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Discussions

Interactive comment on "Quantifying new water fractions and transit time distributions using ensemble hydrograph separation: theory and benchmark tests" *by* J. Kirchner

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Dear Riccardo, dear Editor,

I thank Riccardo Rigon for his further comments. As before, I will quote his comments verbatim (in plain text) and intersperse my responses (in boldface).

Dear Editor, Dear Author,

in brief my thinking:

a - Finding a way to estimate hydrograph separation or travel time distribution averages through regression is an interesting achievement

Thank you.

b - Doing linear regressions, either with plenty of data or data scarcity, cannot be considered an advanced topic in 2018. Reference to appropriate literature should be enough and could substitute many pages of this paper.

As section 4 makes clear, this is not just "doing linear regressions", even if the starting point is something that looks like a regression equation. And it's not a question of "data scarcity", but of data gaps *that would make these solutions impossible* using standard techniques.

"Reference to appropriate literature" is not enough because the approach documented here *does not exist* in the literature yet. For one thing, the covariance matrix must be altered, in two different ways, to account for the two different reasons that precipitation tracers can be missing (no rainfall, and lost samples). This *has never been done before*, and it is not trivial to figure out how to do it.

The approach also relies on Glasser's method of so-called "available case analysis", which is unknown within the hydrology community. It would also very hard to figure it out from the original literature; the only reason I could grasp it is that I had previously re-invented this particular wheel, so I knew how it had to work.

One could argue, perhaps, that Tikhonov-Phillips regularization is a standard technique in geophysical inverse methods, but it is largely unknown in hydrology except in unit hydrograph papers from past decades. And those papers typically use Tikhonov's original approach, which yields biased results, whereas here I use Phillips' approach, which is much less widely used but yields unbiased results.

It took me nearly two years to figure this all out, and I had the advantage of having taught statistics for many years beforehand. It would be a disservice to readers to just point them to the literature and expect them to figure out how to

perform the analysis presented here.

c - Niemi's relation validity is granted always, if properly modified to account for the missing knowledge of the partition coefficients required. In Rigon et al. 2016 there is a section dedicated to it.

Sorry, that was my mistake. I thought Prof. Rigon's earlier comment was focusing on Niemi's flow-normalized time approach (which was the focus of the cited 1977 paper), and I now see that he was referring to something different. The factors of Q/P in Eqs. 27, 59, and 62 are ensemble estimates of the partition coefficients that he refers to. I will see what I can do to clarify the revised manuscript on this point.

In the revision, I will also address an important point that was missed in the first version, namely the need divide by the length of the time step when estimating transit time distributions. This is necessary for dimensional consistency, and also to make estimates comparable across different sampling intervals.

d - The explanation given to account for evapotranspiration is not clear, at least to me. For what I understand, the Author did not introduce a new modelling procedure but tried to simulate the effects of fractionation on the final outcomes by introducing a sinusoidal alteration of the output signal obtained.

This is correct.

If I did not understand properly, the Author should make an effort to express things better. If I understood properly, that was not so easy, anyway. I personally have doubts on the procedure he used, but I understand the point of the Author.

I will see what I can do to clarify the presentation on this point.

e - I think that the technique developed by the Author is worth to be published. However, it accesses a limited number "m", as called in the paper, of instants (less than the number of recorded inputs, much less, for having good statistics). This limitation has

effects both on the backward and the forward probabilities estimations. The technique does not get everything.

I never said, or implied, that it "gets everything". I can of course state explicitly in the manuscript that the approach cannot estimate TTD's beyond m lags, although this should already be clear from the math and the figures.

With respect to the backward probabilities, it is NOT able to get really old water distributions, i.e with expected values of decades years old, unless the time series of appropriate length is available.

This is correct. But *this is also true of every other approach* that uses conservative tracers. It is not a question of having a long enough time series. The key problem is that even if you had a long enough time series, the tracer inputs (typically deuterium or oxygen-18) would also need to be variable enough on decadal time scales that one could separate the (highly damped) signal from the noise.

Regarding to the forward expectations, the techniques does NOT allow to estimate the right partition coefficients if multiple fluxes are present, but only an approximate value for them. In both the cases, long time series in input could be required to get right answers. These facts should be clarified better to the reader and to the potential users of the methods developed.

The factors of Q/P in Eqs. 27, 59, and 62 are ensemble estimates of the partition coefficients that Prof. Rigon refers to. In any case, one would not need partition coefficients to estimate the (ensemble, or "marginal") forward transit time distribution of the water that ultimately leaves by any one specified outflow (ET, for example, or discharge), as long as one has tracer time series in that outflow (and the input, of course).

To estimate the forward transit time of the *entire* precipitation input, one would need tracer data for all of the possible exit fluxes (principally stream discharge

and ET). Thus the primary limitation is the availability of data.

I will make it clear that both the backward and forward TTD's that are estimated from the tracers reflect only the linkage between whatever two fluxes that the tracers are measured in (typically precipitation and discharge).