Reply to Referee #2

We would like to thank the referee for thoughtful and useful comments. In the following, we describe our responses (in blue) point-by-point to each referee comment (*italic*).

*This manuscript presents measurements of areal and surface elevation change, satellite-derived surface velocity data and modelled mass balance and ice dynamics data for three glaciers in the Bhutan Himalaya. One of these glaciers is land-terminating, another is transitioning between land-terminating and lake-terminating, and the third is lake-terminating. The ultimate goal is to be able to test whether proglacial lake development leads to increased glacier thinning rates. The conclusion is that it does, and that the glacier transitioning from land- to lake-terminating will accelerate and thin further as the proglacial lake develops. The manuscript is well-written, appropriately and clearly structured and the figures are good quality, but further work is required before it can be published in The Cryosphere.*

<u>Major comments:</u>

*1. My main concern relates to the lack of any real sensitivity testing to the many components that are assumed or estimated in the modelling – particularly relating to the surface mass balance. The stated uncertainty in the thermal resistance calculations are > 60 % alone. . . As a minimum it would be helpful to see the output from the debris thickness modelling to see if it is realistic. There are further assumptions relating to the linear temperature profile and albedo, for example, that need to be accounted for since the estimated mass balances are very negative compared with previous studies. How much impact are these terms having on the results? The ice flow modelling is simple, which is not a problem in itself, but certainly it would help to see some of the input datasets such as the ice thickness map to convince the reader it is somewhat realistic. And what impact does the chosen sliding coefficients have on the modelled results (beyond figure S3)?*

Although debris thickness was not measured during the field campaign, ice is exposed from place to place over Thorthormi and Lugge Glaciers (Figs R1a and R1b), suggesting that debris-cover is rather thin than that of Lugge II Glacier (Fig. R1c). In addition, few supraglacial ponds and ice cliff exist over Thorthormi and Lugge Glaciers. So we emphasize that spatial variability of elevation change, thermal resistance and SMB are less than those the reviewers supposed. Anyhow, following the referees suggestion, we recalculated thermal resistance with considering sensible heat, for which pressure level temperature and geopotential height of NCEP2 are taken into account (Fig. R2). Scatter plot and spatial distribution of thermal resistances derived from the original method (net radiation only) and from recalculated one (net radiation + sensible heat) are shown in Figs R3 and R4. Spatial distribution of the difference between the two results is also shown in Fig. R4c. Thermal resistance significantly increased after the consideration of sensible heat (Fig. R3). However, large difference appeared only near the western margin (Fig. R4) probably because of relatively thick debris covering the area. We will recalculate the SMB distribution with the revised thermal resistance in the revised manuscript. We evaluated sensitivity of calculated meltwater against meteorological parameters (Fig. R5). We chose the meltwater instead of SMB to quantify the uncertainty in percentage. The tested parameters are surface albedo, air temperature, precipitation, relative humidity, solar radiation, thermal resistance and wind speed. Uncertainty of thermal resistance and albedo were assumed to be 100% and 40% based on Figs R2b and R2d. Uncertainties of each meteorological variable were assumed to be RMSEs of ERA-Interim reanalysis data against the observational data (Fig. R6). Variations in meltwater within a possible parameter range are estimated by quadratic sum of results from each parameter shown in Fig. R5. Estimated uncertainty of meltwater is less than 50% at a large part of Thorthormi and Lugge Glaciers (Fig. R7). We will replace figures by the recalculated results and add Figs R5, R6 and R7 to the revised supplement.

Detailed bedrock topography is unavailable for all the glaciers. Available bedrock topography data were shown in Figs 6a and 6b in the discussion paper. Because our model geometry is not based on data, we performed sensitivity analysis using the broader range (±30%) of the sliding coefficient and ice thickness (Fig. R8). The RMSE between the modeled and measured flow velocities were computed as a measure of the model performance (Fig. R9). For Thorthormi Glacier, the model is similarly sensitive to sliding coefficient and ice thickness. For Lugge Glacier, the model is more sensitive to ice thickness than sliding coefficient. Figs R8 and R9 will be added to the revised supplement.

