Modelling steady states and the transient response of debris-covered glaciers

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Author Response to Both Reviews

We would like to thank both reviewers for two very helpful reviews. We believe their comments can be easily addressed via the intended revisions outlined below. We first discuss major themes and then go line by line, first for Leif Anderson's review (starting on page 2) and then for Fabien Maussion's review (starting on page 11).

Major Themes

C1 Dialogue, literature, foundations

It was raised by both reviewers that we could improve the manuscript by having a better discussion and integration of the advances made by previous work, what research gaps are present, and where our work fits into this dialogue with other existing research. We agree that our paper would gain from a better discussion in context of literature. We will include the additional references suggested by the referees and beyond and better incorporate these into the existing text. We will also use this to better motivate our study and model choice.

C2 Model description

The reviewers felt that some aspects of our model were not so clearly explained or motivated. We will improve the explanation and details, explain why the features of this model were chosen, and what is different from other models used in previous studies. We will have a particular focus on differentiating the relevant parts of our model (e.g. frontal boundary condition) from that used in Anderson and Anderson (2016) and on the aspects of our cryokarst model.

C3 Additional figures to aid visualization

There were a few places where additional figures could be useful for making our results easier to understand. We agree with these points and we will add such figures where noted (e.g. visualize impact of cryokarst effect on along-flow surface mass balance). In order to better illustrate the role of the debris for the transient response, we further plan to add animations of some of the modelling experiments in the supplements.

C4 Variable climate forcing

We used 'white noise forcing' in the text and both reviewers felt this was incorrect. We agreed that the terminology we used was not fully correct. We will note that it is white noise on a century timescale and we will use the term 'random climate forcing' instead.

C5 Uniform debris concentration assumption

One reviewer makes an important point about the assumptions inherent in our choice of constant debris concentration. We did not yet discuss the impact of these assumptions and limitations very clearly in the text. But we feel this issue needs more attention and is especially important given that numerous other studies use the same assumption. Therefore, we will add a clearer and more substantial discussion of the impact and limitations of the assumptions of this model choice. However, based on our existing results we are confident that this assumption does not affect the main conclusions of our research.

In what follows, we show first the reviewer's comments in black and then our response in blue.

Major comments (Reviewer 1: Leif Anderson)

This work is emerging from a dialogue between other DCG modeling and observational efforts. As the paper reads now, that dialogue is not yet properly developed. Often times previous work immediately relevant to points being developed in this manuscript are cited (or not) as having worked in the general topic at the start of a paragraph. But the insights gained from past efforts are not yet allowed to be in dialogue with the results from this work.

We will better incorporate the previous work into the manuscript both for context and to explain what is new (and what isn't) in this paper.

This partly means that a stronger foundation should be laid in the manuscript (in the introduction) with regards to what insight has already been gained from previous work and how this effort builds off of those previous efforts. This also means that the writing does not clearly delineate between conclusions made by previous work and the new findings here (especially in the discussion section and the toe parameterization appendix). I raise this point not to diminish the important contributions made here. On the contrary engaging past work with the new insights will highlight the work done here more clearly and make for an even more valuable contribution to the community.

Because the model developed in Anderson and Anderson (2016) and the one presented here very similar I think it is appropriate to be a bit more explicit about how the models are different. As it reads now it is not always clear what was originally derived by A & A 2016 and what is new here. A bit more care should be taken when discussing the differences between the toe parameterization approaches. It is unclear how different the approach derived here is different from the range of parameterizations explored in A & A

2016. A more explicit statements about the toe method will allow the method developed here to be reproduced.

We will clarify the description of model, and in particular expand and better explain and justify both the terminal boundary condition and the cryokarst parameterization.

Figures in general are well composed, though a few more simple figures will expand accessibility to a broader audience. It would be helpful to see some modeled mass balance profiles plotted in the main paper since they are essential for showing the difference between debris-free and debris- covered glaciers. And the added effect of cryokarst on DCG mass balance profiles.

