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Interactive Comment

Interactive comment on "Aggregation in environmental systems: seasonal tracer cycles quantify young water fractions, but not mean transit times, in spatially heterogeneous catchments" *by* J. W. Kirchner

Anonymous Referee #1

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GENERAL COMMENTS

This is an interesting paper that explores which information can be gained, in terms of catchment transit times, from the analysis of seasonal tracer cycles. The paper is written in a clear form that makes it easy to read. The contents can be divided into two parts: 1) it is shown, through rigorous benchmark tests based on a virtual experiments, that the stationary travel time distributions estimated from seasonal tracer cycles are typically unreliable and biased towards younger mean transit times. 2) a new

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metric (the “young water fraction”) is introduced that can be more accurately derived from tracer cycle information. The results suggest that, for a range of plausible TTDs (apparently, every TTD that can be derived by combining gamma distributions with shape parameter alpha in [0.2, 2]), the amplitude ratio derived from sine wave fitting is representative of the fraction of water younger than about 1.5 – 3 months. Both the parts are of good scientific significance, but while part 1 is also straightforward and easy to understand, part 2 is at times unclear to me. Considering that part 2 is the basis for Paper 2, and that potentially the method will be widely used in the future by the scientific community, it would be advisable to revise part 2, so as to permit a better understanding of the contents. Below, I included some comments that may help making the manuscript clearer:

i) There is some ambiguity between the general idea of “young water fraction” used in common speaking and the specific definition “young water fraction” developed by the author (the fraction of particles younger than 2-3 months). It may be desirable using a different name for the new variable defined by the author, to avoid this ambiguity.

ii) The Fyw is an interesting and promising concept, but its definition in real catchments is not easy to digest because it is affected by the imprecision in determining the threshold age (on the other hand MTT has a very intuitive definition, but it is an uncertain metric). The paper would benefit from a deeper analysis of how the threshold age varies in the virtual experiment when the tributaries are aggregated (see Detailed Comments on Section 4.1).

iii) The author often mentions the catchment “spatial heterogeneity” and the related “aggregation error”. However it is not clear what the author’s definition of “heterogeneous” and “homogeneous” is. This has implications, because the essence of the problem with the traditional derivation of MTT from sine wave fitting methods is the use of a wrong assumption on the TTD shape. I would call this an error caused by the wrong assumption of using a simple TTD for a complex system, and I don’t see why the author calls this an “aggregation” error.

iv) The paper presents several interesting inferences on the relationship between the amplitude ratio and the Fyw. As these are not causal relationships, one would expect to see a paragraph with a summary of the fundamental working hypotheses (e.g., the shape parameters alpha in [0.2, 2]), that can guide the reader towards the limits of applicability of the outlined method.

v) Sections 4.1 and 4.3 include details that are not always clear to me, and should be better explained (see Detailed Comments). In particular, I could not find in the manuscript a description of how to incorporate the phase shift information in the determination of Fyw.

DETAILED COMMENTS

3063, l. 3: It would be important to better define the working framework at the beginning of the paper. The author may mention here that the flowpaths and the catchment connectivity change in time, potentially by large factors. The catchment has no stationary behavior and stationarity is a legitimate assumption, but it must be stated that it is an explicit assumption, which allows the use of one TTD instead of several TTDs. The author may also move up here lines 3-14 of page 3066.

l. 7: (connected to comment on line 3) “have simply assumed that the TTD is stationary and has a given shape”

l. 8: it is not so “obvious” to me that MTT is the ratio between storage and fluxes. While it surely is for a well-mixed system (which produces an exponential TTD), I am not so confident that the same holds for other storage mixing hypotheses.

l. 16-19: it may be appropriate referring to the recent commentary on WRR by McDonnell and Beven (2014) on this topic.

