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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Age-ranked hydrological budgets and a travel time description of catchment hydrology" *by* R. Rigon et al.

Anonymous Referee #1

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The value of a travel time based description of catchment hydrology has been increasingly acknowledged over the recent years. Therefore, this manuscript comes timely and could eventually be an interesting contribution to literature. Presenting a considerable string of formalisms, describing various aspects of travel times, and which are, as far as I can see, mathematically sound, the authors delve deep into the topic. However, even after reading the manuscript three times I struggle to see what the actual intended contribution is. What do the authors want to convey to the reader? This needs to be made much clearer. Is it a review of existing concepts? Is it an extension of existing concepts? If it is a review, the description of the concept needs to go further back to include earlier work and detailed descriptions thereof. If it is rather an extension of existing concepts, it needs to be clarified what the novelty is and how it fits into our current understanding. In other words, what are the main findings? What do we learn?

In either case, the presented work needs to be put into a broader context. The authors refer to a few key publications, but they ignore many other recent contributions that address the issue from similar and/or different perspectives. Such a broader context will help the reader to better appreciate the relevance of the presented work. I would thus invite the authors to discuss their methods and findings with respect to methods, results and findings from a wider range of other (also more experimental) studies including, for example Birkel et al. (2010, 2011, 2014), Fenicia et al. (2010), Van der Velde et al. (2010, 2015), McMillan et al. (2012), Hrachowitz et al. (2015), Rinaldo et al. (2015) but also with the work of Cvetkovic, Fiori, Dagan, etc over the past years.

Other comments:

(1) P.1, l.17-18: there is, in my understanding, little that remains unclear. Perhaps provide some examples.

(2) P.2, l.26: again, I sort of disagree, there is little that remains unexplained. Please also give an example here to clarify.

(3) P.2, l.33-41: please be more specific here: what is the research hypothesis to be tested?

(4) P.2,l.47 and 48: this should read as "...the time at which..." to avoid confusion, as we are (as a simplifying assumption) not talking about a time interval over which the input occurs but an instantaneous input.

(5) P.3,l.57-58: please add the respective dimensions

(6) P.3,l.67: this can only be solved analytically if piecewise linear functions of inputs are available and, more importantly, with the assumption of only one storage component in the system, which may be quite an oversimplification for most catchments. The water balance as given here cannot resolve the non-linearites in the system, including interception, vadose zone dynmaics, storm flow connectivity, etc.. Thus the practical utility of such an analytical solution, if not used in an operator splitting strategy that ac-

counts for different system components, remains limited. Please qualify the statement accordingly.

(7) P.4,l.86: Really? I would be surprised by that as this is implicit (and has to be) in essentially all approaches that somehow track fluxes through the system. I could imagine that this has already been explicitly formulated earlier. Please check, in particular papers by Cvetkovic, Dagan, Fiori, Russo, etc.!

(8) P.5, section 4: that is all fine and true, but nothing new. It remains unclear what the purpose of this section is. Please clarify!

(9) P.6,l.136: I may have missed something here, but how can P(Tr/t) not integrate to one (and it seems it actually does in figure 2)?? As far as I understand, it is the sum of storages of all given ages present in the system over the total storage at any time t. Also: how does ageing contribute here? Please clarify.

(10) Figures 2 and 4: I am a bit confused by this figure. How can three injections at three different injection times (tau1, tau2, tau3) plot on top of each other when the x-axis is the actual time t measured by a clock? Should here the x-axis not rather be the time since injection?

(11) P.8,l.161ff: that is correct, but has been shown and discussed earlier (e.g. Benettin et al., 2015, Fig.6; Hrachowitz et al., 2013, Fig.9). Please put into context.

(12) P.10,l.173-176: sure, nothing wrong with that. It remains, however, unclear, what the relevance of this is. We may be able to extrapolate the splitting coefficient for the future, but what exactly does the knowledge of this help us when future climatic forcing is unknown? Please clarify.

(13) P.11,l.198: should read as ". . .what was written. . ."

(14) P.11,eq.45: please clarify what the difference is to the relations discussed by Botter (2011) and Benettin et al. (2015)

(15) P.14,275ff, eqs.57-61: again, this is what is essentially done in most recent tracer based approaches. Please put into context.

(16) P.14,l.294-296: there are definitely obstacles to adequately determine SSF in reality. Not only due to uncertainties in precipitation and tracer input, but also due to oversimplified models and the related uncertainties (e.g. in Q) that typically lead to considerable equifinality (i.e. the well-known closure problem; Beven, 2006)

(17) P.14,l.304ff: sure, but not new. See for example the work of Bertuzzo et al. (2013) or Benettin et al. (2015).

(18) P.15,l.317: maybe mention that a linear reservoir here entails complete mixing/uniform SSF

(19) P.15,l.319: please clarify in detail what Rtau is.

(20) P.15,eq.63: as it is presented right now, this ignores the critical difference between celerity and velocity, or in other words that the response time distribution of a pressure wave routed through the system is significantly different to the travel time distribution of an actual input signal (McDonnell and Beven, 2014). This needs to be made clear!

(21) P.15, eq.68: this does essentially boil down to the convolution integral used in many earlier studies starting from the 1960s or so. Please put into context and highlight the relevance here.

(22) P.16,l.335: of course, as already argued by others previously (e.g. Rinaldo et al., 2011)

(23) P.18,l.423: should read as ". . .damped. . ."

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