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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 #3

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The manuscript by Tsutaki et al. presents a comparison of three glaciers in the Bhutan Himalaya. Two of the three glaciers are studied to determine differences in glacier dynamics, retreat and mass wastage between land-terminating and lake-terminating glaciers, and whether the presence of a proglacial lake increases dynamics and ice wastage. To do this the authors: (a) present in situ measurements of surface elevation made using DGPS in 2004 and 2011 and compare them with remotely sensed elevation changes reported in literature; (b) derive glacier surface flow velocities using feature tracking on ASTER satellite imagery; (c) manually delineate retreat of the tongues using Landsat 7 imagery; (d) model surface mass balance of the debris covered glaciers, and (e) present a two-dimensional ice dynamics model and two model

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experiments. The results show that the lake-terminating glacier (Lugge) has considerable higher thinning rates than the land-terminating glacier (Thorthormi), but that this is mainly caused by differences in ice dynamics and not by differences in surface mass balance. A strong emergence is present for Thorthormi due to its longitudinally compressive flow regime that offsets its much more negative surface mass balance, and this is largely absent for Lugge. The manuscript in generally well-written besides some style issues, and the subject is of interest to the readers of the Cryosphere. There are, however, some technical issues and uncertainties with the modelling. At least moderate revisions are required before the manuscript can be published.

Major comments:

The authors present three glaciers in the manuscript: Thorthormi, Lugge and Lugge II. Lugge II was measured using the DGPS, was included in the spaceborne flow velocity measurements and was included in the SMB calculations, and its results are presented in figures 1–3. However, it is not included in the ice dynamics model experiments, is barely discussed in the results and discussion, is not included in the abstract and therefore seems of little significance to the overall story. The authors argue in the introduction that Lugge II is at a different elevation and is therefore difficult to compare to the other two glaciers, but there are many more factors that control the dynamics and mass balance of the glaciers that can complicate the comparison, also between Thorthormi and Lugge, which should be acknowledged. In this light it is also odd that the authors state that the surface mass balance of Thorthormi is 37% more negative than Lugge because it is situated at lower elevation (L360). I would suggest that the authors decide to either remove Lugge II Glacier completely from the manuscript to focus more on a clear comparison of Thorthormi and Lugge, or to consistently include all glaciers in all analyses.

I think more discussion on and comparison with other lake/land terminating glaciers reported in literature is required in the manuscript. This is touched on lightly in the manuscript but needs be more elaborate, especially because at present only two

glaciers are used in draw conclusions and hypothesise on the dynamics of lake/land terminating glaciers. Can the differences in dynamics that are found for these glaciers transfer to others? Why, why not?

Most of the methods deployed by the authors have considerable ranges of uncertainty and those should be addressed and discussed better. Especially the SMB modelling that is largely based on the rather uncertain thermal resistance obtained from ASTER data and a few (risky) assumptions seems to prone to uncertainty. The relatively very large negative SMB of Thorthormi is therefore questionable in my opinion. It has been shown in a number of articles in the last few years that using spaceborne thermal infrared imagery over debris-covered glaciers (e.g. Rounce & McKinney, Foster et al., Mihalcea et al, Gibson et al.) provides opportunities but also numerous difficulties and these should be acknowledged. The accuracy of the '1 m resolution' DEM obtained by IDW interpolation of a seemingly very limited number of moderately well distributed DGPS points (Fig 1a) is also uncertain. Maybe it's spatial variability could be validated/substituted with other DEMs. What about the new High Mountain Asia DEMs available at NSIDC DAAC? I think the manuscript would greatly benefit from a more comprehensive sensitivity analysis (e.g. Monte Carlo) to show the total range of uncertainties affecting the final interpretations.

Line by line comments:

12: Why 'more' than offsets? Rephrase

18: Maybe add Scherler 2011 (10.1038/ngeo1068)

30: There are some more recent papers on this, also by Huss and Hock themselves: 10.1038/s41558-017-0049-x, 10.1038/nature23878, 10.1038/s41558-018-0093-1.

54: 'of most spaceborne DEMs'. DEMs of high resolution stereo satellite imagery (e.g. Pleiades), LIDAR and UAVs are remote sensing methods perfectly capable of deriving several metres (even sub-metre) elevation change

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59-66: The aim of the paper is not entirely clear to me from the introduction. This paragraph now is more of a methods summary. Consider changing it to clearly convey the research aim and question.

69: Is the glacier area measurement really accurate to 0.01 km2?. Same for other glaciers.

75: Use separate paragraphs per glacier to improve readability.

77: moraine-dammed

75: No space between '∼' and the number. Throughout.

86: 'were carried out on/around' -> 'were performed for'

92: These are surface flow velocities, right, not integrated over the vertical? Rephrase into something like 'surface flow velocity of Thorthormi'.

101: I've never seen full web URLs in a body of a paper. Use a reference entry for the website in the bibliography instead.

104: Are the elevation variations caused by a person carrying the pack on a debris-covered glacier really only 10 cm? How were these estimated?

106: Mentioning UTM is not quite relevant.

107: Why this 1 m resolution?

122: remove 'the' after calculated.

125-126: There is no info whatsoever on the accuracy of the orthorectification of the ASTER images? Could these maybe be retrieved? This can be quite an issue in steep mountainous regions.

131: Why did you select the statistical correlation mode. I was under the impression that the mode that works on the frequency domain is better and is better suited detect subpixel displacements. Please elaborate on the choice.

134: So no filtering was applied using the signal to noise ratio statistics that are provided by COSI-Corr?

137: The SLC-off gaps in the ETM+ imagery did not provide an issue in the analysis? This should at least be mentioned.

139: 'that possessed the' -> 'with'

141: Use of QGIS in particular is not relevation and, again, the weburl is unnecessary. Just state that delineates were performed in a geographical information system.

143: So there is no user-induced accuracy error?

162-163: These are quite bold assumptions and this should be acknowledged.

260: Preferably also show the off-glacier displacements in figure 3a.

273: Is does not appear that heterogeneous to me. Especially since the actual heterogeneity is likely much higher given the hummocky surface of most debris-covered glaciers. I understand that this is not captured by the ASTER data, and that this is the variability that is obtained, but it should be reworded.

330-333: The authors speak of accelerating mass losses, but the numbers and accompanying year ranges do not show this per se.

341-342: Please elaborate.

344-365: I found this section rather confusing. There are methods and results presented in the discussion section. I strongly suggest relocating this to the appropriate sections.

360: This is not due to differences in debris cover and debris thickness?

375: A difference of 5 ma-1 is a lot. As suggested in the main comments, I think a comprehensive sensitive analysis would be a great addition to the paper and could help to support the conclusions.

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