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Comment on esurf-2021-88

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Referee comment on "Modeling the spatially distributed nature of subglacial sediment transport and erosion" by Ian Delaney et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-88-RC2, 2022

As a preliminary note, I would like to state that F. Herman, who is last author of this manuscript, was also last author of a paper where I was involved (Prasicek et al., EPSL, 2020). However, I never met the first and second author of this manuscript in person, and there is no further connection. So there is no conflict of interest in any direction. As a second note, I would like to mention that I have not worked in the field of glacial erosion and sediment transport for a long time. So I may be not familiar with some of the concepts used in modeling glacial landform evolution. In turn, I have worked with fluvial models and hydrogeological models for a long time and believe that my mathematical background might be helpful here.

Owing to my background, my review focuses on the methods part, while I wrote only a few remarks on the results and discussion sections.

The topic of subglacial sediment transport is important in the field of glacial landform evolution, and it is perhaps the component with the biggest gaps in knowledge. In my own work, I used simple analogies from fluvial sediment transport with almost no validation by real-world data or by more elaborate models. So I find this topic very interesting, but I am quite sure that it is interesting and potentially important for the glacial erosion community.

On the other hand, the description of the theory and the numerical part falls much behind my expectation on the group of authors and also much behind the 2019 JGR paper about the 1D version. At some places, it even looks as if it was wrong. So think think that considerable parts of the theory need to be rewritten.

(1) Sect. 2.2

The main part where I am not immediately convinced that is is correct is Sect. 2.2 about sediment transport, starting from the balance (Exner) equation (Eq. 4). In its genuine form, such an equation would be written in 2D in terms of a sediment flux per unit width (m^2/s) instead of the sediment flux (m^3/s). In the 1D version (Delaney et al., JGR, 2019, Eq. 9), the sediment flux is used in combination with a channel width w. However, the version introduced here uses a length scale l that describes a "characteristic length-scale for sediment mobilization, over which sediment mobilization adjusts to sediment transport conditions." This length scale is a longitudinal length scale (in flow direction), while the formulation of the balance equation in terms of the flux requires a length scale perpendicular to the flow direction (such as the channel width). So a am quite sure that Eq. 4 is not correct in the form it is written, but I cannot assess whether it is correct in the implementation and whether it affects the results in case it is not correct.

There also seems to be a problem with the physical dimensions in Eq. 5 (beyond that the divergence of the sediment flux at the left-hand size should not be called "sediment discharge"). If dot(m)_t is indeed an erosion rate (m/s) as defined earlier, it is not consistent with the other properties, which are fluxes per length (m^2/s).

(2) Sect. 2.3

While I am confident that the numerical implementation is sound, I am not happy about the way it is described in this section. To be honest, I found it even more confusing than enlightening. The cited work by Bovy et al. (2016) used a standard finite-volume discretization of the fluxes, while the algorithm proposed by Braun and Willett (2013) used a single-flow-direction (D8) scheme (if I am not wrong). I read that you use a multi-flow-direction scheme, but it is not clear what the difference toward typical continuum schemes (finite volume) is or whether it is the same. Instead of (or in addition to) mentioning functions and software packages, I would ask you to state the equations that are finally solved.
(a) How exactly is the flux (water/sediment) distributed among the available directions? It looks as if this changes trough time, but I did not get it completely.
(b) What does "integrating Equation 1" (line 166) exactly mean here? Typically, "integrating" a differential equation is somehow one-dimensional or upstream in a tree, like the algorithm made popular by Braun and Willett (2013). However, if there are multiple flow directions, upstream paths may meet again, so that you would end up at different values of phi by integrating over different paths. I see that you are solving a system of equations, but I cannot see how.
(c) It would be good to state the balance equation for the water and for the sediment for each grid cell explicitly and in such a way that it becomes clear which system of equations finally has to be solved.
(d) Also about the discretization: I guess you are assuming one single conduit of a given hydraulic radius for each grid cell. This may be questionable, but of course not necessarily bad. A similar assumption is typically made in karst evolution models. It should be discussed at least briefly. And is this conduit directed, or will it change its direction if phi changes?

There are also some more parts in the model description that should be clarified, where I think this can be done quite easily.

(3) Lines 104-105: Q*_w or Q_w? And I did not understand how the water pressures can increase to unreasonable values if the flux rapidly increases. I can imagine that this happens if D_h is small for a single-flow-direction model, but why do the alternative flow

directions not help here? (4) Line 137: Here you use "capital S" (the cross section area), which is definitely different flow "lowercase s" used in Eq. 1 (probably a nondimensional factor).
(4) Line 137: Here you use "capital S" (the cross section area), which is definitely different flow "lowercase s" used in Eq. 1 (probably a nondimensional factor).
(5) Line 144: As far as I know, the lower bound $l_{er} = 2/3$ mentioned here was obtained from a worldwide comparison of glaciers under different conditions. So I am not sure whether this low value is relevant for Eq. 10.
(6) Lines 144-149: Some other studies use the relation from Eq. 11 for the deformation velocity and a similar relation with an exponent $n-1$ instead of $n+1$ for the sliding velocity u_b. This relation predicts that deformation becomes more and more relevant if the thickness of the ice layer z_s-z_b increases. Using this relation in my own work, I was even told by reviewers that the sliding velocity cannot be predicted from the deformation velocity at all. I do not share this point of view, but I think it should be discussed why even a weaker assumption than the relation typically used is employed here.

I am not familiar to the test cases used in Sect. 3, so that I just list some points that came into my mind when reading it.
(7) Line 239: Why is there a need to increase DeltaT, and what is the value of DeltaT used here?
(8) Line 246: I did not get the point why H_{max} grows.
(9) Line 285: What is the meaning of the -5 in Eq. 16, given that DeltaT is an (adjustable) temperature offset?
(10) Lines 339-342: Nice numbers, but you somehow let the chance to analyze your model more thoroughly pass by. From Eqs. 7 and 8, we see that the transport capacity Q_{sc} is proportional to v^5 and thus also to Q_w^5. this is what you put into your model. So you could integrate Q_w^5 over, say 1 year periods, and look how well your sediment output correlates to this integral. If it correlated perfectly, then your sediment output was just what you put into your model equations, and everything else would be unimportant. I think it will not correlate perfectly, so that you can discuss which of the components of your model is important. However, this is just an idea how you could sharpen the discussion.

Please do not get me wrong -- it is definitely not my intention to tear down your work. However, there are already many modeling papers in the literature where readers cannot see clearly enough what was done and reproduce the basic ideas. As modelers, it is our task to act against this tendency. I am quite sure that this piece of work will finally end up at a very good paper, but you will have to spent some more work on it.

Best regards,
Stefan Hergarten