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Interactive
Comment

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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This paper introduces a new glacier evolution model that includes a fully dynamic treatment of debris cover and thereby allows a quantitative investigation of interactions between glacier dynamics and the influence of debris on surface melt. With this model then a very detailed sensitivity analysis of the effect of debris cover on steady state geometry of glaciers is undertaken. Although, the insulating effect of debris on surface melt has long been studied, the interaction with flow dynamics has up to now not really been properly addressed, at least not in a quantitative way. This publication exactly addresses this issue and thereby provides a firm basis for better understanding the dynamics of debris covered glaciers and has implications on the interpretation of glacier evolution and change and on paleo-glacier/climate reconstructions. In the current context of global warming, the related rapid glacier recession and the large uncertainties for debris covered glaciers, this paper is a very important and significant contribution with wide ranging implications. The modelling sensitivity work are also set into context of a real-world data set. This paper is focussing solely on the steady state case which I think is fully justified as a detailed understanding of the steady state is important (and complex enough) before considering the case of climate change. I would encourage the authors, however, in the future to also look at the time transient case. The paper is in general very well written, is well illustrated and mostly well explained and it integrates the existing research and literature. Clear conclusions are made that are well supported by the undertaken research. The methods are in general well explained and sound, although in the methods on the debris incorporation and advection (and numerics of it), i struggled a bit to follow (see details below) and some clarifications maybe needed. Also some figures (fig. 8+9) could perhaps be a bit more effective. Overall this is a very valuable paper, that provides an important, novel and timely contribution to the relevant topic of the dynamics of debris covered glaciers and I therefore highly recommend to publish this paper after addressing the issues/comments listed below (more substantial and minor comments).

Main more substantial comments

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. -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. -Thirdly, Co does not seem to have the units of the concentration C (kg/mˆ3) osed loer 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. Further, am I right that the concentration here is a mass concentration (kg/mˆ3) rather than a volume concentration (%), maybe should be made more explicit?

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. 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?). 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.

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 surface) 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?

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 techni-

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cally 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.

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

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 colored dots/lines with a color scale for dloc would be better, Fig 8 is already a color figure anyway.

Minor comments:

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. . .'

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

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)

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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).

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)

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

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.

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?

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

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 substracted already here. Should be clarified.

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

Line 196: 'The model replicates. . ..', it is not clear to me to what 'model' the authors refer to here, the models of the authors above or their model for surface debris deposition. . ..?

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 dˆdot I agree, but for

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d_width or d_loc it is not revefering 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).

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

Line 223 (see main comments above (a)): something odd about this definition of Co

Line 250: should maybe refer to appendix B here.

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?

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

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.

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,. . .’

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%.

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 maybemore useful.

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).

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.

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

Line 408: here high dependence of time evolution on dflux^snout 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.

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).

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)

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

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

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....???

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.

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

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.

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. 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.

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.

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

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?).

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