Reply to Referee #1

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*).

In this study, the authors combine field measurements (DGPS), satellite image analysis (debris thickness estimation, surface velocity fields) and modelling (2D flow model, surface mass balance (SMB) simulated from an energy balance model) to assess how sensitive the thinning rate of two glaciers in Bhutan is to the presence or not of a proglacial lake. This question is important because many studies have already observed higher thinning rates for lacustrine terminating glaciers than for land terminating glaciers (a complete list of references addressing such observations is available line. But the reasons for this different behavior are still not entirely clear (accelerated flow and calving, flotation).

This study includes different steps:
1. Thinning rates estimation using DGPS measurements;
2. Debris thickness estimation using thermal ASTER images
3. SEB modelling
and 4. Flow modelling.

Major comments:

My main concern comes from the fact that each step mentioned above has large uncertainties (see my comments below) that are not possible to quantify because there is almost no validation (except surface velocity fields derived from optical satellite images). As a consequence, the results are rather subjective and are, in my opinion, not supported. This is a pity, because the study is interesting and exhaustive, but in state, it looks like more a theoretical numerical exercise than a field case study. I have some suggestions below to try to evaluate (only qualitatively though) the reliability of some results, but without validation dataset, I doubt that the results can be fully supported. I found interesting the *experimental strategy (starting from the present state for experiment 1, and exploring the opposite situation removing or adding a proglacial lake for experiment 2), but according to me, given that the debris thickness spatial variability is likely to be badly reproduced, the SMB is highly uncertain, or the bedrock topography extremely simplified, such experimental exercise concerns more synthetic glaciers than true case studies. If there is no possibility to validate the results at each step, I recommend to stick with the theoretical approach (applying the flow model to a synthetic glacier with and without proglacial lake, prescribing a vertical mass balance gradient observed on Himalayan glaciers, including a debris cover part such as Chhota Shigri Glacier, India for instance – Azam et al, Annals Glaciol. 57, 328–338, 2016) than trying to relate this study to a true case study.*

Because some in-situ data (e.g., SMB and bedrock topography) is unavailable for the studied glaciers, validation of the results at each step is difficult. We therefore performed (1) validation of spatial representativeness in thinning rate obtained from DGPS with that from satellite-derived DEM (see reply to the major comment #2). We also performed sensitivity analyses for (2) SMB modelling and (3) glacier flow modelling to estimate uncertainties. For the SMB modelling, we recalculated spatial distribution of debris thermal resistance with considering sensible heat flux (see reply to the major comment #3). Detailed bedrock topography is unavailable for all glaciers, but we alternatively evaluated sensitivity of modelled ice speed against changes in ice thickness and basal sliding coefficient. We also computed RMSE between modelled and observed ice speed as a measure of the uncertainty (see reply to the major comment #4).

Surface elevation changes have been obtained by interpolating points surveyed by DGPS in the field with obviously a limited number of points (approx. 5000 to 26000 surveyed points over glaciers TabS1). Given that those glaciers are rather large (approx. 13, 11 and 3 km2, respectively for thorthormi, Luge and Lugge II), the number of points is not very large (corresponding to a relative coverage <1% of the total glacier surface). These glaciers are also heavily debris covered (with *supra glacial lakes and likely cliffs, the latter not being mentioned in the text though), and in turn with a large variability of their thinning rates (e.g., Immerzeel et al., Remote Sensing Env., 150 (2014) 93–103). Consequently, using an interpolation technique to derive the glacier surface thinning rate is questionable. The expected accuracy is therefore probably very bad, and I doubt that the standard errors displayed in table 1 (a few cm) obtained from the surveyed points can be applied to the whole glacier surface. The authors should comment on this, and should explore how sensitive the results of their study are to these glacier surface thinning rates, which are likely to be very different from their point thinning rates (with a difference potentially as high as a few meters in some areas i.e. cliffs, ponds...). In my opinion, the authors should compare their DEM with DEM obtained from satellite images.*

In order to evaluate spatial representativeness of glacier surface elevation change obtained from our DGPS measurements, we compared elevation changes obtained from DGPS-DEMs and from ASTER-DEMs acquired on 11 October 2004 and 6 April 2011, which cover similar period of our field campaign (2004–2011). ASTER-DEMs with 30 m resolution provided by the ASTER-VA (https://gbank.gsj.jp/madas/map/index.html) were used to compute the surface elevation change. Elevation of ASTER-DEMs was calibrated by the DGPS data on ice-free terrain in 2011. The 2004 and 2011 ASTER-DEMs showed positive biases (dZ) of 12.73 and 11.20 m, and standard deviations (σ) of 20.24 and 14.04 m, respectively (Fig. R1). Vertical coordinates of the ASTER-DEMs were then corrected for the corresponding bias. Elevation change over the glacier surface was computed as difference of the calibrated DEMs (Fig. R2). Given the error range of ASTER-DEM, the rate of elevation change derived from DGPS-DEMs is similar to that from ASTER-DEMs (Fig. R3). In order to evaluate spatial representativeness of the DGPS survey, we compared the rate of elevation change of 1-m-grid DGPS-DEMs with that of 30-m-grid ASTER-DEMs along elevation (Fig. R4). Mean rates of elevation change with its standard deviation from DGPS-DEMs are -1.40 ± 0.77 and -4.67 ± 1.36 m a^{-1} for Thorthormi and Lugge Glaciers, respectively, while those from ASTER-DEMs over the elevation range where the DGPS measurements exist are -0.70 ± 1.25 and -4.87 ± 1.29 m a^{-1} for Thorthormi and Lugge Glaciers, respectively. Figure R4 shows that the rates from DGPS-DEMs fall within those of ASTER-DEMs, and thus it supports applicability of our survey results to the entire ablation zone. In the revised manuscript, we will take into account spatial variability in the rate of elevation change from ASTER-DEMs to uncertainty of the mean rate over the entire ablation zone.

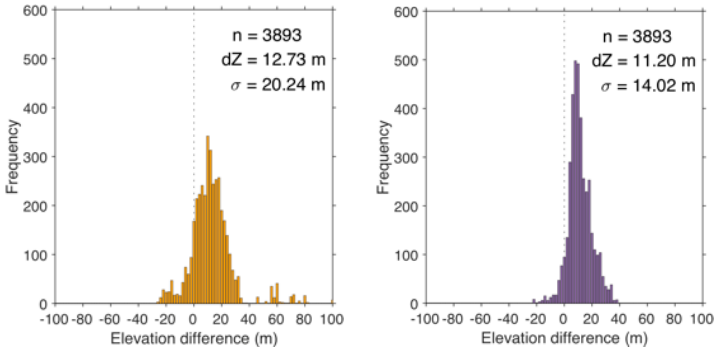

Figure R1: Elevation differences in the ice-free area (left) between 2004 ASTER-DEM and DGPS-DEM and (right) between 2011 ASTER-DEM and DGPS-DEM.

