Response to anonymous referee 1

We thank anonymous referee 1 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.

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'.

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.

We thank the anonymous referee 1 for her/his positive appreciation of our work and we understand her/his concern about the lack of balance in the focus of our discussion and the critics of the extrapolation of our findings based on a single case. We respond on the specific points raised hereafter.

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.

We agree with the reviewer. However, we think that many recent studies, based on DEM differences often neglected the ice dynamics. This article was an opportunity to stress the potential influence of the emergence velocity, and consequently to stress the fact that thinning rates and ablation rates are very different. We address the reviewer's comment by two developments in the text:

1- We changed the structure of the text in order to better emphasize the emergence velocity calculation. The section 3.4.2 is now titled "Ground penetrating radar" and we added more methodological development and background about the emergence velocity in a new subsection within the method section, which is now divided as: 4.1 Emergence velocity; 4.2 Ice cliff backwasting calculation; 4.3 Sources of uncertainty. We substantially enriched the 'update' estimate. We corrected the uncertainty of the GPR estimate (see below and thanks for pointing this out!) and tested different glacier thermal regime hypothesis.

Section 4.1 now reads: **"The emergence velocity refers to the upward flux of ice relative to the glacier surface in an Eulerian reference system (Cuffey and Paterson, 2010). For the case of a glacier in steady-state (i.e., no volume change at the annual scale), the emergence velocity balances exactly the net ablation for any point of the glacier ablation area (Hooke, 2005). For a glacier out of its steady state (as Changri Nup Glacier) the thinning rate observed in the ablation area is the sum of the net ablation and the emergence velocity (Hooke, 2005). On debris-covered glaciers, while the thinning rate is relatively straightforward to measure from DEM differences, for example, the ablation is highly spatially variable and difficult to measure (e.g., Vincent et al., 2016). In order to evaluate the mean net ablation of Changri Nup Glacier tongue from the thinning rate, we estimate the mean emergence velocity (w_e) for the period November 2015-November 2016 and for the period November 2016--November 2017 using the flux gate method of Vincent et al. (2016). As the ice flux at the glacier front is 0, the average emergence velocity downstream of a cross-section can we calculated as the ratio of the ice flux through the cross-section (Φ in m^3 a^{-1}), divided by the glacier area downstream of this cross-section (A_T in m^2):**

$$w_e = \frac{\Phi}{A_T}$$

This method requires an estimate of ice flux through a cross-section of the glacier, and is based here on measurements of ice depth and surface velocity along a profile upstream of the debris-covered tongue (Figs. 1 and 2). The ice flux is the product of the depth-averaged velocity ($\overline{u}$ in m a^{-1}) and the cross-sectional area. For the period November 2015-November 2016 (resp. November 2016-November 2017), the glacier slowed down compared with the 2011-2014 period and the centerline velocity was equal to 10.8 m a^{-1} (resp. 11.1 m a^{-1}), leading to an assumed mean surface velocity along the upstream profile of 8.1 ± 0.6 m a^{-1} (resp. 8.3 ± 0.6 m a^{-1}), as the centerline velocity is usually 70 to 80 % of the mean surface velocity along the cross-section (e.g., Azam et al., 2012; Berthier and Vincent, 2012). We used the relationship between the centerline velocity and the mean velocity, instead of an average of the velocity field along the cross section, because the image correlation was not successful on a relatively large fraction (~ 30 %) of the cross section. Converting the surface velocity into a depth-averaged velocity requires assumptions about e basal sliding and a flow law (Cuffey and Paterson, 2010). Little is known about the basal conditions of Changri Nup Glacier, but Vincent et al. (2016) assumed a cold base, and therefore no sliding. This leads to $\overline{u}$ being approximated as 80 % of the surface velocity, additionally assuming n = 3 in Glen's flow law (Cuffey and Paterson, 2010). As an end-member case, assuming that the motion is entirely by slip implies $\overline{u}$ equals to the surface velocity (Cuffey and Paterson, 2010). Consequently, we followed Vincent et al. (2016) and assumed no basal sliding, but we took the difference between the two above-mentioned cases as the uncertainty on $\overline{u}$. This leads to $\overline{u}$ = 6.5 ± 1.6 m a^{-1} (resp. 6.6 ± 1.7 m a^{-1}) for the period November 2015-November 2016 (resp. November 2016-November 2017).