We will include surface mass balance plots in the manuscript (for the cryokarst case also) and add some animations in the supplements for illustrating the differences in transient response.

A figure or schematic showing how the cryokarst formulation is implemented in the model would be helpful. Maybe driving stress could be plotted and an example of the effect of the cryokarst features on the mass balance profile would aid the reader in understanding the new parameterization and its effect on the glacier. As the manuscript is now I have trouble visualizing the pattern of cryokarst features on the glacier at any one time. I ultimately think this parameterization is an important and useful contribution and showing a bit more how it works will only benefit the manuscript and the community. This is an important contribution! The authors might also consider shifting from the use of 'cryokarst,' as ice cliffs themselves are not necessarily the result of the collapse of englacial tunnels.

We already plot the driving stress in the appendix and in the supplement, but we will try to visualize the thresholds in driving stress as well and try to add a sketch for explaining the cryokarst parameterization.

The authors might consider adjusting the use of the term 'white noise' as it refers to the climate forcing. In terms of climate, white noise forcing almost always refers to year-to-year variability in the climate. The climate forcing applied here is actually red noise because the timestep is 100 years and there is therefore autocorrelation from year-to-year. This manuscript uses persistent climate changes to fore the model. I am not actually sure of the correct phrasing but maybe climate changes that are randomly sampled from a normal distribution would interface better with previous work. Or just the response of a DCG to variable climate?

We agree, as noted above in main response C4. We will use 'random climate forcing' instead.

The discussion section would be improved with a more thorough discussion of the uniform englacial debris concentration assumption. It is very important to consider what a steady, uniform englacial debris concentration implies for headwall erosion rates when glacier geometry is changing. I have included/expanded on some points lower down.

We agree, as noted in main response point C5 above. We will note this in the manuscript and be more explicit about how this pertains to the debris input and discuss the implications on our findings. Based on our existing modelling results we are however confident that our main conclusions with regard to general dynamical behavior of debris covered glaciers (in contrast to clean ice glaciers) remain unchanged.

The manuscript in general should be streamlined and repeated statements should be cut out. Individual sentences are well composed, but I find myself a bit overwhelmed at times in the text. The modeling results section will benefit the most from some textual work. The number of experiments and the changing focus from various parts of the DCG system make it hard to follow. Anything that can be done to simplify and distill the description of these experiments will help the reader.

We will better streamline the text and try to avoid repetitions.

Line-by-line comments (Reviewer 1: Leif Anderson)

Line 13. “as is also observed in remote sensing” this could be a little clearer. Maybe just remove ‘in remote sensing’

Agreed – will remove “in remote sensing”.

line 40. “the relatively recent advent of remote sensing data.” consider re-phrasing here.

Agreed – will rephrase.

44. The introduction is a bit parsimonious towards previous efforts. What are the contributions of previous debris-covered glacier models? What have we learn up to now? By setting the stage more the novel and interesting contributions of this work, which there are many, will be better highlighted.

Agreed – as mentioned in the main comment C1, we will explain in detail what was done previously, what the state of the field is up until now, and be more explicit about what is new in our work/model.

Line 46 and 47. Recognizing that you have cited several of our papers here, but there are additional transient simulations of debris-covered glaciers responding to climate change using essentially the same model as A &A 2016 in Crump et al., 2017 and Anderson et al., 2018. The references are fully written at the end of the manuscript. Also Anderson et al., 2019a does not include any model simulations.

Good point – as mentioned in C1, we will add / correct the references here.

Line 57-59. The way this sentence is written it is fuzzy what the actual differences are between the models. Is it the same besides the differences listed here? If not it would be good to make it a bit more clear what the other differences are in the methods section.

Agreed – as mentioned in C2, we will give more detail on our model and what is different from previous models.

Line 63-65. This is a great way to support the use of SIA!

Thanks.