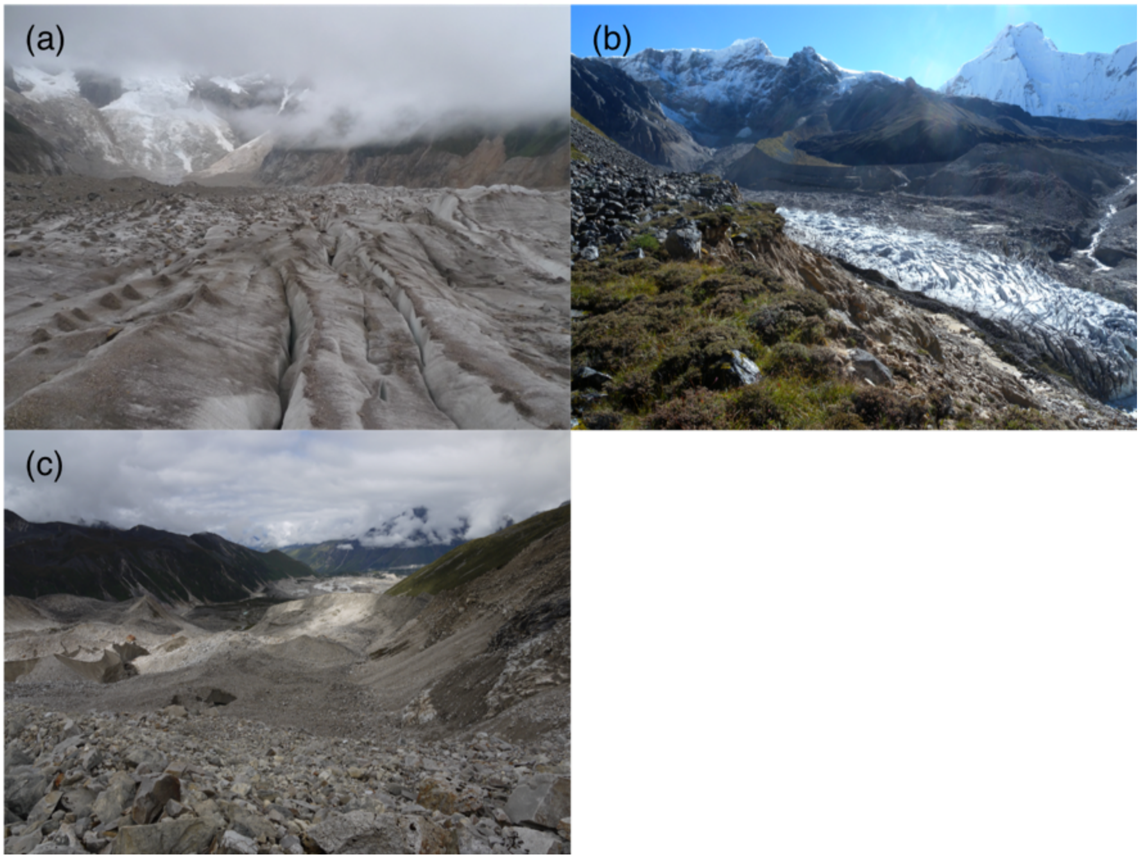


**Figure R1:** Photographs showing surface condition near the termini of (a) Thorthormi (18 September 2011), (b) Lugge (20 September 2011) and Lugge II Glaciers (21 September 2011).