3070 l. 5: Eq. (7) is not enough to derive Eq. (8). Maybe start the sentence with “from Eq. (1) and Eq. (7), using the Fourier Transform properties, one can. . . ”

3075 l. 6-14: This is a very important result, and should be better explained. As

HESSD
12, C1430–C1435, 2015

the author says, it is not intuitively obvious (and it is actually quite surprising) that the tracer cycle amplitude in the mixture is almost exactly equal to the average of the tracer amplitudes in the two tributaries. This looks like an interesting property of the gamma filtering for the shape parameters investigated by the author, where the damping of the tracer cycle prevails over the shifting. Other filters would not behave the same (e.g. gamma distributions with shape parameter alpha>2?), suggesting what the limits of applicability of the method are. Indeed, one may expect the same behavior from the advection-dispersion model TTDs derived by Kirchner et al., 2001, and not from the lognormal TTDs reported by Selle et al., 2015.

Section 4.1: 3076 l. 15: at this point in the paper it seems like it is the opposite: you look for the threshold age for which the Fyw closely approximates As/Ap across a wide range of scale factors. I would suggest stressing that the existence itself of one single threshold age, which verifies almost exactly the equality Fyw=As/Ap for very different scale parameters, is already an interesting result.

3076 l. 20 to 3077 l. 4: in this paragraph there is a fundamental perspective shift that needs to be explicitly clarified. Before this point, the young water fraction was defined to be equal to the amplitude ratio. After this point, due to the results shown in Figure 9, the perspective changes and the amplitude ratio will be always assumed to be a good predictor of the relative amount of water younger than 2-3 months. If this is not stated clearly, the sentence will sound circular (the amplitude ratio is a good predictor of a new variable that has been explicitly defined to be equal to the amplitude ratio!).

3077 l. 11: “leads to the important result”. Is it not a hypothesis that is going to be demonstrated, rather than a result?

3077 l. 15-20: from the same procedure used to determine Fyw for the gamma distribution, it would be possible to determine the “real” young water fraction (as well as the “real” threshold age) in the mixed runoff. So why did the author not perform this test? It would make the statement “the amplitude ratio predicts the young water fraction also in the combined runoff from heterogeneous landscapes" much stronger. Moreover, it would be interesting to see the effect of the aggregation on the threshold ages (particularly from tributaries with different shape parameters). Does a single threshold age still verify the equality As/Ap=Fyw, for different parameters alpha? Do the threshold ages fall in the same 2-3 months range in the mixed runoff? Do they average linearly?

Section 4.2: same comment as 3077 l. 15-20: the "real" young water fractions and threshold ages could be determined from equation 16. So is the amplitude ratio a good predictor of the "real" young water fraction? This would really make the young water fractions independent from the gamma distributions they were initially defined from. Also, is there any hint on what causes the larger departures from the 1:1 line in Figure 11 and Figure 12? Could it suggest anything for the limits of applicability of the method?

Section 4.3: it is really unclear how the phase shift can affect the determination of the young water fraction, as it does not appear anywhere in its definition. So I am not able to interpret Figure 13 a-c.

3083 l. 19: "the most useful metric" seems like an overstatement.

Section 5: The uncertainty induced by sine-wave fitting is not mentioned (while it is, briefly, in Paper 2). In my opinion, the manuscript would benefit from a simple analysis on how the uncertainty in sine wave fitting translates into uncertainty in the estimation of the young water fractions. Besides showing that Fyw is a reliable metric while MTT is not, the paper does not suggest what the young water fractions can be used for. This is partially addressed in Paper 2 (section 3.7), but some hints also in paper 1 would make the impact of the manuscript stronger.

TECHNICAL CORRECTIONS

3078 l. 3: minimal

Figure 11 caption: horizontal axes

HESSD
12, C1430–C1435, 2015

Literature cited:

McDonnell, J. J., & Beven, K. (2014). Debates - The future of hydrological sciences: A (common) path forward? A call to action aimed at understanding velocities, celerities and residence time distributions of the headwater hydrograph. Water Resources Research. http://doi.org/10.1002/2013WR015141

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