The spatially distributed nature of subglacial sediment dynamics: using a numerical model to quantify sediment transport and bedrock erosion across a glacier bed in response to glacier behavior and hydrology

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Referee's report

Well, the title is a mouthful. How about 'A numerical model of subglacial sediment transport'? It would shorten the paper considerably.

The paper describes a model for subglacial sediment transport, implemented as a numerical code. For reasons which will emerge below, I think this paper should be rejected. However, I suspect that this is a rather unfashionable view, and certainly there are many examples of cellular computational models which have been published, notably in the field of hillslope evolution (Willgoose, Tucker, etc., etc.). Whether such an approach is justified in the present instance may be a matter of opinion. From my perspective, however, there is nothing I can usefully learn from this paper.

Already in describing water flow, the basic physics is shelved. A Röthlisberger-type theory for channel flow involves an evolution equation for cross-sectional area, and thus the hydraulic radius; this is avoided here by parameterising the hydraulic radius. But actually, I think it is worse than this. Eventually, the model is applied to sediment transport beneath both glaciers and ice sheets. We know that there are R channels under glaciers, but then most of the erosion is elsewhere; how is it thought the sediment gets into the channel? Eventually we get bedload or suspended load, but these concepts are for rivers. Although this is a model, it does not seem to be one which engages with physical principle.

I think the Exner equation (4) is muddled. The relaxation length l should not be there. The Exner equation is just $H_t + \boldsymbol{\nabla}.\mathbf{Q} = m$, and then $\mathbf{Q}$ has to be prescribed. Commonly one just takes $\mathbf{Q} = \mathbf{Q}_b(\tau)$, but if one wants to include the relaxation length, then one can take (in one dimension) $lQ_x = Q_b - Q$, as is commonly done in modelling dune formation (e. g., Kroy *et al.* 2002, equation (6)). In two dimensions, you would need a bit of differential geometry. The basal stress is a vector $\boldsymbol{\tau}$, and if $\mathbf{T} = \dfrac{\boldsymbol{\tau}}{|\boldsymbol{\tau}|}$ is the tangent unit vector along a (water) flow line, then you would have $\mathbf{Q} = Q\mathbf{T}$, and $\mathbf{T}.\boldsymbol{\nabla}Q = Q_b - Q$, I suppose. Equations (5) just look silly.

I'm very surprised to see the exponent 5/2 in equation (7). Most of these bedload transport laws have 3/2. I don't have the Engelund-Hansen report, but in his 1970 JFM paper, he uses Meyer-Peter/Müller (and doesn't reference this report). Ah, I see reading on (140) that there is a reason for this, as it supposedly includes suspended sediment. Of course, a proper treatment of suspended sediment then requires an evolution equation for the suspended sediment concentration. It seems to me that if you go to the trouble to include bed erosion to the bedload layer, then it is logically commensurate to include suspended load concentration, at least in some fashion. But again, we must be thinking of streams, and then such streams do not cover the glacier bed; how should sediment transport to the streams be modelled? The statement "the continuous nature of the relationship improves the model stability" is poor. First you pose the model, then you deal with trying to solve it. You don't decide what is in the model on the basis of what you can solve (or you shouldn't).

At equation (11), I begin to wonder what is the point of this exercise. Yes, what happens at the bed is complicated. But to choose the sliding velocity to be a fixed fraction of the shearing velocity is simply making things up. Particularly, sliding depends on the subglacial hydrology through its dependence on the effective pressure. One might argue that the emphasis here is on sediment transport, but that fundamentally depends on water flow (and also actually till deformation), and I see little point in trying to deal with one at the expense of the others, at least if the results of the model aim to be realistic.

As we come to the numerical implementation, I belatedly realise that the point of all the simplifications to the physics is that it allows a cellular model to be constructed. It reminds me a bit of the paper by Barchyn *et al.* (2016). I am not a big fan of this approach, which seems to me to be motivated by the wish to produce pretty pictures at the expense of doing science. A model is only as good as its formulation, and I find the modelling here to be weak in a number of points.

This point is perhaps illustrated by the comment at 371, the 'model successfully captured the inter-annual variability in sediment discharge from the Griesgletscher'. But a parameter search was used to find parameter values which worked. So can we conclude that the model is a good one? No. Does it then have predictive value? No. And, most importantly, should we expect it to be a good representation of physical process? In view of my comments above, I would have to say no.

Smaller points:

At equation 1. This looks a bit odd to me. In my way of thinking, for force balance in a channel, you would have $\tau l = \rho g S A$, where τ is the stress, l the wetted perimeter, S a suitable slope and A the cross-sectional area. So $\Psi = \rho g S = \dfrac{\tau l}{A}$, and if $\tau = f\rho u^2$ and $Q = Au$, then $\Psi = \dfrac{f\rho Q^2 l}{A^3}$. You can get equation (1) if $l \propto A^{1/2}$, as for a circular or triangular cross section; but it seems to me that equation (1) is not the basic law. For example, a wide stream has $l \propto A$, approximately.

Line 105. This is unclear. D_h is a length, not an area.

115. and $\rightarrow$ or, presumably.

181. The wording here suggests that a partial differential equation is being solved, but if I understand this correctly, this is not the case. (4) with (5) form a set of ordinary differential equations.

Reference

Barchyn, T. E., T. P. F. Dowling, C. R. Stokes and C. H. Hugenholtz 2016 Subglacial bed form morphology controlled by ice speed and sediment thickness, Geophys. Res. Lett. **43** (14), 7,572-7,580.

Engelund, F. 1970 Instability of erodible beds. J. Fluid Mech. **42** (2), 225-244.

Kroy, K., G. Sauermann and H. J. Herrmann 2002 Minimal model of sand dunes. Phys. Rev. Letts. **88** (5), 054301.