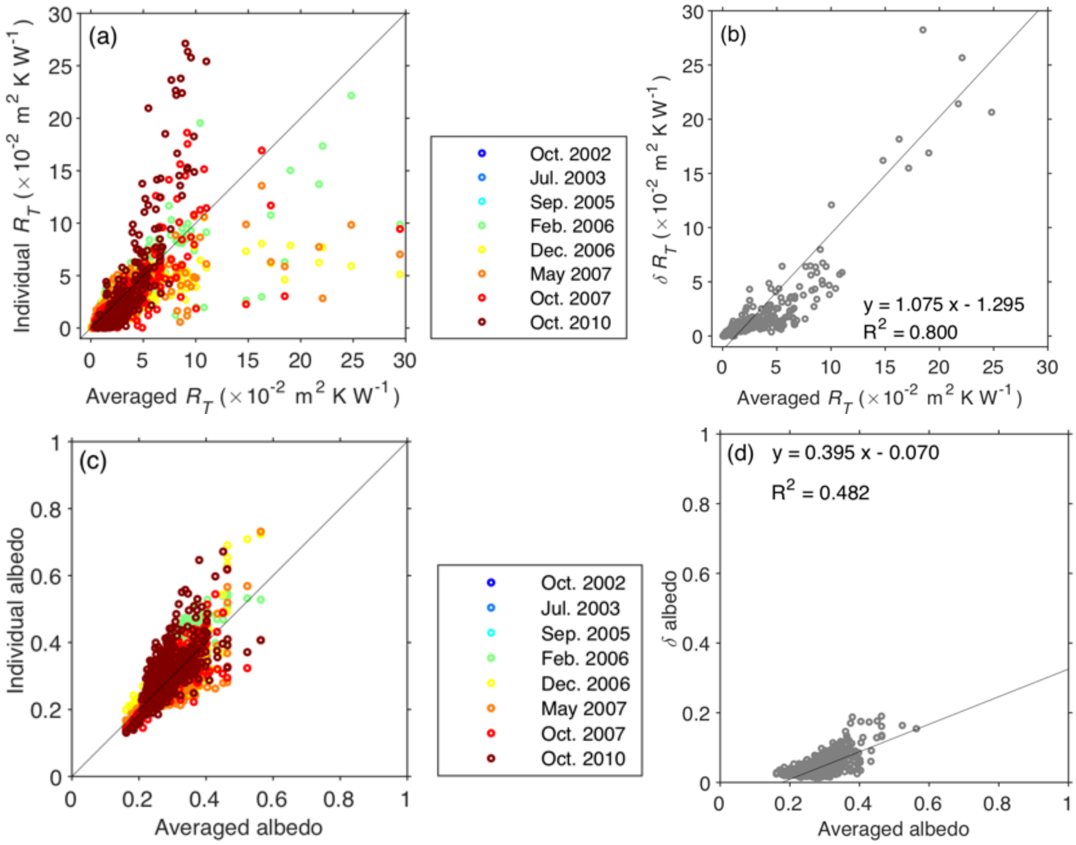


**Figure R2:** Scattergram of (a) thermal resistance ($R_T$) of the multitemporal ASTER data against their average derived from net radiation + sensible heat, which is used to calculate ice melting under the debris-covered surface of Thorthormi, Lugge and Lugge II Glaciers. (b) Standard deviations ( $\delta$ ) of thermal resistance. (c) Scattergram and (d) standard deviations of albedo.

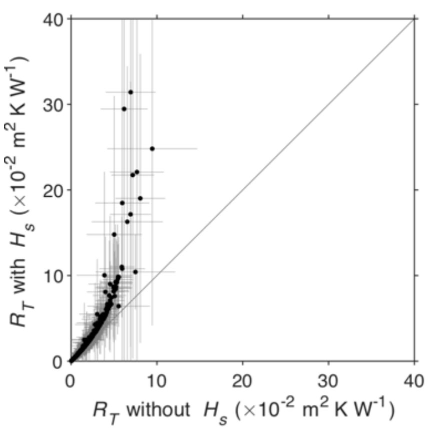


**Figure R3:** Scatter plot between thermal resistance calculated from only net radiation (without $H_s$) and from net radiation + sensible heat (with $H_s$).

