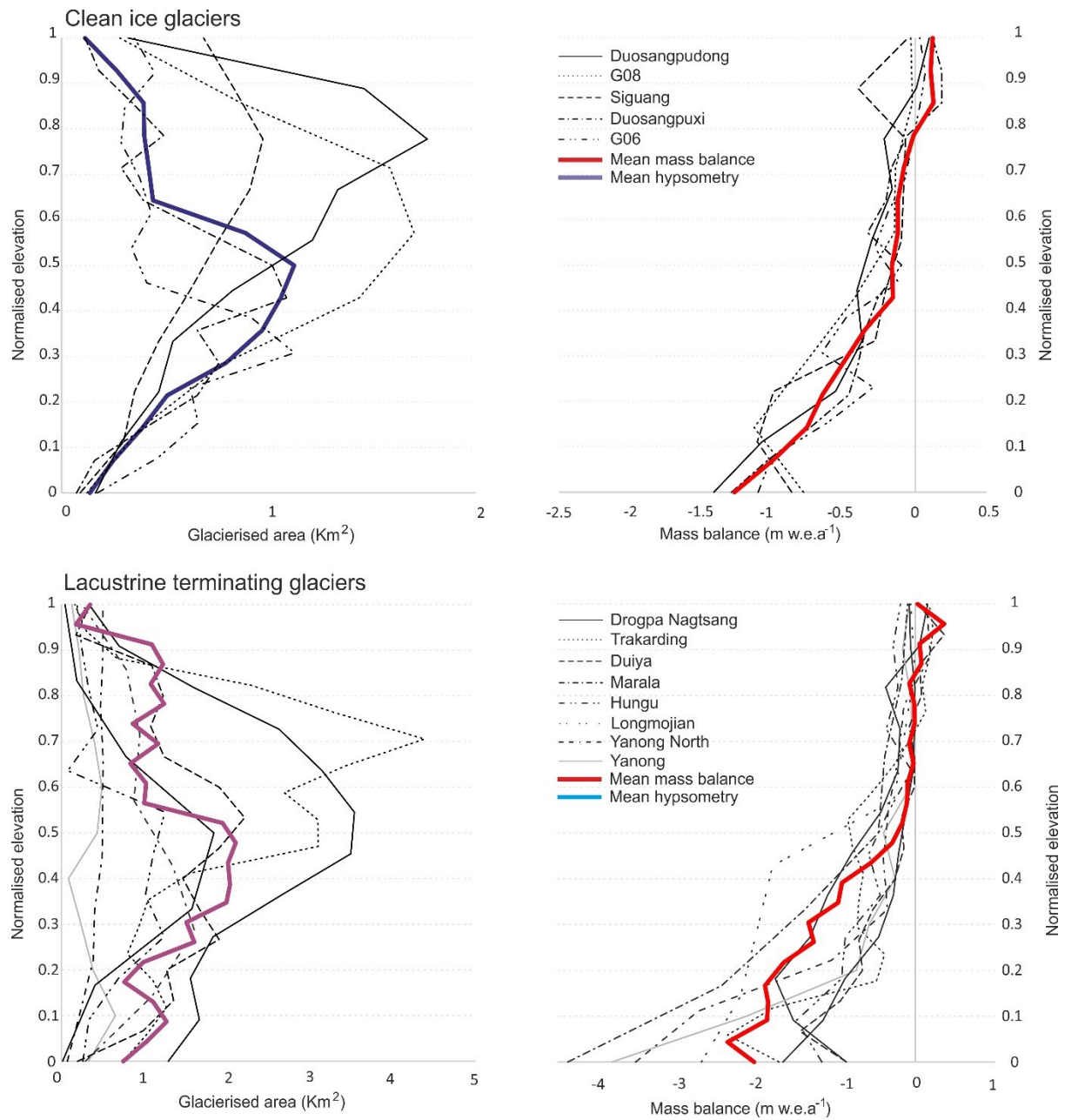
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Figure 6. Mass balance and glacier hypsometry curves for clean ice and lacustrine terminating glaciers in the study area.
