Responses to reviewer comments and changes made to
"Suspended sediment and discharge dynamics in a glacierized alpine environment: Identifying crucial areas and time periods on several spatial and temporal scales in the Ötztal, Austria"
by Lena Katharina Schmidt et al.,
Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-85-RC1, 2022

Dear Editor,

in the following, we are addressing point-by-point the comments of the two referees, Anatoly Tsyplenkov and Ronald Pöppl. We will list our response and the changes made to the manuscript below each referee comment. Unless specified otherwise, the line numbers mentioned in our changes refer to the latest version of the manuscript (with revisions hidden). To prevent problems due to formatting, we also included a pdf version (without markups) with line numberings.

Thank you very much for your support,
Lena Katharina Schmidt on behalf of all authors

Comments of referee 1, Anatoly Tsyplenkov

Changes in how river catchments function due to global environmental changes are an essential topic that increases interest from scientists, managers, and policymakers. The present study addresses this. As such, the work is timely and relevant to ESurf. The manuscript authors provide a valuable long-term dataset of changing suspended sediment loads and water runoff in the Ötztal Alps.
Although I liked the manuscript very much, it cannot be published in its present form. The article requires major revisions. The structure of the article is seriously messed up. It was hard for me to read, and I may have missed a lot of technical errors. Therefore, I would like to reread the manuscript after the corrections.

General comments:

- *There is a bit of a mix-up regarding terminology in the article. In the Introduction and Methodology section, you discuss "suspended sediment yields" (SSY, t/km²). However, later in work, you describe sediment mass fluxes expressed as suspended sediment loads (SSL, t/yr). Moreover, you didn't mention how you calculated sQ, SSY, and SSL. This should be added in the first instance to understand what is going on.*
  Reply: Thank you for pointing this out. Indeed, we decided to use both suspended sediment yields (SSY, for comparability among the sub-catchments) and suspended sediment loads (SSL, to enable the readers to get a feeling for the absolute magnitudes). We are happy to add the respective equations to the methodology and to review whether it will be more easily understandable if we change from SSL to SSY in some instances.
  Changes made: We reviewed the usage of SSL and SSY throughout the manuscript and followed the definition of (MacGregor, 2011), who defined the sediment yield SSY as the "total quantity of sediment, expressed in units of mass (or volume) per unit time". Accordingly, we termed the specific yield [t/km²/a] sSSY. To describe short-term sediment fluxes (e.g. for the event analysis), we use the term sediment discharge Q_{sed} [t/s]. We added a short description and calculations in section 2.2.1.

- *The paper's abstract is poorly written and does not tell the story well. Undeniable statements (like L24ff, L27ff) are mixed with results so that after reading the abstract, it is not clear if anything new has been done. Try to be more specific, highlighting material you use (e.g., water discharge and suspended sediment concentration series from 2006 to 2020). Names of gauging stations are worth mentioning. It would be interesting to the reader to have some*

descriptive statistics in the abstract (e.g., mean annual SSC, Q, or SSY) and main results. A paper by Mensh and Kording (2017) might be helpful.
Reply: We are happy to rewrite the abstract as you suggested (more specific description of data, gauging stations, etc.). However, we fail to understand what you mean by "undeniable statements". Both lines you are referring to describe aspects of our results.
Changes made: We rewrote the abstract to give better context (i.e. tell the story better) and added the gauge names and data as suggested. We also rephrased the description of the results in the abstract so that the most important result is mentioned first and explained afterwards to improve readability.

- *Correct the structure of the article. Dissolve the results from methods and discussion. See minor comments for some suggestions.*
  Reply: We agree, thank you for the detailed suggestions.
  Changes made: We moved inappropriate parts of results (as suggested in the detailed comments below) to methods or discussion or removed them entirely. As a result, we restructured discussion.

- *You are saying (L144-145) that you have measured turbidity at all stations and then recalculated NTU to SSC. However, Fig2 and the corresponding equation only describe lower distribution bounds (0-20 SSC or 0-10 NTU). From Supplementary materials(Vent_Q_SSC), I can see that SSC increased up to 1000 g/l. How did you calculate suspended sediment concentration for values above 20 g/l? Using the same equation for extrapolating the linear model to a high-value area usually leads to significant errors and uncertainties. This case should be corrected and critically discussed. This is the weakest part of the research, questioning your conclusions.*
  Reply: Thank you for this valuable comment, which highlights that we apparently did not make our description clear enough. Firstly, we only refine the calibration of turbimetry at one station (Sölden) and use data provided by the Hydrographic Service of Tyrol for the other two gauges. Secondly, the output given by the turbidity probe in Sölden is already in (tentative) concentration units, i.e. (m)g/l (not NTU), but had to be recalibrated to the actual concentrations using samples. Thirdly – and perhaps most importantly – SSC in Fig. 2 is given in **g/l** (and refers to the station Sölden, see figure caption) while SSC in the supplementary file you refer to is in **mg/l** (and refers to the station Vent) as stated in the metadata. This should resolve your remark about extrapolation. We will improve the description in lines 144 ff. to be more clear.
  Changes made: We improved the description in L145ff.