Line 90. Reading this sentence makes it seem like this melt formulation (equation 6) was derived by Nicholson and Benn (2006) but it was actually derived in Anderson and Anderson (2016). It is appropriate to cite that work here.

Good point – we will change to reflect this.

Line 99-100. How is it different? It seems to be nearly the same. It might be more appropriate to state that you 'improve upon' or 'start from.' How is this different from Anderson and Anderson (2016)? Explicitly stating what they do will make it more clear what the new contributions of this work are.

As discussed in main comment C2, we will address this by giving more details on our model and how it differs from previous work in both the Methods section and in the appendix.

Also how was your value of D_0 chosen?

We will address this also in the Methods.

Line 102. This sentence could be simplified right now it is a little more complicated than it needs to be.

Agreed – we will rephrase.

Lines 103-105. Your case would be stronger if you develop the justification for the parameterization a bit better here. It seems to me that there are some more citations here for work that has linked ice flow with these features. Like Kraaijenbrink et al., 2016 and/or Watson et al., 2017. It is a clever approach though.

As mentioned in main comment C2, we will add more details on the model here. We will look more carefully at these references as well to see if it makes sense to cite them. As there are many studies on ice cliffs / ponding and some qualitative but somewhat vague relation of these features to dynamics. However, to our knowledge there are currently no explicit quantitative or mathematical models that link ice cliffs and ponds to the dynamics. We will try to better include the more general qualitative relations from the literature here in the revised manuscript and better explain the issue of the lack of a quantitative model.

Lines 105-110. It would be helpful for the reader to include the equation for driving stress here. That way readers can connect to the fact that driving stress scales with ice thickness and surface slope. Is there a physical mechanism why cryokarst features might follow driving stress? Would be good to include that.

As mentioned in the main comments C2, we will add more model details here and add the driving stress equation.

Line 122. Just need to clarify what the CFL condition is here as this is the first time this acronym shows up in the text.

Agreed – we will add this.

Section 3 Modelling results

This section is rather difficult to follow and I am quickly overwhelmed by the number of simulations and how quickly the writing moves between them.

132-135. It could be beneficial to include a what you refer to as a 'baseline' case (with base debris concentration) so the reader has a single simulation to compare the others to. Reading below it is easy to get lost in all of the simulations. Maybe this baseline case could be bolded in the figures below?

Good point – we will revisit this and will more clearly identify and delineate the baseline case from what follows.

Figure 1. Nice figure. What part of the glacier is covered with debris? How does that relate to the ELA. Perhaps adding these would be helpful to bring the various components of the model together for the reader.

It seemed clear to us where the debris is here, since it is also included in the same figure. However, as the reviewer's comment speaks to the important point raised in the main comment C5, we will modify the glacier profiles in Fig. 1a and 1c to make clear that the debris cover starts at the ELA.

Line 157 need a hyphen between 'debris' and 'covered'

Yes – thanks, we will correct this!

Section 3.2 I think these are all important interesting simulations. This section would be improved though with a bit more synthesis. It is a bit difficult to follow because of the number of different experiments. Maybe more clear topic sentences clearly keying on what each experiment the paragraphs correspond to would help? Or sub-section titles for each experiment?

It might also be that the description moves between simulations using different englacial debris concentrations quickly. Perhaps it would be easier to follow if the descriptions of the experiments use one concentration case?

These are good points. We will better clarify, motivate, and differentiate the experiments here to make them more digestible to the reader.

Figure 2 is really a great future! 223-224. Might be good to have a citation here.

Agreed – we will add this.

Figure 4 is also really clear.

Thanks!

Table 4. The table looks very clean but maybe adding in text at the top the definition of each variable again would help the reader follow.

Agreed – we will add this.

Figure 6. The introduction of ‘Bare ice %’ is hard to wrap my head around since it seems to be a new way of describing the cryokarst features. Maybe just label it % of the surface composed of cryokarst features. Consider finding another way to represent the contribution of cryokarst that is more clear.

