Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-532-RC2, 2017

Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Aggregation effects on tritium-based mean transit times and young water fractions in spatially heterogeneous catchments and groundwater systems, and implications for past and future applications of tritium" *by* M. K. Stewart et al.

Anonymous Referee #2

Received and published: 11 January 2017

In their manuscript, Stewart et al. investigated tritium-based estimates of mean transit times (MTT) and the fraction of young water (Yf) in light of aggregation bias due to catchment heterogeneities. Furthermore, past studies are reinvestigated and evaluated in respect to aggregation bias. This topic is highly interesting, as most commonly the stable isotopes of water (Oxygen-18, Deuterium) are applied in tracer studies. In comparison to this, tritium is used more seldom, but it has the potential to elucidate longer transit times, where stable isotopes hit a boundary at about 4-5 years. I hope my

comments and suggestions will be helpful to the authors and improve the manuscript.

General Comments

1) Manuscript structure: I found the structuring of the text to be all over the place, making it hard to read for me, as I was expecting to have all the tools necessary to understand the paper after reading the Methods. However, the "Results" section basically starts with several paragraphs of new Methods. I would suggest to either changing the order of the text to properly divide Methods, Results and Discussion, or rename the header titles from "Results" and so on to something else to avoid confusion. Please see specific comments about my ideas which paragraph could be shifted to different sections.

2) Methods: After introducing tritium (H3)-based TTD estimation, LPMs and their properties, "Results" starts and I am left with an unsure feeling of how the paper addresses the issues raised in the Introduction. I know you use the four GMs from Fig 2a, but in which combinations for the two virtual catchments? Only selected combinations, or all possible ones? How were the catchments mixed? (I know it is 50:50 because it says so later on, in Results. . .which links back to my comment about structure of the paper). Did you use the GM of each sub-catchment in Equation 1 and forward-propagated Northern and Southern hemisphere H3-data, then mixed it 50:50? All this information is missing, and young water fraction calculation or the literature reevaluation is not even mentioned here. Furthermore, I think that the description of the individual LPM can be shortened without losing important information. Also, I think Table 1 and showing that the GM can mimic the shapes of other LPM is not essential for understanding of the paper. It is interesting to quickly summarize which LPM is useful for which application, however.

3) Yf calculation: It is unclear to me how you calculated this. Yf is determined and calculated from the threshold age ty (Equ. 12), yet it seems the threshold age was calculated by comparison to apparent and true Yf already existing (page 9, first few

lines)? True Yf comes from the individual Yf1 and Yf2 (Equ.13), but where are they and their ty1 and ty2 coming from? Also, ty should give good agreement with 10% of apparent and true Yf. 10% of what? What is the 100%? Maybe an explanatory figure would help here, and this should be also in Methods.

4) Apparent Yf determined from LPM fitted to H3 of mixture: To my understanding, Kirchner 2016 showed that Yf of the mixture can retrieve the “true” Yf (calculated from our knowledge of the virtual system) using a gamma function, but only Yf is valid and the corresponding gamma function itself is not valid (otherwise we would have a valid gamma function and thus a correct MTT again, i.e., no aggregation bias). Equipped with this knowledge, how can we reliably trust the apparent Yf result if it comes from a LPM function that is fitted to the H3 mixture and will most likely not be e.g. gamma distributed anymore, but hyper-gamma distributed?

5) Chapter 3.3 seems unnecessary to me, and is very short in itself already. If you want to keep it, please elaborate on its importance.

6) Chapter 3.4: I am unclear as to which results were already obtained by the cited studies and which results were calculated by the present manuscript.

7) I generally doubt the validity of Chapter 3.4, the literature review. To me there is suddenly a huge leap in logic/faith: that using compound LPM will give the true MTT. Or one that is “truer” than the versions of single LPM. It is assumed that just a good fit of tritium tracer data warrants to say that the model gives true results. I do not say they are wrong, I do not say they are true. I do say we cannot know, or I do not see any evidence here that would substantiate your assumption that the compound LPM would give the true MTT. Even if both parts that feed the mixed water in all described studies would be homogeneous in themselves: the virtual experiment catchments of Kirchner 2016 were also homogeneous in themselves, but different from each other, and still led to aggregation bias. We would need proof that the individual catchments are “homogeneous enough” (whatever that means) and that compound LPM, which are

just simplifications of processes that we think occur in a catchment domain, correctly mix the two flows in a way that surely avoids aggregation bias. Just a good fit of observed tritium data is surely not a bad start, but not enough in my opinion. I am in favor or a) deleting Chapter 3.4 OR b) rewriting it much more cautiously, with discussing the considerations uttered here.

Specific Comments (page-line)

2-24: “young water” appears here the first time. Maybe define it a bit more clearly. How young does it need to be to be considered young water?

3-11: “[. . .] the one tracer”. This makes it seem to me that two different tracers are used, but I rather get from this paragraph, that actually “when we only have tracer data of the mixture” is meant. Please clarify.

3-16: Choice of LPM based on hydrogeological situation: please give an example or reference at this point.

3-18: “water-bearing layers” to avoid confusion while reading (had to read three times)

4-6 “times. i.e. The water [. . .]” please correct the capital T and also the period after times seems strange.

