The Cryosphere Discussions

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Comment on tc-2022-17

Anonymous Referee #2

Referee comment on "Estimation of stream water components and 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-RC2, 2022

The paper by Wang and co-authors entitled "*Estimation of water residence time in a permafrost catchment in the Central Tibetan Plateau using long-term water stable isotopic data*" leverages a unique data set in a remote environment to shed light on the 'mean residence time' (MRT) of groundwater and stream water using transit time approaches. They seek to highlight how the active layer and permafrost influence MRT and draw inferences on permafrost hydrology.

The paper is well written and clear. The figures are straight-forward and interpretable. I have a number of editorial comments at the end, yet I have considerable concerns about the analysis that I believe must be addressed before this manuscript is suitable for publication. The data is novel and is of considerable value to the hydrological and permafrost community, yet there are large uncertainties and at times I believe mis-application of methods that the authors need to consider before this manuscript is suitable for publication.

General Comments:

~~The authors use the exponential model as opposed to the more widely used gamma distribution. I am curious as to why this is. This will have considerable impact on the MRT calculation and should be discussed.

~~The correlation analysis is highly flawed and must be revisited. It appears the authors use any type of correlation against variables with different units, etc., to 'look around for relationships'. This is not statistically robust in any way. Values must be transposed/normalized to compare among factors, and the type of correlation must be explained. Figure 6 shows two 'best fit' lines with either low or no relationships. This is a regression analysis. Furthermore, this data is ALL serially correlated which needs to be

accounted for. As it stands, the authors have a lot of work to do to justify this analytical approach. Binning data together from across seasons, etc., truly make this confounding as the active layer changes.

~~More information on the IHS method is needed.

~~More appropriate literature is needed, along with less bold statements about the importance/influence of this work.

~~Many of the conclusions are not supported by the data.

Line Comments:

~~Line 36. "The progressive increase of the permafrost active layer thickness has exacerbated the increased water storage capacity of permafrost and exerted a higher contribution of groundwater to river water." This is statement is incorrect, do the authors mean the active zone? I do not think the active capacity of permafrost has increased.

~~Line 40. "However, it remains unclear how permafrost changes would alter water storage and movement in permafrost catchment." Is this true? I think there is considerable literature suggesting otherwise.

~~Line 52: "Therefore, the influence of permafrost changes, climatic factors, and vegetation variations on catchment MRT in a high-altitude permafrost catchment is seldom evaluated". This is true, but MRT and water ages have been reported and should be cited.

~~Line 68: "The findings from our study will deepen our understanding of the hydrological process in permafrost regions and will be important for water resources supply and safety in the TP." I am unsure if this manuscript does this. There is little talk of water supply and safety and the last sentence of the introduction should be strengthened.

~~Line 171: the first sentence makes no sense and the first paragraph beginning line 170 is very confusing.

~~Line 178: What other source waters would there be other than precipitation?

~~Line 183. I do not believe Xia et al., 2021 is the appropriate reference.

~~Line 185. I am unsure how the thawing and freezing of soils affects this. More details are needed. Also the next sentence regarding the slower slope associated with longer residence time. This is confusing and I'm not sure correct. The final sentence (line 188/9) also needs appropriate support.

~~Line 212. Appropriate historical reference are needed.

~~Line 214. More information on how freeze-thaw cycles affect isotopic composition are needed if the authors are invoking it.

~~The correlations yield some results that do not make a lot of sense and literature cited is incorrect. For example, line 221-223 not supported, and the Landerer 2010 reference is form a very different scale endnote appropriate.

~~Line 233/4 needs to be rewritten.

~~The paragraph starting Line 235 does not make sense to me. Particularly at the end. Precipitation obviously has an influence on active layer water - I'm not sure what the authors are getting at. Is it that there is no relation between the isotopic composition of active layer waters and precipitation isotopes? If so, this should clearly be stated.

~~It is not clear how the two-component IHS is applied, and how the values are determined. Are the average precipitation values volume weighted? How was snow accounted for? Was the IHS applied for the entire period to get these numbers (62 and 38%?). To compare these results to others in the literature, there is a lot more information that is needed. Did the authors consider IHS among years to assess mechanisms of variability to link to process?

~~Line 271 to 273 are very confusing and need to be rewritten.

~~Line 299/300. "The longer MRT reflects more complex soil water retention 300 and recharge processes (Ma et al., 2019b)." This is not clear at all. Why? A link to process must be made.

~~Line 327 - correct the terminology.

~~Line 329: "These strong correlations indicate that soil and air temperature are potentially efficient predictors of supra-permafrost water MRT". I do not support this statement at all. What is the mechanism? Is it the correlation analysis? It is a bold statement with little support.

~~Line 336: "These results also support the findings from previous studies in terms of a relationship between permafrost changes and residence time. In particular, (Wright et al., 2009) have stated that MRT of permafrost catchments is highly dependent on the annual development of the active layer." I could not find any information on MRT in the Wright paper.

~~Line 350: "The larger precipitation corresponds to lower temperature, yielding a thinner active layer, which, in turn making the active layer water to be saturated sooner". Is this with respect to this study? This is no a general or predicted finding.

~~ Line 357: "Interestingly, we found that the stream and supra-permafrost water MRT are both negatively correlated with NDVI (R2= 0.29 and 0.53, respectively)" The entire issue of linking MRT to 'factors' in a regression analysis is flawed. Processes and explanations must be provided. Why would this be? There is some speculation but this could easily be spurious.

~~ Line 364: "However, it remains unclear whether a positive feedback mechanism exists between vegetation and permafrost active layer changes or not." There is considerable literature on this that should be referenced.

~~ Line 365: "Moreover, the optimum residence time for vegetation growth should be elucidated in future studies as well." I am unsure as to what the authors mean.

~~ Line 371: "It can be deduced that the estimated MRT of supra-permafrost water is valuable for evaluating the extent of permafrost degradation. Most importantly, it can be used to infer the effects of long-term climate, permafrost changes, and vegetation on the hydrologic regime in permafrost regions." This sentence is clearly wishful thinking and I am not sure the authors have shown this at all. If they have, they need to suggest how and why and what the implications are.

~~Table 5. The data in this table is highly specific and incorrectly applies correlation methods within and among data sets.

~~References: The authors reference largely literature from China when discussing general permafrost hydrological knowledge. While I am not dismissing any of this work, suggesting that permafrost acts as an aquiclude, then citing Gao et al. 2021. This is not 'new information' and has been identified for many decades in the North American and Russian literature. Perhaps it has also been long-identified in the Chinese literature, and I would suggest the authors here and elsewhere cite appropriate historical works as opposed to ones focussed on the TP unless the work is directly related to processes in the TP and not ones that are more general.