Reply on anonymous Reviewer #1

Overall, the manuscript: *"Channel evolution processes in a diamictic glacier foreland. Implications on downstream sediment supply: case study Pasterze / Austria"* is well prepared and addresses relevant scientific questions on the future channel evolution processes in such systems. The submission is well structured, and the language is fluent and precise. However, when reviewing the manuscript some questions arose (general comments), which need to be addressed/considered during the review of the manuscript.

> ***Reply:*** *Many thanks to the Reviewer for the appreciative words regarding scientific importance, structure, and language. Thank you for critically reviewing the submitted manuscript (MS).*
> *We agree that the initial version of the manuscript needs a revision regarding the points the reviewer made. A detailed discussion of the comments can be found in the following section. We believe that we have considered all comments appropriately.*

My main concern is the assumption of a static system to predict future morphological processes in a fully dynamic system. I agree with the authors that different processes can be seen and explained, but interpretations and conclusions are in my opinion associated with uncertainties.

> ***Reply:*** *Thank you for this valuable comment. We agree with the reviewer, that proglacial areas are dynamic systems (stated in LN 49 in the initial MS), and uncertainties are given (stated in the study limitations; LN326-344 in the initial MS). However, the statements are indeed valid, because:*
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> *(1) Glacial diamictic till, characterized by unsorted to poorly sorted sediment with grain sizes ranging in size from clay to boulders (Harland et al., 1966; LN 40 in the initial MS), is present in the proglacial area of the Pasterze Glacier (LN 129 in the initial MS). This common glacially deposited material (Benn & Evans, 2013) is prone to selective glacifluvial sediment transport, where grains exceeding the transport capacity form an infrequently mobile armor layer (Bunte & Abt, 2001; LN 63 in the initial MS). Glacially deposited non-fluvial boulders will have a big contribution to channel bed stabilization (e.g., Hauer and Pulg, 2018) and vertical landform decoupling (Fryirs et al., 2013), a conclusion of the presented study (LN 307; Fig. 7 in initial MS).*
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> *(2) Although the channel is formed in a highly dynamic environment (LN 49 in the initial MS), the lateral confinement by dead ice and ice-cemented sediment led to the deep canyon section due to the slower ablation rate of debris-covered ice (by up to 35 %; Kellerer-Priklbauer et al., 2008; LN 100 in the initial MS). Melting debris-covered (dead) ice will lead to a broadening of the canyon in the future, resulting in greater wetted width, which implies less transport capacity and bed shear stress for bedload mobilization. Furthermore, a stabilized channel bed by grains exceeding the transport capacity and glacially deposited non-fluvial boulders will remain erosion resistant.*
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> *(3) Moreover, the calculation was done with the predicted maximum mean monthly discharge within the modeling period of the applied glacio-hydrological model GERM (for this revision, the*

calculation was also done for the maximum mean daily discharge until 2050 – see later reply). The results of GERM follow the well-known prediction of the expected moment of peak water (e.g., Huss and Hock, 2018; Schaefli, 2015). This turning point, where the meltwater discharge will decrease after the exceedance of this turning point due to less glacier ice volume, is predicted for European glaciers before 2050 or has already passed (LN65 in the initial MS; Huss & Hock, 2018). The results by GERM (according to the A1B scenario) predict the maximum meltwater discharge for the 2030s ($Q_{m.melt.max.2034} = 9.6\ m^3s^{-1}$*) with a decreasing mean monthly meltwater runoff until the end of the modeling period in 2050 (LN 228 in the initial MS). This statement is also valid for the maximum mean daily runoff until 2050 (best resolution possible by GERM; see later reply).*

(4) That the observations, results, and key findings of this study are transferable to other proglacial areas is described in a reply below.

1. General comments

A) The authors obtained a DEM in 2018 and used predicted runoffs for steady-state 1D simulations (2018, 2035, and 2050), which means the system (topography, roughness) was static over time. However, from the figures, it becomes evident, that the system is fully dynamic and the canyon part changed drastically within the last three years. Hence, I am wondering how reliable these predictions are, as there are no morphological changes considered.

