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Discussions

Interactive comment on "Spatially Distributed Characterization of Soil Dynamics Using Travel-Time Distributions" *by* F. Heße et al.

F. Heße et al.

falk.hesse@ufz.de

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We would again like to begin by saying that we really appreciated the comments of the reviewer and we think we could much profit from them. We are sure that they helped us to significantly improve our manuscript. In the following, we present these comments as well as our point-by-point response to all of them. In addition, we added the revised version of the manuscript with changes tracked as a supplement.

Anonymous Referee 2

This manuscript uses the theory of travel time distributions in time variant flow systems and a spatially distributed hydrological model (mHM) to analyze the spatial distribution of mean travel times and life expectancies in a German catchment. To the best of my knowledge, this is one of the first attempts of using spatially explicit formulations to analyze the main physical controls on travel time distributions.

Overall, I'm in favor of publication of this manuscript in HESS. The topic is timely and interesting, the technical analysis of the authors is largely robust, and the paper is quite clear (event though some improvements in the presentation are recommended, see below).

1. Title: I'm wondering if "soil moisture dynamics" would be a better choice instead of "soil dynamics" We agree with the Reviewer's assessment and changed the title accordingly.
2. Page 5, section 2.2: I suggest adding more information about the rationale behind these equations, and the assumptions (e.g. random sampling) We extended the discussion of these equations mainly by addressing their limitations. However, to keep their introduction concise, we refer for further information to the established sources.
3. page 6, line 10: maybe it is worth adding more info about the nature of these global parameters gamma

   We added more information to the paragraph to better highlight the role of these parameters for our investigation. See Section "Numerical model" in the revised version of the manuscript.
4. equation (5b): need to add "if $x_5 > TV$" We changed this as suggested.

5. page 7 line 28: better specify where the runoff data are gathered (only at the outlet) Yes, they are collected at the outlet. We clarify this in the revised version of the manuscript.

6. page 8, lines 3-4: maybe it is worth to show the result mentioned here, or provide explicit reference about where these results can be found in the existing literature In the revised manuscript, we now provide more data on the soundness of the calibration scheme. This includes a representative plot for the generated outflux as well as a comparison of the predicted AET and measured values for a measurement station in the catchment. This demonstrates the soundness of the calibration scheme with respect both to data that was used for calibration (discharge) and data that was purely the result of the model (AET). See also Section "Study area and model set-up" in the revised manuscript.

7. Figure 5: units are missing We added the units to the caption of the figure.

8. Equations (7), (2) and results: it has been shown that the storage involved in solute circulation is much bigger than the hydrological storage that can be estimated using a rainfall runoff model. Most of the existing tracer data suggest this instance in many place around the world (Plynlimon, Hubber Brook, etc). This would imply the use of a larger storage in the denominator of eq (7) for the calculation of TTD. While I think this issue can not be addressed in the absence of chemical data, I think it would be important to make a discussion on this point and clarify the assumptions underlying the analysis (i.e. absence of residual storage). We fully agree with the comment of the reviewer that hydrological models (e.g. mHM) are more concerned with fluxes that with states (i.e. storage) since that's what they are calibrated against and consequently, that's what they are sensitive for. As a result, the storage used in this models has a high degree of uncertainty. From the very beginning of our analysis, we therefore tried to minimize the influence of such possibly erroneous results from our model. This notion became even

more pressing when our results demonstrated the strong influence that the state variable, i.e. the storage, often has on travel-time behavior. One of the decisions we made, was to confine our analysis to soil moisture only (hence the title of the manuscript), since previous results indicate the overall ability of mHM to estimate soil water content. However, unless better estimates could be provided for the groundwater (which we are working on), where most of the water is stored, we completely excluding this compartment of the water cycle from our analysis. Due to the comments of the reviewer, we became aware that these points were not properly formulated in the original manuscript and better emphasize them now in the revised version (see Section "Numerical model"). In addition, we also consider the strong sensitivity of mean travel times on storage to be one of the main messages of our study. Contrary to discharge data, which is determined by the fluxes, mean travel times are sensitive to both fluxes and states. This opens the door for a more robust and informative calibration procedure, which we try to outline in Section "Relevance of TTDs for hydrological inference". To better convey this important notion, we revised this section accordingly.

9. When you apply the formulation to the scale of a single grid cell, then you have to include the effect of input and output lateral fluxes. Maybe it is worth to specify how the TTDs are calculated in a spatially distributed setting. In mHM subsurface lateral fluxes are assumed to be unimportant compared to the vertical fluxes. This is a modelling assumption that allows for much of the implementation of mHM but at the same time puts some limitations on its applicability. For our modelling this has two limitations: (i) surface lateral fluxes must be present within every grid cell which limits the application to grid sizes in excess of several hundreds of meters and (ii) groundwater states and fluxes are highly error prone due to large sugsurface lateral flux components in aquifers. As a result, we limited ourr analyses to grid sizes with 500 m minimum and we also confined our analysis to soil water content, only.

