The Cryosphere Discuss., 9, C3071–C3083, 2016
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The Cryosphere Discussions

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9, C3071–C3083, 2016

Interactive comment on "Modeling debris-covered glaciers: extension due to steady debris input" *by* L. S. Anderson and R. S. Anderson

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Received and published: 26 February 2016

Thank you for your detailed, helpful comments. We appreciate your time dedicated to this manuscript.

>a) Advection of debris: I do not understand the formula/calculation of the near surface debris concentration Co, nor where it comes from: -Firstly, Co depends on the number of grid-cells in the vertical, which makes no sense, unless Co is the total mass of debris per vertical grid-cell unit but then the units do not fit.

Thank you for this observation. The wording certainly needs to be cleared up in this paragraph. Co is the concentration of debris in the surface-bounding englacial cell with units of (kg/mˆ3).

We have added a sentence: “Co is the mass concentration of debris in the surface-bounding cell” after the definition of Co.

>Secondly, surely the concentration of debris in the accumulation area should at the surface depend both on debris deposition rate AND accumulation rate of ice (snow). For example if ice accumulation is increased for the same debris deposition rate dˆdot the debris concentration should be lower.

We agree with your statement. But because the englacial grid is coarse compared to the processes occurring at the surface of glacier (in the accumulation zone) we simply add debris into the upper most englacial cell as in the equation for Co. This model neglects the processes of firn compaction and snow metamorphism in the accumulation zone.

>Thirdly, Co does not seem to have the units of the concentration C (kg/mˆ3) used lower down. So I really do not get what is done with debris concentration at the surface boundary in the accumulation area, in my opinion bz (accumulation rate) should also be relevant and be included! Should be clarified! Anyway, however it is done, as bz is constant with time (steady state case) I guess all the conclusions are qualitatively not really affected.

Co does have the same units as C (kg/mˆ3). Ddot [=] mm/yr, rho_rock [=] kg/mˆ3, dt [=] yr, H[=] m. There is a dx in the numerator and denominator that cancel out.

>Further, am I right that the concentration here is a mass concen- tration (kg/mˆ3) rather than a volume concentration (%), maybe should be made more explicit?

Concentration is the mass concentration and has been added to the text at P6434 line 23 and line 24. We have added a sentence: “Co is the mass concentration of debris in the surface-bounding cell.”

>b) Advection equation for debris: I assume this equation (14) for concentration is ok, but I am a bit confused about it, as I thought one should be able to describe it by a

simple advection equation. It probably is that but it is written in the zeta-coordinate system with vertical gridsize h_zeta changing along flow, so I am just not familiar with it.

We added a simple advection equation above the equation we use in our manuscript to make it clear that the equation with the zeta coordinate system is used in the model. We also expanded the explanation of this equation.

>Further, I thought that an ice parcel with a certain concentration will keep this concentration all the way while it is advected, but of course it will be stretched or sheared or vertically extended on the way but within the parcel the concentration should stay constant (or am I wrong here?). This means if ice with debris of a constant concentration is deposited over a certain area on the surface in the accumulation area, this will be advected through the glacier as a band of constant debris concentration, although this band can be thinned or extended vertically. Ice is incompressible and the debris particles are fixed within their ice packet thus within the band I expect constant concentration (or am I wrong here?).

You are indeed correct here and we have edited the text to remove the word 'parcel' and instead discuss an englacial cell. Changed P436 line 1.

>I know that numerical diffusion can be an issue in advection schemes but this would be at the edge of the margin of the debris band and the authors seem to have accounted for that. From looking at fig. 5a) I guess eqn. 14 seem to do what I expect it to do, but from the formulation and the text explanation I am not able to fully follow it, so maybe could be clarified a bit.

There is still some numerical diffusion in the model. It is just greatly reduced compared to what would occur without the diffusion correction scheme. We are not sure how to quantify the effect of numerical diffusion so we follow Smolarkiewicz's lead and note that "numerical diffusion is greatly reduced."

We added a note that numerical diffusion is greatly reduced by using the diffusion correction scheme in the implementation and numerics section:

“This iterative scheme imposes a two-step anti-diffusion correction algorithm to the advection scheme which greatly reduces numerical diffusion (Smolarkiewicz, 1983).”

>Further, and somewhat related, from methods I understood that debris deposition over the area d_width is constant, so near the surface debris concentration (along the sur- face) should be almost the same (constant), but this is not the case in Fig. 5a, it looks as if it has been smoothed out (or diffused). Did I miss something here?

Part of the smoothing visible in Fig. 5A is due to the ’contourf’ plotting tool in matlab. The other portion is related to a bit of numerical diffusion. Because the width of the debris deposition zone effects the response of the glacier to a very small degree we are confident that a minor amount of numerical diffusion does not effect the validity of our results.

