Anonymous Referee #1 from 20 Oct 2025

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Comments

I should note that my background is not in the kind of precipitation analyses presented in the paper but rather sediment sources and transport in glacial and paraglacial systems. This means that my review of the paper is only partial.

Overall, this paper is worth publishing in my view. It is extremely well-written and well-presented. It has an importance as whilst it builds upon similar papers from authors of this one, and with overlapping geographical foci, the topic is important as there is an ongoing debate over how the transition from glacier-melt dominated to rainfall-dominated catchments impacts suspended sediment yield from deglaciating basins. The analysis is largely very well done (I make some more minor comments below). On this basis I would hope that the paper can be published after some revision.

That said, I do think the authors need to be much more careful in how they present and interpret this work and I would recommend that they make some revisions.

1. The general positioning of the paper is stronger on the hydrology than on the glaciology. There is actually a very established literature on how glaciated catchments produce suspended sediment and the processes that drive it (e.g. the work of Darrel Swift, Angela Gurnell, David Hannah) and I think this should be brought into the paper. Note David Hannah's work is also important for hydrograph analysis in this kind of setting. This is not least because you cannot assume that extreme precipitation and melt-driven suspended sediment production events are entirely independent. Some of the previous work by the authors recognises this and the point is made at l502. It is quite possible that your extreme rainfall events do not change erosion but rather increase sourcing of subglacial suspended sediment. We know that surcharge of subglacial drainage systems by water, whether melt, precipitation or precipitation-indeed melt, leads to very major suspended sediment yield, especially in "spring events". These don't just occur in spring but can occur at any time and they likely interact with other variables such as snow cover on the glacier. The discussion that then follows from around l410+ here appears to be highly speculative as we have nothing on this kind of driver but a lot on classical kinds of suspended sediment sources associated with very different kinds of environments.

> ***Reply:*** *The referee makes an excellent point about precipitation and subglacial sourcing. We now introduce the melt component to the extremes much earlier in the manuscript, and also include this in the interpretation of processes responsible for the detected relationships and patterns.*

2. Part of the problem here is that with two gauging station records of this kind, separating out individual and joint effects is not easy, and not least because (see below), your flux and yield data are collected from discharge which in turn will be a function of precipitation; so when you correlate precipitations variables with flux and yield data you have an inevitable correlation. The authors might have been much better to do a more event-based analysis than they have done.

> ***Reply:***

Regarding correlation between precipitation characteristics and sediment response variables

While we do include correlations between heavy precipitation event characteristics and corresponding suspended sediment response (e.g. Table 2), we by no means argue that the fact that rainfall characteristics and sediment flux is correlated is the main result. The two main findings we wish to highlight is:

1. Differing responses in fluvial sediment transport during sub-daily and long-duration extremes.

2. The role that heavy precipitation events play in the fluvial sediment transport regime and whether their influence in changing.

We believe this is also reflected in this taking up most of the discussion. However, as the reviewer notes, we should be more careful in our interpretation and discussion of processes responsible for the detected relationships and patterns. We will rework the discussion to include a more nuanced interpretation of processes.

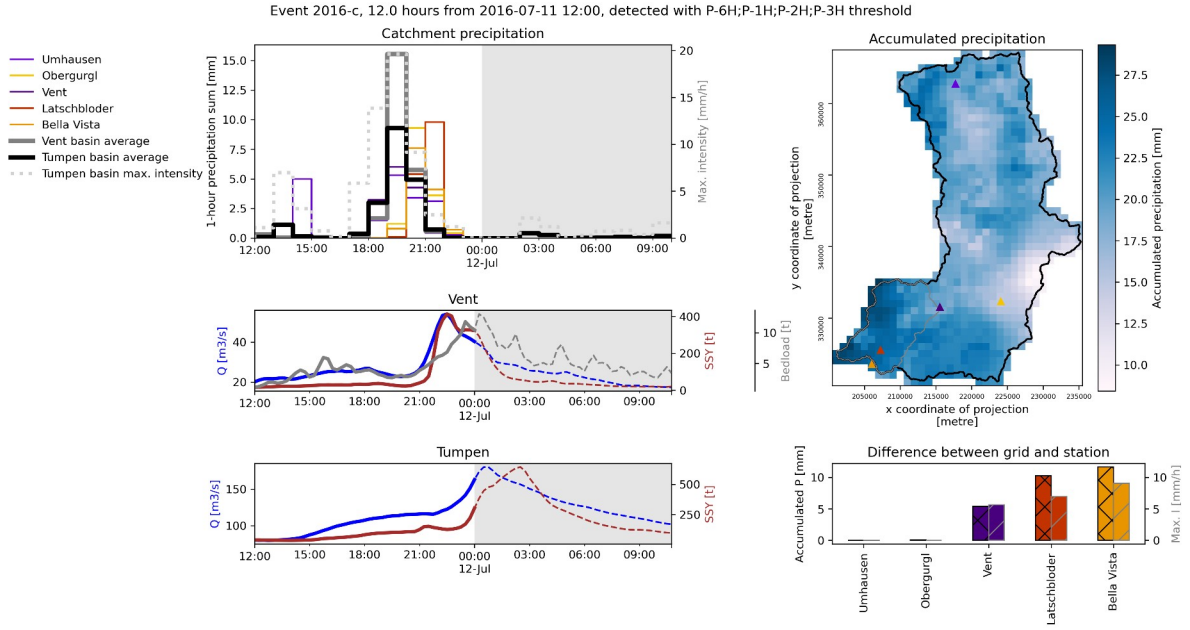
Regarding the comment to do a "more event-based analysis"