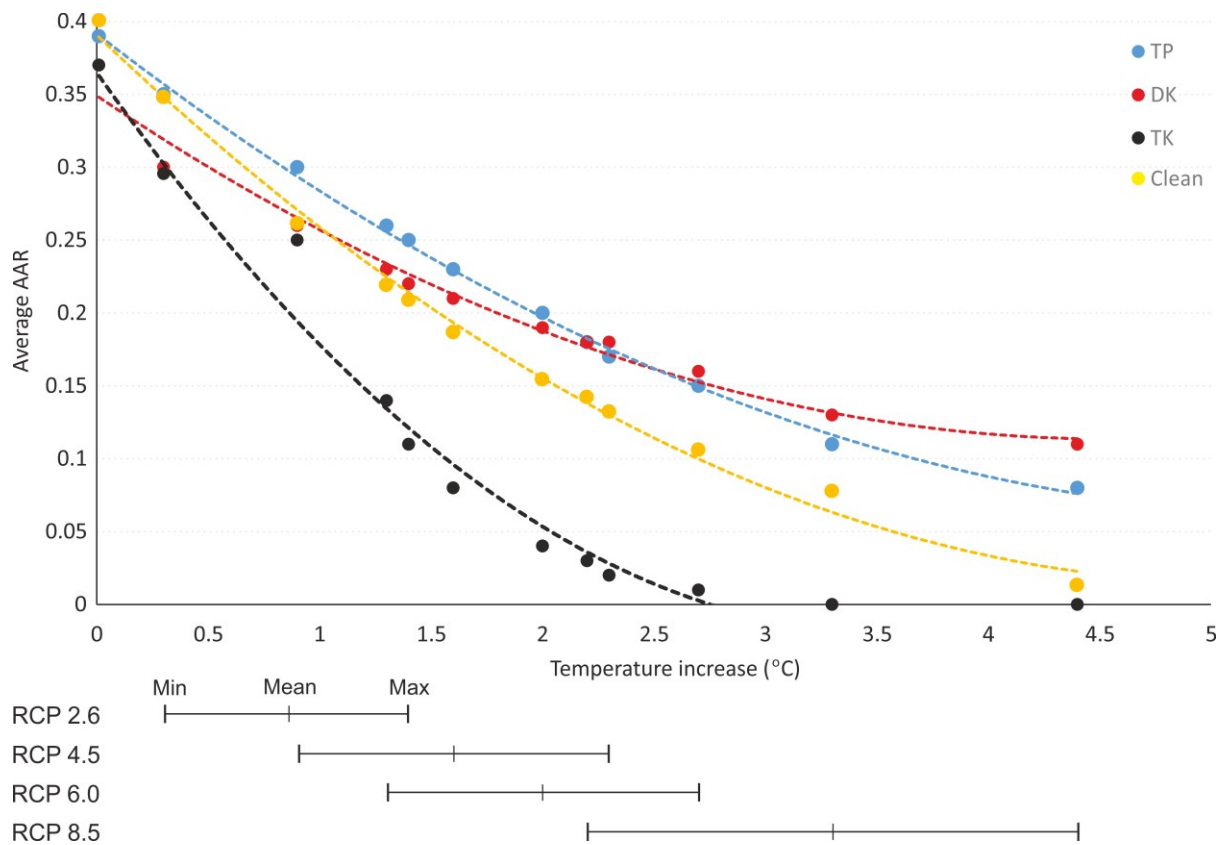
Figure 7. Projected AARs (averaged across each catchment) based on different scenarios of temperature rise and accompanying ELA rise. Temperature rise scenarios have been used from the IPCC AR5 Working Group report. TP- Tibetan Plateau; DK- Dudh Koshi; TK- Tama Koshi.
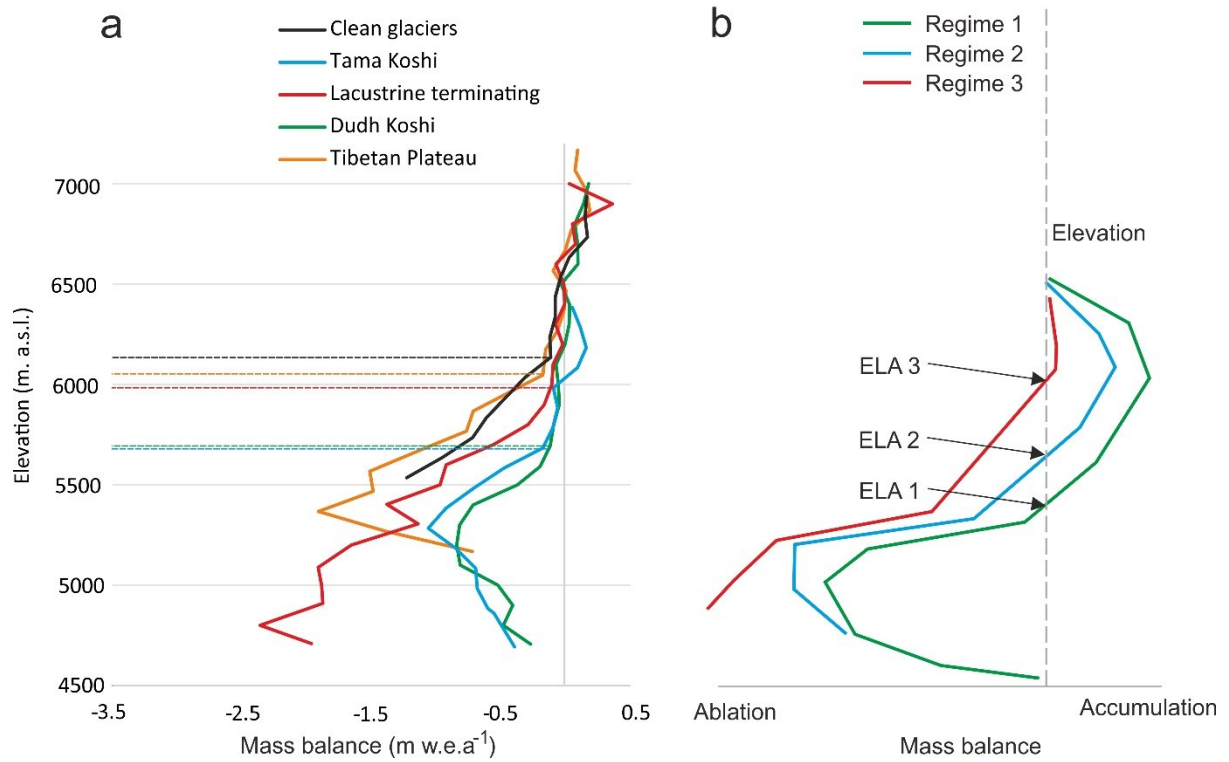
Figure 8. A- Mass balance curves for land terminating glaciers averaged across each catchment and for the populations of clean ice and lacustrine terminating glaciers we highlight. ELAs estimated