4-15: with “calender time” you refer to daily time steps? Monthly? Yearly?

5-23: Starting the sentence with a side-sentence in brackets “(Maloszweski [. . .]” looks weird, in my opinion.

7-6: I would write 2.25 instead of 2.5, if you already use two digits after the comma for 0.05. Except for that I recommend not showing this information anymore, see General comment #2.

7-12 to 7-25: Methods

7-20: Please explain Fig. 3 a bit more in the manuscript to assist in fully understanding it. As far as I understand it, the black curves show TU that one would measure in streamflow. It seems to be the fitting result of the LPM to the mixed H3 signal (p7-L29f). How did you find the two catchments TU concentrations (necessary to find the mixed TU signal) based on the desired MTTs of 3 and 197 years? There must be some other MTT-TU function behind it, that is not shown? I'm basing my last assumption on Kirchner 2016, where the combination of e.g. two exponential distributions did not lead to another exponential distribution, but a hyper-exponential distribution. Thinking along these lines, this confuses me even more now: every red dot in Fig 3, that is, every TU-MTT combination of the two individual catchments, lies exactly on the black curves that come from fitting the mixed runoff TU signal. But according to the logic here, the black curves should be wrong. How can the red dots lie on the black curve, if the shape of the distribution of the mixed runoff is not known and should be some hyper-something version?

7-24: With the assumption of a constant H3 input, are you not basically assuming that no groundwater much older than 50 years significantly contributes to runoff (that is, no groundwater which could possibly include the bomb peak). How realistic is this?

7-28: I would get rid of the reference to Equ. 9 here

7-30: Equ. 10 is the standard deviation and seems to be not fitting the text here. Do you mean Equ. 1?

7-31: All the water in streamflow has the same age, not in soil/aquifer. I would specify that here.

8-12: Regarding Fig 4: Earlier it was mentioned that real input TU data would cause scrambled results in Fig 3. But you use real data now. Please clarify why we can suddenly use them, or if the scrambling would just have made analyzing the figure more difficult, but not prevent the data from being used. Also, the paragraph explains Methods (8-12 to 8-18). Additional information is needed: what were the MTT2 increments? Was the GM model used, as you talk about alpha parameter later? How was MTT2

changed then, by changing beta parameter in certain increments?

8-20: How were the uncertainties for fitting young waters of MTT1 calculated? Might also be good to call it MTT1 here one time since it is only used in Fig. 4 and leads to some confusion initially when looking at Fig. 4.

8-22: The fitting errors are important because of more complex LPM in a good or bad way?

9-4: I guess it is 197 instead of 397.

9-14: Please define a "reasonable" choice of young water threshold.

9-16f: I disagree that cutting-off of the long tail after ty and thus leaving only the short tail will ensure that the apparent Yf does not deviate from the true Yf. As the TTD sums to "1", the long tail influences the short parts of the TTD and vice versa. If one is changed, the other changes too. If we know that the long tail is wrong, we can't be certain that the short TTD part is correct if we basically just ignore the existence of the long tail by cutting it off. To use a metaphor, this is to me like healing a bleeding wound by just not looking at it. And if REALLY the part of the TTD model before ty is correct, how can it be that the part after it is NOT correct? The equation and the parameters to calculate the complete TTD do not suddenly change...we just cut off a certain section of it. If the part before the threshold is true, the part after it is true. If the part after it is wrong, the part before it is wrong. Maybe the question is: how wrong? Significantly? Probably not, looking at results from Kirchner 2016.

9-22: No explanation follows why the reason for this relationship is found in the gamma distribution.

9-26: 6 to 16 years

9-28 to 9-31: Discussion

10-25: MRT, which I assume to be Mean Residence Time, is introduced for the first time and replaces the MTT without explanation. Please rectify. Also, it should be Fig 10c.

12-21f: Following the reasoning about the different bias thresholds: Does this not mean that using tritium methods for streamflow, we would get bias-free estimates for transit times smaller than 6 years, which must include the seasonal cycle results if we would apply it to the stream, and ultimately agree with them?

Title of 4.2: Consider changing to “How much has aggregation affected tritium MTTs in past studies?”

12-30: Conclusion #1: I disagree with ONLY affected by bias if “older than 6 years” and “if determined by simple LPM", for reasons already explained above.

13-1f: If we take the variance into account in the given examples of 10 plusminus 8 and 10 plusminus 5, there seem to be quite a few catchments that have less than 6 years MTT.

13-8 Conclusion #2: As mentioned above, I see no evidence for this statement.

14-23: I must have missed the part in the manuscript that shows that simple LPM still work in case of long series of tritium measurements? Where is that shown?

Table 1: In the description change “The shape parameter of the best-fitting versions of the other models [. . .]”, since it is not always the shape parameter for the other models, e.g. it is the dispersion parameter for DM.

Figure 2a: the scale parameter beta was fixed for each GM? Which value did it have?

Figure 4: In the legend: the orange MTT1 actually says “MTT!”

Figure 7 is not mentioned in the manuscript.

Generally the figures should be unified more in layout, e.g., get rid of the outer border.

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