We have adjusted the text when discussing the numerical diffusion scheme to note that it only reduces the diffusion, but does not eliminate it.

>c) Debris flux at snout: I understand the reason of the extra flux divergence term for debris transport at the snout (dflux_snout/dx) but I do not understand how it is technically implemented (also not from Appendix B). In particular I do not understand, to what location/area the ‘snout’ exactly refers to. Is it the last two gridpoints of the glacier (last ice covered and first ice-free?)? or is it a fixed length-area measured from terminus? For the former it would then be gridsize dependent (the authors may address this or a similar issue in the appendix A). So this should really be explained in some more detail, maybe in a sketch. In particular: at which locations (grid points) is eqn 16 being used for example and what and where exactly is the ‘snout’. Clarifying this is important as the analysis in Appendix B (and fig B1) shows it is important for the length evolution.

Thank you for catching this. We agree that the explanation needs to be improved. We have removed the term 'snout' and replaced it with terminus wedge. We have included a new figure (A1) to explain how the terminus wedge is implemented and where the terminal debris-flux is removed from the glacier. We also note that we are implementing a terminal wedge parameterization on p6436 line 19 and reference Appendix A. Several new sentences were also added to Appendix A. We appreciate this catch!

>d) implementation and numerics there could be a bit more information on how the debris thickness and advection scheme is numerically solved. More specifically: -I assume the debris thickness equation (15 and 16) is solved in the same way as the ice thickness equation (1) with a second-order Runge-Kutta difference scheme -what is used for the debris advection scheme (eqn 14), a 'correction-method' is given here (Smolarkiwicz) or is this already the whole advection scheme

We use the iterative "upstream" advection scheme of Smolarkiewicz, 1983 which limits strong numerical diffusion.

We updated the manuscript text to reflect the above sentence:

"Next, we use a second-order Runge–Kutta centered difference scheme to evolve H(x,t), followed by the implementation of an iterative "upstream" debris advection scheme following Smolarkiewicz, 1983. This iterative scheme imposes a two-step anti-diffusion correction algorithm to the advection scheme which greatly reduces numerical diffusion (Smolarkiewicz, 1983)."

>what boundary condition has been chosen for the ice flow at the upper end of the glacier (x=0)

We added in the 'implementation and numerics' section that there is a no flux boundary condition at (x=0): "We impose a no flux boundary at the upper end of the glacier."

>e) Figures 8 and 9(A+B): I do not find the labelling of the d-loc variation very effective, it is hard to see how dloc is varying, in which direction and by how much. Maybe using

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colored dots/lines wit

Great suggestion. Both figures 8 and 9A have been modified as you recommend using marker width.

>Line 3: strictly speaking it is the mass balance gradient in the ablation area, or maybe it is rather the '. . .ablation rates can be reduced. . .'

We have changed 'the mass balance gradient' to 'ablation rates' as you suggest.

>For introduction and discussion in general, the very recent Rowan et al (2015, Earth and Planetary Science Letters, 430, 427-438) maybe relevant

Thank you for the note. We have included this paper in both the introduction and discussion. We unfortunately did not see this paper before we submitted the paper but we have cited it several times in the text now!

Added citations p. 6426 line 18. We also cite the paper in reference to future research noting that the Rowan et al., 2015 paper is running a model that takes into account the planview dimension of glaciers.

>Line 94 and 95: regarding the use of SIA for modelling glacier geometry evolution the intercomparison study of Leysinger and Gudmundsson JGR (2004, Vol 109, F01007) would be relevant here as it demonstrated the validity of such a simplification on modelling glacier evolution (comparing SIA with a full system flow model).

Good suggestion. This citation has been added.

>Line 133: I guess the authors refer to exponential curve fittings here as other studies have used such fitting, so it would be useful to add these references. Otherwise it is not clear why exponential is relevant here. (similar on line 427).

We added several references for folks using an exponential curve fit: "(e.g., Konrad and Humphrey, 2000; Hagg et al., 2008)"

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>Line 158: a very minor point: but these ‘other sliding relations’ have a theoretical physical basis behind, maybe some reference to such other models could be given.

This is true. Our results here are not sensitive to the selection of sliding parameterization. We added a reference to Cuffey and Patterson, 2010 here as you suggest.

>Line 162, eqn 8: it is not clear to what ‘u’ is referring to here. Is it the vertically averaged velocity, the surface velocity or the basal velocity. Should be clarified.

’u’ here is the vertically averaged ice velocity.

This has been added to eqn. 8 and the sentence following it.

