The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-38-RC4, 2018

Interactive comment on "Can ice-cliffs explain the "debris-cover anomaly"? New insights from Changri Nup Glacier, Nepal, Central Himalaya" *by* Fanny Brun et al.

Anonymous Referee #4

Received and published: 3 May 2018

Summary:

The authors estimate the total ablation associated to supraglacial ice cliff melt on the debris-covered tongue of Changri Nup Glacier, Nepalese Himalaya, based on high-resolution topographical data. They use terrestrial photogrammetry surveys on selected cliffs for validation and UAV- and satellite-imagery for the entire cliff population on that glacier for two consecutive years. They then derive the contribution of ice cliff melt relative to glacier melt on the tongue of Changri Nup Glacier by taking into account emergence velocity and estimate ice cliff melt to be ~3 times higher than the average melt of the glacier tongue. They conclude that ice cliffs cannot explain the debris-cover

anomaly and that the anomaly in turn could be a result of lower emergence velocities and reduced ablation.

General comments:

The main outcome of this study is that UAV- and especially high-resolution satellite imagery can be used to estimate glacier-wide volume losses associated to ice cliff melt, as the authors showed by a sophisticated analysis of various topographic datasets. The second important conclusion of the study is the fact that emergence velocities have so far not been considered carefully enough in terms of glacier ablation estimates and should be investigated further. However, as much as I appreciate the topic of the paper including all its careful analysis, I think the authors' main conclusion of explaining the debris-cover anomaly with reduced emergence velocities in general is not appropriate and a bit out of the context of the paper, given the limited sample size of just one glacier tongue. I suggest the authors adapt the title accordingly and focus more on the nice outcome of the cliff volume loss estimates at the glacier scale, especially in the discussion of the manuscript. Further, I think the balance between ice cliffs and emergence velocity is not given, as the main part of the paper regarding methods description and results, is mostly about ice cliffs and in contrast the discussion/conclusion part is mostly about emergence velocities. The processing of the ice cliff data is well described in general and the results are elaborated carefully by taking into account uncertainties. I miss a better description of one of the key improvements compared to an earlier study, the correction for distortion. Further, I am not convinced that the calculation of the p-factor is feasible or should be done in a different way. All in all my impression of the paper in terms of quality, writing, and relevance in the context of the actual literature is good and it fits into the scope of The Cryosphere, but I have major comments that should be addressed. If these are addressed, I am sure the paper will be a useful contribution to the glaciological community. See further comments below.

Main issues:

1) Explanation of "debris-cover anomaly"

I like that you bring the emergence velocity component into the focus of the analysis and interpretation of debris-covered glaciers. It is clear that this upward movement of parts of the glacier tongue has been neglected so far in most of the studies related to debris-covered glaciers especially. In the case of Changri Nup Glacier the emergence velocity is, based on your observations, very small and thus similar downwasting rates of debris-free and debris-covered glacier surfaces might be explained by this differential emergence. However, I am not convinced by the reasoning of the authors to generalize this result and explain the debris-cover anomaly solely by the confusion of glacier elevation changes and net ablation. It is not clear if Changri Nup Glacier has ablation rates similar to that of debris-free glaciers (of the same elevation range) at all, but this would be the case for the debris-cover anomaly. For sure emergence is an important component and might explain many issues related to this topic. But I think, just based on one single glacier making a general conclusion is too ambitious and therefore the title of the manuscript should be adapted accordingly, away from the debris-cover anomaly more towards the volume loss of the ice cliffs. The study still presents interesting points and provides nice results, such as: estimation of the volume loss associated to ice cliff melt at the glacier scale; incorporating rotational behavior of ice cliffs in volume loss estimates; sophisticated comparison to net ablation by taking into account also glacier emergence.

2) Calculation of p

I am not sure if your calculation of the p ratio makes much sense. Like this you compare ice cliff melt to the melt of the entire debris-covered glacier tongue. This means you compare ice cliff melt to a mixture of subdebris- and ice cliff melt (the glacier tongue). Wouldn't it be more feasible to compare ice cliff melt to subdebris melt (i.e, exclude ice cliff areas)? In order to be comparable to the previous studies you cited, you should check if they also compared ice cliff melt to the ice cliff-subdebris mixture or to subdebris melt only. Also, it is not clear how many ice cliffs you detected on the entire

debris-covered glacier tongue in both seasons, does this number vary? Did you observe the formation of new features over time or disappearance of them? This might also affect the p ratio, in case the cliff areas are not excluded in its calculation. Additionally, could you derive melt rates for the cliff surface (perpendicular to their surface) instead of pure elevation change rates? This would be helpful in order to compare your results to previous studies.

3) Correction with local field of displacement

From the text I cannot follow how the rotational component of the ice cliffs/glacier surface are implemented in the calculations of the ice cliff volume changes (Section. 4.2). Since this is a main improvement of this study compared to an earlier one using a similar method, much more emphasis should be given to explain this implementation. How do you use the local field of displacement? Where does the 3D flow field that you use for the correction come from? This is a key comment that should be addressed for the paper clarity. A schematic figure would be helpful. Also, can you estimate how much the difference is compared to the previous method used in Brun et al. 2016? More importantly, can you validate the new method to show that it is appropriate and sound? From the short description provided: i) the method cannot be reproduced; ii) there is no evidence that it is the correct approach. You should discuss this new method in the discussion part of the manuscript, as it seems to be a clear advancement from previous studies, where a simple homogeneous direction of displacement was assumed.

4) Uncertainty of emergence velocity assumptions

As mentioned above in the general comments, the balance between ice cliff associated melt estimates and emergence velocity assumptions is not well elaborated in the manuscript. E.g. I miss a more in depth discussion of the uncertainties related to the calculation of the emergence velocities in the methods section, such as the one associated with having only one cross section profile for glacier thickness estimates, or general uncertainties in ice flux assumptions (glacier thickness measurements, bed topography, subglacial conditions, distribution of emergence etc.). As stated in line 19, p. 10, "the main source of uncertainty on the cliff volume change is the uncertainty on the emergence velocity", but this is not discussed later on in the text. Also, the reduction in emergence velocity of ∼20% compared to the period 2010-2015 (line 16, p. 6) is striking, isn't it? Can you try to explain it more convincingly? If glacier emergence is so (relatively) variable, this might also have implications on your assumption of generally explaining the debris-cover anomaly by lower emergence velocities.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-38, 2018.