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Discussions

Interactive comment on "Controls on spatial and temporal variability of streamflow and hydrochemistry in a glacierized catchment" *by* Michael Engel et al.

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Response to Reviewer #2

"Controls on spatial and temporal variability of streamflow and hydrochemistry in a glacierized catchment" by Engel et al.

General comments: I have only one major concern: with almost 3000 meters of elevation gradient and highly variable aspect and shading, only one meteorological station is used for the niveo-meteorologial variable determination. For example the snow depth (maximum depth, timing of melt) in Fig. 7 would likely be very different at different el-

evation ranges. The spatiotemporal variability in snowmelt at different altitudes can be a major reason for masking the tracer variability, and not creating a "coherent" tracer signal of snow and glacier melt (see discussion on L 627). Some discussion present on P20L593, but in my opinion the uncertainty caused using only one meteorological station this should be more discussed.

We thank the reviewer for her/his work in reviewing this manuscript and appreciate the comments and suggestions made to help improving this manuscript. We agree on the concern regarding the representativeness of using data from only one meteorological station. We will address this aspect within the discussion by arguing as follows: first, the network of meteorological stations available in the study area comprises 3 high-elevation stations and 1 valley stations. However, only the Madritsch weather station as high-elevation station includes snow depth measurements. As we state in the manuscript, its elevation is similar to the lower tongue of surrounding glaciers, so that we assume its data representativeness for similar elevation bands within the catchment and thus the lower glacier covered areas. This fact motivated our aim to focus on the importance of high elevation meteorological conditions and their relation to downstream streamflow and hydrochemistry variability.

In this context, however, it is true that not only the same elevation controls snowmelt but also spatial variability such as aspect, slope, and microtopography (e.g., Anderton et al. 2002; Grünewald et al. 2010; Lopez-Moreno et al. 2013). This usually leads to different melt rates and thus affects the isotopic snowmelt signature (Taylor et al. 2001; Taylor et al. 2002; Dietermann and Weiler, 2013; Schmieder et al. 2016) and the hydrometric response in the main channel such as the timing of the discharge peak (Lundquist and Dettinger, 2005). Another point we will mention in the discussion considers the representativeness of the outlet sampling time with respect to the peak discharge time at that location. In fact, the peak of hydrochemical response may not be synchronized with the hydrometric one and therefore may lead to stronger or weaker relationships.

As a consequence of this aspect on uncertainties mentioned above and with respect also to the comment of reviewer#1 on the storyline of this manuscript, we will remove the figures 9 to 11 in its current form. Instead, we will show both the nivo-meteorological parameter variability, their relationships among each other and the temporal sensitivity of these parameters by a different graphical way, such as using boxplot diagrams. Choosing boxplots as diagram style will also underline the variability given by each parameter. Resulting from the different uncertainties associated with this data presentation, we decided that potential parameter correlations can also be derived from visual inspection. âĂČ Comment 1 P2L37: Cannot understand this sentence: what is meant with best agreement when time lengths varied?

We agree and will rephrase this part.

Comment 2 P4L112: Why would you assume this? The hypothesis sounds somewhat trivial, and too tailored to what you found in your data.

We agree and will modify the hypothesis.

Comment 3 P4L121: aim to characterize the hydrochemical signature of thawing permafrost: this does not get much attention in the rest of the manuscript, and you don't have that many water samples from permafrost thaw water either. Either reformulate the objective, or discuss the success/failure of this objective in the manuscript.

We agree that this aspect requires more care. We will consider this aspect by rephrasing the discussion accordingly. See also the response to the first comment by the first reviewer about the reformulation of the research objectives.

Comment 4 P5L141: permafrost is "sparsely located"? Can you use typical terminology for permafrost occurrence: isolated, sporadic, discontinuous.

We agree and will add "Discontinuous permafrost".

Comment 5 P6L176: I'm not familiar with "rock glaciers", perhaps explain the landform when first mentioned in the text.