10. Page 10, lines 2-4: gamma models for stationary TTD are much more widespread than exponential models in the literature. Moreover, the ref to Rodriguez1979 can be misleading, as in that case only the IUH is concerned. We agree with the Reviewer's assessment and amended the manuscript to better convey this notion.

11. Page 10, lines 22-24. SAS functions have been introduced before Rinaldo2015 only the name has been introduced later in those papers. We fully agree with the reviewer and changed the manuscript to reflect this fact.

12. Page 10, line 33: "the most simple SAS" should read "uniform SAS" with some references. We agree and changed this as suggested.

13. Page 11, lines 1-4: I would suggest to expand this discussion and provide more arguments/clarify your reasoning. Maybe the point here is that the mixing taking place at spatial scales smaller than 2 km X 2 km is not relevant? When we started out our study, we were aware of the limitation regarding the specification of an age-dependent outflow function. The parametrization of such a function would necessitate measurements against we could fit our model predictions. Since we did not have such data (yet), we decided to use the outflow function which assumes least knowledge, i.e. a uniform sampling without any age preference whatsoever. In the next step, we wanted to estimate the overall error that could result from such a decision. To estimate the possible influence of this decision, we reasoned that a scale-dependent bias in the estimation of travel-time behavior would indicate the existence and possible strength of such an error. This is due to the multi-scale nature of mHM, where subgrid heterogeneity is taking into account by virtue of an upscaling scheme. Using a smaller grid resolution would make this heterogeneity explicite and therefore reveal any possible unaccounted subscale influence. The lack of any scale effect in our results indicates that mHM is able to take this sub-scale heterogeneity into account within the investigated

scale range (which is roughly an order of magnitude). We are aware, that this is only covering one possible source of age-dependent outflow behavior and that other unresolved heterogeneity (at even smaller scales or due to other subsurface properties not accounted for in mHM) would influence the outflow generation as well. We therefore regard our analysis as tentative. In the revised manuscript, we now discuss and better highlight this reasoning.

14. Pages 13 -20: It would be good to see more discussion here about the physical interpretation of the results. This would increase significantly the breadth of the paper. We agree that a physical interpretation of these purely statistical analysis should be provided whenever possible. We therefore extended the discussion and provided physical reasoning for several of the effects observed. This includes particularly precipitation, ET and land cover, which have demonstrated to have a major effect on travel-time behavior (see also the respective sections in the revised version of the manuscript).

15. Page 20, equation not-numbered. I'm wondering why this equation is used to introduce Figure 15 as the life expectancy in not involved (unless I'm missing something) The equation is now numbered. The connection to Figure 15 is such that the scatter plot describes the relationship between mean life expectancy and mean age. This is now better highlighted in the revised manuscript.

16. Figure 15: physical interpretation of these results? units are missing We were not able to come up with a plausible physically-based explanation for this relationship. In particular, since the observed effect is quite minor. Consequently, we only acknowledge that, whatever difference exists between forward and backward formulation, this difference is not very strong. We did, however, add the units to the figure.

17. Page 21, line 1 and Page 23, line 29: which is the underlying physical interpretation of these results? With respect to Page 21, Line 1: we did not come up with

anything other than hindsight reasoning. Therefore, we want to refrain from speculation. With respect to Page 23, line 29, i.e. the unimodal structure of the mean life expectancy: This observation was made for wet years. This means that the soil was largely saturated and acting more of a conduit for precipitation events without imposing any characteristics on its own. We revised the manuscript to better reflect this reasoning.

18. Page 21, line 10: in a general cell the influx is just precipitation of this include also lateral fluxes? As mHM is conceptualized to perform on mesoscale catchments, it is assumed that no lateral fluxes between grid cells do exist. Thus, only vertical fluxes are considered at a particular location, for which the only influx is precipitation. Only the surface routing accounts for lateral fluxes for estimating river runoff.

19. Page 27, equation (8): I think there is an extra Q before the "=" sign. We agree with the Reviewer and changed this as suggested.

20. Page 28, last line: why does this happen? In the revised version of the manuscript, we added a paragraph to the Section "Land cover properties", where we elaborate on the possible causal factors for this observation (see also our answer above).

21. Appendix A, equations (a1) and (a2): I guess the signs of the Q terms are wrong. That's true. We changed this in the revised version of the manuscript.

22. Appendix A, lines 14-17. MKVF equations are expressed in a different form, in which TTDs and output fluxes are grouped together. This "caveat" makes a huge difference: the explicit presence of the hydrological fluxes in e.q. (a1) allows for the use hydrological models for TTD inferences - as done in this nice paper - and the coupled modeling of flow and transport at catchment scale, which is the next step foreseen by the authors (page 29, lines 25-30). We welcome the overall

positive content of the comment above. We do, however, not see any question or criticism that could be directly addressed. Maybe this was not intended, though.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-232/hess-2016-232-AC2-supplement.pdf

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