Author response to referee comments on "Modelling debris-covered glacier dynamics: transient response to changes and feedbacks in debris and climate forcing" (egusphere-2025-5997)

Review 1 (Anonymous Referee)

Summary

This manuscript of Hardmeier et al. (2025) investigates the dynamics of debris-covered glaciers using 1D numerical flowline modelling, with the aim of improving understanding of how debris affects glacier behavior. The study focuses on the coupled evolution of glacier f low and debris cover, exploring the feedbacks between debris thickness, melt rates, and glacier geometry. The authors implement a modelling framework that simulates debris transport and glacier evolution, enabling the investigation of the development of spatial debris thickness patterns, the influence of debris on melt rates and glacier mass balance, and feedbacks between debris cover and glacier flow dynamics. The study aims to contribute to improved understanding of debris-covered glacier evolution under climate change. The manuscript addresses a relevant scientific problem and poses a clear motivation for attempting the research. Debris-covered glaciers namely represent a significant fraction of glacierized area in many mountain regions and strongly modify glacier response to climate forcing. Numerical modelling studies are essential to investigate the corresponding debris-ice interactions because direct field observations remain sparse and time scales at which these processes work are long. In that sense, the manuscript is scientifically valuable and should ultimately be suitable for publication. However, some scientific and structural issues here and there need to be addressed before publication. I recommend major revisions to (i) strengthen the readability of the paper and improving its structure, (ii) explicitly elaborate more on novelties and distinguishing it from confirmed previous findings, and (iii) better frame the outcomes of the study within the specific model setting and parameter choices. I suggest publication once the comments below have been addressed.

We thank the referee for this extensive review that engages deeply with our manuscript and provides constructive, detailed comments for improvements. Below, we respond to and propose changes according to the referee's comments. Referee comments are in Italic, responses in regular font.

Major comments

1. Paper structure and length: I think this paper is very extensive, and sometimes even a bit too much. Some parts of the paper are very large (e.g. the introduction). Sometimes it reads a bit more like a thesis rather than a scientific publication. Anyway, I think some parts of the manuscript can be reduced in longevity of the text to avoid that the reader gets lost in huge amounts of information, either by condensing the text, avoiding repetition of earlier-stated stuff, or moving text to other paragraphs in the manuscript. See for example my comments on L12, L94-104, L225-236 below. Some relevant information (such as justification of the chosen value of the reference debris input rates, or aspects related to the D_0 parameter) is also intertwined within different sections or mentioned too early/too late in the manuscript (e.g. comments L164, L298, L476-482, L490-495, L622-624). Some statements also deserve more references (e.g. L15-19, L30, L52-53).

We agree that the manuscript could be reduced in length and re-framed to read more like a coherent study about the long-term transient response and feedbacks of debris-covered glaciers, rather than an extended collection of experiments. We propose the following major changes to address this point:

- Title: The title should reflect the novelties better and differentiate itself more clearly from previous work. We would adjust the title to "Feedbacks and timescales in the modelled transient response of debris-covered glaciers" or something similar that focuses on the main (new) points of the paper.

- Introduction: We will adjust the framing to be more representative of the key points, check for repetition with later sections, and compact & thin out current sections (according to comments and wherever else possible).

- Methods: We will condense the existing model description to the most essential and novel parts. We consider sections essential that introduce quantities that are later tested or discussed (such as debris dynamics, debris flux, debris-covered surface mass balance, and the new debris entrainment scheme).

- Results: We will move all sections describing experiments repeated from previous studies with similar results (mostly the steady-state and simple transient experiments) to the Appendix. The remaining results contain only new experiments and newly introduced concepts, or repeat experiments (explicitly denoted as such) with different results.

2. Distinction between new and confirmed model physics and findings: In several occasions, the authors mention that they have found newly discovered effects and integrated new model capabilities. I think the authors can be clearer about what exactly is new in their model when compared to previous models (see e.g. comment on L87, L89, L630) and which findings are new or confirm previous findings (see e.g. comment on L265-274, L440, L635-656). This has not become very clear from the text. The authors should also state explicitly how their (newly found) findings advance the science and understanding of debris-covered glacier behavior (e.g. comment L663).

Some changes proposed in the answer to major comment 1 also contribute to addressing this second concern about separating novelty from repetition.

Additionally, we will frame experiments repeated from other studies as benchmarks and report similar findings as such in this benchmark framing. We will rework the lines pointed out specifically by the referee to highlight novel methodology, experiments, and contributions to scientific understanding.

3. Interpretation of main findings within the context of model setting/limitations: I think it is important that the authors stress more clearly that their findings must be interpreted within the context of the main model setting (see for example comments on L367 and S3.4.2, L656). Also the model limitations and how this can affect the main findings can be discussed more extensively, as this paragraph misses some important aspects (e.g. resolution-dependency of results, explicit quantification of debris mass conservation (i.e. is the debris input flux equal to the debris output flux at the terminus in steady state?, see comment appendix B2), and more general limitations related to the model physics, see comments L135, L627).

We agree that the context of the model parameters/properties needs to be more explicitly stated. We will add such statements at several places in the text (see responses to detailed comments below). The other comments related to limitations are also addressed in the responses to the corresponding detailed comments.

Regarding other model limitations:

- Resolution dependence: we conducted some additional tests that are shown in our response to the comment on L632. To summarize, simulations at ≤100m resolution converge at very similar glacier extents, surface geometries, and debris thickness profiles.