Reply: *Thank you for this valuable comment. We are aware that we have modeled the flow competence with a steady-state 1D simulation. In terms of a prediction model, uncertainties are given (LN326-344 in initial MS), but reliable results are provided for discussion.*

B) What was the reason that the authors have chosen runoff in 2035, whereas the maximum mean monthly meltwater runoff is predicted for 2034?

Reply: *(1) Output of glacio-hydrological model GERM is the discharge in different ‚reservoirs‘ (term used in GERM) – in the case of Pasterze Glacier the reservoirs (i) ‘ice’, (ii) ‘snow’, (iii) ‘surface runoff’, (iv) ‘groundwater’ and (v) ‘permafrost’, each in (mean) daily resolution (Schöner et al., 2013). This differentiation allows the statement, that the peak meltwater runoff of glacier ice is predicted for the 2030s according to the A1B scenario (*$Q_{m.melt.max.2034} = 9.6\ m^3s^{-1} > Q_{m.melt.max.2035} = 4.9\ m^3s^{-1}$*), although the summation of all reservoirs leads to maximum predicted mean monthly discharge for 2035 (*$Q_{m.max.2035} = 17.9\ m^3s^{-1} > Q_{m.max.2034} = 15.2\ m^{3-1}$*).*

(2) The statement that peak ice melt is predicted for the 2030s according to the A1B scenario is also visible in the data of maximum mean daily meltwater discharge (highest resolution possible by GERM) of glacier ice until 2050: $Q_{d.melt.max.2034} = 16.9\ m^3s^{-1} > Q_{d.melt.max.3035} = 8.7\ m^3s^{-1}$ *and all other mean daily meltwater discharge by glacier ice until 2050.*

C) The authors write about the torrential flow characteristics, which may lead to morphological changes, but use mean monthly values for their steady-state simulations. Hence, I am wondering, if such mean values can replicate the morphological system, or if important runoff peaks, leading to a morphological development of the system, are missing. The authors write: "Landform and subsystem connectivity is highly dynamic (Lane et al., 2017), and changes are often triggered by high-magnitude/low-frequency events." Especially short-time events may lead to a break-up of the bed armoring layer.

Reply: *(1) Thank you for this comment. We meant with 'torrential' that the river type (especially of the canyon) is 'torrent-like'. We corrected this term.*

(2) During the revision, we did the modeling with the predicted maximum mean daily runoff (best resolution possible by GERM) until 2050 (predicted for July 2045; $Q_{d.max.2045}= 28.02\ m^3s^{-1}$*; according to the A1B scenario). As it is visible in the table below, it is obvious, that the calculated* $d_{50.cirt.r}$ *is bigger than calculated by the maximum mean monthly runoff (2035), but still smaller than the measured* $d_{50.m}$ *by the digital line sampling. Thus, the conclusion of the presented paper – the development of a pavement layer by grains exceeding the transport capacity and glacially deposited non-fluvial boulders – is still valid with the maximum predicted discharge in the best resolution possible by GERM.*

Tab. 1: *Comparison of the* $d_{50.m}$ *(by digital line sampling – DLS) and calculated* $d_{50.r}$ *(with reduced energy gradient* I_{red} *according to Chiari & Rickenmann, 2007) for the respective years.*

River station *[m]*	$D_{50.m}$ *[mm]*	$D_{50.crit.r.2018}$ *[mm]**	$D_{50.crit.r.2035}$ *[mm]**	$D_{50.crit.r.2045}$ *[mm]**
502	*209.128*	*80.482*	*113.280*	*156.207*
409	*232.729*	*106.117*	*131.443*	*152.754*
327	*302.941*	*57.461*	*112.329*	*172.094*
252	*239.863*	*18.914*	*37.235*	*122.245*
168	*222.786*	*114.986*	*141.777*	*184.109*
52	*179.396*	*32.615*	*71.823*	*139.820*

**The indices has changed in response to a comment of reviewer 2.*

(3) We agree with the reviewer, that an armoring layer can be broken up by e.g., high-magnitude/low-frequency events. Although such extreme events are not predictable by models, this rarely possible situation is mentioned in the initial MS (LN 51 or 317).