- *The second weakest point of your research is the visual identification approach of the strongest sediment flux events. This approach is described by you too vague. I insist on adding some criteria, and event statistics. Addinf og descriptive statistics of all Q (m^3/s), SSC (g/l) and SSL (t/event) events will help us (readers) to understand was is «strongest» mean.*
  Reply: Thank you for this comment. We agree that the visual approach may seem somewhat subjective, but we have put considerable thought into how to best identify suspended sediment events. Unfortunately (and as you also point out in your comment on hydrograph demarcation later on), it is not straightforward to automatically identify events. Due to the considerable intra-annual variation in SSC, a threshold-based approach would overlook events early in the year, which stand out against concentrations or yields that occur that time of the year, but with absolute concentrations or yields much smaller than during summer. In turn, monthly thresholds are problematic because interannual differences are high. To program our logger for sampling, we came up with an event detection routine (as briefly outlined in line 161 f.) considering the concentrations in the days ahead and the increase in concentrations, which worked well enough to sample events, but which is unsuitable for complete event identification. For instance, we observed that the occurrence of one event masked the detection of subsequent events in the days after, which disqualifies this approach for the

problem at hand. Similarly, the beginning and end of each event are not easily put into a formula, as not all events do show clear points of inflection before and after the peak. Thus, we came to the conclusion, that visual identification (i.e. expert opinion) is the most straightforward approach to this problem. We will however improve our description on which criteria guided the delineation. We already describe the identified events with respect to Q and SSL in lines 431f (and your comment brought to our attention that the superscripts (e.g. $6*10^6$ to $13*10^6$ m^3) were lost due to formatting, we will correct that as well).
Changes made: We improved the description on event demarcation in section 2.2.2 and corrected superscripts in section 3.6.

- *I understand that there is some evidence that the sediment load at the stations is simultaneously changing. However, what about water runoff? Figure 7 shows that the mean annual Parde's coefficient for Soelden and Tumpen varies equally. Why does Vent stand out like this? It would be interesting to compare the water runoff with the snowfree area too. However, the visual technique you use in Figure 8 requires some quantification. Maybe compare the week of the year of the beginning of the increase in sediment load and water runoff (i.e., the inflection point) with the beginning of the snowmelt at different elevations for different years?*
  Reply: The difference in Pardé coefficients is due to the higher elevation of gauge Vent (as we explain in L 366 to 375) and was not surprising given the existing knowledge on this (e.g. Gattermayr, 2013; Kormann, 2016; Weber and Prasch, 2016; as cited in the manuscript). The respective figure shows mean annual cycles. As such, we do not think somewhat soft features such as "increase in sediment load" should (nor could) be precisely pinpointed to specific week. Therefore, we propose to leave the comparison on the qualitative level.
  No changes made.

Specific comments and technical corrections:

L128-129 — The sentence is unrelated to the rest of the text. What slope is meant? I guess catchment slope like the one from Table 1. Consider removing or improving the phrase. Personally, I find it redundant here.
Reply: Thank you for this detailed comment, we will remove the sentence.
Change made: We removed the sentence.

L130 — I don't understand where the footnotes in Table 1 are headed. Is it like sources of the data used for calculations? Then additional column named «Data Sources» with references may be the better way of presenting. Otherwise, consider moving the phrase from the Title to the Table's bottom (or footnote).
Reply: Thank you for this comment, we understand. Indeed, the footnotes are headed towards the data sources. We chose not to add an extra column, since the table already is quite wide and the data sources are the same for several entries, so a data sources column would include a lot of repetition. We will move the data sources to a footnote at the bottom of the table, as commonly handled in other tables in the journal.
Change made: We moved the data sources to the bottom of the table.

