Reply on anonymous Reviewer #1

Review of Paster et al.: *"Channel evolution processes in a diamictic glacier foreland. Implications on downstream sediment supply: case study Pasterze / Austria"*
Manuscript ID: hess-2022-347

In their manuscript, Paster et al. investigate the channel evolution of a proglacial stream draining the Pasterze glacier in the Austrian Alps between 2015 and 2018. Using a combination of field surveying, remote sensing imagery and hydrodynamic modelling, the authors investigate the recent and future evolution of the sediment transport along the ~850 m long river reach. Relying on predicted runoff until 2050, the authors model the future transport capacity of the river and compare this to field and remote sensing derived measurements of grain size distributions. From their analysis, the authors conclude that the continuous erosion of finer sediments leaves very coarse grain sizes in the channel, armouring the bed and ultimately stabilizing the proglacial river system.

The manuscript is well written and addresses a topic that is potentially interesting for a broad range of readership. Before acceptable for publication in Hydrology and Earth System Sciences, however, the authors need to address a number of general and specific issues that I outlined in detail below.

> ***Reply:*** *Many thanks to the Reviewer for critically reviewing the submitted manuscript. We agree that the initial version of the manuscript (MS) 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.*

1. General comments

A) Introduction: While the introduction is generally well written, it would certainly benefit from a stronger focus on the research gap that the authors want to address in their study. In my view, the aims should be better linked to the scientific context presented in the introduction. This is also true for the relevance and importance of the study. The authors are addressing the fundamentally important topic of how sediment dynamics might change in a changing climate. This should be stressed more explicitly, especially in a setting where the Margaritzenstausee reservoir is located only a kilometer downstream of the study site. Managing the sediment influx of reservoirs is a big issue that calls for studies that enhance our understanding of proglacial sediment dynamics.

> ***Reply:*** *(1) Thank you for this valuable comment. As stated in the initial MS, the gradual channel evolution (glacifluvial sediment reworking; LN55) and landform decoupling (LN42) are decisive parameters regarding proglacial landscape stabilization.*
> *(2) The predicted (i) establishment of an erosion-resistant pavement layer (ii) will force landform decoupling in the vertical direction with (iii) implications on the fluvial system of the proglacial sediment cascade model (key results of the submitted MS). This gradual development of proglacial rivers by glacifluvial sediment reworking has been neglected up to now in different approaches but is an urgent issue when describing (stabilization) processes of proglacial areas.*

(3) We agree with the reviewer that the knowledge of the Margaritze reservoir downstream of the study site is informative regarding high-alpine sediment management. Thus, we added more information in the revised MS (see other replies).

B) Sediment sampling: Applying two different methods, line-by-number sampling in the accessible river sections and manually measuring grains in orthomosaics in the inaccessible canyon, respectively, makes comparability of the data generated an issue. It remains unclear why the authors did not construct partial grain size distributions from the same method in the entire study area. Given the inaccessibility of the canyon, visually measuring grains in orthomosaics would be suited for this and assure comparability. Another way forward would be to construct partial grain size distributions from both methods for some of the sampling points to assess the difference between methods and quantify a potential bias towards larger grain sizes introduced by measuring clasts in the orthomosaics. The authors should explicitly address the uncertainties introduced by the application of two different methods in their manuscript. The studies cited in L144-145 would suggest a shift towards larger grain sizes. Moreover, the description of the photogrammetrical sampling would benefit from a more detailed description. A final point to consider would be to make use of the automated extraction of grain size distributions from images (photo sieving) that also has been applied to entire reaches (Purinton and Bookhagen 2019). This would allow the authors to construct a more complete data set on grain size distributions by increasing their sampling size (only ten locations so far).

Reply: *(1) First, the accuracy of the applied methods – partial grain size distribution by the line-by-number approach (Fehr, 1987) – is sufficient for the study hypothesis as we are interested in big (non-fluvial) grain sizes and their effect on proglacial channel evolution.*

(2) Generally, sediment analysis is challenging in high-alpine, often inaccessible areas (initial MS – LN293). As the initial MS (LN137) also mentioned, the line-by-number method is a common field measurement method for gravel-to-cobble-bed mountain rivers (e.g., Fehr, 1987; Lang et al., 2021).