Good point – we will explain this term better in the text and consider an alternative wording.

Figure 7. You might consider moving this figure into the supplemental and just describing the effect of cryokarst on long term evolution in the text. Just so the reader does not feel overwhelmed.

Good point – we will consider moving this to the supplement and if not, then we will try to make the text associated with the figure clearer.

4.1 Debris-covered glacier memory

This section highlights some interesting findings. The section, though, would be improved by stating what past studies have concluded related to this topic and then emphasizing showing how your results/conclusions differ. This is especially relevant to interface a bit with past transient glacier model simulations. Do they show a similar effect that support your discussion here?

As agreed in the main comment C1 above, we will better situate and integrate our work in the context of previous literature on this topic.

299-302. This paragraph would benefit from a look at the past literature on the subject, as this point has been raised previously. Additionally Clark et al., 1994 et al. also discuss this effect.

We have found hints of this in the literature but nothing explicit. For example, the paper by Clark et al., 1994 discusses many well-known issues relating to the retarding effect of debris cover on glacier response but we could not find any mention of the idea that the length of a debris-covered glacier depends on the history of its cold phases. However, we will include this relevant reference in the introduction and discussion.

Line 347 -351. There are studies that do connect ice cliff occurrence to ice dynamics, including Benn et al., 2012; Kraaijenbrink et al., 2016, .

We already cite Benn et al., 2012 immediate above this but we will clarify this text and also consider citing Kraaijenbrink et al., 2016. However, it does not change the point we make here – that the onset of cryokarst features is quantitatively not well (or not very explicitly) linked to observations of glacier dynamics.

Section 4.4 Steady state velocity–debris thickness relationship

I think this is a very interesting section. I do think it would be improved if it interfaced with the previous literature on the topic. Especially emphasizing how this work has expanded on those previous insights.

As stated in the main comment C1, we agree with this and will add additional references and context.

392-393. A & A 2018 also do a compilation of 8-10 glaciers that show that debris thickness patterns follow this same pattern. These observed profiles can also be referenced with the Mölg study as well.

Agreed – we will add this point.

395. "It is natural to ask to what extent the debris thickness profile depends on the ice flow model and the debris transport model used. That question can be answered for the steady state case without assuming anything about the ice flow and considering only conservation of mass."

It is unclear how the statement above relates to the rest of the paragraph. This seems like an interesting topic though.

This statement is directly connected to what follows, as we do not assume an ice flow model. We will adjust the text here in order to clarify this point.

Equation 11 is very similar to one derived by Anderson and Anderson (2018) who follow a similar approach. It seems appropriate to cite that you are following that line of logic or interface with that work here.

It does makes sense to cite A & A 2018 in a way that more clearly links this section with that study and we will do so. However, we start from a different perspective as we do not assume velocity is constant (as they do) but rather start with conservation of mass. We will clarify this difference here.

404. How is it possible that there is ice flow at the terminus that is not 0? The SIA is based solely on internal deformation which is requires that ice thickness is larger than 0 which is not the case at the terminus. Just a bit of clarification will help.

Again, we do not assume SIA in this section (and will clarify this in the text). We only assume conservation of mass. Also, even in our main study, our model does not necessarily involve zero ice flow at the terminus, as is discussed in the model development on lines 124-127. In fact, in steady state the ice flow speed at the terminus is always nonzero. In some retreat experiment the velocity can however go to zero.

409. There is an interesting discussion to be had between the insights from A &A 2018 (Fig. 9) and what is discussed in this paragraph, especially regarding the zone of englacial debris emergence as described there. How does this discussion mesh/build off of with what was discussed in A &A 2018?

Good point – we will add a further reference to A & A 2018, specifically linking this section to their Fig. 9.

4.5 Model limitations

421-425. This is a repeat from a point made above. My sense is that this only needs to be stated once.

Good point – we will remove one of the two references so that we make this point only once.