L151 — You said (L151) that 2019-2020 data are preliminary. Why? What makes it preliminary? Is it needed to be checked by authorities?
Reply: Indeed. We received data of the stations Vent and Tumpen from the Hydrographic Service of Tyrol, who quality check the data eventually. That has not happened for the data of 2019 and 2020. We will specify this more clearly here.
Change made: We added a description in L149 and removed the sentence from the table description.

L206 — The first mentioning of the SSY «…we visually identified SSY peaks…» needs abbreviation decoding. Moreover, I suppose you meant SSC here.
Reply: Thank you, we will add the explanation. However, we did indeed mean SSY, since our analysis is aimed at identifying the events with the highest sediment output.

Change made: The first mentioning is now in Line 191 and we added the decoding there.

L144-145 — I'm just curious what turbidity sensors did you use. E.g., model and Manufacturer
Reply: The sensors are Solitax sensors by Hach at all gauges. We will add that to the description in the manuscript.
Changes made: We added model & manufacturer in L 146.

L156 — Again, please mention the model and manufacturer of the automatic sampler
Reply: Thank you, we will add that. It is a P6 L Vacuum by MAXX.
Change made: Added "(MAXX P6 L Vacuum)" in L158.

L175 — write it like an equation
Reply: We will add the respective equation.
Change made: formatted as equation $SSC\left[\frac{g}{L}\right] = 1.8487 * turbidity\left[\frac{g}{L}\right] + 0.0079$ in L 180.

L180 — I'm surprised with the Turbidity dimension. Shouldn't it be NTU or FTU?
Reply: We understand the confusion since many turbidity probes give results in units of NTU or FTU. However, as mentioned above, the raw data of the turbidity probe used here are already given in concentration units as a preliminary calibration happens within the sensor – yet still need to be calibrated with concentrations from a sufficient number of samples.
Changes made: We improved the description in L145ff.

L196 — Please, explain why did you choose a 3 mm threshold. Indeed, you are correct to note that 3 mm is not enough to consider an event as erosional (Renard et al. (1997) suggested a 12.7 mm threshold, for example). Nevertheless, at the same time, it seems that we should separate the snow from the rain more by the air temperature. And that threshold, according to the 2018 study (Jennings et al., 2018) for Tumpen, should be around 1.5 °C, not 0 as you used.
Reply: We agree that the temperature threshold should be changed, thank you for the very helpful reference. However, this will not change the analysis result, since temperature is in fact >1.5°C in all cases.
However, the identification of precipitation events is more intricate. Our precipitation data are point measurements at the gauge in Vent and we know that precipitation within the almost 100 km² catchment above the gauge can be highly variable and is affected by the topography. On the one hand, this is reflected in the precipitation gradient (e.g. L113f.) of about 5% per 100m. Assuming this would be applicable to individual events, a 12.7 mm precipitation event in front of the Vernagtferner glacier at about 2850 m elevation would correspond to about 6.7 mm at the gauge in Vent at roughly 1900 m. This is also reflected in the differences in mean annual precipitation (L303ff: "The mean annual precipitation recorded close to the Vent gauge is 666 mm while areal precipitation of the whole catchment is estimated between 1200 and 1500 mm, and for the 11.4 km² Vernagtferner sub-catchment […] even 1525 to 1900 mm are reported"). Thus, we can generally expect the precipitation measured in Vent to be a lower bound of precipitation falling in the entire (sub-) catchment. Adding to this, considering the possibility of rain on snow events and fluvial erosion, we doubt that the Renard threshold can be meaningful here. Instead, we used the hydrograph shape as additional information (as described in L198f) and used the low threshold of 3mm.
We will improve the explanations.
Changes made: We improved the explanations in section 2.2.2 and changed the temperature threshold in L222.

L200-205 — some additional visualization may be helpful. The hydrograph demarcation by water sources is a very discussable topic, and your way to demarcate it is a bit complex.
Reply: Thank you for this helpful comment. We understand that this part needs more explanation. We have prepared a visualization suggestion (see page 1 in Supplement.pdf; a schematic diagram of an

event in 2020). However, any visualization can only be exemplaric and will fail to describe all possible types of events. Thus, we believe that it would be more helpful to improve the description in the text (as mentioned above). For example, it is very insightful that you are referring to hydrograph demarcation, when we are aiming at SSY events, so we will make this more clear.
Changes made: We improved the description in section 2.2.2.

L239-242 — This chunk belongs to the Methods section
Reply: Thank you. We intentionally placed this paragraph here so that readers would not have to jump back to the methods to understand the graph that follows. However, we agree that we need to describe this in the methods. We will reduce the paragraph here and add the description to the methods.
Changes made: We removed first paragraph in section 3.1 and added calculation of sQ and SSY in the methods section.

