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The Cryosphere
Discussions

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 #1

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Brun et al combine several state-of-the-art observational datasets with a novel correction for glacier dynamics (based on unique field observations) to measure volume losses due to bare ice cliffs exposed on Changri Nup Glacier in Nepal. This is an important question, as recent studies have suggested that ice cliffs play an important role in bringing the thinning rates of debris covered glaciers to parity with those of clean ice glaciers (unexpectedly). The study finds that ice cliffs indeed account for a disproportionate amount of mass loss in the debris-covered ablation area of Changri Nup, but that emergence velocity has been neglected in assessments of the 'debris covered glacier anomaly'.

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Discussion paper

I am impressed with the careful processing of the field and remote-sensing observations, in particular with the correction of point clouds for glacier flow and the treatment of uncertainty in general, and I find this study to be an excellent combination of high-resolution topographic datasets and robust processing to measure changes of highly dynamic features. I am particularly pleased to see attention given to emergence velocity, an aspect of glacier dynamics and mass balance that is often neglected in contemporary studies due to the recent emphasis on remote sensing observations. I have concern with the strength of the authors' refutation of the 'debris covered glacier anomaly' based on observations from a single glacier; I rather think they have highlighted the (largely unacknowledged) importance of emergence velocity, but have not demonstrated that this is the dominant or general mechanism by which debris covered glaciers thin at rates comparable to clean ice glaciers in High Mountain Asia. I suggest the authors consider some textual revision in order to better balance the focus of their discussion and conclusions with the focus of their highly sophisticated processing.

Major points:

1. The manuscript is not balanced in terms of the focus of its methods, results, and discussion. The manuscript is mostly aimed at assessing the contribution of ice cliffs to mass balance; the gold-standard methods are targeted specifically to assess this using multiple (perhaps redundant) high-resolution datasets, yet once the authors have a number for the ice cliff net ablation, the discussion is nearly all about the importance of emergence velocity. This feels like an afterthought (i.e. determination of emergence velocity itself is not given much attention in the background and methods, but this is the main topic in the discussion, whereas ice cliffs received little attention); this disparity is awkward. In particular, additional attention needs to be paid to the uncertainty in both the original emergence velocity dataset (per Vincent et al 2016) and particularly with respect to the 'updated' estimate. For example, what about uncertainties in ice thickness retrieval and differences in emergence velocity due to profile orientation? What about the uncertainty of thermal regime and its effect on column-averaged ice velocity?

If emergence velocity is to be a major outcome of the manuscript, its uncertainty needs to be more carefully assessed.

2. I think some adjustment to the title and latter discussion is necessary: I do not think the authors are able to answer the title question using data from Changri Nup alone. First, the authors provide no evidence that Changri Nup fits within the 'debris cover anomaly' framework (that Changri Nup is thinning at a comparable rate to debris-free ice at a similar elevation). This is partly due to the hypsometric differences of debris-covered and debris-free ice in the Solukhumbu region, but this is largely why the debris-cover anomaly has been determined from numerable populations of glaciers, which will exhibit a variety of hypsometric distributions. It could be possible to assess the thinning rates (and melt rates) just below the GPR transect, where debris and ice surfaces exist at the same elevations – does Changri Nup actually show evidence of comparable thinning rates for debris and ice? However, I am doubtful that this would be satisfactory, as Vincent et al (2016) has already demonstrated that melt rates at Changri Nup would be very different beneath debris and clean ice; it seems that the hypsometric parity of thinning rates for debris-covered and debris-free ice does not hold for this particular location, but for larger regions.

Put differently, there is circular logic at play – it is already known that subdebris melt rates are not equivalent to clean-ice melt rates at this location, so no amount of ice cliff melt could bring the subdebris mass balance to the same level. A way forward is to emphasize that both processes are important: neglecting emergence velocity, one does underestimate melt rates, but similarly one does if neglecting ice cliffs. However, emergence velocity has been neglected, and the Changri Nup data is the first field data to demonstrate the effects theorized by Banerjee (2018). Thus, a meaningful question is how much are the competing hypotheses responsible for boosting the thinning rate of debris-covered glaciers? I.e. how much of a boost in lowering is due to cliffs vs how much is because of emergence velocity? Or, how much 'additional' melt would be needed from cliffs to lead to thinning (or b_dot) -equivalence? Twice as much? Three times? Can you guess how much ablation ponds are responsible for (realising that this is just part of your non-cliff net ablation, and does not affect the role of emergence velocity)?

