

## ***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 #3**

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#### — SUMMARY —

Brun et al. present an analysis aiming at determining the actual role of ice cliffs in the so-called “debris-cover anomaly”. The analysis is performed on Changri Nup Glacier, Everest region, and combines both in-situ and remote-sensing data for a two-year period.

The results are bold: The debris-cover anomaly - the authors say - does not exist, but is the result of confusion around the concepts of “thinning rates” and “net ablation”. The “anomaly” – the authors explain – only comes to happen because past studies failed to account for emergence velocities. In reality, the similar thinning rates observed for

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debris-covered and debris-free glaciers are a result of the difference between net ablation and ice flow emergence being coincidentally similar for the two types of glaciers.

As much as I agree with the first part of the interpretation, I do believe that the second part is too weakly backed-up: The authors try to generalize their field-based results in the discussion section, but the result is not convincing. Some general statements (e.g. debris covered glacier have smaller accumulation-area-ratio and are generally smaller) should be better corroborated (inventories for doing that exist by now).

A part from the above, the manuscript has a very high standard: The topic is of high relevance and actuality, the introduction is well written, the text is easy to follow, the technical analysis is clearly done by experts, the relevant literature is cited in an exemplarily manner, and the figures are illustrative.

I am fully convinced that the manuscript will be an important addition to the glaciological literature once it is revised.

#### — GENERAL COMMENTS —

##### 1) Sample size

The fact that all claims are built on one glacier (sample size = 1) is a clear handicap for the general conclusions the authors are aiming at. For making the point that much of the debris-cover anomaly is due to a confusion of concepts, the sample size is not an issue. This can be shown with one single example, and this is really the paper's merit. Where it becomes more difficult is when the authors start arguing that “[for debris-covered glaciers,] the combination of reduced emergence velocities and lower ablation coincidentally sum up to similar thinning rates as [for] debris-free glaciers” (p. 13, L. 19-20). Arguing for a regional-scale “coincidence” seems at least adventurous with the single data-point at hand.

I see two ways of solving this: Either (1) the authors try to get hold of published data that exist for other glaciers, or (2) they refine their theoretical argumentation (Sec.

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6.3) and back up some of the as-yet little-supported claims (see next comment) with remote-sensing and inventory data.

## 2) “Theoretical considerations”

The “theoretical considerations” presented in Sec. 6.3 is the part of the manuscript that I found the least convincing. Unfortunately, it is the crucial one.

The problem is that the arguments seem to be much based on qualitative considerations, whilst the author’s point is much focused on quantitative statements. Two examples: (1) “debris-covered glaciers have lower accumulation-area ratios than debris-free glaciers” (p. 13 L. 9). The claim is backed up with a reference (Scherler et al., 2011) but it would be ways more convincing to have some actual numbers corroborating this. These can either be re-presented from the original publication or re-compiled from inventory data and remote sensing products (I’m fully aware that the second option would be much more work-intensive). Ideally, a distribution of AARs would be shown for both debris-free and debris-covered glaciers, and the difference quantified. (2) “the glacier response time of a debris covered glacier is longer compared with a debris-free glacier (Rowan et al., 2015), therefore the clean tongue will shrink faster than the debris-covered tongue, further enhancing the difference between ADC and ADF.” (p. 13 L. 15-16). Well, again, although a reference is given, it would be so much more convincing having two distributions shown, and a difference quantified. In this case, however, I’m not even sure whether this is necessary since what actually would matter are the present, actual sizes of the debris-free and debris-covered tongues – and not their response times.

I think that fixing this section is the only major work that is in front of the authors.

## 3) Introduction

A rather minor issue: The concept of “emergence velocity” is, obviously, of central importance to the paper. Since one of the main conclusions is that there is confusion

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around the term, I think it would make much sense to provide a clear definition in the introduction. Some indication on how the quantity is typically calculated from field data (or other types of data) may also be helpful for the one or the other reader.