L254-261 — While this part is a discussion.
Reply: Thank you, we will move this part to an appropriate place in the discussion.
Changes made: We moved the paragraph to the discussion in L434ff.

L249 — Mean annual discharge per area or specific discharge? I suggest using the same wording in the whole paper. Otherwise, it is confusing. Moreover, mm/a is it mm per annum? It is more common to write mm/yr or mm/year
Reply: Thank you for this detailed remark. We will harmonize the wording and change the units to mm/yr.
Changes made: We changed to "Mean annual specific discharge" in L 255. Since it was brought to our attention after our reply that the recognized SI unit for year is the letter a, we did not change the units in L 255f.

L284-289 — This is a discussion
Reply: We agree and will move this to the discussion.
Changes made: We moved the paragraph to the discussion (L 480ff.).

L291 — Are both p-values equal to 0.001? This is surprising considering the various scattering in Fig4.
Reply: Indeed, the p-values are <-2.2e-16 and 0.001857. Thus, both are (well) below the significance level (commonly denoted as α) of 0.01 (not 0.001, this is not a common significance level and was a typing mistake). We will add that in the form of "(significance level $\alpha = 0.01$)" for clarification.
Changes made: We added "(significance level $\alpha = 0.01$)" in L 292f.

L297ff — This is a discussion
Reply: We agree and will move this to the discussion. As a result, section 3.3 will be very short, so we will merge sections 3.3 and 3.4.
Changes made: We moved the paragraph to the discussion and combined it with the existing discussion. As a result, we merged the former sections 3.3 and 3.4.

L341ff — It is necessary to add the corresponding section in Methods. How did you calculate Parde coef?
Reply: Thank you, we agree that this is missing in the methods. However, as we switched to percentage of annual water yield (see comment on figure 7 below), this is now obsolete.
Changes made: We added the calculation of the percentages of annual water and sediment yields p_w(WOY) and p_{sed}(WOY) in section 2.2.2.

L411 — This is a discussion
Reply: We are sorry, in the version we submitted L411 is the heading of section 3.8. We assume that is not what you meant.
Changes made: We moved the sentence from L406 (in old version of manuscript) to the discussion (L496).

L434 — sediment load not yield, I guess
Reply: Yes, thank you for the attentive comment. We will adjust that.
Changes made: No changes made (as we decided to follow the definition of (MacGregor, 2011), SSY is the consistent term here (L396)).

L473ff — you have already mentioned your aim in the Introduction
Reply: That is correct, however we intentionally repeated it here to make reading easier. We suggest to adjust the first paragraph to rather summarize the important findings from the results section as suggested by Mensh and Kording (2017).
Changes made: As we rewrote and restructured the discussion, we felt that this paragraph was superfluous (especially since we added sub-headings to the discussion to facilitate easy orientation for the reader. Thus, we decided to remove the paragraph entirely, and a short summary of the findings can also be found in the abstract and conclusion.

Fig4. These are exciting results, but I'm not sure that linear regression is the right way to analyze the SSY-Glacier area relationship in your case. Or maybe I understand your graph wrong because of the legend absence. First of all, you should mention that you hypothesize that the glacier area didn't change significantly during 2006-2020. However, from table 1, we know that this is not true (up to 6 % for less than ten years). I guess that the actual distribution of sQ and SSY along the glacier area would be different if you compare yearly SSY with yearly glacier area. The plot like on Figure 4 can make sense only if you compare mean annual values for 2006-2020 with the mean annual glacier area for 2006-2015. That will make your plot look less significant (i.e., only 5 points) but will make more sense.
Reply: Thank you, we will add a legend. We agree, of course the glacier cover changed during this time and ideally we would use annual values here. However, annual glacier area measurements are simply not available, all there is are the glacier inventories of 2006 and 2015. Thus the mean annual glacier area for 2006 – 2015 would be the mean of two measurements, which is not very informative in our opinion.
Changes made: We added a legend to figure 4, and changed the color of the points to differentiate more clearly between individual years and the overall mean.

Fig5. It would help if you avoided your qualitative assessment in the figure caption. Better to add R2 on a graph.
Reply: Thank you. The figure caption is a result of a recommendation by Mensh and Kording (2017) (which you also recommended): "[...] the title of the figure should communicate the conclusion of the analysis". However, we are happy to add the R^2 to the figure.
Changes made: We added R^2 to the figures and revised all figure captions.