(3) However, we agree with the reviewer that photo-sieving can be more accurate when you have a high-resolution orthomosaic available. For photo-sieving, the ground sample distance (GSD) is the relevant parameter (stated in the initial MS – LN145), also mentioned by the proposed reference (Purinton and Bookhagen, 2019). Despite the high-resolution orthomosaic processed in this study (0.7 mm px^{-1}; see later reply), photo-sieving in the headwater was impossible due to the fine sediment composition. Thus, field measurements were invertible in this section. Nevertheless, line sampling was also applied in the digital approach to the inaccessible canyon to ensure comparability. As it is stated by Lang et al. (2021), "digital line sampling is the one-to-one counterpart of the current state-of-the-art field method [line-by-number analysis]" for mountain rivers.

C) Evolution of the river reach: Despite the interesting data set the authors present and analyse in the present manuscript, some interpretations in the manuscript are not fully backed by the data and some discussion points do not cover all relevant aspects. Multiple studies (also cited in the manuscript) show proglacial areas to be highly dynamic systems, where changes can happen within single events. Here the authors use an orthophoto from 2015 (from the federal government) and a UAV derived orthoimage from 2018 to investigate the channel dynamics. But this data set does not allow insight with higher temporal resolution, e.g. the interpretation that the bed is actually stabilizing. Here, the authors should make use of additional data sets that might allow a detailed quantification of the short-term dynamics. Furthermore, the interpretation that glacifluvial erosion in the channel leads to bed armouring and ultimately to a stabilized channel is based on the assumption that a) the channel does not migrate laterally or completely changes course and b) that sediment delivery to the channel reach does not change dramatically. Below I added comments in this respect to specific locations.

> ***Reply:*** *(1) We agree that proglacial areas are very dynamic systems (e.g., LN43, LN47 in the initial MS), also valid for the Pasterze landsystem (LN107 in the initial MS). But the special situation of the glacier forefield (channel confinement by the debris-covered glacier) hence lateral migration and force channel bed incision (besides other parameters mentioned in the initial MS – LN51).*
> *(2) We also agree with the reviewer that single events (like high-magnitude/low-frequency events) are drivers for changes in the proglacial channel pattern (e.g., LN49 or LN291 in the initial MS). Thus, armor layer formation and the establishment of an erosion-resistant pavement layer in the long term do not stabilize the entire channel.*
> *(3) The intention of the comparison between 2015 and 2018 should show that lateral migration is limited due to the confinement by the debris-covered glacier. Thus, the study results do not allow a conclusion regarding the entire proglacial channel stabilization but give a tendency of an erosion-resistant pavement layer establishment and thus on <u>channel bed stabilization</u>. During revision, the focus was made on more precise wording describing this gradual development process of the proglacial river bed.*
> *(4) Due to the characteristic sediment composition of a glacial outwash plain (diamictic till), the gradual development by glacifluvial sediment reworking to an erosion-resistant pavement layer is given, even when lateral migration and continuous sediment supply will occur in the future.*

2. Specific comments

L14: analyses instead of analysis

> ***Reply:*** *Thank you, we corrected this sentence.*

L32: "steadily increasing spatial boundary"? Does this refer to the proglacial area that increases due to glacial recession? Consider rephrasing

> ***Reply:*** *Correct, the increase of the proglacial area due to glacier retreat is meant by this sentence. We rephrased this sentence during the revision.*

L36-39: consider splitting in two sentences

> ***Reply:*** *Thank you, we rephrased this part as suggested.*

L40: maybe: be described as a sediment cascade?

> ***Reply:*** *Thank you, we rephrased this sentence as suggested.*

L55-56: River bed incision into glacifluvial sediment and the formation of an armour layer is portrayed here as inevitable. While this might be true on the long run (when the catchment is devoid of transportable sediment), lateral migration, sediment delivery from valley flanks and a complete sift of the channel can happen in highly dynamic proglacial environments.