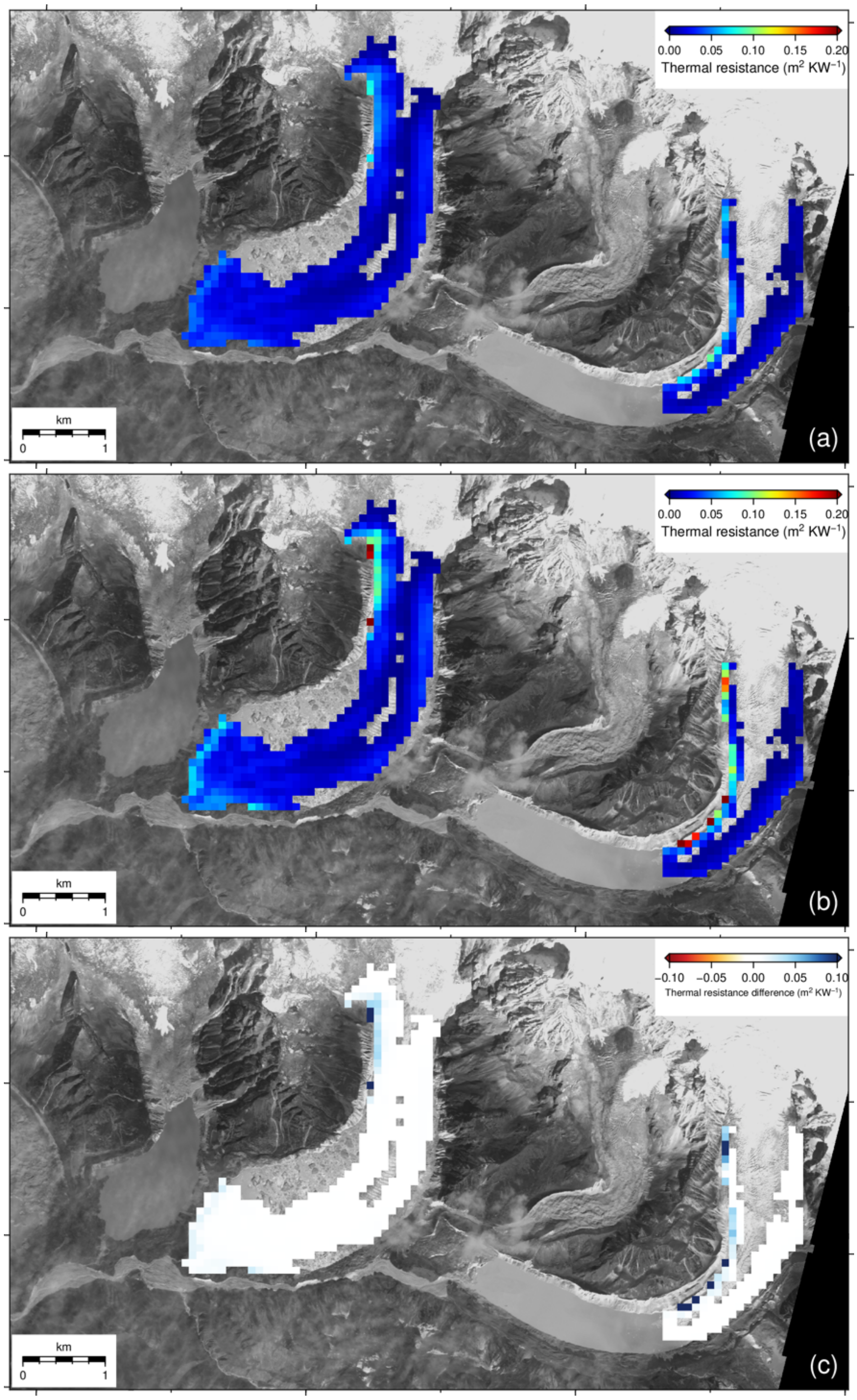


**Figure R4:** Spatial distribution of thermal resistance calculated (a) from only net radiation, (b) from net radiation + sensible heat and (c) difference of thermal resistance calculated by the two methods.

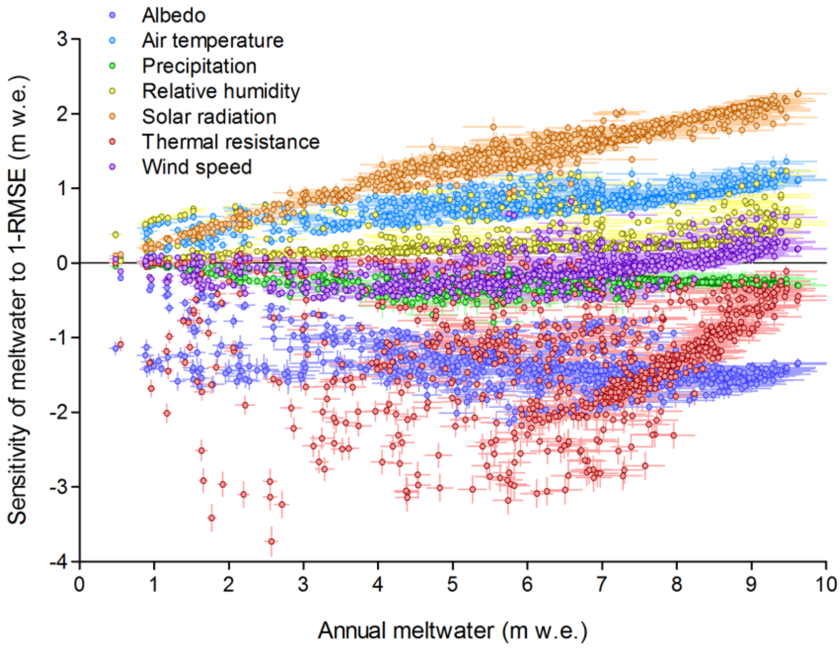


**Figure R5:** Sensitivity analysis of annual meltwater as a function of RMSE of each meteorological parameter at debris-covered area. Horizontal axis is variable annual meltwater calculated each grid in the SMB model. RMSEs except for albedo and thermal resistance are obtained from ERA-Interim and observed data for 2002–2004 (Fig. R6). Uncertainties of albedo and thermal resistance are derived from 8 satellite images (Fig. R2).

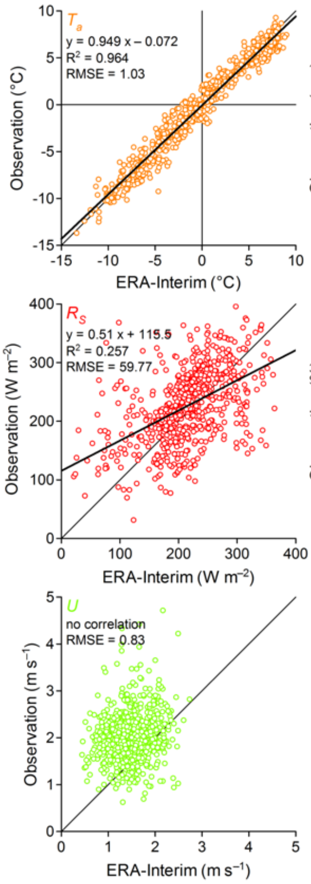


**Figure R6:** Scatter plot of air temperature, precipitation, solar radiation, relative humidity and wind speed between ERA-Interim reanalysis and observational data for 2002–2004.