The referee does not specify what type of event-based analysis they want to see.

First, we would argue that our analysis is already "event-based" in the sense that the basis for our analysis is to partition the data into events. An analysis could for example be based on daily data where the whole sediment transport season is analysed.

Second, there is actually more event-based analysis and evaluations "under the hood" of our analysis presented in the manuscript. The figure below is an example of a "dashboard" created for each heavy precipitation event to examine the precipitation pattern, include differences between the gridded precipitation product and stations, and show the hydrograph and sedigraph at the two gauging stations.

We propose to expand this dashboard figure to include the hysteresis pattern, and, if feasible, information of snow conditions and melt-potential, e.g. positive degree days, which was also requested by another referee. The figures for all events will be added to the supplementary materials. We do however think, that included too much discussion of single events would dilute the main findings of the paper as highlighted above. We see the main contribution of our paper to be a contribution to the "ongoing debate over how the transition from glacier-melt dominated to rainfall-dominated catchments impacts suspended sediment yield from deglaciating basins" as the referee very nicely puts it.



3. The same problem under (1) is then reflected in the literature, how the paper is both set up and discussed. I am not sure how much time the authors have actually spent in this kind of river basin during extreme events, but I am concerned to see a number of papers developed for suspended sediment / soil erosion in very different environments being transferred uncritically into this one. Is Wischmeier and Smith (1978) really relevant to this case? Dunkerley's excellent work from Australia? Processes are invoked to explain results (e.g. soil erosion due to overland flow) when field measurements of such processes question whether they really occur in at least some of the environments described here. Specifically, it is really rare to see classic overland flow in these kinds of catchments in these kinds of environments unless there is some interaction with frozen ground. The unconsolidated sediments you note on l510+ have exceptionally high vertical infiltration rates unless they contain buried ice (they typically contain very low clay fractions, for instance). The (albeit restricted) literature on shallow groundwater flow in deglaciating environments which actually suggests very low degrees of surface hydrological connectivity and hence overland flow generation – Tom Müller's work is a good starting point (e.g. in HESS). So do these environments produce overland flow and soil erosion? What is the state of the soil development processes in this river basin that may lead to higher rates of moisture retention and overland flow generation? Where has soil developed? What is the link to buried ice (in the most upper parts of the catchment) which can be a crucial generator of impeded drainage and surface saturation? What evidence is there that processes matter besides basic fluvial reworking of former glacial sediments in high discharge events? There are many processes that could explain the results obtained and the review needs to be more balanced as there is a gap between the high quality analysis and the interpretation presented. Two things are required in revision; (a) a much more thoughtful and careful use of supporting literature, refocused on what we know of the geomorphology and hydrology of these kinds of Alpine landscapes, both in the introduction and the discussion; and (b) much more circumspect interpretation as there is only so much you can do to disaggregate catchment-scale yield data to identify potential sources within the river basin.