- Debris mass conservation: while debris mass is not 100% conserved in our model and shows some failure modes, this is to be expected considering the numerical approach taken here. We are advecting concentration - an intensive quantity on a Eulerian grid with a moving free surface, a finite difference velocity reconstruction, and source/sink terms that flip sign across a grid cell boundary (the ELA). Each of these choices is reasonable on their own but together make exact closure structurally difficult.
  Other similar approaches either make no mention of how debris mass conservation is enforced (e.g., Anderson et al., 2016) or use a brute-force correction factor (e.g. Mayer & Licciulli, 2021), essentially hiding similar issues as in our approach.
  One actual solution to this problem would be to treat debris as Lagrangian particles (as in Scherler & Egholm, 2020), where debris is implicitly conserved.

Minor comments

L1: 'parts' -> maybe 'aspects' is a better word.

Will be done.

L2: insulating effect -> maybe state it corresponds to relatively thicker debris, since thin debris can enhance melt.

Will be done.

L11: What do you specifically mean with 'non-linearity of transient glacier response'?

The non-linear pattern of retreat: stagnation & thinning, then fast retreat and/or detachment. We will rephrase to make this clearer.

L12: The introduction is quite extensive, maybe try to compact some of the information. For example, I question whether L34-40 are already necessary here but can maybe be moved to S3.1.1 where you mention the effect of debris supply rates; maybe L54-84 can be moved to the Methods section before you explain your own model; or L94-104 may be integrated into S2.7.

As mentioned in our response to major comment 1, we will re-work and compact the introduction. About the lines mentioned here specifically:

- While L34-36 seem quite essential to introduce the concept of debris supply altogether, L37-40 can be moved.

- L54-84 will be summarized to a shorter paragraph, the detailed description of previous approaches moved to the methods.

- We consider L94-99 an important overview of experiments that are performed in this study. L100-104 will be integrated in section 2.7.

L15: I think you can give more references here about work that has attempted to model debris-covered glacier behavior

Will be done.

L16-17: can you give some specific examples here?

Will be done: specifically, debris supply, formation and influence of ice cliffs and ponds, debris re-distribution and terminus processes (Nicholson et al., 2021).

L19: are there not more recent references that you can add? L21 'leads' -> maybe better state 'can potentially lead'

We will add some more (recent) references. L21: we think the causality between debris cover and elongated tongues is well-established. We will clarify that a continuous debris cover is meant (as opposed to partial cover, medial moraines, etc.).

L30: Kneib et al. (2023) is not the first to argue this, maybe add some more references

Will be done.

L52-53: References needed

Will be done (e.g., Rowan et al. (2015), Verhaegen et al. (2020), Compagno et al. (2022)).

L87: Can you elaborate more about what is 'new'? Is it not the same model set-up as in Anderson and Anderson (2016) but with the glacier geometry from Ferguson and Vieli (2021)?

L88-89: uses 'many' elements, but you mention only one example.

L89: 'novel' -> again mention what is new about it.

We agree that calling our approach a "new model" might be misleading. It is rather an improved version of the englacial transport model presented in James Ferguson's PhD thesis.

The model set-up differs from Anderson & Anderson (2016) in mainly in two points:

- Debris entrainment, which is completely overhauled (instead of a prescribed concentration in the boundary cells) to make debris supply consistent under transient climate forcing.
- Terminal boundary condition: here we use the terminal cliff implementation from Ferguson & Vieli (2021).

We will rephrase L87-92 to show more clearly what is new and what is not.

L95: ELA -> define acronym when using it for the first time

The acronym ELA is defined in L55.

L94-104: These are a lot of mentioning of conducted and planned experiments, cannot this information be condensed a bit more to decrease the information load?

See earlier response to comment on L12.

L116-124: Can be condensed.

Will be done, as mentioned in the response to major comment 1.

L129: There is no basal sliding, correct?

Yes, no basal sliding is included in this model.

L135: Can you maybe add something about the ice rheology of your model? The equations that you mention here are only valid for isothermal ice (constant A). Also, what are the kinematic boundary conditions in the model and do they correspond to their theoretical values? Did you check the overall mass conservation within the ice dynamic model (i.e. du/dx = -dw/dz)?

The ice dynamics used in this model are identical to Ferguson & Vieli (2021). Going into further detail in this paper than there would not make any sense, if anything the description of ice dynamics could be shortened.

Yes, the equations are only valid for isothermal (temperate) ice. If applied to any particular glacier, this assumption has to be reconciled.

L139: Can you mention the units of C?

C is a volumetric concentration and therefore a unitless fraction (or could be expressed as vol%). We will add "volumetric" to the description to clarify.

L158: Why does accumulation need to be limited at higher elevations? An alternative would be to just use two SMB gradients, one above and below the ELA.

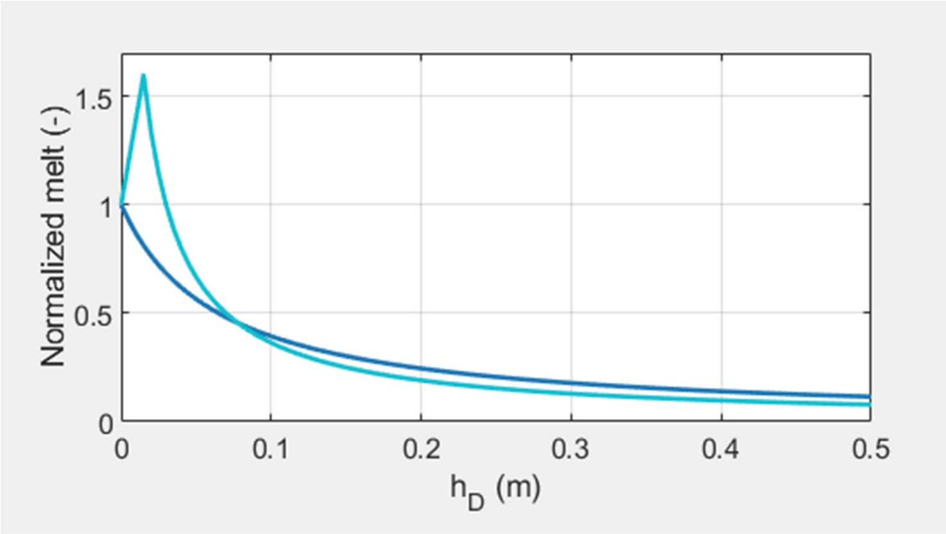
Separate MB gradients for ablation and accumulation area are another possibility to approximate mass balance. However, gradients often change within the accumulation area, flattening out above a certain elevation. Also, Anderson & Anderson (2016) and Ferguson & Vieli (2021) use the same parametrization, enabling better intercomparison. We will mention this in the text.

