Subglacial and subaerial fluvial sediment transport capacity respond differently to water discharge variations

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Referee's report

The paper is concerned with the nature of sediment transport in subglacial channels, and its difference to that in sub-aerial channels. There is a three page introduction which references lots of papers, but I didn't really get much from this. The two models for subglacial and sub-aerial channels are presented in section 3.

The subglacial model is a lumped one, in which the effective pressure controlling channel closure is related to the hydraulic head drop Δh, which thus makes its appearance in both the closure equation (3) and the momentum equation (or force balance), equation (4). I am a bit suspicious about this, as also discussed in the small item (eq. 3) below. I am thinking of Nye's 1976 model as a framework here, although with an added source term in the mass conservation equation to allow for surface meltwater input, subglacial tributaries, and the like.

Now the time scale for changes in water flow is much faster than that due to channel closure, with the consequence that the mass flow equation is effectively at steady state. Further, the mass source term due to wall melting is very small *in the mass flow equation*, so integration of this equation just gives the water discharge in terms of the inflow. Then $v = Q/S$, so the force balance in (4) determines head loss as a function of S, and then (3) is a single ordinary differential equation for S.

The trouble is, that while the first term on the right hand side of (3) makes sense to me, the second does not, and the reason for this is that the closure term involves the effective pressure and thus the hydraulic head, but not its gradient, so I am a bit sceptical about this, because the structure of the Nye model seems to have been altered. You might say that the distinction disappears in the lumping, but it seems to me that if you have an upstream head h_- and a downstream head h_+, the first term on the right of (3) will involve the difference, but the second will involve the average, and there are *two* independent quantities involved. I suppose you can get around this by saying, oh, the outlet head or more accurately the effective pressure is zero, and perhaps that is what is done. But that is a bit disingenuous, because the outflow becomes open channel flow somewhere upstream of the actual mouth of the stream.

Now I went and looked up this Werder 2010 paper, and you can see in its equation (4) the same distinction I am making here. I suppose my overall view is that (I think) the Clarke paper intended to use the flow elements of resistors, etc., as ingredients of a larger scale flow path, and this lumping of a whole channel is an extremely coarse thing to do, and looks a bit like avoiding confronting the spatially-dependent physics simply because it's too hard. So I think you can rescue this aspect of the model presentation, but a bit more exposition would help, in particular the figure 2 or equivalent of the 2010 paper would help. And the model should come with some caveats, e. g., you could say that it is a simplistic and possibly unreliable model. To be fair, this is done in section 5.3, but that is too late.

One of the comments made is that because the sub-aerial model has an algebraic relation between width and discharge (equation 9), the relationship between velocity and discharge is also algebraic; but this is simply a choice of the model. In reality, the width of a channel will also evolve over (long) time through processes of bank erosion, and the use of equation 9 properly only involves a long term time average of discharge, so it is misleading to use it in a time-specific way. Actually, this is admitted at line 370, along with other limitations in the models.

Two kinds of numerical experiment are described in section 3.3, and the results of these presented in section 4. Presumably the input to the model runs is the (time-varying) water discharge, but that seems not to be stated in 3.3, which I would expect, although it is stated in the caption to figure 2.

The second half of the paper characterises the results of these experiments and there is an extended discussion. There is no doubt the authors have put a lot of effort into this, but my interest waned at this point.

In summary, this is one of those difficult papers to judge, because the effort involved is quite substantial and honestly applied, but the whole philosophy of the approach is, in my view, misguided. This is a coxless boat crew who are rowing up a backwater without a rudder, and have lost the main direction of the stream.

I can perhaps come to a conclusion if I imagine future researchers reading and referencing this paper. They might want to say, "Delaney *et al.* (2025) showed that . . . "; but they didn't. What they did was take two models of subglacial and sub-aerial stream and sediment transport, and study their consequences in response to measured hydrographs. So the conclusions are only as good as the models. But there are big conceptual holes in the models themselves; for example, Meyer-Peter/Müller is only one of several such relationships, and it itself is of its nature a steady state result, and its application subglacially has to be a shot in the dark. The subglacial model assumes particular channel shape, and furthermore, the choice of a lumped parameter simplification, while it can be defended to some extent, clearly steps away from an effort to provide the best possible description. The only way some kind of rescue act could be performed would be if the whole ethos were changed; if for example, outlet discharge and sediment transport were both measured, and for example, plotted in a phase plane where circular paths were obtained; that would then suggest hysteresis, and a model, even such a diaphanous one as this, would have some merit. But the model must serve the data, and not the other way round. I think this paper must be rejected for that reason.

Some smaller points (line numbers or equation numbers in parenthesis):

(33): I don't know what 'Following mass conservation' has to do with this.

(99-100): upon that of Clarke.

(102): presumably h_{ice} is the thickness of the ice, but the syntax is not clear, it could be the depth of the channel.

(eq. 3): it's probably in the Werder 2010 paper, but I'd like to see an extra comment about where this $\frac{\Delta h}{2}$ term comes from. Since the bracket represents $N = p_i - p_w$, the pointwise form of this second term would be $\frac{p_w}{\rho_w g}$, so it's not obvious to me where the $\frac{\Delta h}{2}$ comes from. Also in this light, it is worth elaborating the confusing term 'hydraulic head drop change' and defining what Δh is in terms of p_w. Actually, the more I think about this, the more suspicious I become. And in fact, at least when you're dealing with jökulhlaups, the discharge doesn't vary much along the length of the channel, and the consequence of that is that neither does the effective pressure. Of course, that may not apply here but I think the same principle applies. So maybe I am promoting this to a more substantive issue (as indeed now further discussed earlier).

(114): I had no idea what the central angle referred to, but evidently it is the angle subtended at the centre of the circular arc of the channel upper boundary; and then $\beta = 2\alpha$, where α is the contact angle at the channel edge, which seems a more natural quantity to use. Also I checked the algebra of equation (5), and got this formula, but with the factor of two in the numerator, not the denominator, assuming hydraulic diameter is twice hydraulic radius, the latter of which is area/perimeter, so please check this. Incidentally, I did also then check equation (7) and I agree with that, so I do think there is an error in (5).