The Cryosphere Discussions

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Comment on tc-2022-17

Anonymous Referee #1

Referee comment on "Estimation of water residence time in a permafrost catchment in the Central Tibetan Plateau using long-term water stable isotopic data" by Shaoyong Wang et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-17-RC1, 2022

This study uses stable water isotopes to look at the mean residence time (MRT) for a catchment in the Tibetan Plateau. The novelty here is the long-term nature of the data series being leveraged for the MRT estimate as these types of sampling campaigns are challenging to coordinate in cold and alpine regions.

The study is well written and well structured making it easy to read. Still, I do struggle some with the uniqueness of the study presented as while these data types of are challenge to collect and not often presented in the literature, there is a question of what we learn here for this catchment that advances beyond previous regional efforts like in Song et al. (2017)? I think bringing forward the improved process understanding in face of the possible uncertainty is needed here to move this manuscript beyond a presentation of the uniqueness of place that leverages data alone.

One aspect that needs attention is the intercomparison of MRTs between various catchments and studies presented in the manuscript. I appreciate the effort and thinking to place this one catchment in a broader context; however, the different methods and models used when estimating MRT can have significant impacts on the resolution MRT and the entire travel time distribution. Caution is needed when comparing absolute MRT with other catchments. I think if the authors want to keep these comparisons, more information needs to be added (like a column or two in Table 4) indicating the model type and technique used to estimate MRT. Further, a richer discussion of the impacts of the modeling assumptions should be provided as they pertain to this region. There has been significant research and literature on these topics over the last decades and it seems some of the more modern interpretations are missing from this study. All in all, I would anticipate a more thoughtful consideration of the assumptions behind the convolution approach you are implementing here.

In addition, if there is a connection between the MRT and the unique processes in

permafrost environment, it would be more insightful to describe them explicitly. Modeling literature (e.g. Frampton and Destouni, 2015) exists on the subject and would help reduce the ambiguity connecting water movement and process as they are considered in this study. Further, and connected with this comment, there is need to separate the result and discussion section in to two separate sections. Given the amount of data being presented and the analysis put forward, plenty of material for results. Also, mixing the two sections together as is currently done creates confusion about what your data show and how you are interpreting it relative to the science. And it would be good in a separate section of the discussion to consider more the potential limitations of the current study as they pertain to assumptions, data representativeness and the models being considered.

Given the complexity of sampling precipitation in cold regions, more information is needed to help the reader understand how you were sampling here. For example, were how was snow treated throughout the sampling? Were snowpacks or snow melt water collected and considered as inputs in any sense? Also, looking at the variation in elevation in the region, how representative of the catchment is the one meteorological station and precipitation sampling location? Rainfall isotopic composition is rather variable with elevation and snowpack and snow melt rates are really variable. How is the isotopic input variability considered within this study? It seems ignored based on the methodology presented.

The input variability and source water variability of only having one location for monitoring supra-permafrost water sampling seems as if it could confound the results and interpretation to some extent. Specifically, if there are large frozen regions upstream of the stream sampling location, these would have significant impacts on the ability of precipitation to transfer to the stream over the entire catchment. Variability of isotopic compositions in springs and sub-watersheds is well documented (e.g. Lyon et al. 2018). The spatial variability at play in the catchment must be either accounted for or the potential impacts at least taking into consideration via discussion within this study.

Finally, some consideration of uncertainty should be presented. There are several fitted relationships that are being compared across the research. In and of themselves, these are wrought with uncertainty and confidence intervals that can impact the significance of the findings. I would want to see some assessment of the robustness of the results relative to the uncertainty or lack of representativeness of the data being presented. At the least, the two-component hydrograph can directly incorporate the uncertainty via the approach put forward by Genereux (1998). Without characterization of the uncertainty, I am left wondering how much of the results is driven by under-represented variability in the sampling at a catchment scale, the simplifying assumptions within the model, and the fitted equations that smooth out all the between event variability and extremes. That last point is rather important given potential flashy nature of these systems during certain times of the year and more dampened responses as the systems thaw seasonally.

Minor Comments

L100: This sentence is random and does not make sense here. Further, not sure what you men with efficiently?

L171: This first sentence is odd. Separate the results and discussions to increase presentation clarity.

References

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