L163: How does it describe the shape of the Ostrem curve? Does 'characteristic' mean here according to a e^-1 length scale? Explicitly state that melt enhancement for thin debris is omitted.

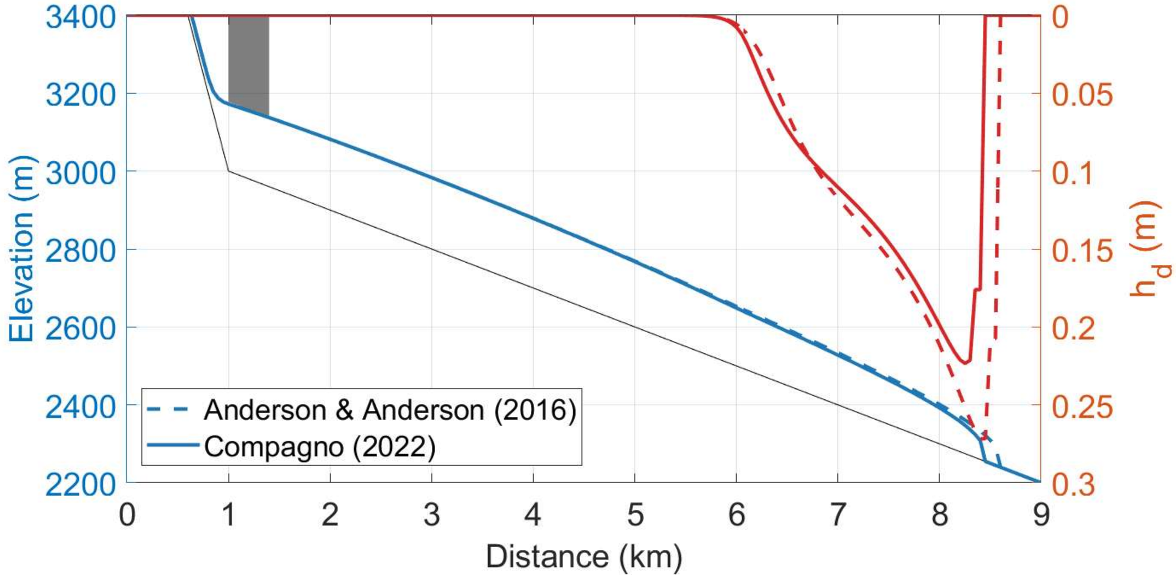
Characteristic means that it describes the shape of the inverse curve from equation 10. When D_0 is smaller, melt reduction happens faster already at thinner debris. We will clarify this right at L163.

While melt enhancement for patchy, non-continuous debris has been shown in empirical studies, measurements of debris "thickness" are highly subjective in this segment. We do not believe that simple parametrizations such as ours can accurately represent this effect, but this certainly needs further investigation (e.g., in a separate study). We will at the very least mention the omission of melt enhancement here in the text.

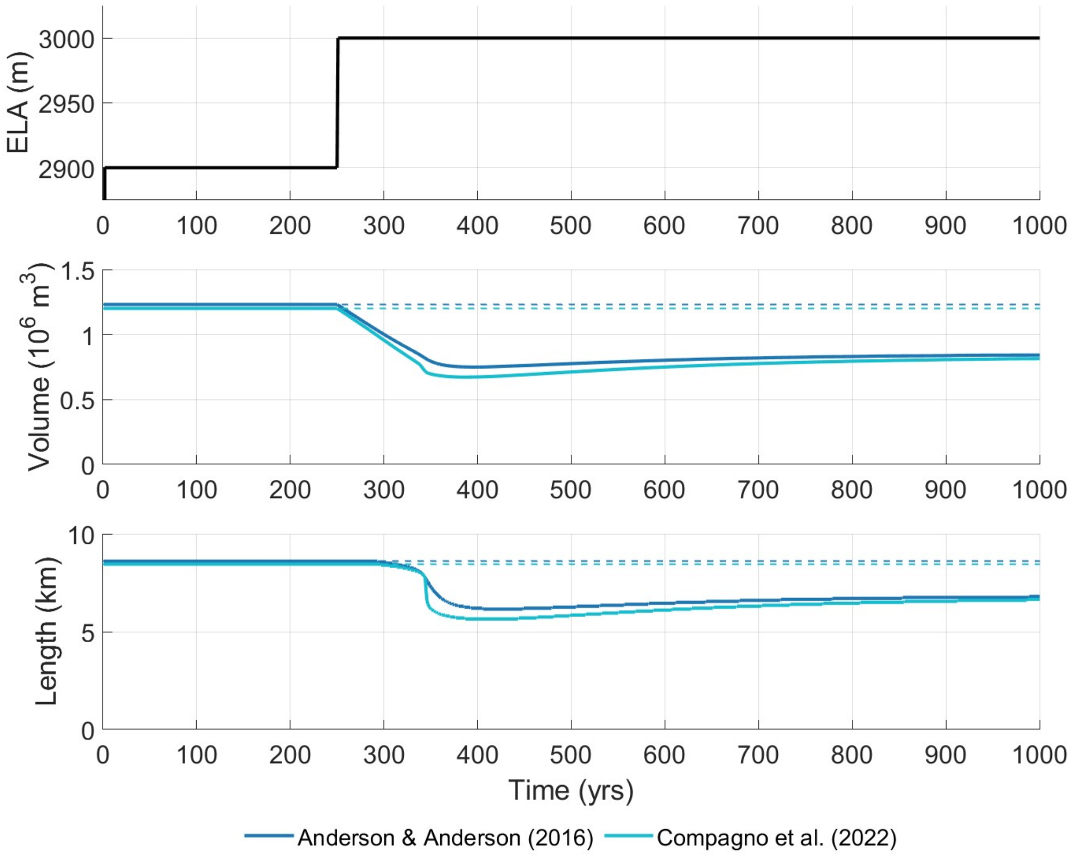
We ran some additional model runs to test how melt enhancement impacts glacier steady-state and response (see attached figures and animation), which could be added to the Appendix and mentioned here. Results from these tests further support our general conclusions about the transient response, showing even more abrupt retreat followed by a stronger re-advance after step-change warming.



