Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/esurf-2021-85-RC1, 2022

Comment on esurf-2021-85

Anatoly Tsyplenkov (Referee)

Referee comment on "Suspended sediment and discharge dynamics in a glaciated 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

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.
- 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.

- Correct the structure of the article. Dissolve the results from methods and discussion. See minor comments for some suggestions.
- 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.
- 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.
- 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 snow-free 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?

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.

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).

L151 — You said (L151) that 2019-2020 data are preliminary. Why? What makes it preliminary? Is it needed to be checked by authorities?

L206 — The first mentioning of the SSY «…we visually identified SSY peaks…» needs abbreviation decoding. Moreover, I suppose you meant SSC here.

L144-145 — I'm just curious what turbidity sensors did you use. E.g., model and manufacturer

L156 — Again, please mention the model and manufacturer of the automatic sampler

L175 — write it like an equation

L180 — I'm surprised with the Turbidity dimension. Shouldn't it be NTU or FTU?

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.

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.

L239-242 — This chunk belongs to the Methods section

L254-261 — While this part is a discussion.

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

L284-289 — This is a discussion

L291 — Are both p-values equal to 0.001? This is surprising considering the various scattering in Fig4.

L297ff — This is a discussion

L341ff — It is necessary to add the corresponding section in Methods. How did you calculate Parde coef?

L411 — This is a discussion

L434 — sediment load not yield, I guess

L473ff — you have already mentioned your aim in the Introduction

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.

Fig5. It would help if you avoided your qualitative assessment in the figure caption. Better to add R2 on a graph.

Fig6 — This is a good illustration for the discussion ð□□□

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.

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.

Jennings, K. S., Winchell, T. S., Livneh, B., and Molotch, N. P.: Spatial variation of the rain–snow temperature threshold across the Northern Hemisphere, 9, 1148, https://doi.org/10.1038/s41467-018-03629-7, 2018.

Mensh, B. and Kording, K.: Ten simple rules for structuring papers, PLOS Computational Biology, 13, e1005619, https://doi.org/10.1371/journal.pcbi.1005619, 2017.

Renard, K. G., Agricultural Research Service, W., Foster, G. R., Weesies, G. A., McCool, D. K., and Yoder, D. C.: Predicting soil erosion by water: a guide to conservation planning with the Revised Universal Soil Loss Equation (RUSLE), 1997.