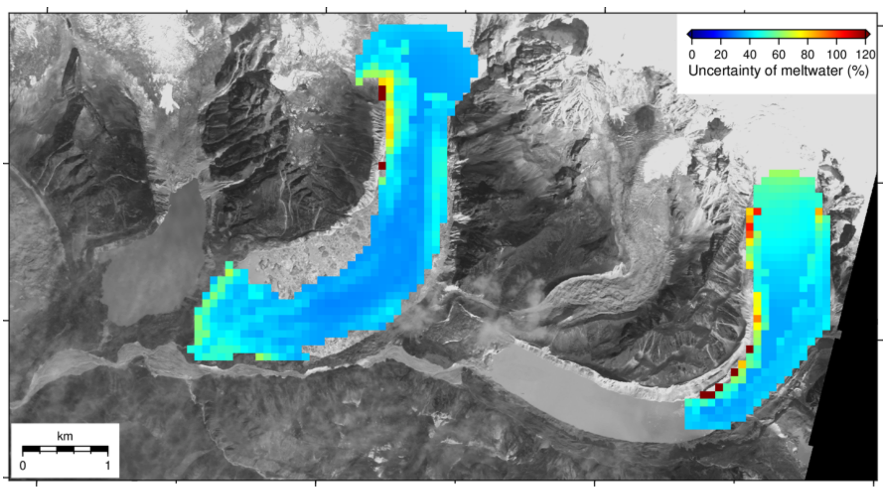


**Figure R7:** Spatial distribution of estimated uncertainty in the computed annual meltwater volume.

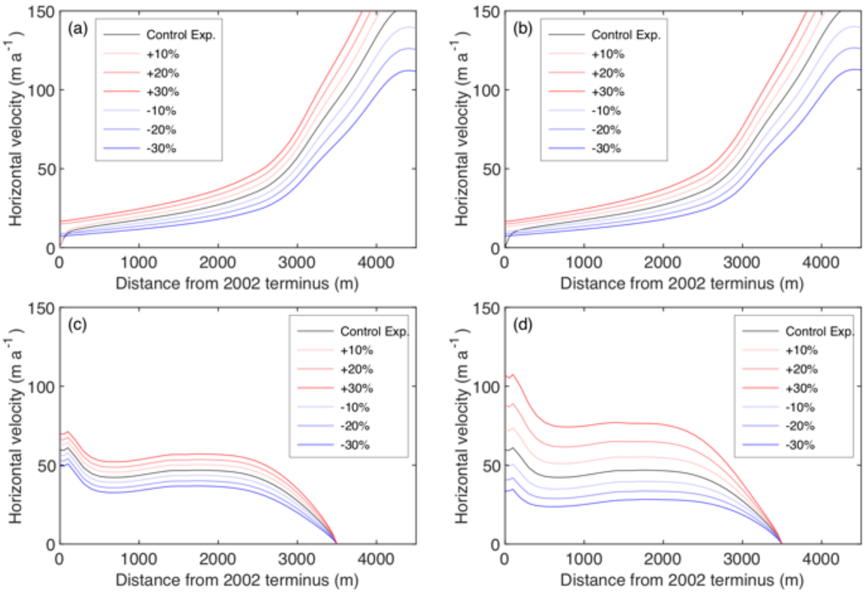


**Figure R8:** Surface velocity computed for (a and b) Thorthormi and (c and d) Lugge Glaciers obtained by changing (a and c) the sliding coefficient (C) by ±30%, and (b and d) ice thickness by ±30%. The black line is the control experiment.

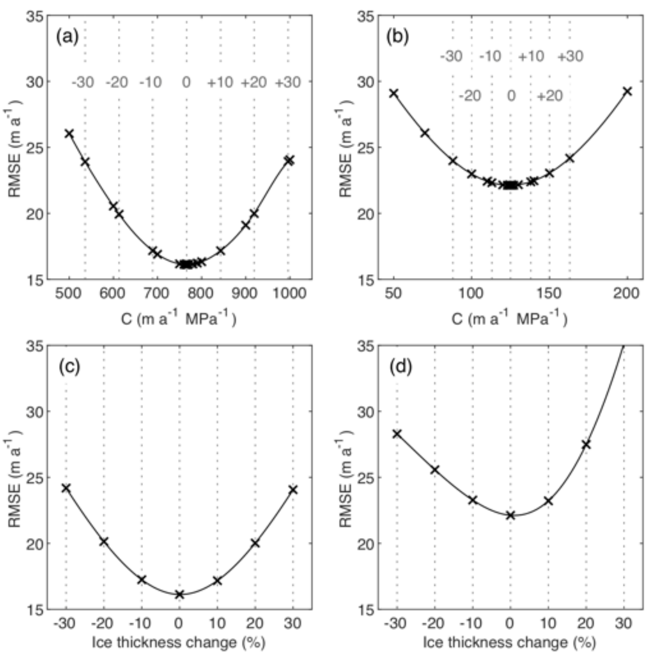


**Figure R9:** RMSEs between the modelled and measured surface velocities of (a and c) Thorthormi and (b and d) Lugge Glaciers, modelled with various (a and b) sliding coefficient (C), and (c and d) various ice thickness.