Reply: *Thank you for this valuable comment. We agree with the reviewer that proglacial environments are highly dynamic areas (see also previous reply) due to processes mentioned by the reviewer (e.g., lateral migration, channel avulsion). That river bed incision or a stabilized river bed by non-fluvial sediment (study results) does not prevent lateral migration is already mentioned in the discussion of the initial MS (LN290). We considered that in more detail and mention this fact also in the introduction of the revised MS.*
(2) An established erosion-resistant pavement layer will remain intact, despite sediment supply by, e.g., lateral migration (like river embankment failure) or sediment input by valley flanks. Thereby, landform coupling is decisive (LN41 in the initial MS). In the investigated study area, the lateral valley flanks (hillslope-channel connection) are decoupled from the fluvial system. Thus, only glacial deposited material is transported glacifluvial downstream (LN103 in the initial MS; Geilhausen et al., 2012). We considered all this information in more detail in the revised MS.

L59: is able to transport sediments
Reply: *Thank you, we revised it as suggested.*

L65-66: For catchments with smaller glaciers, this peak-water effect has probably already been crossed, whereas for larger glaciers, this still lies in the future.
Reply: *(1) Thank you for this comment. It is correct that the moment of peak water in many glacier-fed rivers has already passed, especially in mountain regions with smaller glaciers (Hock et al., 2019). Huss and Hock (2018) suggest that peak water has been reached for up to 67 % of central European glaciers (including the European Alps). As it is mentioned in the initial MS (LN65), the peak water is suggested to be reached for the European Glaciers latest around the middle of the century (Huss and Hock, 2018). We added additional information during the revision.*

L66-68: the second part of this sentence is not clear, please rephrase
Reply: *Thank you, we rephrased this sentence.*

L69: repetition of L44-45
Reply: *We deleted the last part of this sentence.*

L79-86: I think it would be important to mention that the reservoir Margaritzenstausee is located directly downstream. This increases the relevance of the study, as sediment management is an important topic for the reservoir.
Reply: *We agree with the reviewer that knowledge about the reservoir Margaritze is important (see previous reply). Thus, we include this information during revision.*

L91: Please explain the abbreviations here and elaborate how these values have been calculated. Was the length measured as Euclidean distance between start and end point of the segments, or along the channel? This also applies for the calculation of channel slope that can be derived from digital topographic data in multiple ways.
Reply: *(1) The distance between the respective cross-sections (CS) was measured along the thalweg (the lowest point in each CS).*

(2) The gradient of the 'trend line' (based on the lowest point in each CS) was used for the gradient calculation.

L97-99: explain abbreviation "LbN" in the figure caption and provide details on the coordinate reference system used in the figure.

Reply: *We implemented the missing information in Fig. 1 during the revision.*

L105-106: Glacifluvial processes are an important process for paraglacial adjustment, I am unsure why paraglacial reworking is contrasted here with glacifluvial processes?

Reply: *Thank you for this comment. Glacifluvial sediment reworking and paraglacial reworking are indeed not contrasting processes. Thus, we rephrased this section.*

L124: Indicate which version of Agisoft Photoscan (Metashape since some years) was used for processing.

Reply: *We used Agisoft Photoscan version 1.2.6 for processing the DEM and implemented this information in the revised MS.*

L125: add reference

Reply: *We added the reference (Westoby et al., 2012) here.*

L134-135: Here it is unclear what the 478231187 points refers to? Usually, a DEM is a 2D raster with a certain pixel size. Please add details on the ground resolution of the DEM and Orthomosaic here

Reply: *(1) The point number is a meta information of Agisoft. We agree with the reviewer that this is redundant information, so we deleted it in the revised MS.*

(2) The pixel size (ground sample distance GSD) of the DEM has already been specified in the initial MS (LN135). In the initial MS, we used the GSD for describing the ground resolution, which is now corrected in the revised MS.

(3) We added additional information about the ground resolution of the DEM (1.59 cm px^{-1}) and the orthomosaic (0.7 mm px^{-1}).

L140-141: incomplete sentence

Reply: *Thank you, we improved this sentence during the revision.*

L143: partial grain size distributions

Reply: *Thank you, we revised it as suggested.*

L151-153: Please, can the authors add more detail on the data set and method by Schöner et al. (2013)? As this is a crucial input for the study, the readers will want to understand how the Glacier Runoff Evolution Model (GREM) works. Also, please add more detail to the reference Schöner et al. (2013) as cited in the manuscript. Searching for this reference I can only find a presentation on the EURAS-CLIMPACT project that does not contain any detail on the GREM.

Reply: *We added more information about the GERM in the revised MS as colleagues (GeoSphere Austria, formerly known as Central Institution of Meteorology and Geodynamics) were involved in processing GERM for the Pasterze Glacier and are co-authors of this study.*

L152: GREM?