426 to 431. The authors should discuss the implications of the assumption of uniform englacial debris concentration further. From my view it seems more fair to say 'that the effect of a uniform englacial debris concentration should be explored further.' I mention this because there are a number of simplifications that go into this assumption.

I think its is a reasonable first order approach, but this means the entire ablation area will be covered with debris.

It needs to be added here that different ice flow paths will change the englacial debris concentration even with a uniform input of debris everywhere on the glacier. It is really impossible to have a glacier with a uniform englacial debris concentration because of the straining of ice and the inevitable variability of debris input (in space and time) to the glacier surface.

One additional point that should be discussed is how applying a uniform englacial debris concentration relates to headwall erosion rates. If the headwall erosion rate is constant in time then as a glacier gets bigger the englacial debris concentration by definition must become smaller.

This effect is not included in this model. By keeping the englacial debris concentration uniform and steady there must be a requisite increase in headwall erosion rate as the glacier grows in size. If the glacier doubles in size then the headwall erosion rate would need to also double. I think this is simply an underlying assumption of this approach that should be clear to the reader and if possible should be quantified and placed in the supplemental material.

This is an important point, as we note in the main comment C5 above. We agree with the reviewer and will clarify the implications of this assumption. We will also discuss how this affects the interpretation of our results.

427. missing period.

Thanks – we will add this.

434. It should be made explicitly clear what the differences are between the toe condition applied here and the one presented in A and A (2016) in the main text. Is it simply a modification of the approach presented in A and A 2016? Are they not also quantitatively similar? See the text regarding the toe parameterization in the Appendix below.

As stated in the main comment C2, we agree to state more clearly how our boundary condition differs from A & A 2016.

436-437. I could not find where this statement is discussed in the appendix. Is there a citation that notes this or is it a new observation? I am unaware of this effect.

We will clarify this point in the appendix.

437-438. The way this paragraph is written implies that A &A 2016's approach would not capture the effects of a stagnating tongue. Is this actually true? Looking at the other publications after A &A 2016 like Crump et al., 2017 and Anderson et al., 2018 the length change curves are similar to those presented here and based on the toe parameterizations the dynamics should be represented similarly to the work here.

We do not believe this paragraph implies anything about the limitations of the boundary condition used in A & A 2016. The context of this paragraph is on limitations of our model, not the model used in any other studies.

Appendix A and the toe parameterization in general

It is a substantial effort to develop a toe parameterization and any improvements on the exploration from A & A 2016 are welcome, important, and vital for the future development of debris-covered glacier models. It is also important that the method presented here also be reproducible. It would be good for the authors to describe the sub-grid interpolation scheme in detail. What shape/formula do you assume? What H* terms are viable?

465-475. It seems like there should also be some discussion of how this formulation relates to the original terminal condition described by A & A 2016. How are the approaches different?

A & A 2016 explore a range of possibilities for the terminal parameterization which the toe is drowned in debris because it cannot leave the glacier and also a case in which an ice cliff persists at the terminus and debris is effectively rapidly removed from the glacier. See Figure B1 and section 5.2 in A & A 2016. Ultimately, A & A 2016 use a scheme where debris is removed based on the bare ice melt rate which is basically the same as is implemented here. I think it would benefit the readership to have a more complete description of the differences between the two schemes and how different they actually are.

It seems that the parameterization presented here is a smart approach. Despite the way the text is written it seems the approach follows the A & A 2016 formulation closely and fits almost within the range of parameters explored there. The new approach presented here essentially sets no limit on the d_flux term from A & A 2016, and the formulation presented here would be close to the c =10 case in Figure B1 from A & A 2016 for debris removal. The main difference is that this approach keeps the ice cliff backwasting at the bare ice melt rate despite the removal of more debris than that backwasting of the ice cliff actually would allow.

The down side of the approach presented here is that the removal of debris from the glacier is not necessarily physically representative of the process of debris removal at the terminus of real debris- covered glaciers.