Assuming independence for the cross-sectional area (σ_S) and the depth-averaged velocity ($\sigma_{\overline{u}}$), the uncertainty on the ice flux (σ_Φ) can be estimated as:

$$\frac{\sigma_{\Phi}}{\Phi} = \sqrt{\frac{\sigma_{\bar{u}}}{\bar{u}}^2 + \frac{\sigma_S}{S}^2}$$

Given the above mention values for the depth-averaged velocity, the cross-sectional area and the associated uncertainties, the relative uncertainty of the ice flux is ~30 %. As a result, for the period November 2015-November 2016 (resp. November 2016-November 2017), the incoming ice flux was thus 499 700 ± 150 000 m^3 a^{-1} (resp. 503 840 ± 150 000 m^3 a^{-1}). The glacier tongue area was considered unchanged at 1.49 ± 0.16 km^2, corresponding to w_e = 0.33 ± 0.11 m a^{-1} (resp. 0.34 ± 0.11 m a^{-1}). It is notoriously difficult to delineate debris-covered glacier tongues (e.g., Frey et al., 2012). In this case, we assumed an uncertainty in the outline position of ± 20 m, leading to a relative uncertainty in the glacier area of 11 %, which is higher than the 5 % of Paul et al. (2013). In this case, the uncertainty on the glacier outline is not the main source of uncertainty in w_e, but for automatically delineated glacier outlines, this would be an important source of uncertainty. The updated emergence velocity is ~20 % lower than estimated for the 2011-2015 period (Vincent et al., 2016), due to both the thinning and deceleration of the glacier. As the difference in w_e between November 2015-November 2016 and November 2016-November 2017 is insignificant, we consider w_e to be constant and equal to w_e= 0.33 ± 0.11 m a^{-1} for the rest of this study. It is noteworthy that some spatial variability is expected for w_e, however, we have no means to assess it."

2- We now describe in more detail the cliff evolution (number of cliffs, backwasting rates, area changes) in section 6.1 of the discussion and we shortened and substantially rewrote section 6.3. Section 6.1 is now entitled "**Cliff evolution and** comparison of two years of acquisition", and the two first paragraphs read as:

"The total ice cliff covered area did not vary significantly from year to year, ranging from 70 ± 14 × 10^3 m^2 in November 2017 to 72 ± 14 × 10^3 m^2 in November 2016. The twelve individual cliffs surveyed showed large variations in area within the course of one year, with a maximum increase of 57 % for the large cliff 06 and a decrease of 34 % for cliff 03 and 09 (Table S2). The total area of these twelve cliffs increased by 8 % in one year. Interestingly, over the same period, Watson et al. (2017) observed only declining ice cliff areas on the tongue of Khumbu Glacier (~6 km away). All the large cliffs (most of them are included in the twelve cliffs surveyed with the terrestrial photogrammetry) persisted over these two years of survey, including the south or south-west facing ones (Table 1) , although south facing cliffs are known to persist less then non south facing ones (Buri and Pellicciotti, 2018). However, we observed the appearance and disappearance of small cliffs, and marginal areas became easier to classify as either ice cliff or debris-covered areas, highlighting the challenge in mapping regions covered by thin debris (e.g., Herreid and Pellicciotti, 2018).

We calculated backwasting rates for the twelve cliffs monitored with terrestrial photogrammetry for the period November 2015--November 2016 (Table 1). The backwasting rate is sensitive to cliff area changes (because it is calculated as the rate of volume change divided by the mean 3D area) and should be interpreted with caution for cliffs that underwent large area changes (e.g., cliffs 01, 02, 03, 06, 09 and 11; Table S2). The backwasting rates ranged from 1.2 ± 0.4 to 7.5 ± 0.6 m a^{-1}. The lowest backwasting rates are observed for cliffs 11 and 12, located on the upper part of the tongue, roughly 100 m higher than the other cliffs (Fig. 1 and Table 1). The largest backwasting rates were observed for cliff 01, which expanded significantly between November 2015 and November 2016. The backwasting rates are lower than those reported by Brun et al. (2016) on Lirung Glacier (Langtang catchment) for the period May 2013-October 2014, which ranged from 6.0 to 8.4 m a^{-1} and lower than those reported for surviving cliffs by Watson et al. (2017) on Khumbu Glacier for the period November 2015-October 2016, which ranged from 5.2 to 9.7 m a^{-1}. These