Reply: *This hydrological model is called 'Glacier Evolution Runoff Model (GERM)', meaning the acronym in the initial MS was correctly spelled. Accordingly, we corrected the name of this hydrological model in the revised MS.*

L153-154: Is there a reason why the high-resolution digital elevation model derived from UAV imagery cannot be used for a roughness determination here?

Reply*: (1) As it is well summarized by Pearson et al. (2017), different approaches exist for roughness determination by a high-resolution DEM. The wide spreading results show the difficulty of applying this approach. Furthermore, almost all approaches deal with grain sizes clearly smaller than we investigated.*

(2) For 1D hydrodynamic modeling, however, the manning value (reciprocal value of the Strickler value) is needed. The well-known formula according to Strickler (1923) – determination of k_{st} by d_{90} ($k_{st}= 26/d_{90}^{1/6}$) – is only valid for rivers (i) with ideal smooth river bed and (ii) with gravel and sand as bed material. However, this study mainly investigates a torrent with all grain sizes up to macro-roughness elements.

(3) Thus, a representative value was set based on sensitivity analysis and literature. According to Naudascher (1992), a $k_{st}= 19\text{-}22\ m^{1/3}s^{-1}$ should be used for torrents with macro-roughness elements and high active bedload transport. As the canyon of the investigated study reach matches with this torrent description, $kst=20\ m^{1/3}s^{-1}$ is appropriate for the canyon. For the headwater and delta, $k_{st}=28\ m^{1/3}s^{-1}$ is used as suggested in the literature for reaches without macro-roughness elements but with high active bedload transport.

L167-169: Might this data be subject to underestimation/overestimation as the clasts are not lying flat on the ground with their b-axis visible?

Reply: *The big clasts in proglacial rivers are usually outwashed and exposed and thus clearly delineated. The missing knowledge of the third stone axis on sediment analysis based on images also applies to all other digital sediment analysis approaches like photo-sieving.*

L193-195: this is a decrease by factor two, but not by two orders of magnitude.

Reply: *Thank you for this valuable comment, we corrected it in the revised MS.*

L199: what are "big roughness elements"?

Reply: *(1) Big roughness elements like boulders or step-pool sequences create increased flow resistance in steep mountain streams and are important controls of bedload transport.*

(2) In literature (e.g., Nitsche et al., 2011), the more common term "macro-roughness elements" is used. Therefore, we adopted this more appropriate term in the revised MS.

L204-206: Delete "so-called" as knickpoint (or knickzone) is a standard geomorphic term. Maybe add a small explanation here: […] knickpoint, a pronounced convexity in the longitudinal channel profile, […]

Reply: *Thank you for this comment, we revised it as suggested.*

L220-221: But as Fig. 5 shows, the channel has moved considerably in the three years between 2015 and 2018. Except for a few meters, the entire channel shifted considerably, in some locations more than ~50m. I agree that this dynamics are to be expected, as the channel is actively incising. If the authors really want to show that channel migration is lower in 2018 than in 2015 (which again can be expected), they need to show this by data. The automated imagery might help to quantify channel mobility over time.

Reply: *With this comparison, we want to show that lateral confinement has existed since the channel development's beginning. Pictures from the automated camera show a very stable channel since 2015 – explained by the very slow melting rate of the debris-covered glaciers – and a very dynamic delta area (as shown and described in Fig. 5). In the revised MS, we are more precise in wording.*

L221-222: As this area is highly dynamic, I am not sure whether these changes can be attributed to upstream controls. The collapsing front of the debris-covered glacier changes takes away the lateral confinement in this area and the channel can turn to a steeper course and incise (see August 2016 and August 2017 in Fig. 5a).

Reply: *Disappearing lateral confinement leads to channel widening with increasing wetted width, lower water depth, and reduced shear stress.*

L222-224: Also here, I am not convinced that the data presented support this claim. The authors use the 2015 orthophoto and the 2018 UAV derived data here. From these two points in time, lateral changes in the channel can only be quantified for the entire three years long interval. The lateral confinement by "debris-covered dead ice landforms" towards the south is crucial in this setting. It can be anticipated that in a few years from now, the channel will not be active anymore, but will have shifted towards the centre of the valley. This can already be seen in satellite imagery from the summer of 2022 (see Figs. R1 and R2 below).