The A & A 2016 scheme honors that the removal of debris from the toe in the ice cliff case is determined by the backwasting rate, but this in turn leads to a greater grid scale dependence than the scheme presented here. From my view the benefit of either of these schemes depends on the decision to value either grid-scale dependence or the physical representativeness of debris removal from the toe.

Either way a more nuanced description of this toe scheme and how it relates to the work of A & A 2016 is needed to ensure the community can follow these methodological differences.

As we state in the main comment C2 above, we intend to better explain our toe boundary condition and to clarify the differences in our model boundary condition at that used in A & A 2016.

Figure B1. It seems like this figure should plot the mass balance curve with time, since the the cryokarst parameterization adjusts that directly. I would also like this figure with the SMB curves included in the main manuscript since the cryokarst parameterization is a central, new contribution of this work.

Agreed - we will add SMB plots in the main text and the appendix.

General comments (Reviewer 2: Fabien Maussion)

• At the end of the introduction, you write: "*to date no study has used a coupled ice flow-debris transport model to study in detail the transient response and characteristic response times of a debris-covered glacier. This study aims to fill this gap...*". "It has never been done before" is not a good motivation for a study, and I think that the paper would gain from clearly stated research questions. In particular, it would help to understand what motivated the model design and the design of the idealized experiments (why this bed profile, why this model design, etc.). Research questions will also help to place the study in the context of previous literature, and prepare the reader to understand what you are trying to achieve with this paper.

Agreed – as stated in the main comment C1 above, we will more clearly contextualize our study in light of what has been done before and this should better motivate our approach.

- The word "idealized" does not show up in the title, abstract, or introduction. I think it should be clearly stated much earlier (maybe not in the title, but at least in the abstract). "Numerical modeling" could be understood as "applied to real glaciers".

Good point! We will emphasize that these are idealized studies by stating this explicitly.

- This may be subjective, but I don't find any of the comparisons with Jóhannesson's response times informative or useful. Even without debris cover, you can find numerical response times of glaciers which are widely different than the analytical ones, since the e-folding times are highly dependent on parameters such as bed depressions, mass-balance (MB) gradients, etc. (see e.g. Zekollari et al. 2015 or Schuster, 2020 - unpublished thesis work).

While it is certainly true that one can generate widely different response times due to different geometry and/or climate forcing, here we have the same bed geometry (and very similar upstream surface geometry) and the same climate forcing. However, the response is quite different. Since the Jóhannesson time scale is often used to give a general idea of how changing the geometry or forcing changes the response time for debris-free glaciers, we still feel that a comparison here is warranted. The purpose of the Jóhannesson time scale is actually to be able to compare glacier dynamics between different glaciers and it is a useful measure that characterizes the dynamic response. However, we can include a caveat that explains the limitations of this approach.

- Your code availability statement ("available upon request") is against this journal's data and open science policies: https://www.the-cryosphere.net/policies/data_policy.html. I strongly recommend to make your code available (under a clear license), which will increase the visibility and re-usability of your work.

We will make our code available when we submit a revised version of the manuscript, in accordance with TC's open science policy.

Specific comments (Reviewer 2: Fabien Maussion)

Abstract I'm not very familiar with the debris-covered glacier literature, but I had to search for "cryokarst" online

We will better motivate and explain our use of this term in the text.

Abstract add "idealized numerical simulations"

Agreed – we will add this.

eq. (1) consider using b instead of a for mass-balance (more common I believe)

We used *a* instead of *b* so as not to confuse with the bed *b(x)* but we can state early in the manuscript that we are going against convention, just to clarify for the reader.

L72 "for a given a bed elevation" - remove "a"

Agreed – we will fix this.

L96 having read section 2.1.4 and the appendix, it's still not clear to me how you compute H^* (and I don't want to check up on Anderson et al 2016). I notice later that H^* is a constant and a model parameter: mention this earlier in the text.