Normalized melt (-)
1.5
1
0.5
0
0
0.1
0.2
0.3
0.4
0.5
h_D (m)



3400
3200
3000
2800
2600
2400
2200
Elevation (m)
0
0.05
0.1
0.15
0.2
0.25
0.3
h_d (m)
Anderson & Anderson (2016)
Compagno (2022)
0
1
2
3
4
5
6
7
8
9
Distance (km)



L164: Because there is a substantial reported variability of the parameter D_0, do you do some sensitivity experiments with respect to this parameter? Maybe already mention the info from lines L474-476 here. Maybe it would also be an idea to integrate the relationship between melt modification and debris thickness as an inset somewhere (for example in Fig. 1) to better visualize the patterns instead of mentioning it only in the Appendix (I think it is quite an important figure and an important parameter to keep in mind for interpreting your results). Is there for example a debris thickness where melt goes to 0 (we don't see what happens for a debris thickness >0.45m in your Fig. C2)?

Yes, there is substantial variability in D_0, and application to a single glacier should probably rely on local measurements to calibrate the Oestrem curve. We agree that L474-476 would better fit here already, as the curve from DebDab (Fontrodona-Bach) includes all combined debris thickness and ablation measurements globally. We think this also better justifies our default value for D_0.

Figure C3 shows the sensitivity of the parameter, showing exactly what one would expect:

- the larger D_0, the shorter the glacier
- the larger D_0, the closer the transient behavior is to a clean-ice glacier.

We will include a visual representation of the debris-SMB relationship (e.g., as an inset of a figure), but probably not in Fig. 1, which concerns debris entrainment.

L181: You say you keep the entrainment rate independent of the accumulation rate, but is *it also not logical that if the accumulation layer remaining at the end of the balance year is smaller, less debris can get buried into the glacier? By the way, in L192 you mention that it is dependent on the accumulation rate. Please be clear here.*

We want model flexibility, allowing us to have debris supply linked to something other than glacier mass input because that is the reality in nature, e.g. a glacier supplied by both snowfall and ice avalanches (but only the ice avalanches contain debris). More accumulation does not equal more debris entrained, so we want to keep the two separate, entraining the same absolute amount of debris regardless of ice accumulation.

We built this entire implementation because we realized that just forcing a debris concentration on the uppermost grid cell leads to an unintentional coupling of the debris supply to ice surface mass balance, where lower accumulation of ice also meant lower accumulation of debris.

In L192, we mention that the concentration depends on the accumulation rate (which makes sense, since we want to change the concentration to always reflect the same absolute volume of debris with a variable absolute volume of ice).

L184-195 and Figure 1. Not the easiest to understand. What do you mean with the 'surface grid cell' in L187? Only at the debris input location or the surface grid cells along the entire flow line? I also might be confused but why does the advected concentration C_s(t-1,j-1) in Eq. 11 only have an index related to horizontal (j) and not vertical directions (for example in the ablation area the movement of ice is upward relative to the ice surface)? And why does the surface debris concentration (Eq. 11) specifically need to be calculated in a different way then the rest (Eq. 6)? How does C_s(t) relate to ψ here?

We agree that the phrasing in L187 is not precise enough and will change this. Essentially, C_a is the debris concentration in newly accumulated ice within one timestep (i.e., the debris-ice mixture). This is only forced in the uppermost layer in the debris input area (not along the entire flowline).

Regarding the indexing in Eq. 1: we leave out the vertical indices, since we only consider the uppermost grid cell (with index m). More precisely, Eq. 11 would be:

$$C_s(t,j,m) = \frac{w}{h_{\hat{z}}} C_a \;+\; (1-\frac{w}{h_{\hat{z}}})(1-u) * C_s(t-1,j,m) \;+\; (1-\frac{w}{h_{\hat{z}}}) u * C_s(t-1,j-1,m),$$
$$\text{with} \quad C_a = d_{in}/a,$$

Figure 1: Can you maybe explicitly state that the red box inset with title 't-1' corresponds to the red box in the debris deposition zone? Also you mention grid point 'j' in the caption as the uppermost cells, but 'j' in the insets is placed at the lowermost cells? And what is 'm' (it is not yet defined up to this point)? Which grid point is 'j' here? Also C_0 enters the image here to influence C_s(t), but it is not mentioned in Eq. 11, where does it come from? I'm sure the figure makes sense when you know the model and equations well, but for first-time readers I think the figure deserves some extra clarification.