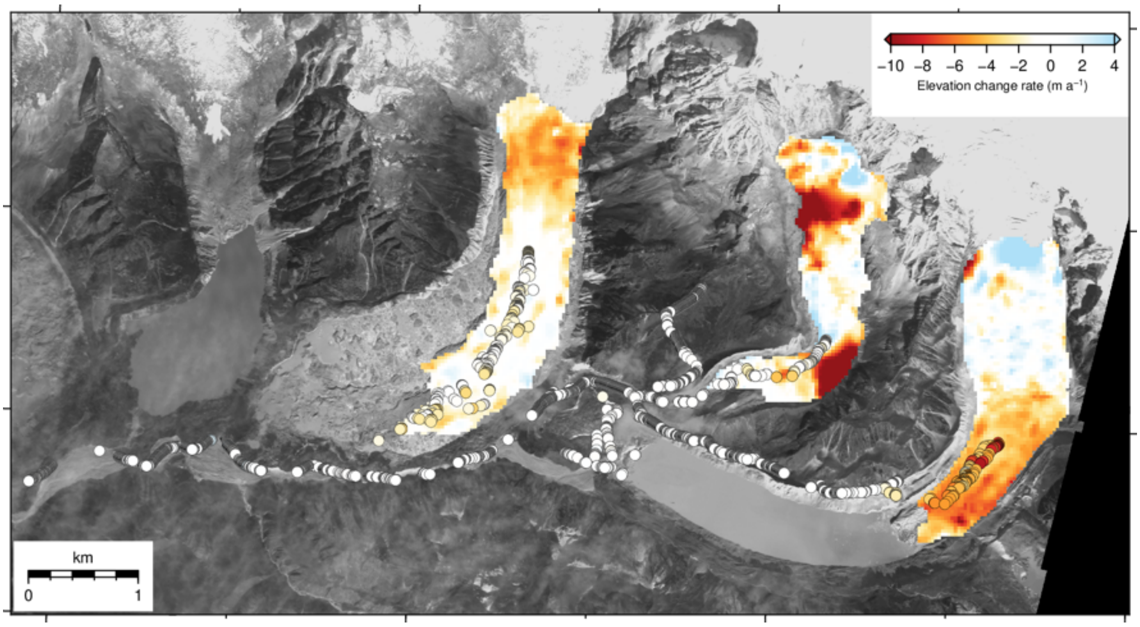

Figure R2: Rate of elevation change for the 2004–2011 period derived from ASTER-DEMs (background shadings) and DGPS-DEMs (circles filled with the same color scale). Glacier outlines are of December 2011.

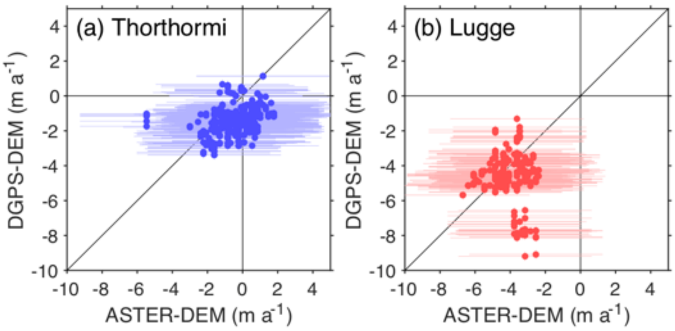

Figure R3: Scatter plot of the rate of surface elevation change between from ASTER-DEMs and DGPS-DEMs at (a) Thorthormi and (b) Lugge Glaciers. Error bars denote standard deviations of DEM differences over the ice-free terrain.

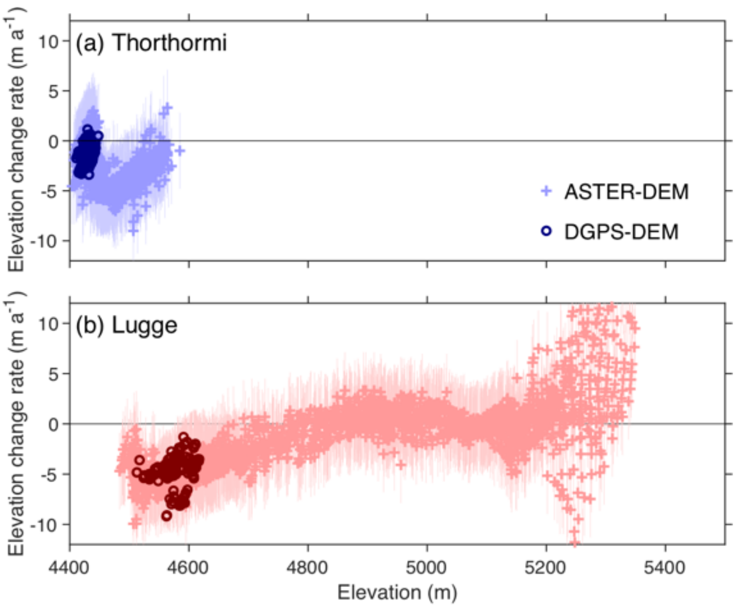

Figure R4: Rate of elevation change along elevation at (a) Thorthormi and (b) Lugge Glaciers. Dark-colored circles are from DGPS-DEMs and light-colored crosses are from ASTER-DEMs. Error bars denote standard deviations of DEM differences over the ice-free terrain.