D) The authors write about bed armoring and that within the canyon an armor layer has already developed. Have the authors considered the status of the armor layer and if it was fully developed? A fully developed bed armor layer can be calculated depending on the grain size distribution, e.g. given by Günter (1971).

Reply: *(1) Thank you for this comment. The started development of an armoring layer is an assumption from the authors, as the measured* $d_{50.i}$ *(in 2018) is already bigger than calculated by the maximum mean monthly discharge in 2018 (*$Q_{m.max.2018}= 12.2\ m^3s^{-1}$*; see table before). The specific critical discharge* q_c *(calculated by Eq.1, modified by Eq.2; see initial MS) necessary* *for the movement of the corresponding* $d_{50.m}$*, is higher than the actual maximum specific discharge in 2018.*

(2) Calculation approaches for the development of an armor layer (e.g., Little and Mayer, 1976; Knoroz, 1971, Günter, 1971 or Jäggi, 1983), sediment data from the surface and subsurface is necessary. As the canyon was inaccessible during the fieldwork, no sediment data of the subsurface is available.

(3) However, the glacial diamictic till does not have a clearly defined/differentiated subsurface sediment (as we know it from armored lowland rivers; e.g. Hunziker and Jaeggi, 2002; Bunte and Abt, 2001), because very coarse grains and even non-fluvial boulders occur randomly distributed over the entire thickness of the glacial diamictic till (Harland et al., 1966).

E) Even though the authors show many references, I am wondering how transferrable the results are to other glacier regions in Europe or even worldwide. The authors write that there are many boundaries involved, such as slope, bed material compositions, but a statement on that should be given.

Reply: *Thank you for this comment.*

Yes, the results are transferrable to other proglacial areas because:

(1) Slope: The channel gradient is an essential controlling parameter for river channel pattern and is one essential hydraulic parameter for calculating the initiation of motion of bedload (Church, 2002). Thus, slope is a relevant hydraulic parameter but not a boundary for a specific study case.

(2) Sediment: Diamictic sediment (unsorted to poorly sorted sediment with grain sizes ranging in size from clay to boulders; Harland et al., 1966) is very common in glacially influenced depositional environments (Benn and Evans, 2013). That means, the sediment composition like in this study area can be found in many different other proglacial areas of the Alpine (e.g., Le Heron et al., 2021; Lane et al., 2017; Morche et al., 2012; Carrivick and Rushmer, 2009) and Arctic recently deglaciated areas (e.g., Tomczyk et al., 2020; Rachlewicz, 2009).

(3) Confinement: The channel (especially the canyon section) is confined by (buried) dead ice (LN 102, LN 114, LN 130 in the initial MS), which exists in many proglacial areas in addition to ice-cemented sediment (Gärtner-Roer and Bast, 2019). Debris-covered glacier surface, a prerequisite for dead ice development (Gärtner-Roer and Bast, 2019; Benn & Evans, 2013), exists at the Pasterze Glacier (Kellerer-Pirklbauer et al., 2019; Fischer et al., 2018) – mentioned in LN 98 and Figure 1b in the initial MS – and is increasing at many European glaciers (Lardeux et al., 2016) and glaciers worldwide (Mayr & Haag, 2019). Thus, dead ice or ice-cemented sediment can be confinement landforms in many proglacial areas.

F) It was not clear to me if newly generated sediments, as a result of glacier melt, are considered in future predictions. These fine sediments may alter the morphology. Here a statement given by the authors: "Combined with the high sediment supply by glacifluvial erosion of glacial diamictic till,.." Here another question arose, how is the suspended sediment transport and the interaction with the bed considered in the study?

Reply: *Bedload is responsible for channel bed stabilization and the development of an armor layer. Therefore, suspended sediment (for alpine rivers – grain diameter <1 mm; Maniak, 2005) was not considered in the presented research design.*

(2) The calculation approaches for the initiation of motion of bedload do not consider suspended sediment.

G) The chapter on the hydrodynamic model needs more details, such as information on the chosen roughness or if a calibration was performed.