We will add a visual indicator that the two red boxes are connected.

The indices are j for the horizontal dimension and m for the vertical. In the figure and in the caption, it should say "the uppermost cell (J, M)". Also, we will add a better explanation of the grid in this case (and the associated indices j and m).

C_0 only applies to a special exception at the left boundary of the debris input area (where j=1). It represents the debris-free ice being advected from the cell to its left. However, the same equation also applies here, the difference is just that concentration being advected from index j-1 is always zero in this boundary case. Ultimately, making this exception might be more confusing than helpful, so we will remove it from the figure. We will also make sure to explain the figure a bit better in the main text. It deserves a bit more attention, as it is the main novelty in the methodology.

L216: Can you justify the chosen geometry? Or why do you think this geometry is representative for the reference tests?

This geometry was (as with other parameters) chosen to maximize comparability with previous studies. We will include this reasoning in L216.

L222: What do you numerically define as steady state?

Steady state here describes a state where glacier geometry (dH/dt = 0 along the entire flowline) and all debris fluxes are constant. We will amend this in L222.

L225-236: overlap with L94-104. I feel there is a lot of repetition, and the info can be condensed.

As mentioned in the response to the comment on L12, we will integrate parts of L94-104 into this paragraph. We will try to somewhat condense the paragraph, but we believe it provides an important summary of the model setup, explaining already quite concisely the default parameter set, where it comes from, and how where sensitivity tests are found.

L232: Model 'robustness' -> so in the end, based on which qualifications or findings would you call your model 'robust' or not?

Overall, we would call the model robust—with a few conditions:

- model spin-up needs to be done in the right way and should include a perturbation before the actual experiment.
- Debris input is consistent anywhere along the flowline, except in the lowest part of the accumulation area, where we found issues with mass conservation.

We will expand on these issues (see responses to major comment 3, L632) in the text.

Table 1 and S3.1.1: d_in -> Can you also transform it into a debris mass input flux (kg yr-1)? And where does this specific value for d_in come from? Can you justify this as being a representative value?

Yes, this value could also be converted into a debris mass influx. For example, with the default 4.5mm per year and an assumption of debris density = 2000 kg m-3, this amounts to a rate of 9 kg yr-1 per square meter in the input area. Over a 400m input area, this sums up to a rate of 3600 kg yr-1.

Our default value is the mean value of the range used by Anderson & Anderson (2016), 1—8 mm yr-1, which they justify with literature values. In reality, debris supply rates are highly variable and a largely unconstrained element of the process chain.

As section 3.1.1 will be affected by major changes and will mostly end up mostly in the Appendix (as it describes repeat experiments), we will also explain this a bit more thoroughly in the new Appendix text.

L249-256 and Fig. 2. That is a nice figure. Can you maybe integrate the flow vectors into the plot? This could enhance the visibility of the englacial flow paths. I also feel like the debris does not go that deep inside the glacier, despite the input location being quite close to the headwall. How should the little step of the debris thickness at the glacier terminus be interpreted in panel b?

While we agree that the visibility of the debris flow path could be improved, we believe that this is best achieved by extending Fig. 2a vertically. Otherwise, we think the directionality of flow is quite clear.

The transport trajectory of the englacial debris is relatively shallow because, in this configuration, the model applies the mass balance in the headwall and produces a thin, fast-flowing ice layer that then flows near the base.

If we look at debris supply processes on many debris-covered glaciers with a complex configuration of glacierized areas and headwalls / lateral walls, these shallower englacial paths are actually often more realistic than debris transport that is exclusively close to the base. We do, however, not make any claim that this configuration is representative for some real glacier. We will add a justification of the debris input location to the text.

The little step in debris thickness at the terminus is caused by the terminal boundary condition, where debris is removed and mass balance is computed at the terminal cliff at a sub-grid scale (see Fig. A2 in Ferguson & Vieli, 2021). It is not an expression of actual debris distribution, but rather a value assigned to the grid cell in this transition to the terminus cliff that does not impact mass balance.

L257-264 and Fig. 3. Looks cool. Are the different d_in values chosen arbitrarily? Please clarify in the text. Can you also maybe include a scatter plot of the debris surface volume vs. d_in as an inset to visualize your statement in L260? Also, it seems the mass balance still has quite a bit of melt (around ~-0.5 m w.e. yr-1) despite the debris being almost 1.5 m thick in the red curve in panel 3b. I would expect it to go even more towards 0. In your figure C2, we cannot see how much a debris thickness of 1-1.5m modifies the melt.

Yes, these d_in values are somewhat arbitrary. They represent the following: 1 mm yr-1 as the minimum of the range of values from Anderson & Anderson (2016), 9 mm yr-1 as double the default value, and 22.5 mm yr-1 as 5x the default debris supply.

For the non-linear relationship between d_in and supraglacial debris volume, we will add an inset or a separate figure in the Appendix that shows this.

In terms of mass balance, with a D_0 value of 0.065m, negative mass balance is reduced by ~94% at 1m debris thickness and by ~96% at 1.5m. The reason mass balance is still relatively negative is that these termini are quite far below the ELA. For example, at the terminus of the red glacier in Fig. 3, the debris-free SMB would be around -7.5 m yr-1. To make this clearer, we will add such an example to L261-263 to make this clearer.