SMB simulations depend on the debris thickness (obtained from ASTER thermal imagery known to be potentially inaccurate), as well as a surface energy balance model based on a large set of hypothesis and parameters (i.e. T=0 ◦ C at the ice-debris interface, linear debris temperature profile within the debris (lines 162-63), *surface roughness, albedo of the debris, or bare ice to list only some sensitive parameters – see table 1 of Fujita and Sakai, 2014 for a complete list of parameters). Even though there is no information regarding the used parameter set, I presume that most of these parameters have been taken from a previous study conducted on Tso Rolpa catchment in Nepal (Fujita and Sakai, 2014) where the surface energy balance has been validated using hydrological and meteorological observations. We do not know if the parameters used in Fujita and Sakai (2014) are transferable to this present catchment in Bhutan. In short, there are a large amount of sources of uncertainties (not discussed in this present study), which prevent the results from being reliable if not validated. Looking at results of SMB (Fig 1c), point surface mass balance are very negative. The authors compute SMB of -7 m w.e./a over debris cover areas (section 4.4). To my knowledge, such very negative values of point SMB have never been observed in the Himalayas beneath debris. Plausibly, such values could correspond to very thin debris cover (a few mm or cm, before the maximum of the Ostrem curve) but given the location of these areas (in the lower part of the glaciers where the debris thickness is expected to be the largest), it is highly unlikely. Moreover, the studied glaciers are debris covered, with potentially cliffs and ponds at their surface (is it true? No information regarding cliffs in this study) so the SMB spatial variability is supposed to be very high (e.g., Immerzeel et al, Remote Sensing Env., 150 (2014) 93–103; Buri et al., Ann Glaciol. 57(71), 199–211, 2016, Miles et al, Ann glaciol., 57(71), 29–40,2016) although the SMB map displayed in Fig1c does not show large spatial heterogeneities. In order to evaluate the reliability of the SMB results, a map showing the debris thickness over the 3 glaciers would be necessary. It would be useful also to show the SMB gradient as a function of elevation. And a sensitivity test including all parameters is necessary to test the reliability of the results.*

Although debris thickness was not measured during the field campaign, ice is exposed from place to place over Thorthormi and Lugge Glaciers (Figs R5a and R5b), suggesting that debris-cover is rather thin than that of Lugge II Glacier (Fig. R5c). 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. R6). 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 R7 and R8. Spatial distribution of the difference between the two results is also shown in Fig. R8c. Thermal resistance significantly increased after the consideration of sensible heat (Fig. R7). However, large difference appeared only near the western margin (Fig. R8) 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. R9). 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 R6b and R6d. Uncertainties of each meteorological variable were assumed to be RMSEs of ERA-Interim reanalysis data against the observational data (see Fig. R13). Variations in meltwater within a possible parameter range are estimated by quadratic sum of results from each parameter shown in Fig. R9. Estimated uncertainty of meltwater is less than 50% at a large part of Thorthormi and Lugge Glaciers (Fig. R10). We will replace figures by the recalculated results and add Figs R9 and R10 to the revised supplement.

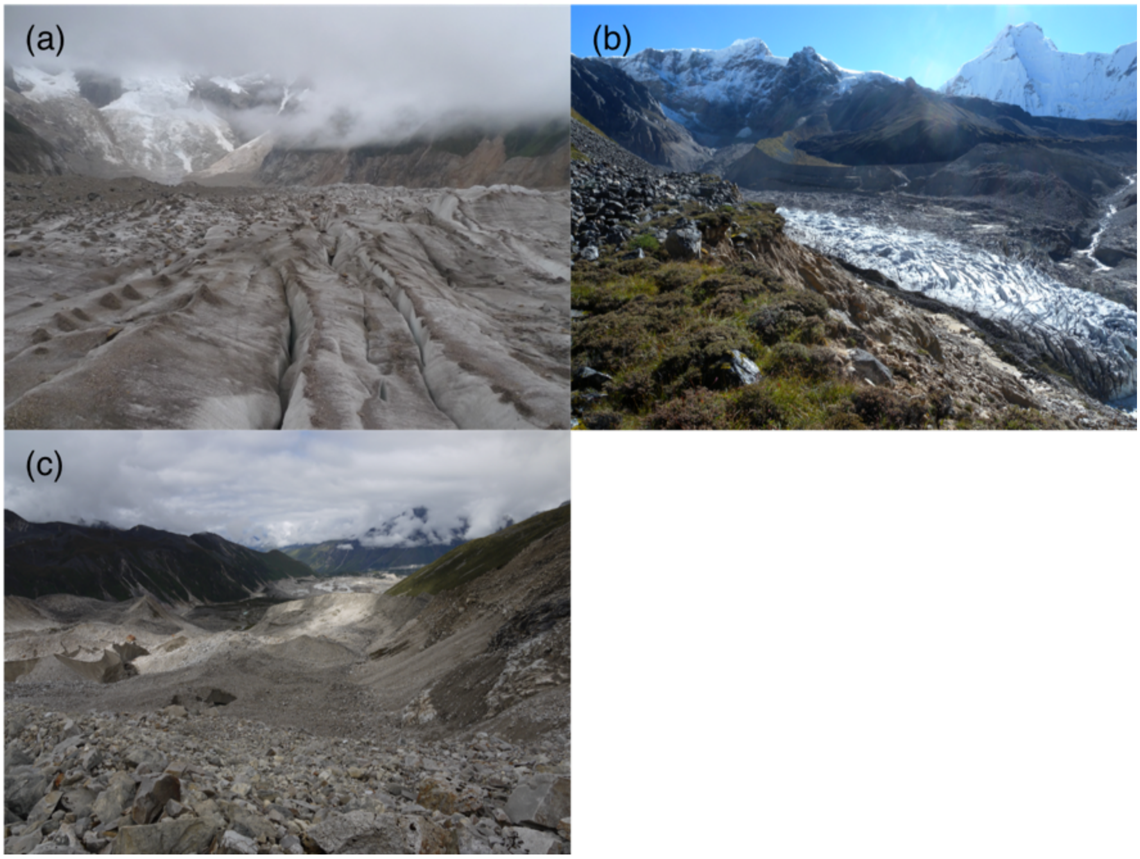

Figure R5: Photographs showing surface condition near the termini of (a) Thorthormi (18 September 2011), (b) Lugge Glaciers (20 September 2011) and Lugge II Glaciers (21 September 2011).