Minor points

Some nomenclature formality is needed for the cliff area terms. Variably through the manuscript there are ‘planar’ (cliffs are often considered inclined planes, so this is confusing), ‘2D’ and ‘3D’ areas of cliffs. Please clarify this early on in the manuscript, and ensure consistency.

P1 L20. Suggest ‘have been found’ in place of ‘were found’ for correct tense

P2 L5-11. It may be useful to use the same order for the hypotheses here as for the rest of the text, e.g. you first discuss how to test the cliff hypothesis before considering the role of emergence velocity.

P2 L6-8. This is the thesis of this paper (that emergence velocity is a major player), which it supports very well. Here, however, this is an hypothesis – that differences in emergence velocity ‘can/could lead to comparable thinning rates despite differences in surface ablation. The two studies referenced are hypothetical, idealised flow-models.

P2 L10. This seems to refer to surface ablation only, yet Sakai et al 2000, Miles et al 2016, and Watson et al 2017 (ESPL) also indicate that ponds could potentially lead to significant internal ablation (which would also contribute to lowering as in Thompson et al 2016).

P2 L12. It follows that you also need to determine the melt contribution of supraglacial ponds in order to resolve this

P2 L17. In the formulation of Equation 1, does the ‘tongue’ area include or exclude the ice cliff areas? That is, does p compare ice-cliff ablation to the overall surface mass balance, or to the non-cliff ablation? Is this consistent between the studies mentioned?

P2 L24. Please also mention the source data and method for Brun et al 2016 if you are going to for Thompson et al 2016.

P2 L28. Suggest 'positive emergence velocities will increase the ...' as it is more concrete than 'affect'

P3 L5-10. It is necessary to make some mention of your emergence velocity correction in this paragraph.

P3 L28. 'GCPs' should be singular or possessive here.

P4 L15. 'equal' should be 'equivalent'

P5 L3. Incomplete sentence. 'This ensured our study/our analysis to...'

P6 L9. I don't believe the accuracy of this cross-sectional area. The uncertainty with respect to radar velocity in ice alone is greater than the stated value. The stated uncertainty equates to 10cm of uncertainty in ice thickness all along the cross section. Please ensure that your corrected uncertainty is propagated to your uncertainty in emergence velocity as well.

P6 L19. Constant and equal over the lower glacier for both periods of study, you mean. As the flux gate method can only give you a mean emergence velocity for the lower glacier, but please mention how it is expected to vary in space, and how this might affect your results for ice cliffs and for the whole glacier.

P9 L2. Your kernel sizes are with units of pixels, correct?

P11 L10. Can you calculate or estimate the 3D area of these cliffs in order to calculate a mean backwasting rate for comparison to other studies? As the rate of elevation change over a cliff-affected area is heavily influenced by, e.g. their height and slope, the backwasting rate is perhaps easier to compare between studies (or indeed between years, as your 2016-2017 data is quite different).

P11 L18-19. For p, it makes sense to me that the comparison would be cliff area to non-cliff area, rather than cliff area to the whole area. Please check what prior studies have used for this calculation.

P11 L25. Why the much higher melt rates in 2016-2017?

P12 L8. 'Mean tongue' is not a sensible term. Consider 'relative to the whole tongue'

P12 L10-18. Neglecting the emergence velocity, what portion of the glacier's total ablation would be accounted for by ice cliff melt? Perhaps it would likewise be useful to compare the area-averaged losses due to ice cliffs and emergence velocity – are they of comparable magnitude?

P12 L19. Consider 'the' debris-cover anomaly

P12 L22. This emphasizes the problem with your p calculation – it is not comparing ice cliff to debris, but ice cliff to drbis-and-cliff mixtures. Your values of p will increase with this correction. I.e. total melt due to cliffs was 440000m3 for 2015-2016, and they covered an area of 113000m2. Total melt for the whole glacier was 1,918,000m3 over an area of 1.49 km2. Thus the non-cliff melt was 1478000m3 over an area of 1.377km2. And thus p is 3.6 (20% higher). Can you also calculate what p would be neglecting your emergence velocity estimation (for comparison to the studies mentioned?