differences are likely due to temperature differences between sites. Indeed, the cliffs studied here are at higher elevation (5320-5470 m a.s.l.) than the two other studies (4050--4200 m a.s.l. for Lirung Glacier and 4923-4939 m a.s.l. for Khumbu Glacier)."

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.
We modified the title of the article, which now reads "Ice cliff contribution to the tongue-wide ablation of Changri Nup Glacier, Nepal, Central Himalaya"

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.
The "debris cover anomaly" (i.e. similar thinning rates over debris-covered and debris free glaciers at similar elevations, although ablation is expected to be reduced over debris covered glaciers compared with debris-free glaciers) is to our opinion an interesting but fuzzy concept, which has been used to motivate previous studies that looked for processes responsible for enhanced ablation on debris-covered tongues. Based on the data of Brun et al. (2017), we show that the thinning rates of debris-covered areas are comparable to thinning rates of debris-free areas for glaciers in the Khumbu region (Figure R1). The thinning rate of Changri Nup Glacier agrees well with this regional pattern and therefore we do conclude that the tongue of Changri Nup Glacier is a representative "debris-anomaly" glacier.

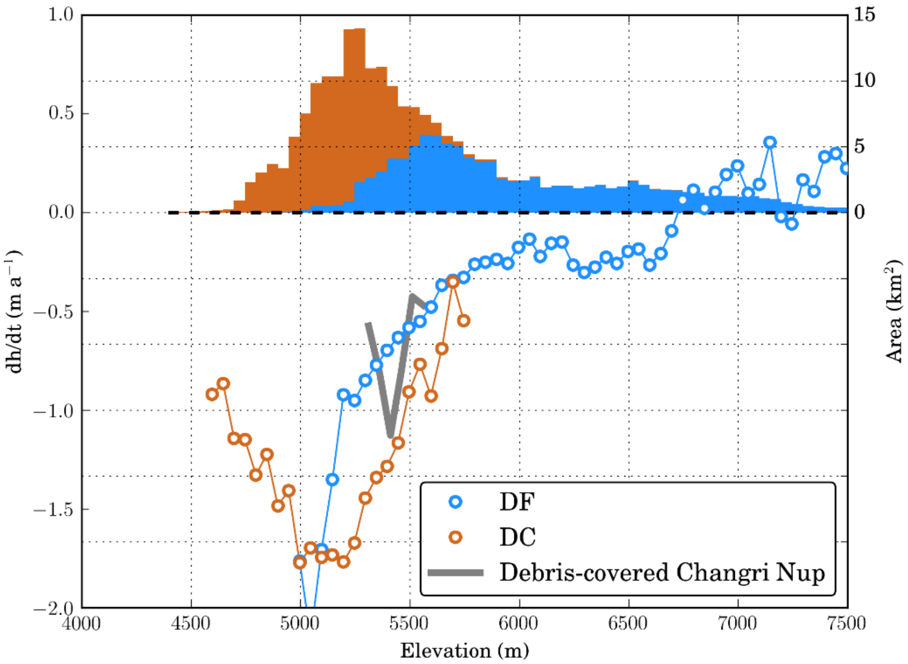

Figure R1: rate of elevation change for debris-free and debris-covered ice in the Khumbu region, based on Brun et al. (2017) data. The brown histogram represents the hypsometry of the debris-covered ice and it is stacked above the blue histogram, which represents the hypsometry of the debris-free ice. The thinning rate for the debris-covered part of Changri Nup is overlaid in grey.

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?
We do not think that comparing melt rate just beneath the GPR section to compare clean ice and debris covered ice is possible, because the transition between clean ice and debris-covered ice is very smooth and it is hard to distinguish between the two categories. Moreover, this area is very small and it is not representative of heavily debris-covered tongues.