*2. The main conclusion of the manuscript is, as I understand it, that lake development does impact ice dynamics, and therefore thinning rates. I didn't get this from first reading, mainly because the two glaciers on which the manuscript focuses (Thorthormi and Lugge) are not easy to compare – they have very different geometries, different debris distributions, and different flow regimes (even before accounting for lake vs nolake). Given this, perhaps spending a bit more time looking at the lake- vs no-lake simulations for Lugge Glacier might help (the latter of which is given little attention at present). And/or looking further at what has happened at Thorthormi following lake development (see point 4 below). There are also several statements about the low impact of ice dynamics on the thinning rates of Lugge Glacier, yet a final forecast of rapid changes at Thorthormi Glacier once the lake develops – how can these two assertions be reconciled? Is it that emergence velocity at Lugge would be (more) positive in the absence of a lake? Overall, spending some further time sharpening the take-home message would be beneficial.*

The main conclusion of this study is that the dynamically-induced ice thickness change is small, and thinning of Lugge Glacier is mainly caused by negative SMB. On the other hand, more negative SMB is counterbalanced by dynamically-induced ice thickening, resulting in a smaller thinning rate of Thorthormi Glacier. Based on this conclusion, we hypothesize that the emergence velocity will decrease at Thorthormi Glacier after the expansion of the supraglacial lake, resulting in an increase in ice thinning rate. To test this hypothesis, we will discuss the influence of lake expansion on the emergence velocity based on lake- (Experiment 1) and land-terminating (Experiment 2) simulations for Lugge Glacier in the revised manuscript. We do not conduct additional analysis on surface elevation change of recent Thorthormi Glacier using satellite data (see reply to the major comments #3 below).

*3. Somewhere it needs to be explicitly acknowledged that this is a very (very) small sample. While the field data clearly cannot be replicated, an abundance of satellite remote sensing data are available to test some of these ideas across the broader*

*Lunana area. I acknowledge this would require significant further data processing, but augmenting the dataset would certainly give the study more substance.*

Satellite-based observations of glacier elevation change across the Bhutan Himalaya were carried out by Gardelle et al. (2013) and Maurer et al. (2016). We acknowledge the studies covering a large area and a greater number of samples. Nevertheless, our study has advantages in accuracy, and we performed additional analysis as described below. We evaluated surface elevation change of the studied glaciers by ASTER-DEMs, which is however to examine spatial representativeness of DGPS-DEMs. According to the accuracy analysis, we found that ASTER-DEMs ( $\sigma$ =~20 m; Fig. R10) has 10 times larger uncertainty in vertical coordinates than DGPS-DEMs ( $\sigma$ =1.91 m; see Fig. 2a in the discussion paper). The unique point of this study is to evaluate glacier surface elevation change by highly accurate DGPS data. We also investigate the contribution of ice dynamics to ice thinning that has not been quantified in the previous studies. We explain these points in the revised manuscript and take the suggested satellite analysis over a broader area as a future work.

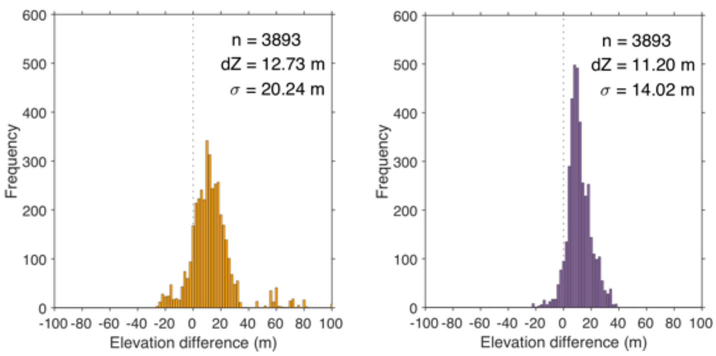


**Figure R10:** Elevation differences in the ice-free area (left) between 2004 ASTER-DEM and DGPS-DEM and (right) between 2011 ASTER-DEM and DGPS-DEM.