L256: Can you confirm here that both the glacier geometry and debris mass is in steady state based on how do you defined it (i.e. what is steady state quantitatively here)? Does the debris model conserve mass (i.e. is the debris input equal to the debris output mass at the terminus) in steady state? It would be nice to show the transient evolution of (i) the glacier volume and (ii) debris input/output terms or debris mass towards steady state to get an indication of how long it takes to reach it.

According to the definition given in the response to the comment on L222, the glacier is in steady-state here. The debris model conserves mass (with the exception mentioned at L232), and Fig. B5 shows that debris mass flux is constant not just at the input and output, but over the entire transport.

Showing model spin-up as a figure in the main paper would be a bit too much, since we mainly want to focus on the experiments. We will, however, add a figure to the Appendix or an animation as a supplement.

Table 2: very useful. Can you maybe also include the respective sections where you treat the experiments?

Will be done.

L265-274 and Fig. 4: All very cool, but is this not just a repetition of the experiments of Anderson and Anderson? What is the added value? Maybe you can frame it within a qualitative model validation attempt or something, where you check whether the model does what is expected from earlier models and theory.

Yes, this intentionally meant to repeat the experiments performed by Anderson and Anderson. This is certainly one of the sections we want to re-frame as validation and move to the Appendix.

L275-287 and Fig. 5: I wonder if this linear trend can be extrapolated when even higher debris input values are inserted into the model. Maybe you can include an additional experiment or two/three with even higher debris input rates to see whether the relationship also holds under very extensive and very thick debris zones (or maybe some non-linearity kicks in?). Therefore, I would also add in L287: "linearly dependent within the explored debris input magnitude range" or something.

A long-term debris input rate (as opposed to intermittent large events) of 22.5 mm yr-1 is already far higher than expected values for glaciers of this size. Nevertheless, we will run some additional experiments with higher debris input. If anything changes about the linear relationship at those values, we will add it to the figure and adapt the text.

L298: Change of ELA of 100 m corresponds to what typical temperature change? (This is later mentioned in L369 but could already be moved to here.)

Will be done.

L305: Can you please define how you calculate glacier length and volume in the case of detached dead ice bodies? Is it the sum of all ice-covered grid points or only the index of the last ice-covered grid point of the active glacier?

With the idealized geometry with no topographical disturbances, we do not get detachment of dead ice bodies, which is why this is not relevant here.

In later experiments, however, we do get short periods of detachment (Fig. 13). There, we would measure two separate glacier "lengths": the terminus of the remaining main glacier body and the final ice-covered grid point. We will clarify this in the text of the methods.

The volume is more straight-forward in our view, it always remains the total sum of ice mass along the flowline, independent of detachments.

Figure 6. Panels e and f have no units in the vertical axis labels.

Will be done.

L337: You say here you change d_in from 1 to 9 mm yr-1, but in Table 2 you indicated 0-22.5 mm yr-1 for this experiment?

The values in Table 2 are remnants of an additional experiment included in an earlier version. The true range should be 1 to 22.5 mm yr-1, which includes both the step-changes shown in Fig. 7 and B1. The sentence at L337 should specifically reference Fig. 7, where debris input varies from 1 to 9 mm yr-1, so we will change that.

L342: A bit confusing to start talking about the deb-3 experiment here (within the section about deb-1 and without even talking about deb-2 first).

Absolutely, we will re-order the experiments to reflect the order of mention.

Figure 8. Difficult to read from the graph what the actual d_in value is with the logarithmic scale in panel a.

As mentioned in the caption, d_in spikes from a baseline of 4.5 mm yr-1 to 1 m yr-1. We will add some indication in panel a to make this more easily apparent.

L367: 'changes in ELA seem much more impactful than changes in debris input' -> Yes, but I think this statement should be interpreted within the context of your reference values. Maybe for another characteristic debris thickness value in the melt-modification parametrization or bedrock slope (which impacts ice flow velocity and hence debris accumulation on the surface), changing d_in could become more important?

We agree and will add a qualifying statement to the sentence about the dependence on our parameter set.

S3.4.2: Do you think this detachment is solely caused by irregular bedrock topographies? Could it be that this feature is more easily produced by your model if you incorporate the relative melt enhancement for thin debris? As before, I think this statement should be interpreted within the context of your model setting. By the way, why specifically choose this glacier? What makes it representative enough for applying your model to it?

The main factor is certainly the irregular topography. There would be a negative feedback when including thin debris melt enhancement: thin debris -> increased melt -> faster thickening of the debris layer -> decreased melt. While there is an effect of melt enhancement on the steady-state distribution of surface debris and on the transient response time (see attached figures and animation) the impact is not specifically local to the thin debris area itself, but rather that the glacier thins somewhat more quickly in general. We expect detachment in thin debris areas to only become relevant if englacial debris transport is (too) close to the base (i.e., when a large part of the ice column melts before debris thickness starts increasing).

We chose this glacier because there has been a lot of work (and a lot of data) on it in our group. All the authors have visited Zmuttgletscher many times and understand its dynamics very well, enabling a more informed (subjective) perspective on model results. It is not meant to be representative of anything, but rather to show the effect of irregular bed topography on a similarly sized glacier compared to the default set-up.

We will again add a qualifying statement about the context of the results.

Figure 10b: add the to label whether this is glacier or debris volume for clarity.

Will be done.

Figure 11: this is the same experiment as the grey curve in Fig. 10, right? Maybe add in the caption for clarification. Re-arrange the labels of the vertical axis on the left, they almost touch each other.

