Response to anonymous referee 3

We thank anonymous referee 3 for the detailed review. In this response, the reviewer's comments are in black standard font. Our response is in standard blue font and the modifications to the manuscript are in blue bold font.

— 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 in 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 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.
We thank the anonymous referee 3 for her/his positive appreciation of our work. We understand his/her concern about the disproportion between the strength of our conclusion and the weakness of the theoretical arguments we advanced. We addressed this concern by revising a large part of the discussion (see details in our response to the general comments below)

— 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 stat arguing that "[for debris-covered glaciers,] the combination of reduced emergence velocities and lower ablation coincidently 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. 6.3) and back up some of the as-yet little-supported claims (see next comment) with remote-sensing and inventory data.
We thank the reviewer for the two suggestions and respond to his/her comment together with the next comment.

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.

We agree that some claims in this section 6.3 were supported only with qualitative considerations, preventing our conclusions from being convincing. We re-wrote a large part of this section 6.3 (see below) being more cautious and following the reviewer's suggestions, we also backed up our arguments with quantitative statements.
A distribution of AARs for debris-covered and debris-free glaciers could not be shown because ELAs are not available everywhere with enough accuracy. However, to quantify the fact that debris-covered tongues are most of the time larger than debris-free ones, we plotted the minimum elevation as a function of debris cover percentage for all glaciers in HMA larger than 2 km^2 (approx. 6500 glaciers) (new fig. 10, added in the revised manuscript). We can see that the larger the percentage of coverage by debris, the lower the glaciers flow, which is an indication that debris-covered glaciers have on average a larger ablation area than the debris-free glaciers. Concerning the response time, we agree that this was not backed up, and not really necessary in our analysis, and in turn it has been removed.

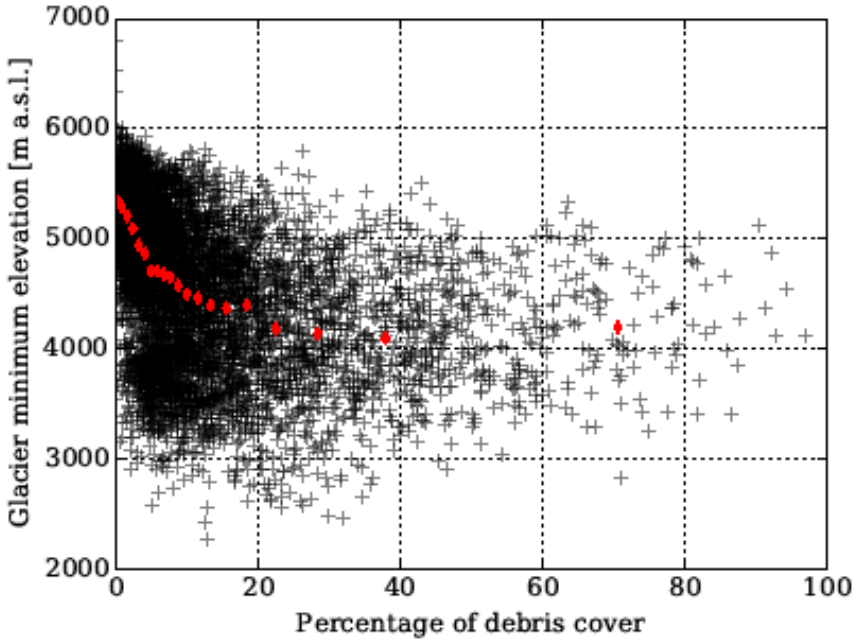

Figure 10 - Glacier minimum elevation as a function of the percentage of debris cover for the glaciers larger than 2 km^2 in High Mountain Asia (6571 glaciers in total). The black crosses represent individual glaciers and the red diamonds shows the mean of the glacier minimum elevation. For instance, the first diamond represents the mean of the glacier minimum elevation for glaciers with a percentage of debris cover between 0 (minimum) and 0.51 (5th percentile).

Revised section 6.3.:
"6.3 Ice cliff ablation and the debris-cover anomaly
Between November 2011 and November 2015, Vincent et al. (2016) quantified the reduction of area-averaged net ablation over the glacier tongue due to debris-cover. They obtained a tongue-wide net ablation of -1.2 m w.e. a^{-1} and -3.0 m w.e. a^{-1} with and without debris, respectively. As ice cliffs ablate **at -3.5 m w.e. a^{-1}, ~3.6** times faster than **the non-cliff terrain of the debris-covered tongue for the period November 2015-November 2016**, and ~1.2 times faster than the tongue if it was entirely debris-free, **approximately** 75 \% of the tongue would have to be covered by ice cliffs to compensate for the lower ablation rate under debris and to achieve the same overall ablation rate as a clean ice glacier under similar conditions. **Since ice cliffs typically cover a very limited area (Herreid and Pellicciotti, 2018), it is unlikely that they can enhance the ablation of debris-covered tongues enough to reach the level of ablation of ice-free tongues.**