from the altitude at which mass balance curves approach zero are marked by dashed lines in matching colours. B- Mass balance curves proposed by Benn et al. (2012) to represent three distinct regimes of ice melt on debris-covered glaciers.
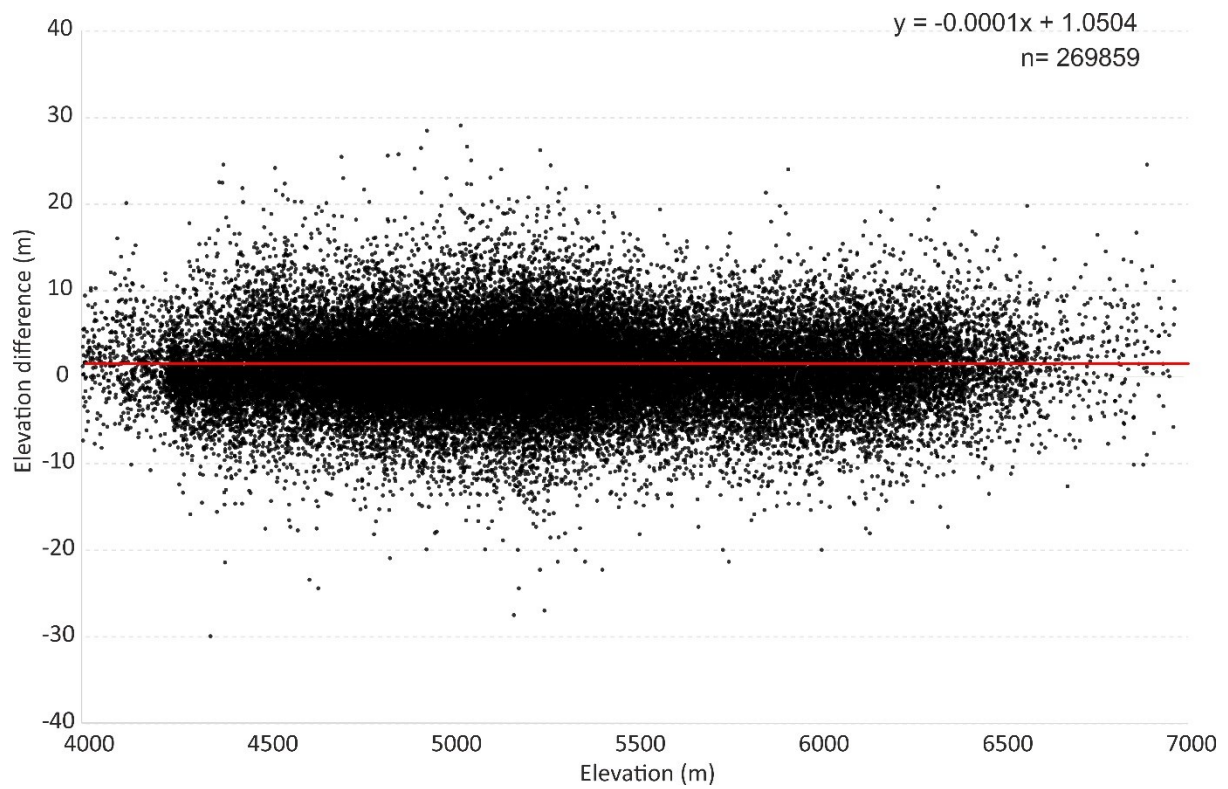
Supplementary figure 1. Elevation differences for stable ground (off-glacier) between SETSM and SRTM DEMs, plotted against elevation. There is no clear relationship between DEM differences and increasing/ decreasing elevation (often labelled an elevation dependent bias).
