Reply to anonymous Reviewer #2

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.

> ***Reply:*** *We thank the Reviewer for the critical and detailed review of the submitted manuscript (MS). We agree that the initial version of the MS needs a revision regarding the reviewer's points. A detailed discussion of the comments can be found in the following section. We are convinced that we have considered all comments appropriately.*

1. 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:

A) 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 contextualization 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.

Reply: *Thank you for this comment. We agree with the reviewer that proglacial areas are highly dynamic environments (initial MS – e.g., LN42, 47, 107, 220). However, special focus was given to the evidence of stabilization processes. Studies have shown stabilized channel sections with increasing distance to the glacier terminus (e.g., Gurnell et al., 1999).*

(1) Based on forecasted hydrological model data (glacier evolution runoff model – GERM), the study results give a prediction/tendency of the future channel bed stabilization in a well-known diamictic environment. Channel stability is strongly coupled to landform (dis-)connectivity (Fryris et al., 2007; initial MS – LN41). Elements that force vertical decoupling are non-fluvial sediment (initial MS – LN56), forming first an infrequent mobile armoring layer (Bunter & Abt, 2001), which transforms in longer term to a (more) erosion-resistant pavement layer. The hydraulic modeling results underline this gradual development, although uncertainties are given (initial MS – e.g., LN143, LN297; and later replies).

(2) The comparison with 2015 (origin of the channel, initial MS – LN172) was done to get an overview of the study area since the channel formation. However, all study results are based on hydrological modeling (GERM). Care was taken in the review to clarify this difference to avoid misunderstanding (see later replies).

B) 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.

Reply: *Thank you for this important comment. We used the line-by-number method (Fehr, 1987) in two different ways: (i) on-site and (ii) digital in the processed high-resolution orthomosaic based on the UAV images. As stated by Lang et al. (2021), "the digital line sampling is the one-to-one counterpart of the current state-of-the-art field method [line-by-number analysis]".*

(1) A methodological comparison of both approaches is not possible due to (i) the very fine sediment composition in the headwater and (ii) the inaccessibility of the canyon.

(2) We supplemented the already addressed study uncertainties (initial MS – LN296) with those mentioned by the reviewer (see other replies, respectively).

C) 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.

Reply: *Thank you for this valuable comment; knowledge about uncertainties is essential (see previous reply).*

(1) The river bed coarsening by glacifluvial erosion (pavement layer formation) for the presented case study is a continuous process over the upcoming years due to steadily increased bed shear stress strongly coupled to the predicted discharge (according to GERM until the year 2030; initial MS – LN192). This continuous process of river bed coarsening gradually increases the river bed's characteristic grain sizes (e.g., d_{50}). In our study results, the actual characteristic grain size $d_{50.m}$ (field measurement) in 2018 is already bigger than the flow competence (hydraulic model results) according to the highest predicted discharge in 2030 ($d_{50.c}$). This difference will increase in the future due to the gradual coarsening of the substrate of the channel bed. After the year 2030, the flow competence is expected to decrease around the level of 2018 (initial MS – LN193), which in turn limit the initiation of motion of bedload. As stated in a later reply, the tendency of pavement layer formation is still given considering uncertainties in sediment sampling.

(2) As described above, the (predicted) gradual (year-by-year) coarsening of the river bed underlines the study's results, which are supported from a hydraulic point of view. As this gradual development of a pavement layer is valid for areas of diamictic glacial till, the new "in-stream" storage type of non-fluvial sediment and, thus, the extension and refinement are valid for all other catchments with the same characteristics. The pavement layer is a relevant landform decoupling process (acting as buffers in the vertical direction – initial MS LN42, 284; Brierley et al., 2006), which is specific and poorly investigated in (diamictic) glacier forelands.

(3) We focused on precise formulation and consistent wording during the revision.

2. Specific comments

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

Reply: *We revised it as suggested.*

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).

Reply: *Thank you for this additional information. We added it appropriately.*

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.

Reply: *Thank you for this valuable comment. We agree and replaced the word "will" with more appropriate terms like "predicted" or "forecasted".*

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.

Reply: *Thank you for this important comment. We added more detailed information about the research gap and the hypothesis of our study:*

Proglacial rivers in a diamictic glacier foreland, a well-known dynamic system, show evidence of stabilization by glacifluvial erosion. Our aim is to verify the gradual channel bed coarsening and the tendency of pavement layer formation by hydraulic indices.

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.

Reply: *In the initial MS (LN41), we described sediment connectivity in different directions (longitudinal, lateral, vertical) as a crucial parameter for sediment contribution for the glacifluvial transport. In turn, different landforms prevent sediment transport as they act as buffers, which decouple different (storage) landforms (Brierley et al., 2006; Fryris et al., 2007). We reworded this section and added new information during the revision.*

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

Reply: *We rephrased this paragraph during the revision.*

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).

Reply: *This section was intended to represent the study area for a more detailed overview. However, we considered this comment during the revision and have been more careful in the exact wording.*

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.