> ***Reply:*** *Thank you for this constructive critique. We agree that our interpretation must be grounded in glacial–paraglacial process understanding in addition to rainfall-driven erosion. In revision we will:*

1. *Refocus the literature: We will substantially reduce or remove citations that are not directly transferable (e.g., Kampf et al. (2016), Shen et al. (2016)) unless explicitly framed as conceptual background, and we will expand coverage of Alpine-relevant work (e.g., Swift, Gurnell, Hannah; studies on shallow subsurface flow and limited surface connectivity such as Müller; and recent paraglacial process studies).*

2. *Interpretation and discussion of processes: We will rework the discussion of processes to be more nuanced, and revisit the argumentation around overland flow and related erosion processes.*

   While the referee makes an important point about infiltration rates and shallow groundwater in unconsolidated sediments, in the recently de-glaciated areas of the Vent-Rofental basin (where the authors incidentally have been for fieldwork on several occasions) the sediments can also have quite high fractions of clay and silt. Therefore we do not find it unreasonable to assume that runoff generation through infiltration-excess can play a role during heavy precipitation events. There is furthermore visible signs of rill and gully erosion within the catchment.

   Also, the authors have, in fact, spent much time in the study area, also during a heavy precipitation event (more specifically event 2023-g).

3. *Adopt a more circumspect tone: We will emphasise that catchment-scale SSY integrates multiple sources and pathways, limiting source attribution. Where we cite studies from other environments, we will state why they are relevant (or note their limitations) for the Alpine setting.*

4. Point (3a) needs to be supported by better river basin characterisation. For each station we need to know contemporary % ice cover (we are just told it is 10% for whole system what is it for Vent). It would also be good to know what % of the catchments were ice covered during the Little Ice Age and how vegetation cover has developed since, given its potential impacts on erosion processes. This would help us to get a better understanding of process regimes associated with the basin.

> ***Reply:*** *Thank you for this helpful suggestion. We agree that a clearer basin characterisation will strengthen the interpretation. Previous work in the basin by Schmidt et al. (2022) has examined this in more detail, and we will add the major points to enable the reader to better interpret the results presented.*
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> *We suggest to add a table with glacier cover percentages for both the whole Ötztal and the Vent subcatchment using available inventories (LIA, 1960, 1998, 2006, 2015). We will also quantify the area deglaciated since the LIA, and, if feasible, show this in Figure 1. In addition, we will add information on current vegetation and sparsely vegetated surfaces to better illustrate spatial contrasts in surface stability and erosion potential.*
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> *Schmidt, L. K., Francke, T., Rottler, E., Blume, T., Schöber, J., & Bronstert, A. (2022). 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. Earth Surface Dynamics, 10(3), 653–669. https://doi.org/10.5194/esurf-10-653-2022*

5. The analytical approach in Table 2 is a bit weak. Your SSY data are effectively the sum of SSC*Q. Q is directly driven by precipitation. So, you would expect strong P versus SSY correlations. What is perhaps more interesting is to look at when P and SSY is not correlated – this

might say much more about what is going on. Note, I also struggled to understand which datasets were being used for Table 2 – Tumpen or Vent?

> ***Reply:*** *We agree that, since SSY largely reflects the product of SSC and discharge, and discharge itself is closely linked to precipitation volume, a correlation between P and SSY is to be expected. Nevertheless, we consider it important to show both yield and flux, in addition to peak concentration, as these variables reflect different aspects of catchment response. We will also add average SSC to Table 2 for completeness.*
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> *What we find particularly interesting and what Table 2 is intended to highlight, is that it is important to consider rainfall and not just precipitation. This is not a trivial point in our setting, because part of the precipitation even in summer can occur as snow, which contributes little to immediate runoff and sediment mobilisation.*
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> *Furthermore, we want to show that rainfall intensity (both peak and mean) is more strongly associated with the sediment response variables than total rainfall amount. The pattern that rainfall peak and average intensity correlate more strongly with both SSY and peak SSC suggests that it is not simply the total water volume that matters. This also holds for peak SSC, which does not explicitly include the discharge (although it is of course not wholly independent, given the positive relationship between discharge and SSC in the catchment).*
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> *We will make these points clearer in the manuscript both in the results and discussion.*
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> *We acknowledge that we omitted to specify the dataset source; Table 2 refers to Tumpen and we will clarify this in the revised manuscript.*

6. Figure 6. Some of these relationships look like a linear fit it being done to a non-linear dataset. This is really difficult to see with these plots, especially with the marker being scaled by even[t] duration. Non-linear relationships here might tell you a lot.

> ***Reply:*** *Note that in the linear regression, the sediment response variables, i.e. the dependent variable, has been log10 transformed. We have examined the residuals for each linear regression model, and they are normally distributed (see figure below). Therefore, find it reasonable to apply a linear regression model. The figure will be added to supplementary materials.*