

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

Author(s): Delaney and others

General comments

In my view the paper is interesting and relevant, and hence worthy of publication. I have a couple of general observations, followed by a list of minor suggestions.

My general observation relates to the structure of the main findings reported by the manuscript. In reading the Results and Discussion I was sometimes lost as to what I was learning that was new. I think this was mainly because the structure lacked a clear and logical progression from one general finding through progressively more specific findings. This may well stem an initial imperfect expression of the objectives. Here, most of what is novel seems to fall under Objective 2 and not under Objective 1. Indeed, isn't Objective 1 ('to establish whether sub-seasonal water discharge can co-vary with sediment transport capacity in subglacial systems') already established beyond reasonable doubt? To me, and I believe the introduction to this manuscript, it clearly 'can'... (note the objective refers to 'can it', and not to 'does it' [to which the answer is also almost certainly 'yes' but perhaps with a little more room for maneuver]). I would recast the objectives to reflect better the new material presented in this manuscript (closer to objective 2 in fact, which I feel could readily be sub-divided). Having considered this, I feel the progression of the Results could be far more accessible to the reader and allow the manuscript to focus on a few really key points (which I think are currently slightly lost in the density of the presentation).

Interpretation is presented within each Results section (e.g., see 'due to...' on L174). This continues throughout Results, and I would separate all of the explanation and hence interpretation out from the results.

There are some very important points in Results, which I would attach some key data to and make more explicit here and in the Conclusion and Abstract. I'm thinking for example of associating the (increased) hysteresis as a function of discharge with a headline % change in sediment transport capacity. E.g., "thus, a more variable, but typical, Alpine Q record can transport up to *% greater sediment per cumec than a less variable ice sheet-type Q" and/or "the greater variability in Q from Alpine than from ice sheet style Q can account by an offset of peak sediment discharge from peak meltwater discharge by up to * % of the cycle involved". At present, I feel the manuscript lacks this incisive illustration to bring the scale of the results home to the reader.

Less importantly, but something I'd like to check, is that the fundamental conclusions are applicable more generally. Since they derive from modelling based on two 'type' datasets, there may be a possibility that not all key findings hold for certain other variations of subglacial discharge pattern. I have every reason to believe they do, but I feel the manuscript should report that this has been done more widely and the key results/patterns still hold. I don't think there's any need to include any such results into a revised manuscript – but I would just make the statement that while, for simplicity of illustration, two typical but contrasting datasets are explored here, the model has been run with other data sets and the key do hold more generally. Incidentally, I would also include an explicit disclaimer early on that the analysis only relates to pressurized subglacial flow and not to open-channel flow prominent approaching the terminus of many valley glaciers etc.

Specific comments

Line/Location	Comment/Suggestion
Title	The title states the obvious from prior research and I would recast it to reflect more accurately the new and original contribution of this manuscript. Also, if the authors elect to not change the title I would change the existing wording to 'transport capacities respond differently'. Grammatically, this is not clear given the two capacities noted earlier in the title, but stylistically I think 'capacities respond' reads better than does 'capacity respond'. (I would change the whole thing anyway... Why not start by considering 'Sediment transport capacity response to variations in water discharge in pressurized subglacial channels')
3	'full' subglacial channels?
5	There is no need to mention 'over days' here (indeed, it is misleading anyway without quantification of how much change as a function of time). I would just leave it as changes slowly (or slowly relatively to variations in water Q).
7	'Sheet' is missing (the authors really should have picked up on this obvious typo)
8	This hysteresis causes (no need to qualify the type of hysteresis since the use of 'this' serves the purpose)
64	I think there is earlier use of transport-limited (in which case the definition should be presented there, and not here)
75-76 and 80-81	I'd remove summary of the results of this study from the Introduction. They are not yet established.
70-71	In phrasing objective 1 (in more than one place in the manuscript), shouldn't the dependent variable come first? Thus, I would write it as 'to establish whether sediment transport capacity can co-vary with water discharge in subglacial systems'.
79	Given the apparent relevance of the Alley et al. study, I feel it's key findings should be presented in detail here to provide more direction to the specific objectives of this study.
159	The parameter values are not purely random (but random within certain sensible ranges, no?).
171-	I get a little muddled right at the start here – not helped by the sub-heading, which I would recast as "Influence of subglacial channel size on the timing and variability of...". The section (and others following) also combines results with explanation (i.e., interpretation) which I feel also adds unnecessarily to the difficulty in following the progression. I would separate results from interpretation.
172	This narrative jumps straight in with the ambition to 'quantify the sources of increased variability...' but the nature of that increased variability has not yet be established or illustrated. This really needs building up more progressively – bringing the reader through with the first-order model results. Then go on to explore more and more detailed influences and relationships. Most of this is, in fact, in Figure 2 – but the reader is not guided through the fundamental relationships here.