Other ablation-related processes such as supra-glacial ponds (Miles et al., 2016) or englacial **ablation** (Benn et al., 2012) may contribute to higher **ablation** rates than what can be expected on the basis of the Østrem curve. Yet this does not apply to the case of Changri Nup Glacier, as Vincent et al. (2016) already showed that the debris part as a whole is responsible for a significant reduction of ablation. As a consequence, **and based on this case study, we hypothesize** that the reason for similar thinning rates over debris-covered and debris-free areas, i.e. the "debris-cover anomaly" is largely related to a combination of surface mass balance change and dynamics.

This hypothesis currently applies to the Changri Nup Glacier tongue only, and it is unclear if it can be extended to the debris cover anomaly identified at larger scales. The high quality data available for Changri Nup Glacier are not available for other glaciers at the moment, and consequently we provide a theoretical discussion below.

The mass conservation equation (e.g., Cuffey and Paterson, 2010) gives the link between thinning rate ($\overline{\frac{\partial h}{\partial t}}$ in m a^{-1}), ablation rate and emergence velocity for a glacier tongue:

$$\overline{\frac{\partial h}{\partial t}} = -\frac{1}{\rho}\dot{b} + \frac{\Phi}{A}$$

where Φ (m^3 a^{-1}) is the ice flux entering in the tongue of area A (m^2), ρ is the ice density (kg m^{-3}), and $\dot{b}$ is the area-averaged tongue net ablation (kg m^{-2} a^{-1}). **Consider two glaciers with tongues that are either debris-covered (case 1- referred hereafter as "DC") or debris-free (case 2 – referred hereafter as "DF"), and similar ice fluxes entering at the ELA i.e., $\Phi_{DC} = \Phi_{DF}$. The ice flux at the ELA is expected to be driven by accumulation processes, and consequently it is reasonable to assume similarity for both debris-covered and debris-free glaciers. There is a clear link between the glacier tongue area and its mean emergence velocity: the larger the tongue, the lower the emergence velocity. These theoretical considerations have been developed by Banerjee (2017) and Anderson and Anderson (2016), the latter demonstrating that debris-covered glacier lengths could double, depending on the debris effect on ablation in their model. Real-world evidence for such differences in debris-covered and debris-free glacier geometry remain largely qualitative. For instance, Scherler et al. (2011) found lower accumulation-area ratios for debris-covered than debris-free glaciers. Based on the data of Kraaijenbrink et al. (2017), we found a negative correlation (R = -0.36, p < 0.01) between the glacier minimum elevation and the percentage of debris cover (Fig. 10), hinting at both reduced ablation and a larger tongue for debris-covered glaciers.**

Consequently, the qualitative picture we can draw is that debris-covered glacier ablation area **is usually** larger ($A_{DC} > A_{DF}$), leading to lower emergence velocity ($w_{e,DC} = \Phi/A_{DC} < w_{e,DF} = \Phi/A_{DF}$). If the glacier is in equilibrium, in both cases, the thinning rate at any elevation is 0, because the emergence velocity compensates the surface mass balance, but with lower magnitudes for both variables (w_e and $\dot{b}$) in case of a debris-covered tongue (Fig. 11). In an unbalanced regime with consistent negative mass balances**, as mostly observed in High Mountain Asia (Brun et al., 2017)**, similar thinning rates between debris-free and debris-covered tongues could be the combination of reduced emergence velocities and lower ablation **roughly summing** up to similar thinning rates as debris-free glaciers (Fig. 11). Additionally, there are evidences of slowing down of debris-covered tongues and detachment from their accumulations area, both leading to reduction in ice flux and consequently in w_e (Neckel et al., 2017).

In conclusion, our field evidence shows that enhanced ice cliff ablation alone could not lead to a similar level of ablation for debris-covered and debris-free tongues. While we acknowledge the existence of other processes which can substantially increase the debris-covered tongue ablation, we highlight the potential important share of the emergence velocity in the explanation of the so-called 'debris-cover anomaly', which partly originates from a confusion between thinning rates and net ablation rates."

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 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.
We modified the method section to include a more detailed description of the emergence velocity in section 4.1. The alternative methods to measure the emergence velocity are mentioned in the discussion and in the conclusion.

— 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.
We now name this quantity f_C and defined f_C^*which is the cliff ablation enhancement factor compared to non-cliff area (instead of the average glacier tongue).

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.
Cliffs is plural, and cliff is singular. In some cases however it is necessary to use the singular to denote the singular sum of the plurals: e.g. *net ice cliff ablation* refers to ablation from all ice cliffs. We have attempted to clarify this and be consistent throughout the revised manuscript.