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?
The point raised by the reviewer is interesting but we believe that it is not possible to address it (at least using our data), for two main reasons: first, we do not know much about the emergence (of both categories of glaciers), and consequently it is not possible to answer directly the question "how much is because of emergence velocity" raised by the reviewer. Second, the cliff melt is highly localized, whereas the emergence velocity effect is spatially distributed. Consequently, we cannot really calculate the values suggested by the reviewer, as they are expected to be very different for each glacier, because they depend on the relative areas and they depend on the ice dynamics. Instead, we calculated the area covered by cliffs which produce ablation similar to a debris-free tongue (P12-L22-24).

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)?
We tried to map the area occupied by ponds, but it turns out that this task was not straightforward. Based on the UAV orthomosaic, we could not distinguish between very shallow ponds/supraglacial river systems, which have a limited contribution to ablation, and ponds that are deep enough to develop vertical mixing and therefore enhanced ablation. We mapped approximately 20 000 m² of surface covered by ponds (i.e. approximately 1.5 % of the tongue area) on the November 2017 imagery. It is noteworthy that a single pond contributed to half of this total by itself (area of 9 600 m²). This pond is located at the bottom of cliff 06 and triggered a large calving event between November 2015 and 2016. We cannot say much more about pond ablation with this dataset.

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.
We have removed "planar" from the manuscript and replaced it with "map view", following (Herreid and Pellicciotti, 2018). The cliff 2D area was defined as the cliff footprint (P5 L28).

P1 L20. Suggest 'have been found' in place of 'were found' for correct tense
Modified accordingly

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.
Modified accordingly

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.
We added "**could**"

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).
We separated this sentence into two parts: the first one mentions only the cliffs (we removed the reference to Miles et al 2016) and the second one mentions the supraglacial and englacial ablation due to water circulation: "**Other processes linked to supraglacial and englacial water circulation could lead to substantial ablation** (e.g., Benn et al., 2017; Miles et al., 2016; Sakai et al., 2000; Watson et al., 2018)."

P2 L12. It follows that you also need to determine the melt contribution of supraglacial ponds in order to resolve this
Modified accordingly. "**In order to partially test the first hypothesis, there is a need to calculate the total contribution of the additional melt processes to the tongue-wide surface mass balance. In this work, we focused on the ice cliff contribution, as the other processes are currently not quantifiable at the scale of a glacier tongue.**"

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?
This point was also mentioned by other reviewers. The *p* factor is now named f_C factor to avoid a confusion with the "*p*-value" (comment from reviewer 3). The f_C factor has the same definition as *p*, but we added the definition of a new factor, named f_C^*, which is the ratio of the cliff ablation divided by the non-cliff terrain ablation (denoted by the subscript NC):

$$f_C^* = \frac{\Delta V_C}{A_C}\frac{A_{NC}}{\Delta V_{NC}} = f_c \frac{\Delta V_T}{\Delta V_T - \Delta V_C}\frac{A_T - A_C}{A_T}$$

Based on our data, for Changri Nup Glacier, $\frac{\Delta V_T}{\Delta V_T - \Delta V_C} = \frac{1}{1-0.23} = 1.30$ and $\frac{A_T - A_C}{A_T} = \frac{1-0.07}{1} = 0.93$, consequently $f_C^* = 1.2\, f_C$.

For the consistency between the studies mentioned, we interpreted the studies as follow:

- Juen et al. 2014 : "Although the ice cliffs occupy only 1.7% of the debris covered area, the melt amount accounts for approximately 12% of the total sub-debris ablation" -> $f_C^* = \frac{12}{1.7} = 7.1$
- Reid and Brock 2014: "Analysis of the DEM indicates that ice cliffs account for at most 1.3% of the 1m pixels in the glacier's debris-covered zone, but application of a distributed model indicates that ice cliffs account for ~7.4% of total ablation." -> $f_c = \frac{7.4}{1.3} = 5.7$