Reply: *(1) We agree with the reviewer that dead ice melting will lead to lateral channel migration and channel avulsion (as already discussed in the initial MS in LN303-309).*
(2) As the channel is confined by debris-covered dead ice, the lateral changes strongly depend on dead ice melting. Due to this 'special environment', dead ice melting is more relevant as bank erosion for lateral migration and is now appropriately mentioned in the revised MS. Furthermore, collapsing dead-ice landforms may enable completely new channels.
(3) This expected future development is more appropriately addressed in the revised MS.

Figure 5b: It is hard to tell the difference between the lines indicating the start and end of the study area and the beginning of the canyon. Maybe colorize? Also in the legend, label should be "start of canyon" or "beginning of canyon"

Reply: *(1) As figures should be readable in black/white, we revised this figure but used a black (and white, respectively) dotted line for the start of the canyon instead of colors.*
(2) We adopted the suggestions for labeling.

L231-232: Again, this is a bold claim relying on only two points in time. In my view, this would require a thorough quantification of channel dynamics with high temporal resolution.

Reply: *As mentioned in several replies, we used more precise wording during the revision - stabilization in terms of "channel bed stabilization****"!***

L233-235: The knickpoint is located in a conspicuous position at the left lateral margin of the valley. From the picture in Fig. 6 one gets the impression that bedrock is exposed in this specific situation. This would strongly limit the mobility of the knickpoint and limit its potential for headward erosion. Can the authors give more detail on the specific setting of the knickpoint?

Reply: *Thank you for this very valuable comment. Further analysis will be done on the knick-point condition and its future effect on the (upstream) channel development.*

L235: This is the first time since the abstract (L15) that river bathymetry is mentioned. Please elaborate in the introduction, methods, and results section how and why river bathymetry was measured and what this adds to the study.

Reply: *Bathymetry wasn't measured, as it was impossible due to inaccessibility. We revised the wording of this paragraph and the Abstract during the revision.*

L237-239: While there might be a tendency of river channels to be more stable in greater distance from the glacier terminus, other factors, most importantly channel slope, are playing a crucial role as well.

Reply: *We agree with the reviewer that (i) channel slope, (ii) sediment composition, and (iii) runoff variability are decisive for channel pattern changes and river stabilization (already mentioned in the initial MS - LN52). However, landform decoupling by different reworking processes with a greater distance to the glacier terminus is also a decisive parameter (e.g., Bakker et al., 2018; Geilhausen et al., 2012; Fryris et al., 2007).*

L241-242: If this knickpoint is produced by underlying bedrock, knickpoint migration will be very slow.

Reply: *Thank you for this comment, we agree with the reviewer and we are now more precise in the revised MS (see previous reply).*

L244: "non-fluvial sediment"? In extreme cases, steep rivers can transport large blocks… I guess the authors want to make thr7e claim that these sediments are glacially deposited and remain in position, while the finer clasts are eroded and transported by the river?

Reply: *Correct, non-fluvial sediments are those glacially deposited and never transported by hydraulic forces. These non-fluvial sediments mainly form the erosion-resistant pavement layer.*

L247: before, the formation of the canyon has been described as glacifluvial, why are the authors using the term "post-glacial" here?

Reply: *Thank you for this comment. During the revision, we focused on precise and consistent wording throughout the MS.*

L250-261: But this stabilization "from a hydraulic point of view" (L252) or the establishment of "an erosion-resistant pavement layer" will only happen under the assumption that the channel will not migrate laterally, or even shift to a new course. Baewert and Morche (2014) show that in a proglacial environment of the Gepatschferner the channel completely shifted to a new course following one extreme precipitation event. Proglacial areas are highly dynamic, and this is especially true for their upper margin where melting dead ice constantly reshapes the topography of the valley floor.

Reply: *(1) With the term stabilization, the <u>channel bed</u> is meant and not the cross-sectional shape. We had the focus on this precise wording during the revision (see previous reply).*

(2) But we agree with the reviewer that proglacial areas are highly dynamic – as stated in the initial MS (e.g., LN43, LN47) – and our study results cannot predict the stabilization of entire proglacial channels.
(3) If lateral channel migration will occur, a lot of sediment is available for fluvial transportation (mentioned in LN290). But this fluvial sediment transport leads again to the armor-layer formation and later to the erosion-resistant pavement layer (see previous reply).