>Line 171, eqn 10: it is not clear how u_coupling is determined/calculated, eqn 8 only refers to how tau_bx is modified. Is u-coupling actually used (and relevant) for calculating the vertical velocity profile? Or is u-coupling determined from substracting udef from u-total?

u_coupling is determined from subtracting u_def from (utotal-u_sliding).

We added an explanation on p. 6432 line 5.

>Line 178: Is this equation referring to the deformation velocity (udef)? (see explanation in next point). Also not clear how u_coupling is integrated into this.

See comment above.

>Line 180-181, eqn 12: I might be wrong here, but I think w=0 is not the correct boundary condition is there is basal sliding on a slope, then there is vertical component from the along bed sliding velocity. I guess this bed parallel vertical component from sliding has been subtracted already here. Should be clarified.

w=0 at the bed assumes no melting. the ice has to remain in contact wight the bed. the sliding is taken into account by the fact that it is not u but du/dx that counts in the integration, and hence any gradient in sliding generates a gradient in vertical velocity.

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but the bottom b.c. remains w=0.

>Line 192-197: maybe some typical values for headwall erosion could be given here.

We have included examples of headwall erosion rates as requested. “(typically ranging between 0.5 and 2 mm/yr)”

>Line 206-207: a detail on terminology, I do not think all the these debris deposition variables all need a dot on top, for the debris deposition RATE dËĘdot I agree, but for d_width or d_loc it is not referring to RATES, and if the authors insist on the dots, the d_flux should for consistency have one as well (here it is actually a RATE).

Thank you pointing this out. The dot has been removed from debris variables that are not a rate and added to d_flux term throughout the manuscript and figures.

>Line 210: where do these values of deposition rates come from???

We consider these to be viable deposition rates based on viable parameter inputs to equation (13) as noted on lines 2015-2018. We explored deposition rates up to 32 mm/yr but the results did not add to the patterns shown in the results so we limited deposition rates to 8 mm/yr.

“The deposition rates explored in this study are viable based on headwall erosion rates (typically ranging between 0.5 and 2 \unit{mm\,yr^{-1}}), headwall heights, and headwall slopes for high-relief mountain environments (e.g., Heimsath and McGlynn, 2008; Ouimet et~al., 2009; Scherler et~al., 2011; Ward and Anderson, 2011).”

>Line 228 (see main comments above (a)): something odd about this definition of Co

See our comments and corrections above.

>Line 250: should maybe refer to appendix B here.

The reference is added.

>Line 250-258 (see main comment (c) above): not clear to me to which

area/location/gridpoints the ‘snout’ (and its equation 16) applies. Sketch?

See above also. A new figure as been added to the appendix and appendix A has been modified.

>Section Implementation and numerics (see main comment (d) above: some more details on numeric needed.

See above response.

>Line 277: This is just my personal opinion, but not crucial: I find it not that useful in giving the location dloc as percentage of the non-debris covered glacier length as in nature such a length is usually not available, so maybe it would be better to relate dloc to the ELA position. Anyway, it does not change anything.

This is an good point. But it is not clear how we would define d_loc below the ELA without using the debris-free glacier length. We left these percentages as is.

>Line 289-290: again not that crucial: M_input is the ‘cumulative’ mass that has been deposited/added, so I would rather say something like ’. . .where Minput is the total rock mass deposited on the glacier and accumulated over time,. . .’

’and accumulated over time’ was added at line 289. Thank you for the note.

>Line 293: I guess the base run is not the most representative example for testing (showing) debris mass conservation as the englacial part is very small, the case of dloc=7% (fig 5a) maybe would have been better. But it seems the authors tested this for all cases anyway and the errors are still below 1%.

We agree that it would be better to show the case of dloc = 7% for the debris mass conservation plot in figure 4 but we decided to show the case from the base parameter set instead for consistency. The model does conserve debris as you note.

>Line 304 (and some figures): a small detail: not so clear to me why they use the letter epsilon for this debris emergence position, epsilon has already been used for

backweathering rate. It is a position so ‘x’ with some subscript maybe more useful.

Also a good suggestion. We have changed the symbol for the epsilon for this debris emergence position to x_emergence_int.

>Line 335-345, section 4.2.2: From line 336 I take that the authors would like to investigate the relative importance of d and dwidth, which they do by an extensive sensitivity study in which they vary them independently. The issue is that dflux is also changing for variable d and dwidth. If the relative importance should really be addressed in detail I would keep dflux constant while varying d and dwidth (and plot it this way).

Good idea. Figure 9 has been revised and now only has 3 panels. 9B now clearly shows the effect of changing deposit width and deposition rate using color and marker size.