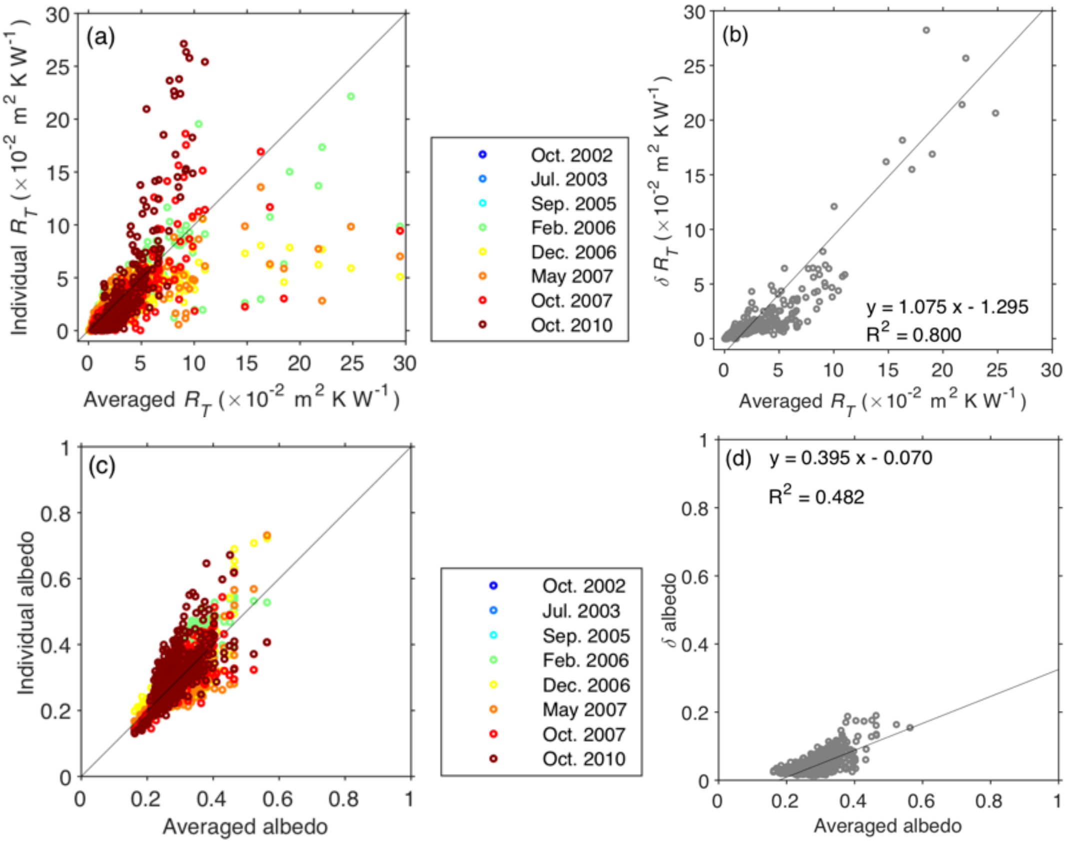

Figure R6: Scattergram of (a) thermal resistance (R_T) of the multitemporal ASTER data against their average derived from net radiation + sensible heat. The mean thermal resistance is used to calculate ice melting under the debris-covered surface of Thorthormi and Lugge Glaciers. (b) Standard deviations (δ) of thermal resistance. (c) Scattergram and (d) standard deviations of albedo.

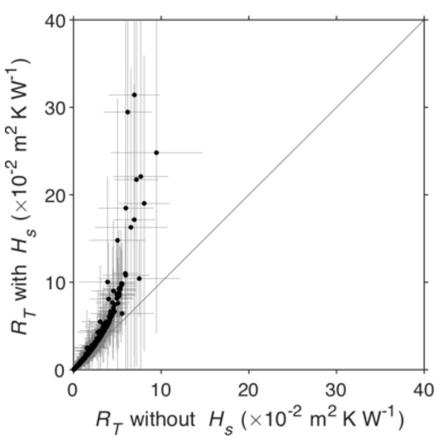

Figure R7: Scatter plot between thermal resistance calculated from only net radiation (without H_s) and from net radiation + sensible heat (with H_s).

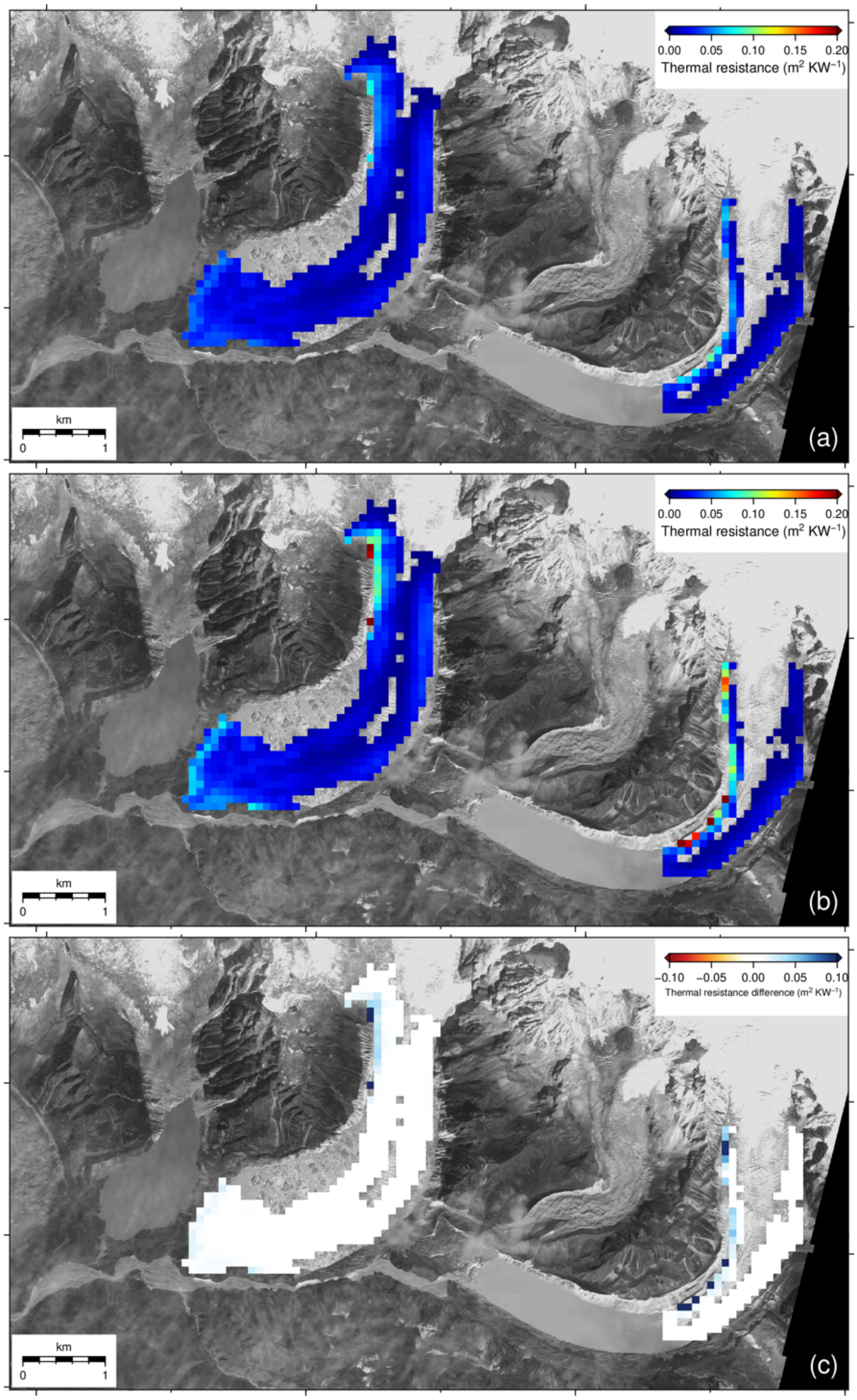

Figure R8: Spatial distribution of thermal resistance calculated (a) from only net radiation, (b) from net radiation + sensible heat and (c) difference of thermal resistance calculated by the two methods.