Reply: *Thank you for this comment, additional information will be added to the MS:*

(1) Based on the measured sediment composition, sensitivity analysis, and literature (e.g., Naudascher, 1992), three different roughness coefficients k_{st} *were used: (i) headwater and delta:* $k_{st}= 28\ m^{1/3}s^{-1}$*; transition and canyon:* $k_{st}= 18\ m^{1/3}s^{-1}$*.*

(2) Due to the inaccessibility of the canyon, the measurement of calibration parameters (e.g., discharge, water levels) was not possible. Thus, we performed a sensitivity analysis comparing different hydraulic parameters (e.g., flow velocity, shear stress, Froude number, and wetted width) to define the adequate roughness coefficient for the hydraulic model.

H) I recommend having a separate chapter on the Glacier Evolution Runoff Model (GERM), and more details on the calibration of the model (bias-corrected data, regional climate model) and the output. Here my question is why daily values ("One of the model output data is runoff in daily resolution") are used and not e.g. values with an hourly resolution, or maybe three hours maximum.

Reply: *We will add additional information about GERM to the MS:*

(1) Precipitation in the catchment has been extracted from a gridded precipitation dataset (Hofstätter et al., 2013) using the mean value of 4 neighboring grid points and by applying a vertical precipitation gradient. For the scenario runs, data from regional climate model runs of the reclip:century simulations (Loibl et al., 2011) have been bias-corrected to the station dataset. For temperature, a monthly bias correction and for precipitation, the quantile-mapping has been applied to adjust the distribution of the climate model output to the measured one.

(2) The best resolution of the glacio-hydrological model GERM is discharge in daily resolution. This is a widely used tool for predicting runoff evolution (e.g., Huss et al., 2014; Sorg et al., 2014; Farinotti et al., 2012; Huss et al., Huss et al., 2008).

I) Page 10, line 214: "The sediment analysis shows a downstream coarsening". I think this is misleading, as in the delta the same grain sizes are visible as in the headwater. In general, I think we see here typical morphological patterns, where the different grain sizes depend on the boundaries, such as slope. As the canyon has the highest slope it is evident, that the coarsest sediments can be found there. I think here it is necessary to dig into the data of the HEC-RAS model to get more insight into the hydraulics of the system.

Reply: *We agree with the reviewer, that 'downstream coarsening' is a misleading term in this case. We changed the wording with the coarse sediment composition of the canyon as a reference for the description of the finer sediment composition in the headwater and delta.*

J) Page 10, line 242: "the maximum mean monthly runoff." As mentioned before, a mean value may not be representative for predicting morphology and channel evolution, as the "Glacifluvial sediment reworking is strongly coupled to runoff characteristics."

Reply: *(1) As written in a reply before and presented in Tab. 1, it is evident that the calculation results ($d_{50.crit.r.i}$) with the predicted maximum mean runoff in daily resolution (best resolution possible by GERM) are bigger than with maximum mean monthly runoff, which leads to a smaller difference to the measured $d_{50.m.i}$.(see Tab. 1)*

(2) As stated as well in the reply before, the key message and conclusion of the paper are still valid with the highest predicted discharge in the best resolution possible by GERM.

K) Page 10, lines 233-235: The paragraph needs some modification and may be rewritten. The authors introduce three discharges: Qm.melt.max.2018= 4.9 m³s-1; Qm.melt.max.2034= 9.6 m³s-1 >> Qm.melt.max.2050= 3.5 m³s-1) and use Q2034 for comparisons. I think here only comparisons between the sampled sediments and the Q2018 can be made, as it is unsure what the channel and the grain size distribution will look like in 2034 or 2050. Hence, I think the statement "In the transition section (CS 622 m – CS 552 m) with a slightly increased channel gradient (Sm= 2.4 %), a much bigger characteristic grain size was calculated then measured (d50.c:LS.3= 170 mm > d50.m:LS.3= 79 mm; Fig. 5a)" is not valid, as no measurements are available yet. Why is 2050 not included in the figure? Can the authors maybe draw lines of a fully developed bed armor layer? Here I think it needs to be statistically proven that the modified approach by Chiara and Rickenmann led to better results.