173	Change rather than evolution? (elsewhere too)
Fig 2 caption	Is it really an arbitrary y axis scale? If it is truly arbitrary, does it even need a scale range?
179-183	Is this not already established? If so, it should be presented clearly in the Introduction and developed as necessary here. It is also interpretation.
Fig 3 (and Fig 4)	Shouldn't the dependent variable be plotted on y and the independent variable on x? I for one at least think this way around and therefore find these plots a little confusing.
	Is there a need for the axis labels to be at an angle rather than parallel to the axis? If not, I would reorientate them parallel in all cases.
193-4	This is interpretation
195	The reference to 'increased variability' needs to make it clear what property is being referred to. Like the previous sub-heading, this sub-heading is a statement of findings before those results have been presented. I would avoid this and present it as the less assuming: 'Influence of...'
205-6	Interpretation in Results
210-11	Interpretation in Results
220	Typo (subglacial)
Section 4.3	A lot of this reads more like methods and background modelling operation and/or equation terms than Results of the modelling. If it is all results then the key findings are difficult to pull out from the density of the presentation – perhaps some of it could be omitted or moved to supplementary?
Fig 5	I find the figure too small. If larger the font would be easier to read and the y axes could have the property written out rather than represented by symbols. I'm sure there is no need for it to be this small.
232	Typo? ('typesc')
235	The text: '...as used as proxy for...') doesn't make sense (or at least could be worded more clearly).
284 (maybe elsewhere too)	The wording should be "greater than" and not 'greater... compared with...).
297	"...itself dependent of..." doesn't make sense to me
298	I don't believe there is sufficient evidence to make the claim: 'While transport limited states likely do not occur at many glaciers'. In my own experience most glacier do have at least parts of the bed that are sediment-rich and hence have the capacity to be transport limited.
302-4	I would try to pull some key quantification out here and present it as a headline figure to illustrate the magnitude of the effect.
319	I'm not sure 'sporadic' is the right word here. Please check you mean this.
321-22	I see the point being made here but it's not black or white. Surely there is some control exerted by discharge; it's just that it is complicated by the additional forces in the subglacial scenario. The way this is presented here may be technically ok but the reader could be forgiven for coming away with the feeling that there is no solid relationship between Q and sediment transport capacity in these cases – which would not be accurate. I feel it would be particularly helpful if the effect could be quantified in some generalized or headline way in terms of the likely (or maximum if you

	prefer) effect on a typical alpine and/or ice sheet hydrograph. That figure could then also go into the Conclusions and Abstract.
394	Surely the relationship can be characterized a little more usefully than simply to state that it is 'incoherent'. This also relates to my point above relating to lines 321-22. Can this be characterized accurately or systematically so the reader has a little more to learn in terms of the nature and magnitude of the effects?
407-10	It would not do any harm here to point to advances in subglacial sediment tracking, which speaks to this issue. I am well familiar with Jenkins' work – but there may well be others too.