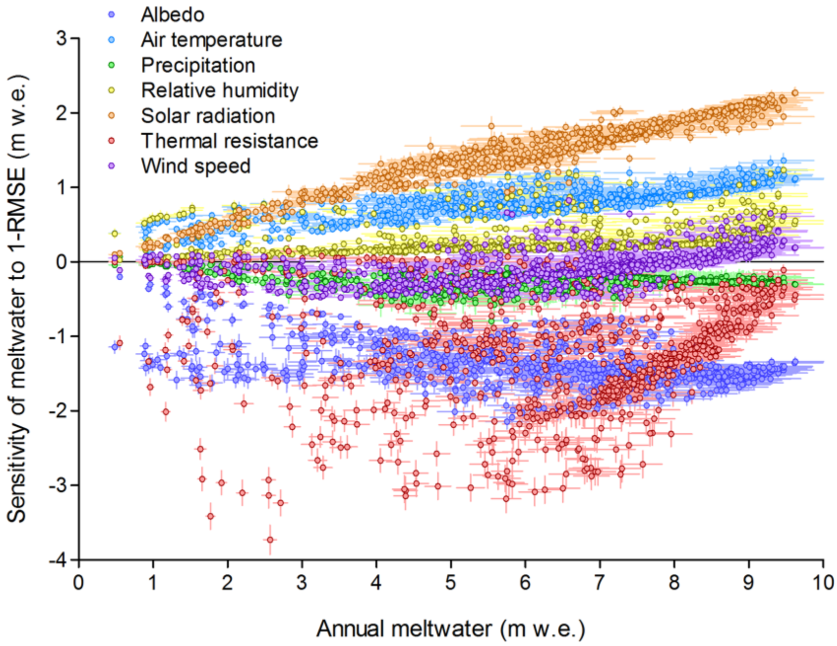

Figure R9: Sensitivity analysis of annual meltwater as a function of RMSE of each meteorological parameter at debris-covered area. Horizontal axis is variable annual meltwater calculated each grid in the SMB model. RMSEs except for albedo and thermal resistance are obtained from ERA-Interim and observed data for 2002–2004 (Fig. R13). Uncertainties of albedo and thermal resistance are derived from 8 satellite images (Fig. R6).

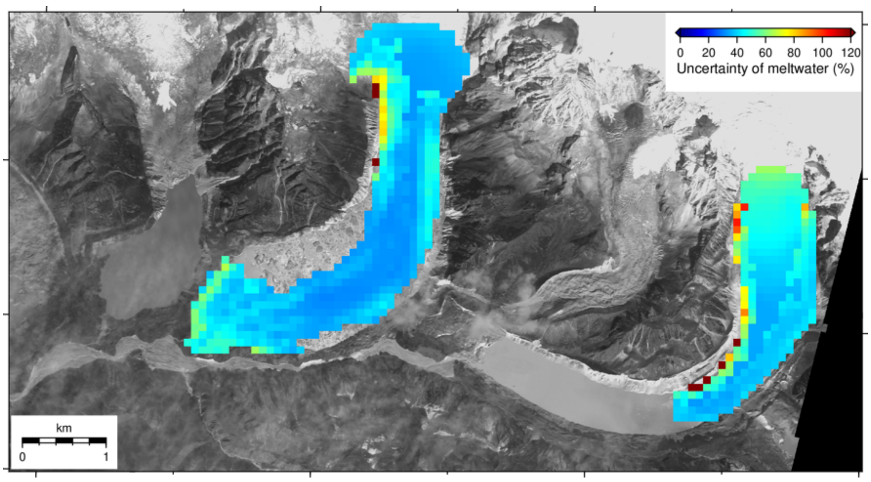

Figure R10: Spatial distribution of estimated uncertainty in the computed annual meltwater volume.

The application of the debris flow model in 2 opposite configurations (experiments 1 and 2) is interesting but the bedrock topography is potentially very different from reality. Either the authors stick with a theoretical case (using an idealized synthetic glacier with a prescribed bedrock topography) or they make a sensitivity analysis using different bedrock topographies, sliding coefficients. . . A sensitivity test has been performed (section 5.2) but I believe that the explored range of ice thickness (+/-10 m) or sliding coefficient (+/-10%) should be much wider.

We performed sensitivity analysis using the broader range (±30%) of the sliding coefficient and ice thickness (Fig. R11). The RMSE between the modeled and measured flow velocities were computed as a measure of the model performance (Fig. R12). 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 R11 and R12 will be added to the revised supplement.

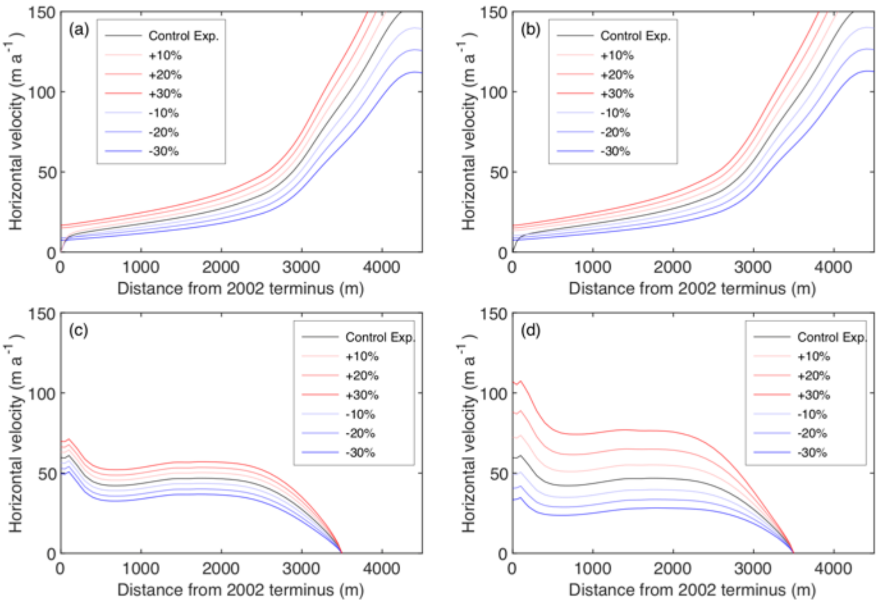

Figure R11: Surface velocity computed for (a and b) Thorthormi and (c and d) Lugge Glaciers obtained by changing (a and c) the sliding coefficient (C) by ±30%, and (b and d) ice thickness by ±30%. The black line is the control experiment.