>Line 370-376: It maybe useful to already here mention that in the model the width does not vary along flow where as in reality the width in the accumulation area is often much wider which of course affects AAR.

We added that our ssdf glacier has an AAR of 0.5 due to no width variation along the flow. As suggested.

>Line 383: related to above: I would add here. ‘. . . has an AAR of 0.5, due to no width variation along flow.’

The sentence has been modified as you suggest.

>Line 408: here high dependence of time evolution on dfluxĚĘsnout is mentioned but this model investigation has never been presented or mentioned before in the results/text, it is however in the appendix B. So it should be mentioned in the results that it has been undertaken (but refer to appendix and fig. B1) and then here a reference to the appendix B and its figure B1 should be added.

Agreed. A sentence referring to these results was added in the results section p. 6437

l. 6-7 The reference to the appendix/B1 is added in line 408.

>Line 422: I guess here it should be clarified that for the '2dim-case' dloc is of secondary importance (I expect for 3d it may different).

A reference to the 2D case has been added in line 422.

>Line 427: again, it would be useful to add a reference of studies who have used exponential curve fittings, otherwise why is exponential relevant here. (similar on line 133)

A couple of references were added.

>Line 463: after '. . .removal from the toe' refer to (see Fig B1 Appendix B)

Line 463. The reference has been added.

âĂŃ>Line 463: remove 'a' before 'high melt rates'

The 'a' was removed from line 463.

>Appendix A (in particular lines 525-528: I struggle to understand this 'gridsize dependence', this should be explained better. What is meant by 'increasing dx from 100m to 200m?' change if grid size or an advance. . ..???

I wrote a few sentences to help clear up what we mean here:

"When the model is allowed to evolve from the ssdf glacier to the debris-covered steady state, debris is advected into previously debris debris-free cells on the glacier surface. In our model, the debris thickness h_debris(x,t) represents a layer of equal thickness on any cell. Debris thickens slower on the finite-difference grid with a larger dx because the debris advected into a cell is spread over a longer distance (due to the larger dx). There is therefore a timescale built into the thickening of debris in a cell that is dependent on dx. Because ablation rates are sensitive to debris-cover thickness, changing dx in the model has an effect on the evolution of the glacier.

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Interactive Comment

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Discussion Paper

In order to test the effect of changing the grid spacing in the x-direction on the steady state debris-covered glacier length we increased dx from 100 (used in all simulations outside of this test) to 200m. This test led to differences in steady state debris-covered glacier length which were less than 200m even when d_flux was varied."

>Appendix B: again (see main comment (c) above) the 'area/location of the 'snout' is not clear at all, maybe explain here first and add a sketch.

We made Figure 1A and expanded Appendix A to address this comment.

>Table 1: here slopes are given in % but in fig. 11 where different slopes are considered in the figure ratios are used. make consistent.

We changed the slopes to percentages in figure 11 to match those in the table.

>Fig. 5: would be useful to add a fine line at the elevation of the ELA. Further, explain in caption what dark grey dashed vertical line is (I assume the non-debris glacier lengths position.

These are good catches.

The ELA line has been made thicker. The vertical dashed line is now better labeled in the figure as well with an arrow.

>Fig. 6: the scale on the right of A-C is very small and as yellow very hard to read. I would increase the size of this figure.

The yellow has been changed to brown and the figure size has been increased.

>Caption: 'Modelled glacier changes. . .' is very vague. Why not say 'Modelled changes in ice fluxes, thicknesses and velocities due to. . .'. Further: figures D-E are not really explained, so add after '. . .shown in Fig. 6.'(D-E) Comparison of surface velocities and ice thicknesses for the debris covered and debris-free cases.

The suggested changes were made in the figure 6. caption.

>Fig. 8: I do not find the labelling of the d-loc very effective. For (a) it seems ok but for (b) the labels are far from the arrows. Maybe using colored dots/lines with a color scale for dloc would be better, it is already a color figure anyway.

Figure 8 has been updated using the marker size to represent d-loc more effectively.

>Fig. 9 A+B: again the same issue as in Fig. 8, it is even harder to see to what dloc the different lines refer to. Maybe using colors would address the issue.

Fig. 9 A has been updated using marker size to represent d_loc. Fig. 9B in the original manuscript has been removed.

>Fig. 10: why having shifted y-axis on the left. Could one not use one axis on left and one on right?

One axis was shifted to the right as you suggest.

>Fig B1: not so clear what the blue arrow refers to. Does it mean from the onset of the arrow down no steady state is reached (continues to advance?).

This has been removed from the figure and a sentence has been placed in the caption instead.

Interactive comment on The Cryosphere Discuss., 9, 6423, 2015.