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Reply on RC3

Ravindra Dwivedi et al.

Author comment on "Tandem use of transit time distribution and fraction of young water reveals the dynamic flow paths supporting streamflow at a mountain headwater catchment" by Ravindra Dwivedi et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-355-AC1, 2021

RC3: 'Comment on hess-2021-355, Anonymous Referee #', Anonymous Referee #3

General comments

3.1 According the reviewer the paper needs major changes for the following reasons: (1) the overall manuscript is confusing (2) the contributions of this study are not clear.

Despite some scientific contributions can be easily seen, it is necessary to make the contributions of this study clear. In general, the manuscript is difficult to read because there is no clear common thread, some sections seem to come out of the blue. Some sections are inconsistent with previous affirmation which reduces the truthfulness of the manuscript. The overall manuscript is too long, especially Methods and Results section.

Our response: We appreciate Reviewer 3's constructive review. These comments are consistent also with those of Reviewers 1 and 2, and so it is clear that we need to revise our introduction and justification. Please see our specific responses to comments # 1.1 and 1.2. If "Some sections are inconsistent with previous affirmation" is meant to refers to our previous TTD work [*Dwivedi et al.*, 2021], we note that that study was based on wavelet analysis of high-density tracer-flux time series data, and that tritium tracer concentrations or fraction of young water metric were not used in that study.

General comment of each section.

3.2 The introduction section is confusing and does not show specific novelty and sound scientific value at global scale. I suggest to restructure the introduction reinforcing the state of the art of previous studies using isotopes and TT models and after explain the novelties of this paper. The term deep and shallow groundwater flow at mountain range is referred without previous explanation. Due to the complexity of this terms, I will recommend a previous definition of those. The term fractured rock system is referred in some occasion but it is not clear if this is the case of the study area. If this is the case, I would be appropriate to explain how fractures are going to be taken into account.

Our response: Please see our response to comments # 1.1 and 2.1. In the revised version of our paper, we will clearly state our definition of both shallow and deep groundwaters. We note that as there no deep wells within our study site, and therefore the nature of the

fracture network within the study area is not known. For this reason, any impacts of fracture network on groundwater flow paths in our study area are considered to be represented by the tritium TTD.

3.3 At some point it seems that the authors try to reproduce his previous work Dwivedi et al. (2021) but for the "deep groundwater" however, the author says that this has already been done by Ajami et al. (2011) and Dwivedi et al. (2019b). Again, a detailed reasoning about why is this paper a novelty is needed, it is confusing. The authors highlighted the contribution of using multiple year isotope data however only one year of 3H data is used, again confusing.

Our response: Please see our response to comment # 3.1 above. That said, please note that neither *Ajami et al.* [2011] nor *Dwivedi et al.* [2019] have used/evaluated the usefulness of the fraction of young water metric to reveal the dynamic nature of flow paths either at the same or similar sites. Please also note that our statement related to use of multiple years' data is applicable to the fraction of young water metric because the data represent water years 2008 through 2012. While our compiled/collected data for tritium is sparse, it includes sampling dates from 2009 through 2018.

3.4 The Data section needs a better description of the collector type to understand the representativeness of the data. I strongly suggest to improve Fig1. In the manuscript Fig1 A and B are referred but there is no A and B in the figure. I suggest to incorporate the rivers, a standard scale (0, 0-5 and 1 km for example) and a higher resolution DEM, it looks poor.

Our response: Thank you. In response to your comment, Figure 1 will be improved and properly labeled in the revision version of the paper document. Additionally, the data section will be updated to describe the collector type.

3.5 The Methodology section seems to be a state of the art of the existing methods than something new. There are detailed descriptions of some methods that make the reader to lose the main goal of each approximation/estimation. I suggest to delete all dispensable information. The authors say at the introduction that one of the novelties is the use of multiple year data and only one year of 3H data is used. There is a repeated need to redefine the main goals and the novelty of this study.

Our response: Please see our response to comment # 3.3 above. In the revised version of the paper, the methodology section will be shortened and reorganized to remove non-essential information. Please see also our response to comments # 1.1 and 2.1.

3.6 The Results section is firstly organized by method, then change to shallow and deep groundwater and the mix between isotope type and TTD method and finally include FyW and Tyw. This section is dishful to follow.

Our response: In the revised version of the paper, the results section will be reorganized to ensure that our study findings are conveyed as clearly and accurately as possible.

3.7 Explaining which is the better method or the most reliable on in each case instead of only talking about the existence differences will strongly improve the Discussion section. It is not surprising to obtain different results with different methods. I will suggest to direct the discussion to explain line 577-579.

Our response: The revised version of the document will include explanation of method reliability when using multiple methods (e.g., Fraction of young water or Fyw). We appreciate your suggestions related to Fyw and its discharge sensitivity.

Specific comments

3.8 Line 52: Water stale isotopes: although this term has been used in other works, the term " stable water isotopes" is not correct. Water itself does not have isotopes. The correct term is stable 18O, 2H isotopes of water.

Our response: Our use of the term stable water isotope is similar to use of this term in other studies (e.g., *Ajami et al.* [2011]; *Heidbüchel et al.* [2013]; *Heidbüchel et al.* [2012]). In the revised document, we will clearly state that by stable water isotopes we mean stable $\delta^{18}O$ and $\delta^{2}H$ isotopes of water.

3.9 Line 61: Underestimating or overestimating transient times have other consequences than the correct understanding of the water chemistry. I will be appropriate to explain the most important ones.

Our response: The sentence between lines 59 and 61 is rephrased to read "Underestimated transit times can have cascading impacts on our understanding of subsurface weathering rates, leading to incorrect understanding of stream water chemistry [*Clow et al.*, 2018; *Frisbee et al.*, 2013]."

3.10 Line 92: The second goal is not clear; I do not understand what are you trying to study.

Our response: Our second study objective is related to estimating fraction of young water metric and the corresponding subsurface storage for dynamic and slow flow components of a catchment system. This objective is achieved by using stable water isotope and tritium tracers sampled during different flow conditions. Please not that when using the fraction of young water metric, subsurface storage in terms of short-term storage can be estimated without knowing aquifer properties [*Jasechko et al.*, 2016]. Thus, in a way, the subsurface storage supporting streamflow through baseflow to a high-elevation catchment can be estimated without knowing the effective aquifer properties for fracted bedrock aquifers, which is a great advantage for sites where fractured bedrock aquifer are not well characterized, e.g., our study site. In the revised document, we plan to more clearly state this goal.

3.11 Line180: â„□(ð□œ□) needs to be defined here instead of line 242.

Our response: This will be properly addressed in the revised version of the paper document.

3.12 Line 296: Why only one year period?

Our response: We invoked an annual tracer cycle or period on line 296 of the original version of the main document because the previous literature on the use of Fyw has mostly focused on annual tracer cycles. However, using our proposed mathematical model, we evaluated Fyw for various periods using tritium tracers (e.g., Table S5 in the original version of the supporting information document).

3.13 Line 426: I would say 2-3 years.
Our response: Agreed, we will make this change.

3.14 Line 440: 10.7 "mm"

Our response: Thank you!

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