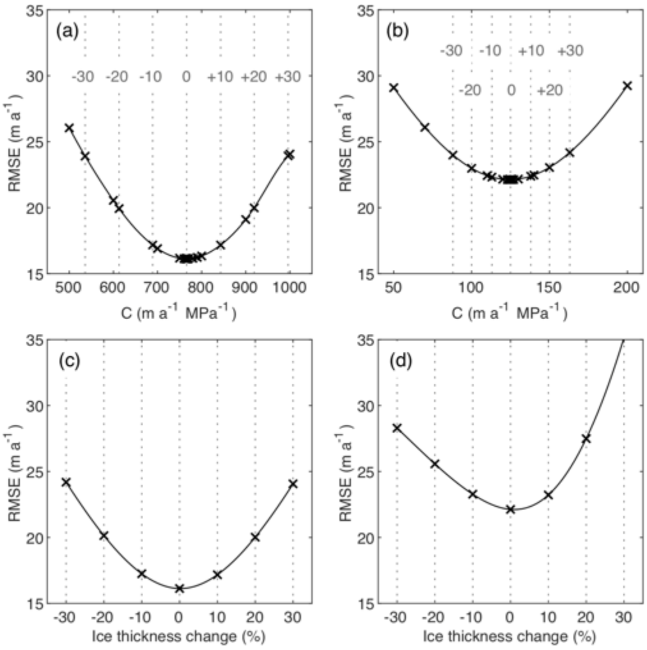

Figure R12: RMSEs between the modelled and measured surface velocities of (a and c) Thorthormi and (b and d) Lugge Glaciers, modelled with various (a and b) sliding coefficient (C), and (c and d) various ice thickness.

May be I missed something but SMB is estimated over the period 2002-2004, elevation change over the period 2004-2011 and flow velocities are simulated over the period 2002-2010 (but it is not clear for the latter). The periods do not match although results of thinning rate, or SMB are compared each other. How are data/results extrapolated in time? This might bring another layer of uncertainty to the results.

We extended calculating period of SMB for 38 years (1979–2017), which covers the period of the surface elevation change survey (2004–2011). Meteorological variables such as air temperature, precipitation, solar radiation, relative humidity and wind speed in the ERA-Interim reanalysis data (2002–2004) was calibrated with observational data (Fig. R13). Wind speed of ERA-Interim was simple multiplied with 1.3 for obtaining the same average with the observational data. SMBs calculated with observed and calibrated ERA-Interim data for 2002–2004 were compared with those from ERA-Interim data for 1979–2017 (Fig. R14).

SMBs for 2002–2004 (both from observational and ERA-Interim data) showed no clear anomaly against the long-term mean SMB (1979–2017). We will add Figure R13 in the revised supplement. We will replace the SMB result by the 1979–2017 ERA-Interim version in the revised manuscript.

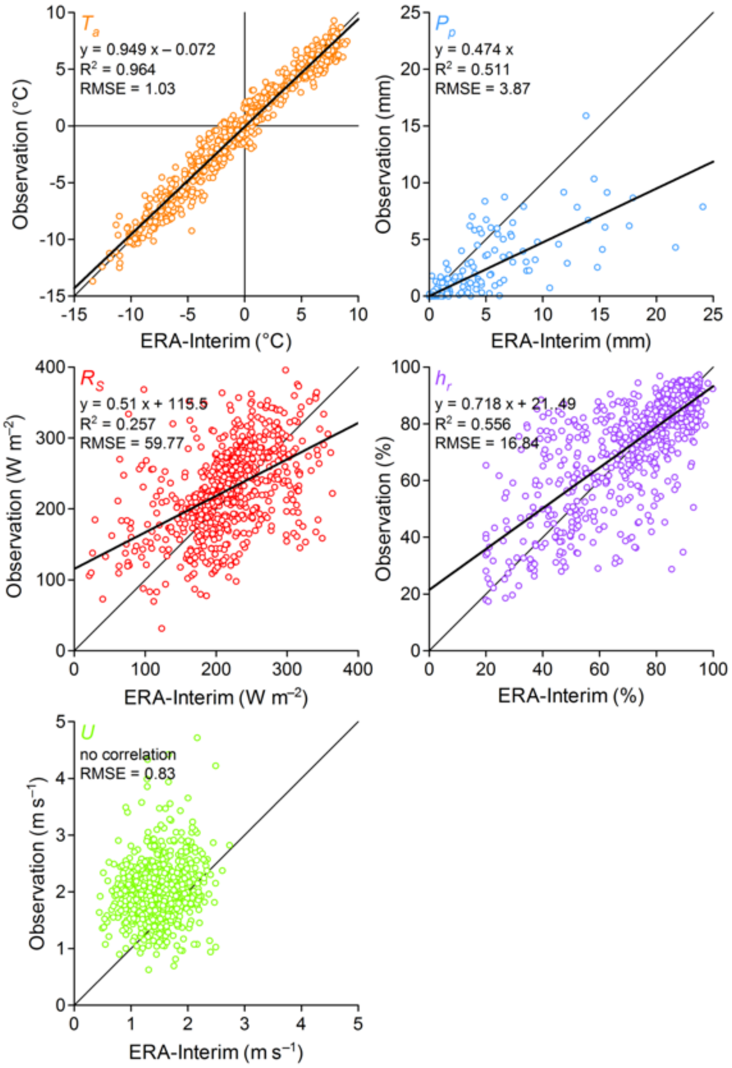

Figure R13: Scatter plot of air temperature, precipitation, solar radiation, relative humidity and wind speed between ERA-Interim reanalysis and observational data for 2002–2004.

