We thank the reviewer for the critical assessment of this work. While we disagree with many of the perspectives presented below, we also have considered them thoroughly. Broadly, it is our opinion that the reviewer's comments point to a need for additional explanation of our work. We address these items below. We have placed the reviewer's comments in bold, our commentary is in normal font, and the new text is in italics.

Best regards,

Ian Delaney on behalf of all authors

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

  This is a perspective shared by another reviewer, using other words. We have adjusted the title to read: *Modeling of the spatially distributed nature of subglacial sediment transport and erosion*.

- **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.**

  This comment points to shortcomings in the model description that Dr. Overeem and Dr. Hergarten addressed as well. For the sake of manuscript length, we chose to leave the complete description of the hydraulics model to the previous publication (Delaney et al., 2019), where it was originally published. Upon receiving the feedback here along with other reviewers we have decided to include the complete description of the hydraulics model in the manuscript. This has been added to the response to Dr. Overeem's comments. In this material, we describe how the hydraulic radius responds to hydraulic conditions below the glacier and is converted in to cross sectional area.

- **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?**

  Given the spatial resolution of this model, we assume that the amount of sediment available in a cell is available for transport, with the uptake of sediment limited by the till height H. This is described in Equation 6.

- **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 I should not be there. The Exner equation is just Ht + $\triangle \cdot$Q = m, and then Q has to be prescribed. Commonly one just takes** Q = Qb(τ), **but if one wants to include the relaxation length, then one can take (in one dimension) lQx = Qb 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** τ, **and if** T = τ **is the tangent unit vector along a (water) flow line, then you would have** τ Q = QT, and T.ΔQ = Qb Q, **I suppose. Equations (5) just look silly.**

We appreciate the comments regarding the Exner equation and Dr. Hergarten also brought these issues to light. We have now adjusted the Exner equation so that it is consistent, and the exact text is in the response to Dr. Hergarten's review. The adjusted equation and accompanying are also included in that response.

The reviewer suggests that Q = Qb(τ) results in sediment discharge. While this generally true in transport-limited situations, in supply-limited cases, sediment transport depends also on till height H. When till height is small, sediment is not available for transport, and sediment transport exists in a supply-limited regime. As our equation 5 (now 8) describes, sediment transport is reduced when H is small, to account for supply-limited transport. The dependence on till height H means that we must evolve the Exner equation (equation 4, now 7) in response to bedrock erosion and sediment transport.

- **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).**

  We have chosen in this version of the model to include suspended sediment and bedload transport together for simplicity and because the point of the paper is to demonstrate the importance of sediment transport in two spatial dimensions. We do not aim to parse the relative roles of suspended sediment and bedload transport, especially because the fraction of sediment created by bedrock erosion that is bedload or suspended adds more assumptions to the model. The statement regarding the continuous nature of the relationship, upon the reviewer's recommendation, it has been omitted from the manuscript.

- **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 discussed in the response to Dr. Hergarten, we have exchanged the fixed sliding fraction with a Weertman sliding parameterization based upon the driving stress, which has very similar erosional patterns and results compared to the fixed fraction of deformation. While sliding does depend on effective pressure, over the time periods needed to create substantial sediment at the bed for transport and on land terminating glaciers, Weertman sliding is largely accepted as being able to represent sliding velocities (e.g. Gimbert et al., 2021).

- **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.

We appreciate the critical review of our work. At the same time, the model accomplishes describing both bedrock erosion and sediment transport below glaciers and implements a routing scheme to transport this sediment across the glacier's bed in two spatial dimensions.

- **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.**

  Our intent in applying the model to Griesgletscher is to 1) demonstrate its applicability to real scenarios, and 2) to understand potential processes that drive subglacial sediment transport. In Section 3, we describe how the model fulfills these objectives.

  Indeed most models need some kind of tuning in order to be compared to data. Furthermore, by systematically calibrating the model to available data, we find that model performance heavily depends on the grain-size parameter, which is a strong influence on the sediment connectivity below the glacier. Thus we are able to use the model to suggest that sediment availability plays a larger role in sediment discharge from the glacier compared to other factors, such as the background bedrock production rate.

- **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.**

  Nowhere in the manuscript do we say that the model is "good," that it has "predictive value" beyond the dataset it is calibrated to, or that it is necessarily a "good representation of physical processes".

  In the introduction, we outline our objectives as:

  In this manuscript, we present SUGSET_2D, a two-dimensional subglacial sediment transport model. The model implements subglacial sediment transport and bedrock erosional processes presented in Delaney et al. (SUGSET; 2019). We apply a routing scheme to the model that transports sediment down-glacier based upon the hydraulic potential gradient. Synthetic test cases show the model's ability to reproduce known processes and yields insight into the spatially distributed processes responsible for subglacial sediment dynamics. Implementation of the model to existing glacier hydrology, topography, and sediment discharge datasets from the Griesgletscher helps to understand some subglacial sediment transport processes at this site that could be generalizable to other situations. Through these experiments, we discuss the impact of two dimensional sediment connectivity on subglacial sediment transport.

  In our opinion, the paper fulfills these objectives.

  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 I = gSA, where is the stress, I the wetted perimeter, S a suitable slope and A the cross-sectional area. So = gS = I, A and if =fu2 and Q=Au, then = fQ2I. You can get equation(1) if I $\propto$ A1/2, 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 I $\propto$ A, approximately.**

  This comment points to our decision in the previous manuscript to omit the complete description of the hydraulics model. This information has been added and explains the conversion of wetted perimeter to a cross-sectional area. In this way, equation 1 is the generalized law, with factor s depending on the relationship between the hydraulic radius l and the cross-sectional area S. The text is available in the response to Dr. Overeem's review.

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

The text has been changed to read:

We note that to prevent unreasonable water pressures when Q_w^* rapidly increases and D_h is small, the model limits the minimal cross-sectional area S to 0.5 m^2.

115. and to or, presumably.

Done.

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.

The text now reads:

We implement a regular grid for discretization. Spatial discretization must be substantially smaller than characteristic length-scale, l, in Equation 8. We then solve Equation 7 for till height H for given initial and boundary conditions in response to till production $\dot{m}_t$ and divergence of the sediment discharge Q_s using an explicit time integration scheme.

References

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