Fig6 — This is a good illustration for the discussion ðŸ˜Š
Reply: We are not entirely sure, what this comment is targeting, maybe because the last characters of your comment have been lost. We are discussing the relationships between glacier area and mass balances and Q and SSY in the paragraph starting line 483. If you are suggesting to refer to figure 6 in the discussion, we can add a reference (e.g. in L494).
Changes made: We added a reference to figure 6 in L455.

Fig7 — Why are you using Parde coef and not the same % of annual runoff as for the suspended sediments? Maybe adding standard errors or standard deviation will be more valuable than the min-max range. Again, there is wrong wording: the second graph should be % of annual SSL (sus. sed. load), not SSY.
Reply: We used the Pardé coefficient since it is a standard hydrological index for streamflow seasonality. However, we agree that % of annual runoff will be more consistent and thus easier to follow for the readers and thank you for the helpful comment. We will adjust the figure accordingly, which however does not alter the message (page 2 in Supplement.pdf). Additionally, we used the 25 % and 75 % quartiles instead of min-max here, as a suggestion. However, we disagree on the "wrong wording", since the percentage of annual SSL [t] is the same as percentage of annual SSY [t/km²].

Changes made: We termed the percentages of annual water and sediment yields p_w(WOY) and p_{sed}(WOY) throughout the manuscript, added the calculation in section 2.2.2 and updated the figures and revised the figure captions accordingly.

Fig8 — Is this multiyear average % of SSY and Snow free area? Can you add confidence intervals on lines, then? It is correctly to label the dashed lines simply by the station name as they represent not SSY but the ratio of annual SSY.
Reply: Thank you for the helpful comments. Yes, the version in the manuscript before showed multiyear averages. Since we assume that your question is directed towards the desire to visualize interannual variation, we adjusted the figure by adding interquartile ranges (25% and 75% quartiles, see page 3 in Supplement.pdf). Further, we plotted the median of snow free area per week of year instead of the mean, as this reduces the influence of singular classification errors within the original snowcover data (as e.g. above 3500 m between week 20 and 30 in the earlier version of this plot). We will adjust the figure description and the description in the text accordingly.
Changes made: We updated the figure and revised the figure caption.

Comments of referee 1, Ronald Pöppl

Suspended sediment and discharge dynamics in a glacierized alpine catchments = hot topic in science, but also for managers and policymakers (in line with reviewer 1) and the topic fits the scope of ESurf. In principal, the paper is well written and scientifically sound. However, I have some minor to moderate concerns which should be addressed by the authors before this interesting piece of work is ready for publication (see also attached .pdf):

General comments:

1) Results sections contain a lot of data interpretation and discussion content, which should be moved to the discussion section. Moreover, concluding remarks are presented in the results sections (not appropriate).

Response: Thank you, we agree and will resolve these issues.
Changes made: We revised the results section and moved the respective sections to the discussion (as also mentioned above). As a result, we restructured the discussion.

2) The introduction is a bit minimalistic. More could be said about the importance of this topic, also in water and sediment management contexts. The importance of connectivity is mentioned in the introduction, but not addressed in the rest of the paper?!
Response: Thank you for this comment. We agree and will add a respective paragraph. With regard to your note on connectivity, the paragraph you are referring to aims to give an overview of expected or possible future changes in high alpine environments and mentions connectivity for the sake of completeness. Yet we do not address future changes themselves in this study, but hope to provide a good basis for future studies by analyzing the recent past. In this, assessing changes in connectivity is not within the scope of this manuscript. We will rephrase this section to prevent any misunderstandings and to make it more clear.
Changes made: We added a paragraph on sediment management in L 64ff and touch upon connectivity again in the discussion (L528) and conclusion (L568).

3) Outlook/perspectives are missing.
Thank you for pointing this out. Yes, our paragraph providing an outlook / perspective is a bit short (L565ff). We will extend this paragraph and provide more information on possible fields of application and future research tasks.

Changes made: We added an outlook paragraph to the discussion (section 4.6).

4) Very unusual heading titles in the results section.

Response: Thank you, we will rephrase the headings.
Changes made: We revised the section headings. That lead to resolving the former sub-sections 3.1 and 3.2 to the new subsection 3.1, and likewise 3.3 and 3.4 to the new 3.2, 3.5 and 3.6 to the new 3.3 as well as 3.9 and 3.10 to the new sub-section 3.6.