Reply: *(1) As mentioned in a previous reply, the input discharge for the hydraulic model was not only the predicted maximum meltwater discharge ($Q_{m.melt.max.i}$), but it was the maximum predicted total mean monthly discharge by GERM ($Q_{m.max.2035}$).*

(2) The approach in the presented paper is a prediction model, which is a widely used tool also in river morphology studies. Although uncertainties are given (see study limitations; LN 326-344 in initial MS), the results are reliable, and the conclusion is valid.

(3) The calculation results with the predicted discharge from 2050 are not included, as the goal of the study was to find the highest predicted discharge for comparison with sediment data from the actual situation. The predicted maximum mean monthly runoff in 2050 is almost the same as in 2018 ($Q_{m.max.2018}$= 12.2 m^3s^{-1}; $Q_{m.max.2050}$= 12.7 m^3s^{-1}; mentioned in LN 226 and 258 in the initial MS) resulting in similar calculation results for $d_{50.crit.r.i}$.

(4) A statistical test is not applicable, as reference data (e.g., measurement data) are not available due to missing measurement stations. However, as it is stated in the study limitations of the initial MS (LN 226-344) and as it is stated in literature (see later reply), the consideration of macro-roughness elements and increased flow resistance in steep high-alpine rivers is a relevant modification of bedload calculation approaches for more plausible results.

L) Page 12, line 267: "The analysis of the sediment composition and the hydrodynamic-numerical model results tend to the potential for riverbed incision in the headwater and for pavement layer formation (channel bed stabilization) in the canyon." This estimate is in my opinion only valid for a static system.

Reply: *As mentioned in a previous reply, we applied a prediction model for estimating the future flow competence of the proglacial river using predicted discharge data of the glacio-hydrological model GERM. Prediction models are associated with assumptions and uncertainties (see study limitations; LN 326-344 in the initial MS). However, under given conditions, the results are substantial for discussion and the conclusion is valid.*

M) Page 14, line 317: "However, rivers in proglacial areas with an established pavement layer still enable lateral sediment supply, often triggered by high-magnitude/low-frequency events." This is evidence that the system will change over time.

Reply: *(1) Lateral sediment supply often leads to the broadening of the channel resulting in less bed shear stress, especially in combination with the exceedance of the moment of peak water (see previous reply and LN 75 in the initial MS). Furthermore, lateral sediment supply does not change the composition of the already (partly) stabilized channel bed by grain sizes exceeding the transport capacity and glacially deposited non-fluvial boulders (see previous reply).*

(2) As mentioned in a previous reply, the calculation was done with the predicted maximum mean monthly discharge (during the revision also with maximum mean daily discharge – highest resolution possible by GERM). Thus, the calculation was done with the maximum discharges predicted and available for this study area. Extreme events like high-magnitude/low-frequency events cannot be predicted.

N) Page 14, line 326- : The authors write about shortcomings and uncertainties, e.g., in geometry and calibration data acquisition as well as in sediment sampling, but do not quantify it.

> ***Reply:*** *Uncertainties in areas without any measured reference data are difficult to quantify. Thus, uncertainties can only be estimated by e.g., a sensitivity analysis. Therefore, we reduced the measured* $d_{50.m}$ *by 20 % (in parts up to 30 %) and* $q_{max.2045}$ *is still too low for the initiation of motion.*

O) Page 16, line 369: “This paper predicts the future flow competence (the largest particle a flow can move) of the proglacial part of the river Möll according to the glacio-hydrological model GERM (glacier runoff evolution model) of the Pasterze Glacier by 2050.” I think this sentence is misleading, as there were many uncertainties not considered. Hence, I would rather say an estimate.

> ***Reply:*** *Measurements in high mountain areas and especially at glacier forelands are challenging (e.g., weather, accessibility, discharge behavior) as written before and mentioned in the initial MS (LN 326-344). Thus, we agree with the reviewer and changed the wording.*

P) Page 15, line 340: “The importance of considering energy losses by macro-roughness elements to achieve more plausible results was pointed out in various studies (see literature in Chiari and Rickenmann, 2011) for bedload transport calculations, especially in steep mountain streams (S> 4-6 %; Badoux and Rickenmann, 2008).” But when looking at Figure 5, I have the impression that the approach given by Rickenmann (1990) fits better than the approach with the reduced energy gradient (Fig. 5b) in the canyon. See my previous comment.