*4. The forecast for an impact on ice dynamics at Thorthormi is interesting, but represents a missed opportunity I think. Why not test this prediction, using velocity (and perhaps also surface elevation) data derived from more recent satellite imagery (it has been 7 years since detachment from the terminus). If this* *analysis does indeed show that the glacier has accelerated and thinned, it would add great weight to the existing conclusions.*

We acknowledge the test our prediction using surface velocity data derived from satellite images acquired after 2011. Nevertheless, quantifying interannual variability in surface velocity is difficult because of insufficient accuracy of the observation. Surface velocity observations of Thorthormi Glacier after 2013 was carried out by multitemporal Landsat 8 OLI images (Fahnestock et al., 2015). However, it is also difficult to quantify velocity change because of coarser spatial resolution and lower accuracy than our velocity data. We will not carry out additional analysis of surface velocity and surface elevation change of the glaciers by satellite remote sensing data because of the reasons described above and as a reply to the major comment #3.

## Minor comments (per line number)

*1-5: these two sentences are almost identical. Suggest rewording one or the other.*

We will change in the revised manuscript as follows. "Despite the importance of glacial lake development in ice dynamics and glacier thinning, in situ and satellite based measurements from lake-terminating glaciers are sparse in the Bhutan Himalaya, where a number of supraglacial lakes exist. We acquired in situ and satellite based observations across lake- and land-terminating debris-covered glaciers in the Lunana region, Bhutan Himalaya."

*5: spell out GPS in full*

We will change as suggested in the revised manuscript.

*6: move 'for the 2004-2011 period' to end of sentence*

We will change as suggested in the revised manuscript.

*12: does it really 'more than offset' glacier thinning? Surely this would result in thickening? Suggest 'compensates'. . .*

We will change from "more than offsets" to "compensates" in the revised manuscript.

*24: insert 'particularly' before 'sensitive' given that all glaciers are impacted by changes in temperature and precipitation*

We will change as suggested in the revised manuscript.

*28: remove 'therefore' given this sentence is not substantiated by preceding text*

We will remove it in the revised manuscript.

*29: what is meant by 'mechanisms' – this is rather vague. . .*

"mechanisms" here means mechanisms of much greater mass loss of Bhutanese glaciers than other glaciers in eastern Himalayas. We will change from "their mechanisms" to "mechanisms of mass loss of Bhutanese glaciers".

*47: spell out GPS at first use in main text*

We will change as suggested in the revised manuscript.

*54: I'm not sure remote sensing methods can't measure several metres of change. How about lidar? Suggest change to 'small' changes in surface elevation.*

Reviewer #2 and #3 also pointed out accuracy of DEMs derived from UAV and laser/radar altimetry, and we agree. We will remove the statement "However, the accuracy of the remotely sensed DEMs is still insufficient to measure several metres of glacier elevation change." in the revised manuscript.

*55: change 'sub-metre' to 'centimetric'?*

We will change as suggested in the revised manuscript.

*57: change 'performed' to 'acquired'*

We will change as suggested in the revised manuscript.

*59: remove 'rapid' since no results have been presented at this stage of the manuscript*

We will remove it in the revised manuscript.

*63-64: yes, but the glaciers are entirely different in geometry – some better justification for site selection is required here*

We will change reasons of selecting the glaciers as follows. "Thorthormi and Lugge glaciers were selected for analysis because these glaciers are situated around the same elevation. Lugge Glacier terminates a proglacial lake of Lugge Glacial Lake, while the terminus of Thorthormi Glacier is grounded but developing a large supraglacial lake (Bajracharya et al., 2014). Thus, making them suitable for evaluating the contribution of ice dynamics to the observed ice thickness changes. The glaciers are also suitable for field measurements because of its relatively safe ice-surface conditions and proximity to trekking route."

*65: using 'dynamic thinning' here is pre-emptive – it could be thickening too. . . maybe change to 'dynamics'?*

We will change to "dynamics" in the revised manuscript.

*65-66: change 'the surveyed glacier thinning' to 'changes in glacier surface* *elevation'*

We will change as suggested in the revised manuscript.

*72: is this thinning rate a mean value for the ablation area? Needs specifying.*

We will change to "Ablation area of the glacier thinned…" in the revised manuscript.

*75: is this what defines a land-terminating glacier? Does whether it is grounded or floating not represent a better criterion?*

We will change to "In 2011, the glacier terminus was grounded, and thus Thorthormi Glacier was a land-terminating glacier" in the revised manuscript.

*101 and elsewhere: I'm not sure what TCD protocol is for referencing web pages but this is awkward – can the full url not be put in the reference list?*

As replied to a comment from Referee #3, this format was used in a paper recently published in The Cryosphere (e.g., Friedl et al., 2018). Therefore, our manuscript also follows this format.

*112: very few points of elevation change are shown in Figure 1. . . where can I see these 431, 248 and 258 points?*

The grid size of the rate of elevation change in Fig. 1a is enlarged to 50 m, which was averaged from 1-m resolution DEMs for better visibility.