Reply: *(1) We used the approach according to Fehr (1987), which is the state-of-the-art field method for gravel to cobble-bed mountain rivers. According to this method, 150 stones are required to represent the respective site adequately.*

(2) We agree with the authors of the suggested paper, that a larger number of stones result in less uncertainty but – in turn – is more time-consuming (sampling 150 stones for an LbN analysis takes around 30 minutes). However, even with the uncertainty described by the authors (d_{50} varies from ±25 % for a sample size of 100 stones from a gravel bed river), our study results can still demonstrate the tendency of pavement layer formation in a proglacial diamictic environment.

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.

Reply: *The presented study focuses on channel incision and channel bed stabilization processes. Detailed process reconstruction is also limited in a 2D modeling approach. Thus, a 1D model is sufficient and frequently applied in the glacial environment investigating glacifluvial processes (see previous reply).*

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

Reply: *We added more information about the GERM model in the revised MS.*

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.

Reply: *We only used the comparison with the orthophoto of 2015 to show the year of the beginning of the channel formation. To avoid misunderstandings, shift this part to the study site description. All our results are now based on the hydrological model (GERM), the hydraulic model results, and sediment analysis.*

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!

Reply: *Thank you for this comment. We agree and rephrased this sentence.*

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

Reply: *We did the digital line sampling in all non-wetted areas in the canyon (five possible sites in total). We agree with the reviewer and replaced the term 'characteristic' in the revised MS with the more appropriate term 'specific'.*

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

Reply: *It only represents a tendency and is based on the calculation results according to the highest flow competence in the year 2030.*

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).

Reply: *The approach according to Rickenmann is well-known and requires the characteristic grain size d_{50} (Eq.1 in the initial MS – LN159). For considering the increased flow resistance due to macro roughness elements (like in the canyon), the energy gradient was reduced according to Eq.2 in the initial MS (LN163). For this calculation step, the d_{90} is required.*

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.

Reply: *(1) As described in a previous reply, the uncertainties of sediment sampling are now more appropriately addressed in the revised MS.*

(2) Sampling surface grain sizes for analyzing armor and/or pavement layer formation is representative only to a certain extent. However, we present indications and tendencies based on the hydrological model GERM and hydraulic model data (flow competence).

(3) Big roughness elements like boulders or step-pool sequences create increased flow resistance in steep mountain rivers and are essential elements controlling bedload transport. The more appropriate term' macro-roughness elements' is used in literature (e.g., Nitsche et al., 2011). Therefore, we used this term in the revised MS.

L219 – what does 'pronounced river structure' mean?

Reply: *With this term, we described the well-visible proglacial river in the year of formation. As this paragraph describes the past morphological development, we revised the entire paragraph and transferred parts of this section to the study area description (see other replies).*

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.

Reply: *(1) As it is cited in the initial MS, Pralong et al. (2015) observed limitations in the bedload transport triggered by altered discharge patterns. Especially in the presented study site, Avian et al. (2018) as well as Geilhausen et al. (2012), already observed less bedload transport (initial MS – LN252).*

(2) As it was already mentioned by the reviewer and as it is stated in the initial MS (e.g., LN42, 47, 107, 220), proglacial areas are highly dynamic systems. High-magnitude/low-frequency events can increase sediment transport and great morphological changes (initial MS – LN49, 291). However, different landforms are able to interrupt sediment connectivity (e.g., Fryris et al., 2007; Brierley et al., 2006). One of these landforms is glacial deposited non-fluvial sediment forming a pavement layer and forcing landform decoupling in the vertical direction (Brierley et al., 2006; initial MS – LN284).

(3) The revision focused on more accurate wording, as we investigated the channel bed's gradual stabilization by the tendency of pavement layer formation. Therefore, the term 'channel stability' may lead to misunderstandings, and we used more appropriate and consistent wording in the revised MS.

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?

Reply: *As mentioned in a previous reply, digital line sampling was applied in all non-wetted gravel bars in the canyon (five possible areas in total). According to the hydraulic model results, a tendency of pavement layer formation is already given in all five sites.*

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?

Reply: *The line-by-number method according to Fehr (1987) is state-of-the-art for gravel to cobble-bed mountain rivers (see previous reply). As also mentioned by Fehr (1987) and later by Lang et al. (2021), the undersampled finer fractions are considered (or predicted) by a conversion calculation step.*

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!

Reply: *We agree with the reviewer and revised the mentioned sections carefully and reflected our investigation more based on our empirical data. We also agree, that the phrase 'proven by this study' is inappropriate and we deleted it during the revision.*

3. 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.

MacKenzie, L.G., Eaton, B.C. and Church, M., 2018. Breaking from the average: Why large grains matter in gravel-bed streams. Earth Surface Processes and Landforms, 43(15), pp.3190-3196.

Wilcock, P.R. and McArdell, B.W., 1993. Surface-based fractional transport rates: Mobilization thresholds and partial transport of a sand-gravel sediment. Water Resources Research, 29(4), pp.1297-1312.

Wilcock, P.R. and McArdell, B.W., 1997. Partial transport of a sand/gravel sediment. Water Resources Research, 33(1), pp.235-245.

Reply: *Thank you for the references. We have implemented them appropriately.*