Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-429-RC2, 2018
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Hydrology and Earth System Sciences
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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Received and published: 25 October 2018

This paper presents some interesting ideas. The main topic, in my opinion, is the derivation by regression of the average of the backward probability distributions which can be used to infer the (mean) catchment behavior either related to transport or to the hydrologic response. Estimating the role of antecedent conditions by studying the mean shape of the distribution functions, which is the last topic treated in the paper, is a great intuition that could become a classic.

Because I like the most the topic of the backward probabilities with respect to the one of new water/old water, I would change the title to put major emphasis on transit time



distributions than on water fractions. I believe that finding a way to characterize the backward distributions by regression is a more general and better achievement than the hydrograph separation.

In the paper there is also section on the response time distributions, also known in literature as the forward distributions. Estimations made could be biased by the example used, i.e. of having just one outgoing flux. Since there is a simple relation between backward and forward probabilities. i.e. the Niemi relation (e.g. equation 34, and the whole section 7 in Rigon et al., 2016, or equation 1 in Botter, 2012), n outgoing fluxes require to make explicit n-1 partition coefficients, a fact that it is not so evident in this work because of the simple example used as benchmark. As Rigon et al. section 7 shows, there is also an empirical version of the Niemi's finding that works at any time time t, and this is the case treated. As not clearly stated in the paper, the empirical case does not match with a pdf but to a pdf divided by the partition coefficient. In this context, It should be noted by the "astute reader" that while the backward distributions assume knowledge of all the past, forward distributions assume knowledge of all the future, which cannot be the case of the analysis under my review (see also section 6 of Rigon et al., 2016). In any case the Author needs to be a little more explicative on these facts. A brief section is dedicated to talk about evapotranspiration and fractionation effects. Evapotranspiration is not present in the model used as benchmark and the way it is introduced is not clear. Reading the paper I will not be able to reproduce the Author's results and I suggest that this part, being unessential for the present study, could be omitted. The paper is exceedingly long. Some technical parts on regressions, whilst important for research reproducibility, distract from the core topics and can be moved in my opinion to an Appendix or to some complimentary material. A further remark that embarrassed me a little. I believe that the coefficients beta of regressions, could be better understood in terms of the backward travel time distributions, and in my view, Rigon et al. 2016 can be a useful citation.

Therefore, also looking to the minor notes I make below, I think the correct judgment is

to go for major revisions (which I think will not require a lot of time though).

Riccardo Rigon

Detailed comments

Page 2 - Line 15 - Equation 1 - I think that an operational definition of new and old is required. In the subsequent text," new" is the discharge produced by the last rainfall interval (a day or a week) and “old” is the rest of the discharge. Specifying it here could be useful.

Page 6- Line 20, Equation (10). Maybe saying that this is the mean backward distribution of travel times evaluated at lag 1 could be helpful. Probably I am biased to think that way and does not correspond to the generic reader of this kind of papers. Anyway, it is my opinion.

Page 7 - Franky, I do not think sections 2.3 to 2.7 are so relevant. They probably reflect the genesis of the paper but they are full of technicalities and certainly scooped out by the more general section 4 of which these are just a particular case.

Page 26 - Section 3.6 - Effects of evaporative fractionation - This section could be interesting but where is evapotranspiration in the model used as a benchmark ? So, how could have been it evaluated Figure 10 ? The indication given in the section are not exhaustive, and I suspect that going deeper in the subject wold require major work. I suggest to take away it.

Page 28 - Definitions - All of it could be much more clearly explained in term of age-ranked functions (e.g. van der Velde, 2012, Rigon et al., 2016). I understand that the Author is a pioneer of the topic and derives everything from the scratch without being taught by anyone, but this is not useful for the general reader who will have great help from referring to those papers too.

Page 29 - Equation 31 - q_{j,k} -> q_{ij}. Both notations are good but they should be used consistently throughout the paper



Page 30 - Solution method - This is essential for results reproducibility, but, at the same time, not central for understanding the concepts. I think moving it as well as section 4.3 and 4.4 to an Appendix or to the complimentary material would make the paper more readable.

Page 31 - Line 25. I would simply cut sentence 26 up to “missing values” at page 32

Page 42 - Forward time distribution. I make my points in the general comments. I believe using the work by Niemi is easier and founded on literature.

Page 43 - line 20 - Rewriting equation 57 into 58 seems to me a little pedantic.

Page 45 - Section 4.8 - I think it is sort of a Columbus’egg, a brilliant idea. The only possible objection is that results (not the method) can be biased by the use of the benchmark model.

Page 50 - Discussion - Should be shortened.

References

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