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

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

Modified accordingly

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?
This offset is usual, and due to the fact that Pléiades DEMs are derived from orbital parameters only (i.e., without GCPs). Consequently, while the geometry of the DEM is robust it is somehow "floating" in the 3D space, with offsets on each components that can be up to ~10 m. We added: "**This vertical offset is expected, as the DEMs are derived from the orbital parameters only (Berthier et al., 2014)**"

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?)
We clarified this point: "The velocities measured with Pléiades match well with the field data **(ablation stake displacements measured with a DGPS between November 2015 and November 2016)**, with the…"

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? Some discussion is found later, but here is where the question arises
We added: "**It is noteworthy that some spatial variability is expected for w_e, however, we have no means to assess it.**"

P.6, L.26: I'm not sure to understand what "deformed" means in this case. "Deformed" how?
"deformed" meant non homogeneous of the individual points of the PCs: "we deformed the PCs, **by displacing its individual points,** for…"

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?
We added a supplementary figure (fig. S5) showing this:

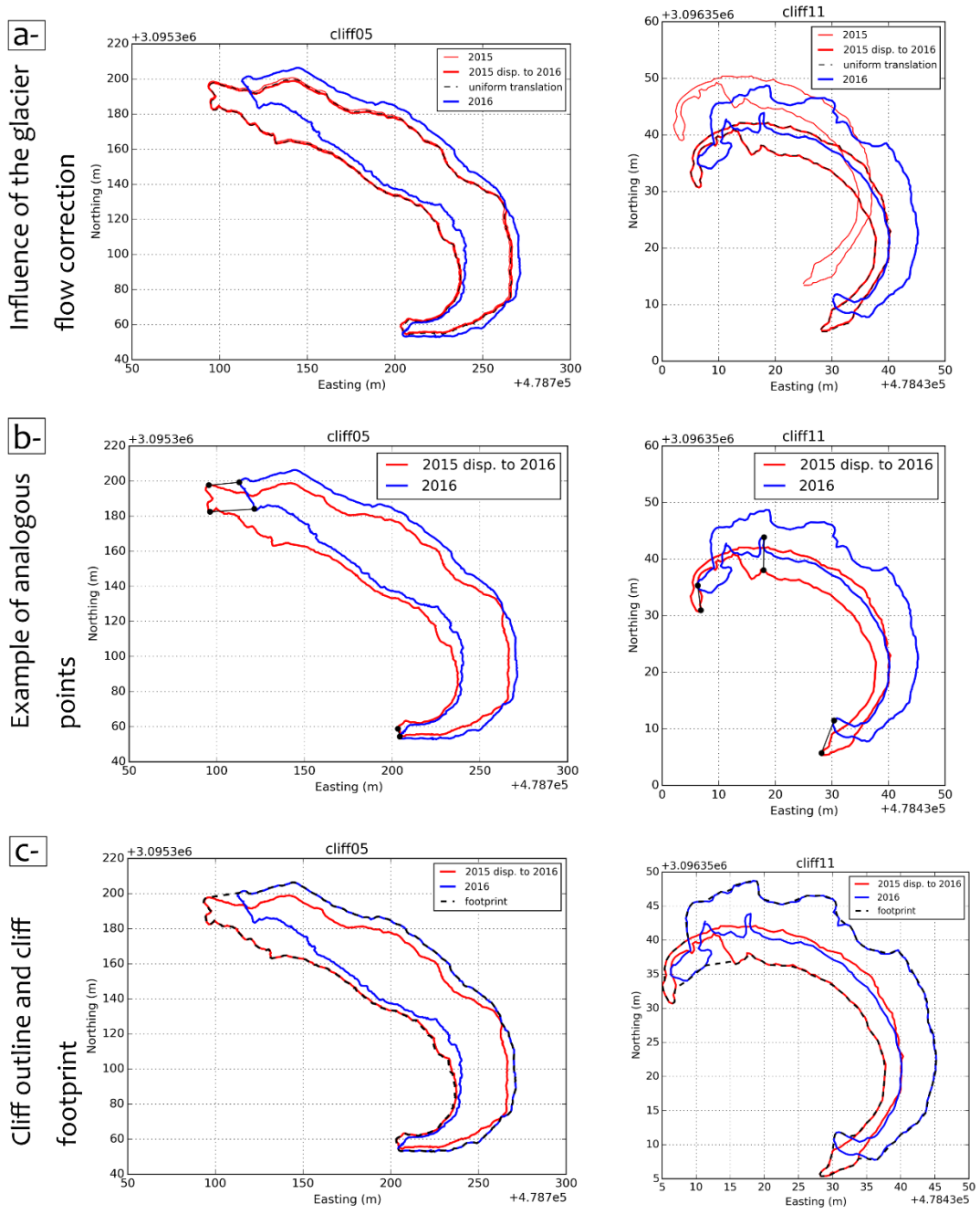

Fig. S5 - Examples of the methodological processing for cliff 05, located on a slow flowing area (left panels) and cliff 11, located in a fast flowing area (right panels). For all the panels the cliff outlines are represented in UTM45/WGS84. a- influence of the glacier flow correction, and comparison with a uniform translation. B- example of analogous points needed for the triangulation regularization. c- difference between the individual cliff outlines and the cliff footprint needed to calculate the cliff contribution for gridded data (DEMs).

