The Cryosphere

Discussions

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Interactive comment on "Contrasting thinning patterns between lake- and land-terminating glaciers in the Bhutan Himalaya" *by* Shun Tsutaki et al.

Anonymous Referee #1

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General comments

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 37). 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. 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.

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.

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.

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.

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.

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.

Specific comments

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

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.

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

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)

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.

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

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rate over the debris cover tongue is more accurate than performing sporadic repeated DGPS profiles.

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

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?

L174 details without e

L200: are the glaciers of this study temperate?

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

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

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?

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.

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

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lations.

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?

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

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?

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

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.

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?

L358: negative emergence velocity is submergence velocity?

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)