- Buri et al. 2016: "Although only representing 0.09% of the glacier tongue area, the total melt at the two cliffs over the measurement period is 2313 and 8282m^3, 1.23% of the total melt simulated by a glacio-hydrological model for the glacier's tongue. -> $f_c = \frac{1.23}{0.09} = 13.7$
- Sakai et al 1998: From the abstract: "The ice cliff melt amount reaches 69% of the total ablation at debris covered area, although the area of ice cliffs occupies less than 2% of the debris covered area" -> $f_c = \frac{69}{2} = 35$
- Sakai et al 2000: From their Table 2: ratio of the "absorbed heat at each type of surface during the observation period (167 days)", including the "whole debris-covered zone" -> $f_c = \frac{256}{26} = 9.8$ or looking only at the debris -> $f_C^* = \frac{256}{21} = 12.2$
- Brun et al 2016: "The ice cliffs lose mass at rates six times higher than estimates of glacier-wide melt under debris, which seems to confirm that ice cliffs provide a large contribution to total glacier melt." -> $f_c = 6$
- Thompson et al 2016: "Although ice cliffs cover only ~5% of the area of the lower tongue, they account for 40% of the ablation." -> $f_c = \frac{40}{5} = 8$

As most of the studies were already within the framework of the original definition of p/f_C, we decided to keep the focus on this factor, instead of f_C^*. This example demonstrates the importance of a consistent framework for comparing these studies.

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.
Modified accordingly

P2 L28. Suggest 'positive emergence velocities will increase the : : :' as it is more concrete than 'affect'
This paragraph has been reworked. It now reads: "**Neglecting the emergence velocities (i.e. comparing thinning rates instead of ablation rates) introduces a systematic overestimation of f_c. This is due to the fact that cliffs ablate at higher rate than the rest of the glacier tongue: ice cliff thinning rates are thus less influenced than the thinning rates of debris-covered ice when neglecting the emergence velocity. As a consequence, the ratio of the cliff thinning rate divided by the mean tongue thinning rate will overestimate f_c. To correctly estimate f_c and the fraction of total ice cliff net ablation, thinning rates need to be corrected with the emergence velocity.**"

P3 L5-10. It is necessary to make some mention of your emergence velocity correction in this paragraph.
Modified accordingly. "We introduce a new method based on DEM differencing, which takes into account geometric changes induced by glacier flow, **and in particular by the emergence**, and apply it to the UAV and Pléiades imagery."

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

P4 L15. 'equal' should be 'equivalent'
Modified accordingly

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

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.

We apologize for this mistake and thank a lot the reviewer for pointing it out!

The section has been quite modified following the reviewer's major comments. The uncertainty on the GPR data is ± 15 m.

The new section 3.4.2 ("**Ground penetrating radar data**") now reads: "**A cross sectional profile of ice thickness has been measured upstream of the debris-covered tongue (Fig. 1) in October 2011, with a ground penetrating radar (GPR) working at a frequency of 4.2 MHz (Vincent et al., 2016). The original cross-sectional area was 79 300 m² in 2011 and 78 200 m² in 2015 (Vincent et al., 2016). Between November 2015-November 2016 and November 2016-November 2017, the cross sectional area decreased from $S_{2015-2016}$ = 76 900 m² to $S_{2016-2017}$ = 76 340 m² (with $S_{yr1-yr2}$ being the mean cross sectional area between the year 1 and year 2), based on the 0.86 m a^{-1} thinning rate measured over the November 2015-November 2017 period along the profile. The uncertainty on the ice thickness is ±15 m (Azam et al., 2012), which leads to an uncertainty (σ_S) of ± 10 000 m², as the length of the cross-section is 670 m.**"

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.

We added: "**It is noteworthy that w_e is likely to be spatially variable, however, we have no means to assess its spatial variability.**"

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

It is in pixel, this is added in the text.

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).

We added a supplementary table (Table S2), which shows the cliff 3D area in 2015 and 2016 and we calculated the backwasting distance in Table 1. We calculated the backwasting as individual cliff volume loss from terrestrial photogrammetry (i.e., only for the period Nov. 2015 – Nov. 2016), divided by the mean 3D area. The backwasting rate is compared with other studies in section 6.1.

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

For a comparison with previous studies, see our response above. We added the results for f_C^* as well.