#### — LINE-BY-LINE COMMENTS —

What follows is a series of line-by-line comments of various nature, ranging from comprehension questions to stylistic corrections and including some specific suggestions for issues the authors may want to think about or change.

P.2 L.14: Maybe a detail but I see a danger of the “p” being referred to as “p-value” at some stage. This would obviously be extremely misleading, since the term is reserved for something very specific in statistics.

P.2 L.15: I was confused by the mixture for plurals (“cliffs”) an singulars (“cliff”). At that stage, I even briefly asked myself if “p” was something defined at the cliff-scale (i.e. one “p” for every cliff). Please avoid the confusion by using consistent wording.

P.3, L15: Remove “in the same outline” (there is no danger of misunderstanding that)

P.4, L.7: “using a [not “the”] Structure from Motion algorithm”

P.5, L.13-14: I was wondering whether the relatively large offset determined for stable terrain (-7 or so meters) requires a short comment/explanation?

P.5, L.30: “The velocities measured with Pléiades match well with the field data” → I may have missed it, but I don’t think any in-situ velocity measurements were described so far? (The only reference to such measurements seems to be at P.3 L.14, but I understood that info only to be a side note on how the glacier outline was derived in another publication?)

P.6, L15-16: Can a word be spent in discussing the implication of assuming a homogeneous  $w_e$ ? That quantity is a distributed field, and I have the impression that assuming a similar  $w_e$  for all ice-cliffs that are considered is an important assumption?

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Some discussion is found later, but here is where the question arises

P.6, L.26: I'm not sure to understand what "deformed" means in this case. "Deformed" how?

P.7, L.19-20. I found the concept of analogous points somewhat abstract. Would it make sense to provide a figure with a visual example?

P.8, L.2-3: Also in this case, a visualization would probably make it easier to understand what is meant exactly.

P.9, L.26-27: "We experimentally determined  $L = 150$  m for the UAV and  $L = 150$  m for the Pléiades data" → "We experimentally determined  $L = 150$  m for both the UAV and Pleiades data" (or should one of the two "150" read something else?)

P.11, L.9: The unit of  $-3.88 \pm 0.27$  is missing

P.11, L.10: Here and elsewhere: In light of the estimated uncertainty, it would make sense to state  $440 \pm 54 \times 10^3$  m<sup>3</sup>/a (instead of  $439\,689 \pm 54\,000$  m<sup>3</sup>/a).

P.11, L.26: I'm not following: Is the stated value ( $1.51 \pm 0.21$  m/a) already corrected for emergence?

P.12, L.7-8: The last part of the sentence is rather involved. Can't you simply say that the cliffs seem to contribute a constant share to the total ablation?

P.12, L.14: Here and below: Consider replacing "original" with "actual".

P.12, L.15: Remove "Doing the same".

P.12, L.17: Replace "emergence velocity" with "ice flow emergence" (saying "the influence of velocity" sounds somewhat odd).

P.12, L.24-25 An alternative (simpler?) wording would be "Since ice cliffs typically cover a very limited area, thus, it is unlikely that they can explain the debris-cover anomaly."

P.12, L.27: Check the wording: "englacial hydrology" is not an "ablation-related pro-

cesses" (it's rather a "discipline", as e.g. glaciology)

P.12, L.28, sentence starting with ".Yet this does not. . ." → Split the sentence somewhere; it is very long.

P.13, L.1-23: This is the part that really needs revision.

P.13, L.7: The unit of "density" should be kg/m<sup>3</sup> (not m<sup>2</sup>)

P.13,L.10: Well, the comparison is somewhat "cheated", as it should certainly include areas of the same size.

P.13, L.12: Not entirely sure what "both variables" is referring to. To mass balance and emergence velocity?

P.13, L.20: If I read this correctly, you imply that the similar thinning rates for debris-free and debris-covered glaciers are only observed now, and that this was different in the past and will be different in the future. Is this correct? If so, state that explicitly.

P.13, L.30-31: I don't understand the sentence. Especially the two "in" within the parenthesis create confusion.

P.13, L.32: Why "nevertheless"? What's the logical link to the previous sentence?

