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Interactive comment on "A nonparametric approach toward upper bounds to transit time distribution functions" *by* Earl Bardsley et al.

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The manuscript "A nonparametric approach toward upper bounds to transit time distribution functions" by Beardsley addresses the problem of these upper bounds being difficult to constrain with the typically available data and which may result in the inference of transit time distributions that can considerably misrepresent the real system characteristics.

The overall topic of this manuscript is of considerable interest as a technique allowing to efficiently constrain the upper bounds of transit time distributions would be extremely valuable to develop a better understanding of the underlying processes. In spite of the general interest, the manuscript remains somewhat superficial and the presented anal-

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Discussion paper

ysis and results are not entirely convincing. It thus may be beneficial to go a bit more into depth and develop the manuscript a bit further to the point, for example by adding some more case studies. This could be done either (1) extending the existing analysis by further toy models, to provide insights how different assumptions and boundary conditions affect the method and/or (2) ideally provide a demonstration with real data.

Specific comments:

(1) P.2, l.1-19: A problem statement that is a bit more detailed would help the reader to better understand the relevance of the analysis in this manuscript (in other words, try to place more emphasis on the sentence in line 12-13). Likewise, it would be good to formulate an explicit science question here and provide a working hypothesis that is going to be tested in the manuscript.

(2) P.2, l.6: why such an emphasis on "cumulative"? The CDF should be known if the PDF is known and vice versa. Please clarify.

(3) P.2,l.8: Please be a bit more specific to avoid misunderstandings. What is exactly meant by "upper bounds"? Feasible and physically meaningful bounds to the tails of these distributions?

(4) P.2,l.9-10: not entirely clear what is meant by "…the extent to which the upper bounds can be located below 1.0,…". Please rephrase.

(5) P.2,l.18: Hrachowitz et al. (2013; HESS) and/or (2015;Hydrological Processes) would fit better here than the 2010 reference

(6) P.2,l.21-22: I am not sure, if this statement (and the emphasis on tracers thereafter) is sufficiently exact. In my understanding, it does mix up general concepts with real world applications. Tracers are essentially tools. Thus in the first instance, transit times are with reference to the movement of individual *water molecules* (which, in reality, and with the available observation technology can only be tracked with tracers).

(7) P.2,l.26: it needs at least to be acknowledged that the assumption of p being con-

stant does not hold for real world systems (as demonstrated by e.g. Harman, 2015; WRR; Hrachowitz et al., 2015; Hydrological Processes).

(8) P.2,l.26-29: see comment (6) – the emphasis needs to be the movement of water molecules, which are tracked with the help of tracers, assuming these tracers are conservative and move with the water.

(9) P.3,l.1: see (6) and (8)

(10) P.3,l.3: pulse magnitude=flux*concentration? i.e. in SI unit symbols (L^3*M/L^3)*(M/L^3)=M^2/L^3. This does not make sense. Please correct this typo. I suppose what is meant is pulse magnitude=tracer mass flux=water volume*tracer concentration, i.e. M=L^3*M/L^3.

(11) P.3,l.13: no, a constant loss proportion is effectively NOT possible, given the natural variability in environmental systems.

(12) P.4,l.22: How is it evaluated/decided if it is permitted by reality? What is meant by "reality" here?

(13) P.4,l.24: I cannot fully follow here. I thought p is kept constant.

(14) P.5,l.5-30: It would be good to clarify/discuss in how far this approach is different to the different approaches suggested by Heidbuechel et al. (2012; WRR) and Hrachowitz et al. (2010; WRR) – both already in the references.

(15) P.5,l.14-15: This statement is confusing. Distributions from the exponential family (also including gamma with shape parameter <1) are also unimodal. Strictly spoken, the mode of a continuous probability distribution is the value at which the probability density function has its maximum value (which is clearly defined for exponential family distributions). Please rephrase.

(16) P.5,l.18: firstly, see (15) – thus, the mentioned gamma distributions *are* unimodal. What is obviously meant here is modes that are found at x>0+epsilon. And

secondly, in addition, the statement also depends on the (time) scale and (time) interval of interest. While a distribution with a mode at x>0+epsilon is likely needed for a high temporal resolution (e.g. <15minutes or so), most models so far were, as dictated by the available data, implemented at temporal resolutions much higher than that. For such higher resolutions, the delayed mode can typically not be resolved anymore by the available data, thus resulting in the necessity of using exponential- or gamma distributions (shape factor<1). Please rephrase.

(17) P.6,l.13: why 51 to 150? Please clarify.

(18) P.7,l.26: again – what is meant by unrealistic? On basis of what is this judged and what is actually meant by "realistic"?

Best regards, Markus Hrachowitz

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