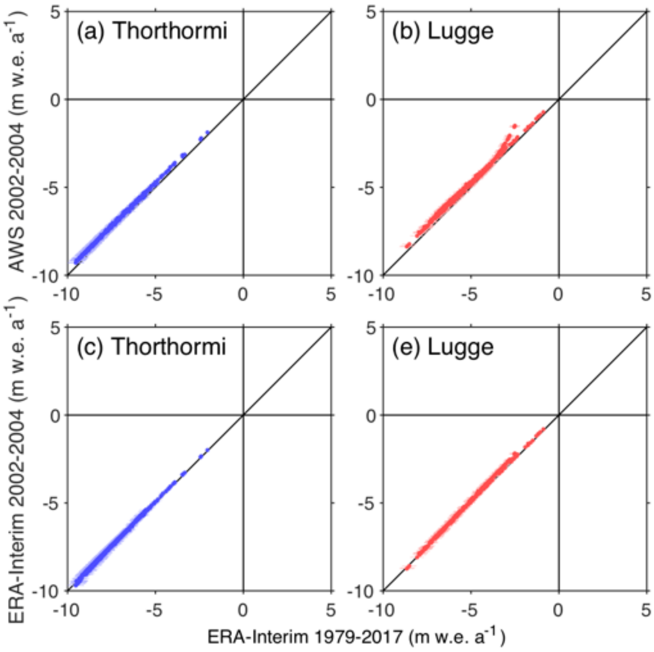

Figure R14: Scatter plot between SMBs of (a and c) Thorthormi and (b and d) Lugge Glaciers. SMBs calculated from ERA-Interim reanalysis data (1979–2017) against (a and b) observational meteorological data (2002–2004) and (c and d) ERA-Interim data (2002–2004). Error bars denote standard deviations derived from the long-term variability (1979–2017).

I found it confusing to have a study focusing on 2 glaciers (Thorthormi, Lugge) but including in fact 3 glaciers (the 2 previous glaciers and Lugge II). I know that finally most of the study is based on the comparison between Thorthormi and Lugge, because the flow model has not been applied on Lugge II, but finally why? Would the results have been different? With this partial study on Lugge II, I do not see any added value.

We excluded Lugge II Glacier from the detailed discussion because lack of the bed topography hampered the flow modelling. Spatial variability in surface elevation change derived from ASTER-DEMs is greater than those of Thorthormi and Lugge Glaciers (Fig. R2). Therefore, it is unsure whether the elevation change derived from DGPS-DEMs is representative. For these reasons we excluded Lugge II Glacier from the surface velocity measurements, SMB modelling and the detailed discussion. We will remove descriptions and figures related to Lugge II Glacier from the revised manuscript but we will leave the rate of elevation change of the glacier as an observational fact.

Specific comments

Line 19: Mölg instead of Mörg, same in the reference list

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

Line 21-22: images used by Bajracharya et al (2014) in 1980 to quantify the area reduction of Himalayan glaciers were full of snow, and in turn the area reduction from 1980 to 2010 is likely to be exaggerated. I recommend to report here the area reduction from 1990 to 2010, likely more accurate. This comment is valid for every places where this study is cited (section study area). It might be useful to compare the glacier area reduction obtained in this present study (section 3.3 – period 2000-2011) with the results of Bajracharya et al (2014) for the period 2000-2010.

We will change the area reduction from 1980–2010 to 1990–2010. Our analysis of glacier area reduction from 2000 to 2011 will be compared with the results from Bajracharya et al. (2014) in the revised manuscript.

L24: -0.22 +/- 0.12 m w.e./a (Gardelle et al, 2013) is not restricted to the ablation area but for the entire glaciers: this figure corresponds to the region-wide mass balance. Same comment for Maurer et al (2016), -0.17 m w.e./a is the glacier wide mass balance, not the ablation area

We will change "the ice thinning rate in the ablation are of" to "glacier-wide mass balance of" in the revised manuscript.

L30: may be worth updating the reference and citing Huss and Hock, 2018 here (Nature Climate Change, VOL 8 | FEBRUARY 2018 | 135–140 | www.nature.com/natureclimatechange)

We will change the citation to their latest study in the revised manuscript.

L54: I disagree with this statement, DEM differencing using satellite images do allow extracting signals of a few meters, especially with the new generation of satellite images i.e. Pléiades, World view. . . The best proof of this are the references just cited above.

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.

L56-58: in Nepal, Vincent et al (2016) show that the repeated DGPS profiles performed in the field were accurate enough to extract a thinning rate along the considered profile, but more importantly, they also said that this thinning rate along the profile is not representative of the whole glacier surface, or cannot be extrapolated in space given that the spatial variability of this thinning rate is extreme over debris covered tongues, due to the large variability of debris thickness and heterogeneity, presence of ponds or cliffs. Therefore, using remote sensing techniques (satellite, UAV) to obtain a thinning rate over the debris cover tongue is more accurate than performing sporadic repeated DGPS profiles.

The repeated DGPS survey accurately measured spatially homogeneous thinning rate over a debris-free glacier without supraglacial ponds and ice cliffs. As Vincent et al. (2016) argued, remote-sensing techniques (e.g., UAV) has advantage to study thinning of a debris-covered glacier. This is because significantly variable debris thickness and surface conditions (ponds and ice cliffs) are more covered by such techniques than DGPS survey. We will describe such an advantage of remote-sensing observations after "(e.g., Gardelle et al., 2013; Maurer et al., 2016; Brun et al., 2017)" and remove "Nepal (Vincent et al., 2016)" in the revised manuscript.

L67: it might be worth including the elevation range of each glacier, at least to have an idea of their maximum elevation, and the fact that they are potentially cold or polythermal. This issue is important for flow modelling

We will add minimum, medium and maximum elevations of each glacier in Table S1 in the revised supplement. We agree that ice temperature condition is important for flow modelling. Polythermal structure was reported at higher elevation (>5000 m) in Yala and Khumbu Glaciers in the Nepal Himalaya (Mae et al., 1975; Watanabe et al., 1984; Ozawa, 1991). However, ice temperature is not known for Bhutanese glaciers. Figs 6e and 6f in the discussion paper show that ice flow is mostly due to basal sliding in the ablation zone of both Thorthormi and Lugge Glaciers. Therefore, we assumed the glaciers as temperate in the flow model, the influence of this assumption to the modelling is small because our model showed active sliding at the bed of the glaciers. We will add this discussion in the revised manuscript.

L100: the benchmarks for DGPS measurements are indicated in fig 1 (4 green crosses) but there is no benchmark visible on Fig 1 2.5 km from Thorthormi snout. Did you relate benchmarks indicated in Fig 1 to the benchmark obtained with PPP processing?

We will extend a plot area of Fig. 1a further to the western side and add the benchmark at 12.5 km from the terminus of Thorthormi Glacier in the revised manuscript.

L174 details without e

We will change in the revised manuscript.

L200: are the glaciers of this study temperate?

No information is available for thermal condition of the studied glaciers. We assumed the glaciers are temperate because our model showed active sliding at the bed of the glaciers.