5) No spaces between numbers and units, and no commas in large numbers have been used.
Response: Thank you for pointing this out. We will scan through our manuscript and correct numbers and units accordingly.
Changes made: We corrected these incidences throughout the manuscript.

Specific comments:

1) Lines 160-165: Temporal resolution of automatic sampler probing? Criteria for sampling time (event-based)?

Response: We assume you are referring to the time it takes for one sample to be collected and the time lag to the next sample? In this case, the collection of one 1 L sample takes about 1.5 minutes and we specified that two samples must be at least 30 minutes apart. For the event-based sampling, we programmed the logger to calculate the absolute difference between the present turbidity measurement and the measurement 30 minutes before for each time step. This difference had to be greater than a threshold we had determined empirically based on past turbidity recordings beforehand. As a second criterion, the present turbidity needed to be higher than the moving average of the turbidity of the last 10 days. This was designed to function as a seasonally adjusted threshold, because intra- and interannual differences can be very large. So, in simple terms, the rise in turbidity needed to be steeper than the empirical threshold and the absolute level of turbidity needed to be higher than the moving average of the past 10 days.
We will update our manuscript and extend the information on the sampling scheme used within the framework of this study.
Changes made: We added further description in L 161ff.

2) Line 196: Why "3 mm"? Why not 4, 5 or 7?
Response: Thank you for this question. We agree that any exact threshold here will we arbitrary to some degree. As we pointed out in out Responses to reviewer I (who suggested a 12.7 mm threshold as commonly used in RUSLE), the identification of precipitation events is intricate. Our precipitation data are point measurements at the gauge in Vent and we know that precipitation within the almost 100 km² catchment above the gauge can be highly variable and is affected by the topography. On the one hand, this is reflected in the precipitation gradient (e.g. L113f.) of about 5% per 100m. Assuming this would be applicable to individual events, a 12.7 mm precipitation event in front of the Vernagtferner glacier at about 2850 m elevation would correspond to about 6.7 mm at the gauge in Vent at roughly 1900 m. This is also reflected in the differences in mean annual precipitation (L303ff: "The mean annual precipitation recorded close to the Vent gauge is 666 mm while areal precipitation of the whole catchment is estimated between 1200 and 1500 mm, and for the 11.4 km² Vernagtferner sub-catchment […] even 1525 to 1900 mm are reported"). Thus, we can generally expect the precipitation measured in Vent to be a lower bound of precipitation falling in the entire (sub-) catchment. Adding to this, considering the possibility of rain on snow events and fluvial erosion, we doubt that the Renard threshold can be meaningful here. Instead, we used the hydrograph shape as additional information (as described in L198f) and used the low threshold of 3mm.
We will improve the explanations.

Changes made: We improved the explanations in section 2.2.2.

3) Lines 209-210: "Therefore, we did not classify the events with respect to precipitation events." … possible implications?
Response: Thank you for this comment. We will add a short sentence on the implication (i.e. that we cannot make a point on how the importance of precipitation events changes in space.)
Changes made: We added the implication to the discussion L 524f.

4*) Lines 229- 231: Is this assumption valid? More susceptible to erosion: yes, but unfrozen?*
Response: This assumption derives from the paper of (Li et al., 2021). We agree that ground no longer covered by snow can still be frozen, thus of course this assumption is a simplification. Yet we believe it is okay to make this assumption, considering the bigger picture (of the almost 800 km² study area and averaging over 17 years) and as we explicitly state that we consider the snow free area as potentially erodible (which implies uncertainty). We will add this to the discussion.
Changes made: We have specified that we understand the snow-free area as a proxy for the activation processes relevant to sediment dynamics in the discussion in L 493ff.

5) Page 14: Move Fig. 8 below the para in which it is mentioned in the text.
Response: Thank you, we will do that.
Changes made: We moved figure 8 below the first mentioning in the new section 3.4.

Li, D., Overeem, I., Kettner, A., Zhou, Y., and Xixi, L.: Air Temperature Regulates Erodible Landscape, Water, and Sediment Fluxes in the Permafrost-Dominated Catchment on the Tibetan Plateau, Water Resour. Res., 57, doi: 10.1029/2020WR028193, 2021.

MacGregor, K.: Sediment Yield, in: Encyclopedia of Snow, Ice and Glaciers, edited by: Singh, V. P., Singh, P., and Haritashya, U. K., Springer Netherlands, Dordrecht, 1014–1016, doi: 10.1007/978-90-481-2642-2_480, 2011.