P.14, L.2: "i.e. the p factor defined in this study" should live in a parenthesis (the sentence is difficult to understand at the moment). Also try to split the sentence as it is very long.

P.14, L.5: "models [. . .] are not directly comparable with the observations" → Explain (or at least give a hint) why not.

P.14, L.5: Not sure to understand what you mean with "flow components". Please clarify. In light of the claims provided above, moreover, I'm not sure to understand the latter part of the sentence (the one that "advocates for a more consistent framework"). This may be clearer after revision.

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P.14, L.10: As much as I agree with the statement, I don't think it is appropriate making it "yours": Geophysicists are working on that since years, after all.

P.14, L.16: "or englacial conduits" → That's a very speculative claim, isn't it? I would suggest to flag it as such.

P.14, L.18: "we hypothesize that" → In the last section (Sec. 6) the claims were stated in a much more decided way. Why this caution here? The text should be coherent in what the level of trust in the results is concerned.

P.14, L.18-19: "the debris-cover anomaly could be a result of lower emergence velocities and reduced ablation" → This is basically the main claim of the paper. Whether it is suitable of having it in the conclusion section or not very much depends on how convincing Sec. 6 will be after revision.

P.14, L.22: "...our suggested framework would inform estimates of ice cliff ablation..." → not sure to understand how "inform" is used here. Can you reword?

P.14, L.24: "[... it] is required to include these results into debris-covered glacier mass balance models" → I'm not entirely sure but it looks like you advocate for mass balance models to include a "p"-factor? If this is actually your message, please be more explicit in saying that.

P.14, L.28-29: If what I understood the message correctly, the sentence could be adjusted to "Two research directions could be (a) to extensively measure ice thicknesses and (b) to install networks of stake measurements to assess the spatial variability of ice flow emergence."

#### — COMMENTS TO FIGURES —

Fig. 1: (a) The red box in the upper-right inset is misleading, since it is not the part enlarged in the main figure. Consider replacing it with a red dot or similar. (b) Last parenthesis of the caption: Why "measured"? (The word can simply be removed.)

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Fig. 2: Please tell what coordinates are used. The black line is the same as the red-dashed one in Fig. 1, I guess? (Readers shouldn't be guessing ;-)

Fig. 3: (a) In the caption, spell out what  $U_s$ ,  $w_s$ , etc are. (b) "Local slope" is misleading; it looks more like the local tangent of the surface (and my guess is that " $\alpha$  = local slope").

Fig. 4+5: (a) Can the colour-bar be stretched and more values be added? At the moment, it is difficult to tell what colour corresponds to, e.g. -2.5 m. (b) What is the meaning of "raw" in "raw elevation change"? (c) Please indicate the time span between the UAV surveys (or the dates of the surveys as such). (d) "from flow for" → Do you mean "for iceflow from"? (e) For consistency, the last sentence should read "Zoom in the dashed rectangle of panel a (c,d)".

Fig. 6: (a) Please (re-) state what "normalized" means in this case. (b) State the period over which the changes refer to. (c) "In the latter" → "In panel b" (d) "because it is more than 150%" → "(since the value is >150%)"

Fig. 7: State the period over which the changes refer to.

Fig. 8: The red markers should be crosses (area), and not dots (volume).

Fig. 10: (a) Please simplify the third sentence (the one starting with "In the transient state, ..."). As far as I understand, it simply means that all but the blue  $w_e$  are taken from Vincent et al (2016)? (b) "ratio of net ablation" → Not sure to understand that. A "ratio" is between something and something else, I would say.

Tab. 1: (a) " $A_{SD}$ " never shows up in the table. Thus, no need of introducing the symbol. (b) "The main aspects" → Why plural? If there is a share of aspects implied in what the table shows, please state that.

Tab. 2: (a) I don't understand the meaning of the reference. Just remove? (b) Please explain in the caption what "virtual" means. (c) In the caption, provide a hint for why some values are "N/A".

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Tab. 3: What is “B/H”? The caption should tell.

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