Yes, it is the same experiment as the intermediate curve in Fig. 10. Will be done.

Figure 12: can you explain why the debris thickness profile is much more irregular (zig-zag) for the ELA=2800m case here?

Local debris thickness depends strongly on local surface velocity. In the region, where the debris-covered tongue of the 2800m-glacier is located, there are a lot more small-scale variations in the bed that impact velocities. We will mention this in the text explaining Fig. 12.

Figure 11 vs. 13: I'm a bit confused here because you say the ELA forcing is from SSP2-4.5 scenarios in both (a) panels of the figures but the ELA forcing looks different in both graphs (one goes up to 3000m and the other up to 3300m at time=3000yr)? Maybe it should be clarified more in the caption if I missed something here. Can you also add labels to the vertical axis of panels (c) and (e) in Fig. 13?

Figure 11 shows ELA variations that are initialized at an arbitrary ELA of 3000m for our idealized bed geometry, whereas Figure 13 is initialized at a lower ELA that leads to a similar sized glacier in this particular catchment. We will clarify the difference in the captions.

The vertical labels in c and e are missing because it is the same axis as b and d. We will still add labels to these panels for clarity.

L440: 'which has not been documented in any other study.' -> I think the paper of Anderson et al. (2018) demonstrated this behavior.

Anderson et al. (2018) found a similar effect, but under very different circumstances. Their re-advance occurs in their "rock glacier" state, where the ELA is above the debris input area and within the headwall, where the only accumulation happens through avalanching. We believe the two effects are not the same, but their observation might still be worth mentioning after this sentence.

L476-482 and L490-495: I think this can be moved to the limitations section.

We agree, will be done.

L588: Formatting of the in-text reference.

Will be done.

L596: Should also be mentioned in the limitations section.

Will be done.

L600: I think the point before '(ii)' should be a comma here.

Will be done.

L605-610: Why is that, do you have an explanation for this phenomenon? How much *mass is (not) conserved when you compare the debris input flux to the debris output flux at the terminus in steady state? You should be more explicit in stating whether your model conserves debris mass or not and illustrate it quantitatively.*

See response to major comment 3. Debris mass conservation is a larger issue with this kind of numerical implementation that has not been adequately addressed. Other approaches conveniently do not answer the question of where, why, and how much debris is lost in their advection schemes.

L620: nothing is mentioned here about the relative melt enhancement that could occur for thin debris…

See responses to L163 and S3.4.2. Mentioning this in three separate places would certainly be too much; we will evaluate where best (in 1-2 places in the text) to mention this.

L622-624: Should have been mentioned earlier (e.g. in S3.1.1).

Will be done.

L627: I think some more detailed processes could be mentioned here: for example, climate-dependent debris input rates (L490-495), simple geometry (what about more complex geometries and the omission of multi-dimensionality in your model?), coupling of debris thickness-melt modification to an energy balance model, spatially and temporally varying debris properties, supraglacial pond formation/meltwater redistribution or other cryokarst-type features, omission of subglacial debris entrainment and basal debris production in your model, omission of gravitational redistribution processes of debris (i.e. rock tumbling and stuff), or any adjustment of the ice rheology associated with the entrainment of debris fragments in the ice or heavy debris loads on top of the ice impacting the driving stress, other processes that can impact debris thickness (i.e. debris compaction/densification over time, falling of debris into crevasses, …), etc. These statements also deserve some references. By the way, concerning limitations, did you test resolution-dependency of your model results? I think this at least deserves a mentioning in the text.

We will extend this sentence to provide some more examples of future directions. But the goal of this section is not to give a full overview of research directions. We will reference Nicholson et al. (2021) here, who give a more detailed overview of the most relevant missing processes in models.

Resolution dependence: see response to L632.

L627: Imagine I want to apply your model to a specific glacier, what should I consider? Is this model easily transferrable and which input data do I need?

For application to a specific glacier, three inputs are essential:

- Bed topography along a flowline
- Climate history (e.g., ELA or DDF timeseries)
- At least a rough idea about the location of debris deposits on the glacier

Additionally, a local estimate of D_0, ice temperatures, or erosion rates could help constrain model parameters.

We will add a sentence summarizing these key inputs.

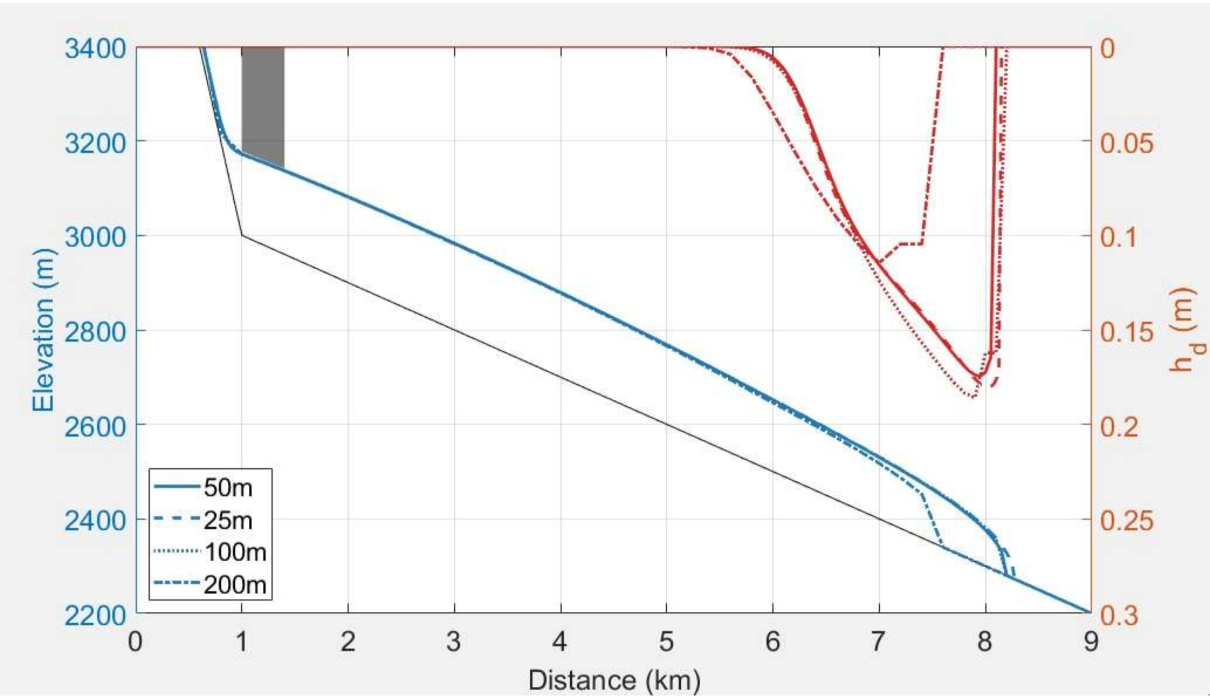
L630: Okay, but what specifically has been improved?

