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

by Leif Anderson

The manuscript develops our understanding of debris-covered glacier response to climate change using a numerical model run in an idealized setting. A vital new contribution is a parameterization of melt hotspots on debris-covered glacier evolution and response time. The manuscript also describes the long-term response of debris-covered glaciers to a variable climate.

Major comments

This is a welcome and important effort that expands our understanding of debris-covered glacier evolution! Overall, I think that this manuscript will be a great/strong contribution once the below issues are addressed. Many of these comments are related to presentation, but they are vitally important so this work can be easily understood by TC audience.

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.

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.

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.

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.

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?

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.

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.

Line-by-line comments

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

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

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.

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.

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.

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

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.

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.

Also how was your value of D_0 chosen?

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

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.

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.

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.

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?

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.

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

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?

Figure 2 is really a great future!

223-224. Might be good to have a citation here.

Figure 4 is also really clear.

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.

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.

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.

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?

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.

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

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.

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.

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.

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.

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.

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?

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.

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.

427. missing period.

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.

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.

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.

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.

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.

References

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