P12 L29. This is a very good point, but highlights a key difficulty for the paper. The authors have not demonstrated that the 'debris-cover anomaly' is applicable to Changri Nup at all! That is to say – the authors have not demonstrated that Changri Nup's debris-covered area is indeed thinning at a rate comparable to clean ice glaciers at the same elevation (the point of the debris-cover anomaly). Vincent et al 2016 has already demonstrated that the surface mass balance of Changri Nup is lower than it would be if debris were not present. Here you demonstrate that ice cliffs cannot bring the debris area's mass balance to the same level, but does Changri Nup even fit the debris-cover anomaly in the first place? This is not so problematic for your analyses and paper, but for the generalisation of your results to other areas (P13 L1-2 especially)

P13 L4. I think this section needs to be tidied up with respect to nomenclature, in particular replace ‘tongue’ with ‘ablation area’.

P13 L8. This hypothetical analysis is very worthwhile, but as stated in the text, ‘has already been shown by Banerjee (2018)’. Please properly reference that study early in this section (you can state that you provide the first field evidence supporting this hypothesis) and reduce this text accordingly. I recommend that you expand the discussion of the responsibility of reduced emergence velocity vs enhanced ablation (how important are cliffs and ponds for mass balance, then?) or consider more fully how the mass balance and emergence velocity (thus thinning rates) of both systems will continue to evolve. Is the apparent parity of thinning rates a temporary feature in this evolution, or should we expect this to perpetuate?

P13 L22-23. The manuscript has demonstrated that emergence velocities (and the difference between emergence velocity for clean-ice and debris-covered areas) are a key part of the debris-cover anomaly, but the manuscript has certainly not demonstrated that this is always (or even generally!) the reason for the thinning rate parity. Consequently I respectfully but strongly think that your statement should be modified, e.g. ‘In conclusion, we have demonstrated that emergence velocity differences are as important as ice cliffs and supraglacial ponds in the calculation of melt rates for debris-covered glaciers, and that the ‘debris cover anomaly’ is in part due to the confusion of thinning rates and net ablation.’

P13 L24. This section is very out of place with regards to the underlying theme of the manuscript, especially as your discussion up until now focuses on cliffs not being important. I suggest as a segue to emphasize that melt rates are substantially higher than without ice cliffs, and that the primary analysis of the study is thus of benefit for modelling studies (otherwise why automatically delineate cliffs at all?).

P14 L6. Please include a mention of where Brun et al (2016) falls in this spectrum.

P14 L7-10. This is an important consideration that should be expanded upon. Your analysis including flow correction is without a doubt more sophisticated and 'correct' than prior efforts, but it extremely limited in its transferability because of the field data requirements. While emergence velocity is clearly an important and neglected aspect of studies addressing debris-covered glacier mass balance, it is extremely difficult to assess (and thus also the reliance on overall thinning rates rather than mass balance). It is not enough to say 'more data would be helpful' when you advocate abandonment of an entire train of thought; rather, I think it is important to acknowledge why such data do not already exist (why debris thickness has prevented widespread ice thickness measurement through debris), and to address alternative methods of assessing emergence velocity (e.g. networks of ablation stakes combined with dGPS).

P14 L24. There is no discussion of this point, but I think it would be useful to expand upon (briefly). What do we do with your results? How does this affect models of debris-covered glacier mass balance and/or dynamics?

Figure 6. These are normalised change in the volume change (rather than cliff volume), correct?

Figure 10. I like the simplicity of Figure 10, but it is deceptive in its simplicity (the scales are of course arbitrary). It would be worthwhile to emphasize that this is one hypothetical transient state (another would be to double b_dot for debris free glaciers, the end-member with no increase in w_e in either case). It also would be worthwhile to highlight here the fraction of b_dot due to ice cliffs (the focus of the study), and to emphasize that w_e is the least measured aspect of the chart.

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