April 2023 review of Paster et al.: 'Channel evolution processes in a diamictic glacier foreland. Implications on downstream sediment supply: case study Pasterze / Austria'

In this manuscript, the authors aim to establish the dynamics and future trajectory of a glacier foreland. They place this case-study in the context of global warming and a key conceptual framework (the sediment cascade approach). Topographic data and surface grain size distributions were used to numerically model hydraulics and bedload transport. In conjunction with projections of future glacial runoff until 2050, these analyses were used to estimate current processes and predict the evolution of the foreland channel. The authors predict the erosion of finer sediments will lead to armouring and stabilisation of the channel, and propose improvements to the sediment cascade approach.

I will mention that this manuscript is not from my immediate field of expertise, however, I have provided comments as a fluvial geomorphologist on what may be required for this manuscript to be satisfactory for publication. At this point, I cannot recommend this for publication as it requires significant work in order to be suitable, and I recommend it be resubmitted in a more advanced form. This is a topic that is potentially interesting to the readership, and consequently I have provided general comments and recommendations that address the current limitations of the manuscript. I encourage the authors to carefully address the points below, and I am of course willing to re-evaluate this manuscript once this has been achieved.

General comments and recommendations

I have two general criticisms here, although they are related. First, there are insufficient data to draw strong process-based conclusions, let alone predictions about the future evolution of the system. This is made more challenging by this being a case study, which would require an especially high-quality dataset in order to contribute to the literature (and to a well-established conceptual framework). This can be broken down into three aspects:

- From my reading, UAV and sediment sampling were conducted in 2018, there was a comparison with a 2015 orthophoto, and over some time photos taken from an automatic camera. This provides a limited temporal comparison but also there is also little contextualisation of these data. More data are required on the history of this area to establish the oscillations (for example, seasonal and annual) at this site as well as its evolution over time. This is particularly important for the study as proglacial environments are highly dynamic over several timescales.
- The manuscript details that different surface grain size methods were used at different sites, but I could not see a comparison of these methods at the same site. This undermines the comparison. There needs to be a more convincing demonstration that differences in surface grain size distributions (Figure 3) are not simply due to differences in the sampling methodology. This may be more easily addressed compared to the above point.
- There was a reasonable quantification of error for the DEM preparation. However, there was not an adequate quantification or qualification or error and uncertainty in other

measurements, notably sediment sampling, hydraulic modeling, and bedload transport estimates. This makes it difficult to assess the results, for example, the predicted mobile D50 vs measured D50 present at the site.

Second, the link between the actual research conducted at the site and proposed improvement to the sediment cascade approach is tenuous. This site may offer some insights into such a conceptual model, although the current dataset and analyses do not currently allow for this due to the reasons outlined above. Lastly, at several points the language and expression need to be revised throughout for polish and clarity about the research findings. However, this is mostly editing, and can be resolved after the above points have been addressed.

Specific comments

L40 (approx) - this introduction paragraph should be divided up for readability.

L60 – comment about transportability of sediment is basically correct but lacks nuance surrounding partial mobility. Flow competence is important but the largest grain size fraction that is transported is transported most infrequently, so I would note the presence of partial transport and mention relevant literature (Wilcock & McArdell 1993 & 1997).

L64 – the word 'will' is used habitually throughout the manuscript when referring to projections of climate change and glacial discharge regimes. These are ultimately predictions and language should reflect this.

L70 – there is no clear research gap or problem that has been communicated. More generally, there needs to be a hypothesis or research question that is tested. Developing this will help in linking up analysis, discussion, and conclusions.

L76 – reference to a 'landform decoupling'. It is not clear what this is exactly, and similar process interpretations throughout need to be explained in specific terms.

L105 – These two processes of reworking are related, however.

L105-108 – Some of these statements about the dynamics of this site and potential for different processes are presented as rather factual, when they appear to be based on 1-2 studies. It may be useful to talk about these key studies and their methodologies so it's clear what has been demonstrated and how (e.g. Geilhausen et al., 2012b).

L139 – How was >150 stones decided? There are several rules-of-thumb across the literature, and this is not necessarily insufficient, however, some recent work has attempted to improve sampling and introduce a quantification of uncertainty. I will note one such effort by Eaton et al. 2019 that would help to demonstrate differences between GSDs more convincingly.

L147 – Usage of a 1D model should be justified given there is good drone data and there are likely important lateral processes occurring here which cannot be accounted for without a 2D approach.

L152 – there is no detail provided here regarding GERM, and this is needed for the study to be reproducible

L170 – There needs to be more detail here regarding both the orthophoto and the automatic camera. Especially with the limited temporal resolution of the dataset, the timing of these captures is critical.

L177 – narrowly graded, based on what criteria? There are indices to indicate the degree of gradation. I would be surprised if a proglacial stream was narrowly graded!

L180 – to me, describing these points as 'characteristic' would imply they are representative of whatever process are of interest. They seem to have been more arbitrarily selected

Figure 3: how has the 'potential future grain size distribution' been developed?

L196 - What is the justification for using D50 as the characteristic grain size? In gravel-bedded streams, a larger-than-average grain size percentile is usually more appropriate as it has greater influence over bedload transport (see Mackenzie et al. 2018).

L198-204: - It is difficult to assess the difference between these values without an estimate of uncertainty and error across the methods. Is a predicted mobile D50 diameter of 60 mm bigger than an observed surface D50 diameter of 50 mm in a way that is statistically significant? Sediment transport equations are not known for their high accuracy. Also, what does 'no big roughness elements' mean? Moreover, the assumption that the surface grain size distribution is representative of the sediment load needs to be more carefully addressed.

L219 – what does 'pronounced river structure' mean?

L237-239 – What is the evidence for this? How can one be sure this will occur at this site? The authors should be careful in the discussion to be clear what evidence there is for the specific area of study, compared to studies of other areas. There also needs to be discussion of what is meant by 'channel stability', as this varies widely.

Figure 6: - How have these 'erosion breakpoints' been defined? They appear remarkably spatially periodic; do they have physical meaning or are they just points that have selected for analysis?

L296-299 – there is some attempt here to discuss the limitations of the methodology, but this deserves a more comprehensive effort to demonstrate that these limitations do not undermine the findings. Relatedly, why is it acceptable that the relatively finer fractions were not accounted for? There is a large literature on the importance of fine sediment for decreasing the entrainment threshold of larger grains. What are the limitations of only sampling surface grains, as opposed to the bulk?

Section 5.2 and Conclusion: I find these sections unconvincing because they are largely unrelated to the data that has been presented. They appear to summarise general findings from the literature rather than detail the empirical and theoretical contributions of the study. The final clause 'as proven and described in this study' is inappropriate. This is, however, mostly an exercise in adjusting the conclusions to better reflect the work that's been done. I encourage them to think carefully through this process!

References

Eaton, Brett C., R. Dan Moore, and Lucy G. MacKenzie. "Percentile-based grain size distribution analysis tools (GSDtools)–estimating confidence limits and hypothesis tests for comparing two samples." Earth Surface Dynamics 7.3 (2019): 789-806.

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