L257-261, L263-265, Figure 6: It is not clear to me how the authors a) define and b) predict these "erosional breakpoints"? Are these "erosional breakpoints" not identical to the locations where partial grain size distributions were constructed from UAV derived imagery? If so, how can breakpoints (i.e. locations where something changes in my understanding) be defined based on six sample locations?

Reply: *(1) Correct, the defined 'erosion breakpoints' are identical to the locations of the partial grain size distributions in the canyon.*
(2) The term 'breakpoint' is, per definition, a location where a continuous process is interrupted. Thus, we implemented the term 'erosion breakpoints' in this study, as the continuous river bed incision is stopped by the tendency of an establishment of an erosion-resistant pavement layer, examined exemplarily at those six characteristic points. There, the tendency of establishing an erosion-resistant pavement layer is given based on our study results.

L273-275: inevitable? I don't think this claim is justified, as I outlined before. Again, Baewert and Morche (2014) show an alteration from single thread to braided and back to single thread over a couple of years in a similar setting.

Reply: *(1) We agree with the reviewer that the study results cannot conclude a strict delimitation between braided and single-thread river sections. Single events or the melt-out of dead-ice landforms can change the system again between supply- and transport-limited.*
(2) However, after a possible channel avulsion in the future, the glacifluvial processes result again in the establishment of an armor layer leading in the long-term to an erosion-resistant pavement layer again.
(3) As mentioned in several replies before, we were more precise in the wording (the tendency of stabilized river bed) during the revision.

L300-316: Also, in this section the authors should attribute the various other possibilities of how the channel surveyed here might evolve in future.

Reply: *Thank you for this comment. As stated in the initial MS (LN304 onwards), the melt-out of the buried dead-ice landforms is able for proglacial channel avulsion (see also previous reply). We added more information about possible future development on the studied channel at the Pasterze landsystem.*

L318-337: Given all the concerns raised above, I would recommend the authors to formulate the conclusions much more cautiously here. While proglacial rivers might have a general tendency to stabilize due to bed armouring and the ultimate formation of a pavement layer, a lot of disturbances will distort this trajectory in a highly dynamic environment. Their survey of a single proglacial river section over the course of three years does not justify very general claims on the evolution of proglacial rivers.

Reply: *(1) Thank you for this valuable comment. We agree with the reviewer that proglacial areas are very dynamic systems (e.g., LN43, LN47 in the initial MS; see previous reply).*
(2) However, based on predicted discharge data by 2050, the study results show that non-fluvial sediment is already present at the bed of the channel in the canyon. These results claim the tendency of channel bed stabilization. Thus, the results justify the refinement and extension of the fluvial system of a proglacial sediment cascade.

References:
Purinton, B. and Bookhagen, B.: Introducing *PebbleCounts*: a grain-sizing tool for photo surveys of dynamic gravel-bed rivers, Earth Surf. Dynam., 7, 859–877, https://doi.org/10.5194/esurf-7-859-2019, 2019.

Reply: *Thank you for this reference. As it is already stated in the initial MS (LN145), the lower truncation for adequate b-axis length measurements in a digital approach is strongly dependent on the ground sample distance (see previous reply): ≥20 px (Purinton and Bookhagen, 2019); ≥10-15 (Detert et al., 2018) or ≥4 px (Lang et al., 2021).*

Fig. R1 – Planet satellite image of the study area (2018-10-22). This is the situation as described in the manuscript. www.planet.com
Fig. R2 – Planet satellite image of the study area (2022-10-07). Note the formation of an incipient channel in towards the southwest of the old canyon. Future melting of dead ice will likely allow water flow in the center of the valley. Also note how the meander in the lower part of the channel changed its course. www.planet.com

Reply: *(1) We agree with the reviewer that melting dead ice bodies are able to open up new channels. This potential process in the future was already mentioned in the revised MS (LN307-309). In the initial manuscript's Summary and Conclusion (LN333-335), we also mentioned the great relevance of melting dead ice landforms regarding proglacial channel evolution and stabilization of entire proglacial areas.*
(2) We added more information about the (possible) future development of the proglacial area from the fluvial perspective during this revision (see previous reply).