> ***Reply:*** *(1) As mentioned in a previous reply, in the study limitations of the initial MS (LN 326-344) and as stated in the relevant literature (e.g., Chiari and Rickenmann, 2011; Nitsche et al., 2011; Badoux and Rickenmann, 2008; Bathurst, 2007; Chiari and Rickenmann, 2007; Rickenmann et al., 2006), increased flow resistance due to macro-roughness elements must be considered in the calculation approach for more plausible and realistic results.*
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> *(2) Moreover, in steep high-alpine streams, the form resistance (e.g., by step-pool sequences) – included in Eq2.; see initial MS – can amount to up to 80 % of the total flow resistance (Canovaro et al., 2004).*
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> *(3) Steep high mountain streams are characterized by (i) wide grain size distribution, (ii) large boulders that remain immobile during high discharges, (iii) step-pool morphology, and (iv) low relative flow depths. All these features lead to additional roughness and flow resistance (Chiari and Rickenmann, 2011; Nitsche et al., 2011). All these characteristics are given for the studied canyon section: (i) glacial diamictic till with grains ranging in size from clay up to boulders, (ii) glacially deposited non-fluvial boulders, (iii) step-pool morphology and low relative flow depths related to the big boulders.*

2. Specific comments

Page 1, line 17: I would replace “sediment was sampled” by “sediment properties were obtained”

> ***Reply:*** *Thank you, we accept the suggestion.*

Page 1, line 18: I would not talk here about bedload transport formulas, as it was the one from Rickenmann used and an approach considering the energy line.

> ***Reply:*** *We agree with the reviewer, and revised it as suggested.*

Page 1, line 18: “Due to the fine sediment composition near the glacier terminus (d50< 79 mm).” I would not call these sediments fine sediment composition.

> ***Reply:*** *We agree with the reviewer, the definition of ‘fine sediment’ is ‘particles smaller than 2 mm’. Thus, we changed the wording of the entire paragraph and used the coarse grain size composition of the canyon as a reference value in this study (a “finer sediment composition than in the canyon was measured for the headwater and delta”).*

Page 4, line 116: “and strong seasonal and diurnal fluctuations.” Can the authors give an approximate?

> ***Reply:*** *(1) As stated in the initial MS (LN 115-116), seasonal discharge fluctuations range between* $Q= 0.1\ m^3s^{-1}$ *in winter and up to* $Q= 25\ m^3s^{-1}$ *in summer (Krainer & Poscher, 1992). As no discharge measurements are available, the daily fluctuations can only be estimated from reconstructed data of the inflow into the Margaritze reservoir, which are of the same order of magnitude as the seasonal fluctuations.*

Page 9, line 190: The authors only used the A1B scenario according to IPCC in their study. This needs to be justified.

> ***Reply:*** *Field measurements in high alpine environments regarding river morphology and hydraulic modeling are prone to uncertainties (e.g., weather conditions, inaccessibility, torrent-like discharge conditions) and lead to some limitations (e.g., geometry and calibration data, sediment sampling; LN326 – 344 in the initial MS). Limitations are also given in the glacio-hydrological model GERM (LN343-344 in the initial MS). The main uncertainties next to the uncertainty of the future climate evolution are in the calibrated parameter set and in the model (Farinotti et al., 2012) of the glacio-hydrological model. Due to missing winter mass balance measurements in the study area, parts of the calibration of the glacio-hydrological model have been carried out using glaciers at close distance but outside the study region. The glaciers considered are in the same mountain range of the Hohe Tauern region. However, to be able to predict glaciofluvial processes, the A1B scenario according to IPCC was used, which assumes balanced land-use changes and balanced progress across all resources and technologies which represents a middle way between different scenarios.*

Page 9, line 191: (v) the glacier edge of 2003 and 2012. What was the glacier edge in 2018 when the field survey was conducted?

> ***Reply:*** *Input data for the glacio-hydrological model GERM are, among others, glacier edges with corresponding digital elevation models of the entire catchment. This data combination was only available for 2003 and 2012 and is therefore used in the model GERM.*