Good point – we will clearly state that H^* is a model parameter here and we will also better motivate how we choose this value.

L100 specify which appendix.

Good point – we will fix this by identifying which appendix.

Appendix A despite of your valid attempts to show that this boundary condition may be found in the real-world, I still believ that the ice-free terminus condition is more a model necessity (trick) than a real-world feature. You don't have to change anything in the text here, I just wanted to comment on that.

That is an interesting comment. While we admit that we do not know exactly what the best boundary condition is, we believe our formulation is consistent with a number of observations and importantly it is also grid-size independent. But we are happy to admit that there might be a better way to formulate the boundary condition here.

Sect. 3.1 (steady states) I really had to think twice about how you can reach steady state with such a model. I think that it would help to write more about it. E.g. by saying again that (i) steady state can be reached only because the MB doesn't go too close to 0 and (ii) that this is only possible by removing debris at the terminus and effectively capping the debris thickness to a reasonable value. You can refer to Fig. S1 in this section (or mention typical values of MB at the terminus in the model) to help understanding.

We will clarify here how a steady state can be reached.

L155 to our knowledge, Jóhannesson et al, (1989) wrote:"The volume time-scale tau can be computed from the volume differences between two steady-state profiles scaled to the causal mass- balance change", but did not mention the e-folding volume response time (yet). Maybe refer to another paper as well: e.g. Oerlemans (1997) or Jóhannesson (1997)

Good point – our text implies that Jóhannesson's paper used *e*-folding time and we will adjust this.

L185 volume-area scaling: since it might be unclear to some of your readers, add here that (in your model) area is directly proportional to length

Good point – we will add this.

L190 is "stagnant" the correct term here? I was confused several times in the manuscript about this, because you seem to use "stagnant" for when the glacier length does not change. Personally, I understand "stagnant" as "ice that is not moving" (u = 0). You cannot have "stagnant" ice with your numerical model setup. I would argue for using "stable terminus" in place of "stagnant", or clearly state in the text what you mean with "stagnant". At the very end of the paper there is a sentence going in this direction ("*stagnation or more specifically the cessation in local dynamic replacement of ice*.").

L277 "stagnated": same here. Is it the correct way to say that? Non-divergence is still happening with u constant and non-zero, i.e. moving ice.

We will clarify our use of the word 'stagnate', which is used in numerous other studies of debris-covered glaciers in the same way as we mean it. That is to say, stagnation here means that the glacier is no longer very dynamic. It is not necessary for the velocity to be zero in a zone of stagnation.

Section 3.3 "white noise" traditionally, white noise climate should be applied on a year to year basis and the periods of cold and warm climates would occur "naturally", as a result of random sampling. I wonder how this would affect your results. Additionally, I wonder if an annually varying MB would still work with your debris cover formulation, since you don't deal with temporary ice/snow cover on debris as for now.

Agreed – we will change the wording here and we will not use 'white noise' anymore. We have run simulations with annually varying ELA as well and it works fine with our model but whether this is physically realistic is open to discussion.

Fig. 5 while this figure carries well your main message, I think that it can be misinterpreted. In particular, the blue line in Fig. 5b gives the impression that the glacier will always grow, i.e. never reach a "steady state" (i.e. an average length around which it oscillates - albeit in a strange, debris covered way). What you could do here is continue the simulation for an additional 5k years (at least) and see what happens. It might have an interesting consequence: the "average length" of a debris covered glacier under a *random* climate might be longer than the length of the same glacier under *constant* forcing. I expect the average length to be some- where between the steady state lengths with the two ELAs (although it might even be longer than that, which would be very interesting to discuss further).

We attempted to raise these very points by showing these plots in Fig. 5. There are a number of further points that could be raised here and indeed, further studies one could do with our model to examine the volume and length response given different climate forcing. We will add some text here to clarify what this part of our study implies.

Figure S1 : write that "SS" stands for "steady state" in the legend.

Agreed – we will add this.