L203: what is the elevation at 5100 and 3500 m of the termini of Thorthormi and Lugge glaciers, respectively?

We will add elevations at the termini (Thorthormi: 4442 m, Lugge: 4530 m) and upper most boundary of the model domains (Thorthormi: 4813 m, Lugge: 5244 m) in the revised manuscript.

L207: strange to see the appearance of Fig 6 right after fig1

We agree to your suggestion. We will change citation from Fig. 6 to Fig. 1 in the revised manuscript.

L255-59: not very consistent to say earlier that the inter annual variability is somehow questionable (l129) and then to discuss here this interannual variability! Is it truly significant?

We will remove interannual variability in flow velocity in the revised manuscript.

Fig 4a: it is strange and not very consistent to see the annual glacier outlines crossing each other, as if from one year to the following, some areas of the glacier were expanding while some others were shrinking. This is likely not to be realistic.

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.

Line 268-69: on fig 4b, we observe the opposite, with the northern half retreating less rapidly than the southern half

In this sentence, we focus on the period of 2009–2011. During the period, the northern half retreated more rapidly than the southern half.

L281: given that the uncertainty on the SMB difference between both glaciers is expected to be very high (see general comment), the result "substantial influence of glacier dynamics on ice thickness change" is not supported as long as there is no sensitivity test on the SMB results, or any additional information to validate SMB simulations.

See reply to the major comment #3.

L292-94: I do not agree with the authors when they are mentioning that the agreement between observed and simulated surface velocities are good (fig 6e and f, lines red and blue, respectively). Looking at fig 7f, it is hard to believe that there is no more than 7% difference between observations and simulations: how is it obtained? More importantly, the velocity depends on the bedrock topography, obtained from Farinotti et al (2009). How reliable is it? how sensitive is the bedrock topography on velocity fields?

Difference between observed and calculated surface velocities (7%) in Fig. 6f was obtained by taking mean velocities (observation: 43.19 m a^{-1}, calculation: 40.22 m a^{-1}) over the calculation domain (0–3500 m). We performed sensitivity analysis of ice thickness. See reply to the major comment #4.

L327: "over recent decades" give the exact period to facilitate the comparison with the period 1974-2006.

We will change "in recent decades" to "during the period of 2004–2011" in the revised manuscript.

Table1: I am confused about the periods: dh or dh/dt are obtained during 2004-2011, but SMB are obtained during 2002-04 and simulated dh/dt during 2002-2010. Not all periods match which makes also the comparison not very reliable. Another question regarding SMB in table 1, over which area of the glacier is it calculated?

SMB was recalculated for the period of 1979–2017 (see reply to the major comment #5). We used simulated dh/dt during 2002 to 2010 as for 2004–2011. We also estimated uncertainty of simulated dh/dt from the interannual variability in flow velocity over the observation period (<5.6 m a^{-1}). The SMB in Table 1 covers only the area of GPS measurements. The mean emergence velocity in Table 1 was calculated only in the elevation range covered by the GPS survey. We will add the above explanation in Table 1 in the revised manuscript.

L332-33: Gardelle, Brun and Kaab studies cover more or less the same period i.e. 1999-2001; 2000-2016 and 2003-2008 respectively (with Kaab study being shorter though) and the results are not always significantly different (i.e. Brun and Kaab) so I agree that we can say that the mass loss is intensified since 2000, but only based on the comparison of these 3 studies with Maurer's covering 1974-2006. I also totally agree that this acceleration is potentially not significant as stated lines 327-328

We agree to your suggestion that the mass loss has increased since 2000 based on the studies by Gardelle, Brun, Kaab and this study with Maurer's result. We will change to "Regional mass balances in northern Bhutan have accelerated from the period for 1974–2006 to after 1999. For example, the region-wide mass balance is -0.17 ± 0.05 m w.e. a^{-1} for 1974–2006 (Maurer et al., 2016), -0.22 ± 0.12 m w.e. a^{-1} for 1999–2011 (Gardelle et al., 2013), -0.42 ± 0.20 m w.e. a^{-1} for 2000–2016 (Brun et al., 2017) and -0.52 ± 0.16 m w.e. a^{-1} for 2003–2008 (Kääb et al., 2012)." in the revised manuscript.

L344-47: somehow senseless and not very relevant to compare SMB and thinning rates over disconnected periods (2002-04 and 2004-11, respectively) especially because SMB may have large inter-annual variability.

SMB was recalculated for the period of 1979–2017. Interannual variability of SMB was discussed by comparing long-term (1979–2017) and observed periods (2002–2004) (see reply to the major comment #5).

L355: the emergence velocity obtained from equation 11 is very sensitive to the choice of the surface slope alpha. How is it obtained? From a DEM, which resolution?

Surface elevation was extracted from the ASTER-DEM (15 m resolution) every 100 m along the central flowline of the glacier. This elevation data was filtered with a bandwidth of 1000 m and used for the upper boundary of the flow model. Surface slope α was obtained every 100 m from the surface topography of the model domain. We will add this explanation in the revised manuscript.

L358: negative emergence velocity is submergence velocity?

Yes, the negative emergence velocity indicates submergence velocity. But, we consolidate to use only emergence velocities to avoid misunderstanding.

L374-76: the mismatch between model and observation may have other origins than only the ice thickness or sliding coefficient: other sources of uncertainties may come from SEB computation affecting the model results or interpolation of

DGPS measurements impacting thinning rate observations. A systematic sensitivity analysis is needed. Farther in the text, the authors claim that the SEB uncertainty is 11% based on fig S1b which shows the standard deviation of the thermal resistance. Actually, there are much more sources of uncertainties and the SEB uncertainty is likely much higher. (see general comments)

We performed a sensitivity analysis of each meteorological parameter in the SMB model. See reply to the major comment #3. We discussed spatial representativeness of the rate of elevation change derived from DGPS-DEMs. See reply to the major comment #2.

References:

- Mae, S., H. Wushiki, Y. Ageta and K. Higuchi (1975): Thermal drilling and temperature measurements in Khumbu Glacier, Nepal Himalayas, Seppyo, 37(4), 161–169 (in Japanese with English abstract).
- Ozawa, H. (1991): Thermal regime of a glacier in relation to glacier ice formation. (PhD thesis, Hokkaido University)
- Watanabe, O., S. Takenaka, H. Iida, K. Kamiyama, K.B. Thapa and D.D. Mulmi (1984): First results from Himalayan glacier boring project in 1981–1982. Part I. Stratigraphic analyses of full-depth cores from Yala Glacier, Langtang Himal, Nepal, Bull. Glacier Res., 2, 7–23