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

The difference in mass balance between 2015-16 and 2016-17 is also observable in the glacier-wide mass balance of the near-by debris-free West Changri Nup Glacier (-0.76 and -2.56 m w.e. yr^{-1}, for 2015-16 and 2016-17, respectively) (P. Wagnon, unpublished data). The exact reasons explaining such large differences need to be analyzed but are not related to air temperature almost similar between both years (-3.6°C measured at the AWS at 5360 m a.s.l. on West Changri Nup, in both years, from 1 November to 31 October). The mean summer temperatures (1 April – 30 September) are also very similar (0.3°C for 2016 versus 0.1°C for 2017). The difference might come from other meteorological variables, but this has not been analyzed in details yet, and it is not the scope of this present paper.

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

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?
We added this calculation and calculated the f_C^* ratios when neglecting the emergence, the section now reads as:
"In this case, the factor f_c would be 4.5 ± 0.6 **(and f_C^*would be 5.4 ± 0.7)**, which is 50 % higher than the actual value. **The cliffs would be found to contribute to ~34 \% of the tongue ablation**. For the period November 2016--November 2017, the factor f_c would be 3.6 ± 0.6 **(and f_C^* would be 4.3 ± 0.7)**, which is 20 % higher than the actual value. **The cliffs would be found to contribute to ~29 % of the tongue ablation.** This might partially explain why previous studies found significantly higher values of f_c, and stresses the need to estimate and take into account the ice flow emergence, even for almost stagnant glacier tongues like Changri Nup Glacier (see Discussion below)."

P12 L19. Consider 'the' debris-cover anomaly
Modified accordingly

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?
As mentioned earlier in this response: $f_C^* = 1.2\ f_C$ for this year on Changri Nup (in agreement with the reviewer's calculation!). We added the influence of neglecting the emergence velocity on f_C^* as well.

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)
We both agree and disagree with the reviewer. Figure R1 shows that Changri Nup Glacier fits within a regional pattern of "debris cover anomaly". Moreover, we think that our calculation related to the cliff area equivalent ablation is true independently of the debris-cover anomaly, as it is based only on field measured ablation rates (for the debris-free surface) and the ablation rates measured in this study. Consequently, we decided to keep the lines 20-26 of page 12 unchanged. However, we understand the reviewer's concern about the generalization to the debris-cover anomaly, which implies additional assumptions, such as the reduced emergence velocity for debris-covered tongues. That's why we substantially modified the rest of this section.

P13 L4. I think this section needs to be tidied up with respect to nomenclature, in particular replace 'tongue' with 'ablation area'.
We prefer to keep the word tongue, because the glacier tongue is not the same as the 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?
This section has been substantially modified in the revised version of the article. It is challenging to discuss the enhanced ablation of ponds, because we know very little about them on Changri Nup Glacier. We tried to map them, but with limited success because we can't distinguish between the large ponds, that are deep enough to produce enough ablation and the shallow ponds, which play a much more minor role (see our response above). While we appreciate the suggestion to orient the discussion towards the future evolution of these processes, we definitively think that we do not have enough elements to discuss this. The revised section reads:

"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 ($\frac{\overline{\partial h}}{\partial t}$ in m a^{-1}), ablation rate and emergence velocity for a glacier tongue:

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

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."

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.'
We modified this sentence. See our modified version just above.

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?).

We removed this section

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

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).
The new paragraph reads: "A significant obstacle to applying our method to other glaciers is the need to estimate the emergence velocity, which requires an accurate determination of the ice fluxes entering the glacier tongues. **The measurement of ice thickness with GPR systems is already challenging for debris-free glaciers, as it requires to drag emitter, receiver and antennas along transects of the glacier surface. It is even more challenging for debris-covered glaciers, as the hummocky surface prevent the operators from dragging a sledge.** 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 recommended. **The precise retrieval of emergence velocity pattern using a network of ablation stakes combined with DGPS is a promising alternative, in particular if combined with detailed ice flow modeling (e.g., Gilbert et al., 2016).**"

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?
The revised sentence reads: "**The main limitation of our study is its short spatial and temporal extent. 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 to develop empirical relationships for cliff enhanced ablation, which could** be included into debris-covered glacier mass balance models."

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

Modified accordingly

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.
Modified accordingly

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