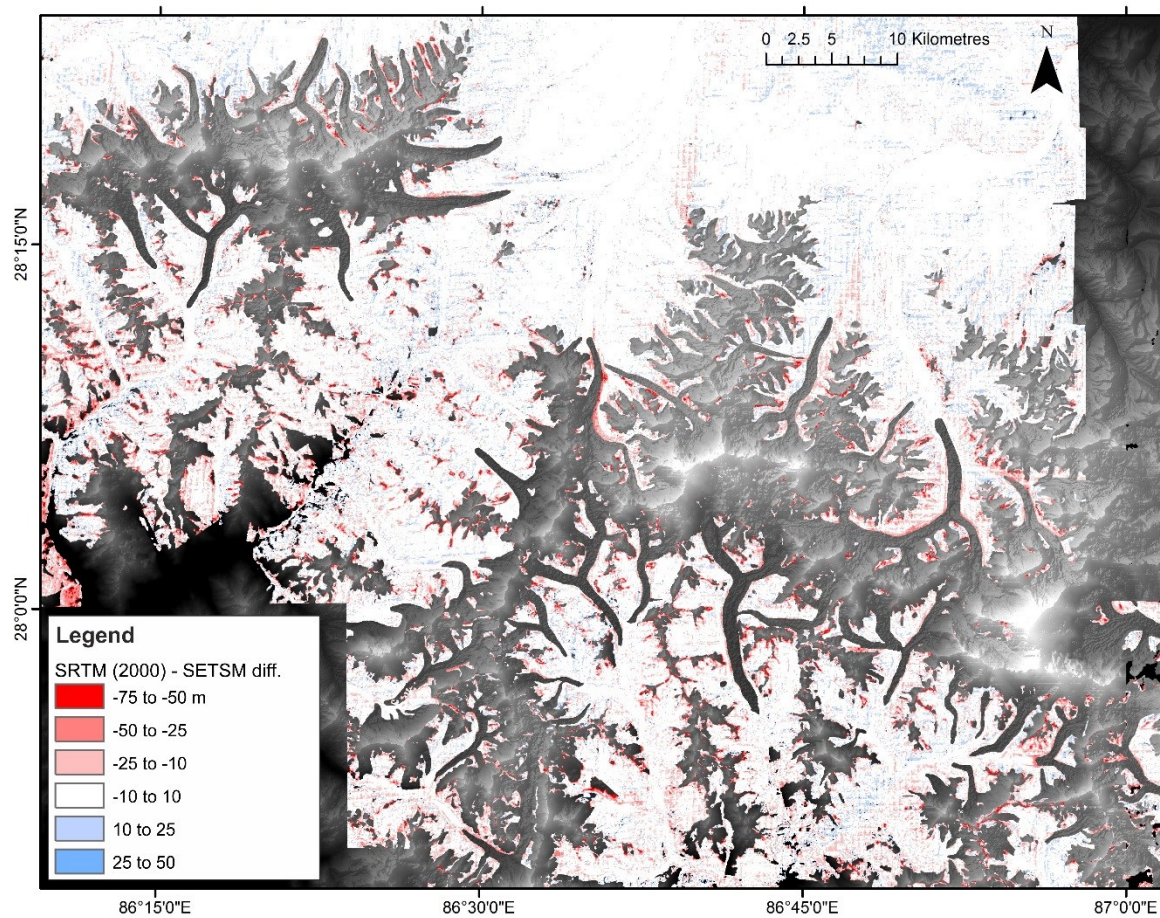
Supplementary figure 2. Elevation differences between the SRTM and SETSM DEMs over stable ground away from glacier surfaces.