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

See the figure above

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?)
Modified accordingly

P.11, L.9: The unit of -3.88+/-0.27 is missing
Modified accordingly

P.11, L.10: Here and elsewhere: In light of the estimated uncertainty, it would make sense to state 440+/-54 x 10ˆ3 m3/a (instead of 439 689 +/- 54 000 m3/a).
Modified accordingly

P.11, L.26: I’m not following: Is the stated value (1.51+/-0.21 m/a) already corrected for emergence?
Yes, we added: “**after correction for the 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?
Modified accordingly

P.12, L.14: Here and below: Consider replacing “original” with “actual”.
Modified accordingly

P.12, L.15: Remove “Doing the same”.
Modified accordingly

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

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."
The new sentence reads: “Since ice cliffs typically cover a very limited area (Herreid and Pellicciotti, 2018), it is unlikely that they can enhance the ablation of debris-covered tongues enough to reach the ablation of ice-free tongues.”

P.12, L.27: Check the wording: “englacial hydrology” is not an "ablation-related processes" (it’s rather a "discipline", as e.g. glaciology)
Modified accordingly

P.12, L.28, sentence starting with “.Yet this does not: : :” –> Split the sentence somewhere; it is very long.
Modified accordingly

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

P.13, L.7: The unit of “density” should be kg/m3 (not m2)
Modified accordingly

P.13,L.10: Well, the comparison is somewhat "cheated", as it should certainly include areas of the same size.
This section was largely modified, please refer to the new version at the beginning of this response.

P.13, L.12: Not entirely sure what “both variables” is referring to. To mass balance and emergence velocity?
This section was largely modified, please refer to the new version in the beginning of this response.

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.
We do not claim this. We just try to reason within a transient framework that is somehow close to the recent situation (i.e., corresponding to the geodetic observations of the last decades).

P.13, L.30-31: I don’t understand the sentence. Especially the two "in" within the parenthesis create confusion.
This section was removed according to reviewer’s 1 comment.

P.13, L.32: Why "nevertheless"? What’s the logical link to the previous sentence?
This sentence has been moved to section 6.1 and “nevertheless” has been removed.

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.
Modified accordingly

P.14, L.5: “models [: : :] are not directly comparable with the observations” –> Explain (or at least give a hint) why not.
“For instance, the f_C values from models (Buri et al., 2016; Juen et al., 2014; Reid and Brock, 2014; Sakai et al., 1998) are not directly comparable with the observations (Brun et al., 2016; Thompson et al., 2016), **because they usually require additional assumptions about e.g., the sub-debris ablation or emergence velocity**.”

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.
We removed this sentence.

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.
“More field campaigns dedicated to ice thickness and velocity measurements (Nuimura et al., 2011, 2017) or the development of airborne ice thickness retrievals through debris **are needed, as stressed by the outcome of the Ice Thickness Models Intercomparison eXperiment (Farinotti et al., 2017).**”

P.14, L.16: “or englacial conduits” –> That’s a very speculative claim, isn’t it? I would suggest to flag it as such.
“Other contributions, such as ablation from supra-glacial lakes**, or even from englacial conduits**, are potentially…”

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.
We adjusted the rest of the manuscript on the level of confidence of the conclusion.

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.

Ok

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?
"A comparison of f_C or f_C^* values calculated for other debris-covered glaciers under our suggested framework would be informative, in order to compare estimates of ice cliff ablation for other and potentially much larger debris-covered tongues."

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.
This sentence was not really clear in the original manuscript. It is revised as: "**It would be very worthwhile to obtain longer-term and multiple sites quantification of the relative ice-cliff contribution to net ablation. Then a compilation of these data would allow developing empirical relationships for cliff enhanced ablation, which could be included into debris-covered glacier mass balance models.**"

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."
Modified accordingly

— 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.)
Modified accordingly

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 ;-))
Modified accordingly

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 "nalpha =local slope").
Modified accordingly

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)".
Modified accordingly

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%)"
Modified accordingly

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

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

Modified accordingly

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.
Modified accordingly

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.
The cliffs exhibit multiple aspects. We made this point clearer in the revised version.

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”.
This is clarified in the revised version.

Tab. 3: What is “B/H”? The caption should tell.
It means “base to height ratio”, it was added in the caption.

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