*114: 'off-glacier' should be hyphenated*

We will change as suggested in the revised manuscript.

*119: specify the sample number is 'n'*

We will change "the sample number *n*" in the revised manuscript.

*125: comment on the quality of the co-registration?*

The accuracy of the co-registration is estimated to be 0.05 pixel (ASTER Science Project, Japan Space Systems, 2012). We will add it in the revised manuscript.

*131: how can a single window size of 16 x 16 pixels be multi-scale?*

We will change to "with a correlation window size of 16 x 16 pixels" in the revised manuscript.

*136: replace 'aerial' with 'areal'*

We will change as suggested in the revised manuscript.

*141-143: why exclude the ponds? Would these not have ice beneath or do you think they have melted down to bedrock? Does this explain the very odd digitising of glacier area presented in Figure 4a?*

It is difficult to identify whether glacial ice exist beneath supraglacial lakes and ponds. However, many floating icebergs ware observed in the lake by in-situ measurements and satellite images. Presumably, these icebergs came from the bottom of the lake by acting subaqueous calving. We excluded floating icebergs in the lake from the glacial area. The annual glacier outlines were judged based on previously proposed manual / automatic digitise methods (e.g., Bajracharya et al., 2014; Nuimura et al., 2015; Nagai et al., 2016). According to the previous studies, supraglacial lakes and ponds are excluded from glacial area.

*152: change 'calculated' to 'estimated' given there are many uncertainties in the* *modelling*

We will change as suggested in the revised manuscript.

*159: that's a large uncertainty. How does it propagate through for the rest of the modelling?*

See a reply to the major comment #1.

*230: make it clear here that you're simulating a lake-free Lugge Glacier – I read this that at present the lake is frozen! Suggest 'For Lugge Glacier, we simulate a lake-free situation, with ice flowing to the contemporary terminal moraine' or similar*

We will change as suggested in the revised manuscript.

*315-316: are these both '-3 to 0 m a-1' by coincidence or is there a typo?*

These two thinning rates are coincidence.

*341-342: but you go on to show that dynamics only play a minor role in thinning at Lugge. . . are you suggesting dynamics were more important following initial lake development?*

We will change from "dynamic thinning was enhanced" to "dynamic thickening was weakened" in the revised manuscript.

*344: specify this is 'simulated' SMB. . .*

We will add "simulated" here in the revised manuscript.

*427: does this statement that dynamic thinning is small at Lugge not undermine* *the main take-home message of the manuscript?*

Our conclusion is that the dynamically-induced ice thickness change is small, and significant ice thinning of current Lugge glacier is mainly caused by negative SMB. So that the statement here is consistent with our conclusion.

*537: replace 'Mörg' with 'Mölg'. . .*

We will change here and in the reference list in the revised manuscript.

*Figure 1: can you indicate the ponds that ultimately coalesce into a lake on Thorthormi?*

Because the location and size of the ponds are significantly varied from year to year, we could not indicate the ponds that coalesce into a large supraglacial lake. Coalescence of these ponds into a lake is confirmed by the Google Earth.

*Figure 3: can you be sure these data towards the terminus of Lugge are not tracking the recession of the ice-front? How do you avoid matching the ice-front (i.e. the dominant feature) in these locations?*

We excluded velocity measured near the glacier frontal margin to avoid such problems.

*Figure 4: how were these outlines derived? They look very odd to me, with no obvious distinction in the debris-cover around any of the digitised outlines. . .*

Glacier outlines were judged from multiple Landsat images, and it was verified using ALOS PRISM images from the same period and Google Earth. Many floating icebergs ware observed in the lake by in-situ measurements and satellite images. Presumably, these icebergs came from the bottom of the lake by acting subaqueous calving. We excluded floating icebergs in the lake from the glacial area.

Although glacier outlines are not necessarily clear because of debris covering, the obtained glacier terminus retreated or advanced depending on the location. According to analysis using Landsat images with 30 m resolution (Paul et al., 2013), a user-induced accuracy error was estimated to be 5% of delineated area of glaciers with more than 1 $km^2$. Following the previous study, we estimated user-induced accuracy error by 5% in the revised manuscript.

References:


- Fahnestock, M., T. Scambos, T. Moon, A. Gardner, T. Haran, and M. Klinger: Rapid large-area mapping of ice flow using Landsat 8, Remote Sensing of Environment, 185, 84–94, https://doi.org/10.1016/j.rse.2015.11.023, 2015.
- Friedl, P., Seehaus, T. C., Wendt, A., Braun, M. H., and Höppner, K: Recent dynamic changes on Fleming Glacier after the disintegration of Wordie Ice Shelf, Antarctic Peninsula, The Cryosphere, 12, 1347–1365, https://doi.org/10.5194/tc-12-1347-2018, 2018.