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Hydrology and Earth System Sciences
Discussions

Interactive comment on "On the shape of forward transit time distributions in low-order catchments" *by* Ingo Heidbüchel et al.

Anonymous Referee #2

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The manuscript presents and discusses an interesting analysis based on virtual (numerical) experiments on the TTD in small catchments / hilsllopes. The work is interesting and well done and it touches a relevant topic, namely the identification of the leading components and parameters in the definition of TTDs. The approach is rather "classic" in the sense that the analysis is somewhat based on the concept of time invariant TTD, while recent approaches have shown the importance of other metric, like e.g. the backward TT distributions, for a comprehensive description of water age and contaminant dynamics. Still, the analysis is useful and instructive.

Perhaps the manuscript is too long and involved at times, with plenty of text (with some verbosity) and figures. See for instance the long Conclusion section (and it is the first time I see a subsection there. . .). I think that this might be detrimental to the work as

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the reader can easily get lost in the many details and miss the important aspects. Thus, I suggest further distilling the principal results, moving the details that are not important for the storyline in the supplementary material and concentrate on the main results that the Authors want to convey. This would strengthen the message of the work and its diffusion.

With so many fine details, I miss a description of the physical processes, as observed in the model runs, which determine the TTD. What is the impact of subsurface stormflow? Saturated and unsaturated flows? Groundwater? This is important in order to explain the impact of the parameters examined.

In the following a few specific comments.

- Line 38. I would also cite the pioneer works by Niemi (1977) and Nauman (Residence time distribution theory for unsteady stirred tank reactors, Chemical Engineering Science, 1969).

- Line 55-57. Here the introduction moves to the field of groundwater hydrology, where the issue of the BTC tailing (power-law or not) has been the subject of intense discussions in the last 2 decades or so; this short text and citation does not even scratch the surface and it looks quite superficial here.

- Line 57: The sentence of the “great” underestimation of mass is very much debatable, in most cases it’s a tiny fraction of the total mass. It may be important for risk assessment of highly toxic compounds, but uncertainty is anyway very large there.

- mTT: please define it (I guess it’s mean TT)

- Line 94-95. This sentence is repeated in other parts of the manuscript. By definition such approach cannot “completely” erase differences. The question is whether the approximation is good enough for applications. The study by Ali et al (A comparison of travel-time based catchment transport models, with application to numerical experiments, JoH 2014) shows that in many cases it does the job, also considering the

several sources of uncertainty, including for instance the estimation of ET (not done here).

- Lines 137-139. Unfortunately the effective hydraulic conductivity cannot replace the dispersive effects of the distributed macropores because it only impacts the mean velocity. I would delete this sentence as it is not needed: the exclusion of such component is legitimate and meaningful in my view because of the important role of macrodispersion in the TTD determination.

- Line 159. vertical or hortogonal to the slope? I guess the latter.

- Line 163. 5m of dispersivity is quite a lot, even more so for the vertical one. Why the choice? In this case the inclusion of Dfree looks irrelevant.

- Lines 174-175. What head is provided in the boundary condition? Where is the water table located? This is quite important.

- Line 204. What is the "subsequent precipitation amount"?

- Line 214. I guess that mm/a means mm/y

- Line 214. Please provide more details on the rainfall time series, e.g. regime, climate etc. As a matter of fact TTD depends also on the rainfall regime, not only the total rainfall per year (e.g. Botter et al 2010).

- Line 338. I don't like the definition, I would rather speak of "The Inverse Gaussian distribution, with parameters D, ..., that is a particular solution of the Advection Dispersion Equation". AD is misleading, as ADE can have several different solutions.

- Line 401. This discussion is based on log-log plots, which many times are misleading. The convergence of curves at large time can be an artifact of the plots.

- Line 408-409. Differences seems larger to me. Again, the log-log plot does not help.

- Section 3.3. Some of the (interesting) conclusions here are very similar to those of

Fiori et al (Stochastic analysis of transport in hillslopes: Travel time distribution and source zone dispersion, WRR 2009) which I think is important for this work. There, the different parts of the Gamma distribution pertains to different mechanisms and parameters (soil, bedrock, etc.). The main difference is that they identify the important role of KBr in the behavior of the tail, which is the exponential part of the Gamma, which in turn is related to groundwater discharge. The aquifer volume, which depends on water table, thickness and slope, has an important role here.

- Line 490. I don't see the power law.

- Line 510. How is the fitting done? What inference methods? How one can say that a distribution performs better than another? Any statistical test?

- Line 668. I don't agree with this analysis, the presumed power-law tail covers less than one logscale. Also, identification of power law tails is not simple (see e.g. Pedretti and Bianchi, Reproducing tailing in breakthrough curves: Are statistical models equally representative and predictive? AWR 2018), the emergence of a (short) straight line in a log-log plot may not be enough. At any rate, I would not say that the inadequacy of the distributions in fitting the TTD is because of the tail, that by the way involves a tiny fraction of the mass, which is magnified by the log-log representation. I think that the issue of powerlaw tails is too much emphasized here.

- Section 4.2. This part is not entirely convincing, I can't see the validity of the prediction based on F. By the way the latter does not include other relevant ingredients, like e.g. KBr.

- Line 750. Again, the method cannot erase "all" differences, but perhaps is adequate for many applications.

- Conclusion section. It is too long, one cannot see immediately the main results of the work. It's a pity because there is a lot of interesting material, that however needs to be better distilled and conveyed.

- Line 754-755. “. . .it is possible to predict the change using the dimensionless flow path number F.”. At the third line of the Conclusion section this seems the major conclusion of the work. Is it so? It does not seems like after reading the text.

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