The new debris supply implementation enables temporally and spatially variable debris supply that is independent of climate. We will rephrase the sentence to explain this better.

L632: 'assess the numerical robustness' -> how? You didn't do any experiments related to numerical resolution-dependency and I think the debris mass conservation could be more explicitly mentioned.

We ran some additional experiments (and will run some more for a better and more detailed visualization in the revised manuscript) to test resolution dependence (see figure below), showing convergence (minimal differences, within a few grid points) for simulations at ≤100m, which cluster at very similar surface geometry and extent. Above 100m, model performance strongly decreases as the model struggles to resolve the small-scale debris input area and terminus processes.

We will add a supplementary figure to the Appendix, along with a mention in the main text. Comments regarding debris mass conservation are addressed at L605-610 and Appendix B2.



L635-656: which of these findings are new, and which confirm previous findings?

Going through all bullet points, identifying new and previously described findings:

1 Partially new (re-advance, for debris-covered glaciers, not rock glaciers)

2 New

3 New

4 New

5 Confirming observations & findings from modelling studies

6 New

7 New

8 New, second part confirms previous findings

We will make this distinction more explicitly.

L656: I think it is again important to frame your findings within the specific context of your model, maybe including relative melt enhancement for thin debris (for example for dead ice body formation) or a different terminal boundary condition implementation would result in (slightly) different conclusions?

We will add a qualifying sentence to the preceding paragraph (L629-634) that frames the findings within the context of the model properties.

L663: I'm missing here an overarching statement why your findings would be important and society relevant (for example glacio-hydrological and glacio-geomorphological processes, natural hazard management, water resources, etc.).

We will add a sentence that ties back to the general relevance that is mentioned in the introduction (long-term evolution of DCGs and understanding of process interactions).

Appendix B: Model 'robustness' -> so in the end, based on which qualifications or findings would you call your model 'robust' or not?

As stated earlier, we consider our model robust under the conditions that (a) spin-up is performed consistently and (b) certain setups are avoided. We are clear and transparent about the shortcomings of our approach, which we believe is essential to enable further applications.

Appendix B2: I think this is an important aspect of the model and should be moved and discussed more properly into the main text. Can you specifically quantify how much mass is conserved? How do debris input fluxes compare to output fluxes at the terminus in steady state (should be the same amount of mass is conserved)? Does the Smolarkiewicz scheme impact mass conservation?

We thank the referee for raising this issue and agree that it deserves more attention. We propose the following adjustments to the manuscript to address this:

We will mention the debris mass conservation problem early in the main text and give a better accounting of it in the Appendix. In short, we want to acknowledge debris advection as a difficult problem to solve. We know where the model breaks down, which is where we would expect it to. Other approaches are in some ways better but not perfect, and no other study points this issue out properly and transparently.

To quantify this problem: how much debris mass is conserved between input and output?

- At ELA = 2700m: 97.4% of debris mass conserved
- At ELA = 2800m: 98.0% of debris mass conserved
- At ELA = 2900m: 97.0% of debris mass conserved
- At ELA = 3000m: 90.2% of debris mass conserved
- At ELA = 3100m: 62.2% of debris mass conserved

- At ELA = 3200m (above debris input): 99.2% of debris mass conserved

The Smolarkiewicz scheme helps with debris band sharpness in the advection step. It is a standard choice for such problems. On a closed domain with no sources/sinks and no moving boundaries, there shouldn't be any mass loss due to discretization because of the flux form of the equations. However, Smolarkiewicz makes sure the debris keeps its original shape much better than not having it (i.e. plain upwind would also conserve mass in these equations in flux form but the debris would not keep its shape). However, Smolarkiewicz doesn't address mass loss issues due to the free upper surface, moving terminus, vertical coordinate that resizes, and source/sink terms at both ends - it just makes sure that the debris is close to the original shape when it gets to those problematic regions of the domain. So, to conclude, the Smolarkiewicz scheme does not negatively impact mass conservation, with the benefit of conserving the structure of englacial debris bands.

Fig. C2: Also, this is an important figure and should be kept in mind when properly interpreting your results. Should also be moved to the main text.

As mentioned in the response to the comment on L164, we will add a sub-figure to the main paper, but probably not with as much detail as here in Fig. C2.