We agree and will modified the sentence as follows: "As rock glaciers are considered as long term creeping ice-rock mixtures under permafrost conditions (Humlum 2000),. . ."

Comment 6

P8L230: do you exclude the events, where there is zero change in snow depth (no snow)? Seems so in Fig. 11.

Yes, we excluded snow depth changes between – 2 cm and + 2cm to remove noisy data. We agree that a better clarification is needed here, which we will address by adding this information.

Comment 7

P9L255: What do old and new water mean in this context? If I understand correctly, with Eqs 2 and 3 you are determining relative contributions from each tributary, and not any event water or other new water contribution

We agree and will remove the misleading sentence. A similar comment was also made by Reviewer #1. âĂČ Comment 8

P9L271: I would not agree that snowmelt isotope signal is enriched from the original through the process of melting. There is an aspect of temporal variability during melting, but I would argue that the "bulk" enrichment happens through gas with water vapour exchange and sublimation in the snowpack. See e.g. Earman et al (2006) and Taylor et al (2001)

We agree and will add these references. We will change the sentence to ". . .through isotopic exchange between liquid water and ice during melting conditions (Taylor et al., 2001),. . .".

Comment 9 P10L284: extra parenthesis?

We will remove the additional parenthesis.

Comment 10 P11L308-321: It is not obvious when the snowmelt period is. Can you provide a hydrograph in the heat map, or describe in the text

We will add more details on the melting period, to which the tracer description is referring, in the text.

Comment 11 P11L329: I don't see how the data presented shows, the relative temporal variability between the two catchment, as suggested by the authors

We agree and will address this point. The temporal description complements the spatial description of runoff contributions, previously mentioned. As these data refer to the two-component HS and are not shown in a table or figure, we will add “data not shown” at the end of this paragraph to make it clearer.

Comment 12

P12L358: discussion, not results section

We agree and will move this paragraph to the discussion section.

Comment 13 P13L367: Did you measure the EC in glacier melt? Would be useful to verify the low EC water is coming from glacier melt

Yes, we measured the EC of glacier melt and found an average EC of 36.1 μS cm-1 and an average of 13.51 ‰ in ïĄď18O. These data and, additionally some data on snowmelt, will be reported in the text.

Comment 14 P14L401: wording: “clearly anticipated”?

We agree and will replace it by “distinctively earlier”.

Comment 15 P14L405: please indicate this event more clearly in Fig. 7, now difficult to find the data you are discussing.

The period of interest is well visible from our perspective as it covers autumn 2015. However, we agree that some modifications in Fig. 7 will be helpful to improve its

visibility.

Comment 16 P16: not sure if section 4.1 is relevant for this work. Please consider removing it, or clarify why it is important for interpreting your results.

We thank for this comment and will address this aspect when restructuring the manuscript story line and its research gaps, as raised by Reviewer #1.

Comment 17 P 17: section 4.2 is interesting speculation on the interplay between geology and hydrology, but geochemical processes discussed here goes beyond my expertise to critically evaluate the discussion.

We appreciate your comment.

Comment 18 P19L575: rephrase or remove “While SD was used in this study,”

We agree and will remove this sentence.

Comment 19 P20L584: I think the control of T and G is specific to glaciated/permafrost catchments, where these variables remain important in sustaining water input even after snow has disappeared. I would not expect such a strong relationship in catchments without the possibility of thawing the glaciers/permafrost on warms days.

We agree and think that this point requires further attention. We will address it by integrating a short discussion paragraph in the discussion section.

Comment 20 P20L586: I think the data you present if a bit far from providing evidence of any kind of tipping points: too speculative.

We think that the comment on tipping points in the context of threshold-like controls is important. However, we agree that the data presented here are not exhaustive to proof the presence of general tipping point mechanism. Therefore, we will modify as follows: “. . .the importance of threshold-like controls at the daily and short-term scale, as described along the cascade from atmospheric circulation and local climate to hydrology. . .”.

Comment 21 P20L612: interesting idea that the different travel times could be detectable for the correlation coefficient.

We appreciate your comment. âĂČ References:

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