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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 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.

This paper describes postprocessing of the state and flux data from the distributed hydrological model mHM, to calculate modelled transit time distributions of water in each model grid cell, in a catchment in Germany. The authors are then able to analyse the temporal and spatial variation in modelled transit time distributions, and relate this to variations in model inputs and parameters.

In its current form, the paper provides an investigation of mHM model behaviour that has not previously been studied, however there is no comparison to measured data to show whether or not the described model behaviour might be realistic, and as highlighted in my comments 2 and 3 below, there are aspects of the model which could be expected to disagree with field data. I therefore believe there is more work to do to convince the reader of the scientific contribution of the paper, and why we should believe these model results.

With respect to a perceived lack of realism in our model results, we would like to provide more information that may help to build confidence in their overall soundness. First, mHM has been successfully applied to a wide range of catchments in different parts of the world and demonstrated its ability to provide reliable predictions on the hydrological dynamics in said catchments. In addition, we can also provide support for the soundness of the predictions of mHM in the considered catchment. In Figure 1 and 2 we show two different results. Figure 1 shows the discharge data vs. our prediction for a dedicated time interval between 1970 - 1979, demonstrating the good performance of mHM in this regard. What is more important, however, is the ability of mHM to also predict data that has not been used for calibration. To demonstrate that, we used data from an eddy-covariance station within the modeled catchment. The comparison shows again a good fit between observation and model prediction (see Figure 2). These results should provide the necessary confidence for our model predictions. To better convey these points, we also amended the manuscript, where we now present and discuss both above results.

In particular, I have the following major comments:

1. The introduction reads as though no previous studies have done what this paper sets out to do, i.e. to use the states and fluxes of a hydrological model to gain additional information about transit time behavior that cannot be directly extracted from field measurements (also referred to as the "virtual experiment2 approach).

However, this is not the case, and previous studies have done exactly this including Hrachowitz2013, Fenicia2010, my own McMillan2012, and Sayama2009 who include an analysis of grid size sensitivity and effect of soil properties on transit time, both analyzed here. The novel aspect of this paper is that a distributed rather than lumped model is used, and I suggest that the introduction is changed to make this point. We appreciate the additional perspective on the topic of travel-time behavior. In the earlier version of the manuscript, we did not fully acknowledge this prior work and therefore created a biased impression of our contribution. In the revised version of the manuscript, we are now able to describe a more accurate picture of the present state-of-the-science and therefore better position our study vis-a-vis this prior work.

2. The method used in the paper implicitly assumes instantaneous total mixing of the water in each model grid cell, without any partial mixing behavior or inactive storage component. However, this type of process description is widely considered to be not representative of field conditions, and typically does not give model estimates comparable with field data estimates of mean residence time (see McDonnell et al. 2010 and the previous references I gave). The authors need to discuss this point and justify the use of model results that rely on this assumption. We fully acknowledge the limitations mentioned by the reviewer. The scheme by **?**, which we use in our study, does indeed assume perfect mixing in every control volume. Although this limitation has later been amended by introducing a function describing age-dependent outflow generation, we did not use the improved scheme as described by Botter2011. This was done because this formalism is still lacking a method for deriving this age-dependency for a given catchment when using water fluxes, only. In the absence of say tracer data, we have no way of inferring the possible mixing in every single cell. Using the framework of Information Theory, one should use in such a situation the assumption with the least information content, which in this case is the uniform model. We

tried, however, to estimate the possible influence of this decision on our study. As pointed out by Harman2015, age-dependent (i.e. non-uniform) outflow generation is the result of several sources of both external and internal heterogeneity in the catchment. The aforementioned ability of mHM to account for such variability, may therefore make the assumption of complete mixing more justified. Since we are lacking actual data to verify this assumption, we tested its plausibility by determining the mean-travel time on different spatial resolutions. We hypothesized that the assumption of perfect mixing should be better meet on finer scales, where more heterogeneity is explicitly modeled. Any divergence from this assumption should therefore be visible when comparing mean-travel times between finer and coarser resolution. Our failure to find such differences adds plausibility that the assumption of complete mixing does not introduce major errors. In the absence of actual data, we have, however, to consider these inferences tentative and open to revision. In the revised version of the manuscript, we now acknowledge these points both in Section "Travel-time distributions for a single control volume" when introducing the equations for the derivation of the TTD's as well as in Section "Impact of modeling resolution".

3. The paper analyses transit time distributions within each model cell. Because the model then routes water between cells in a downstream direction, I infer that the larger the catchment, the greater the modeled transit time because water must pass through a series of cells. However, transit time is not generally found to have a clear relationship with catchment scale (e.g. Hrachowitz2010). Again, the authors need to discuss and justify this point. In mHM the water is not routed from cell to cell but leaves every cell and enters the river network. Once in the river network, the water moves very fast (order of days) compared to movement within each cell (order of months). As a result, there is only a very weak scale effect originating from the catchment itself, in particular for such a moderately sized catchment as used in the study. Given our results, we would anticipate

scale effects only for really large catchments.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-232/hess-2016-232-AC1-supplement.pdf

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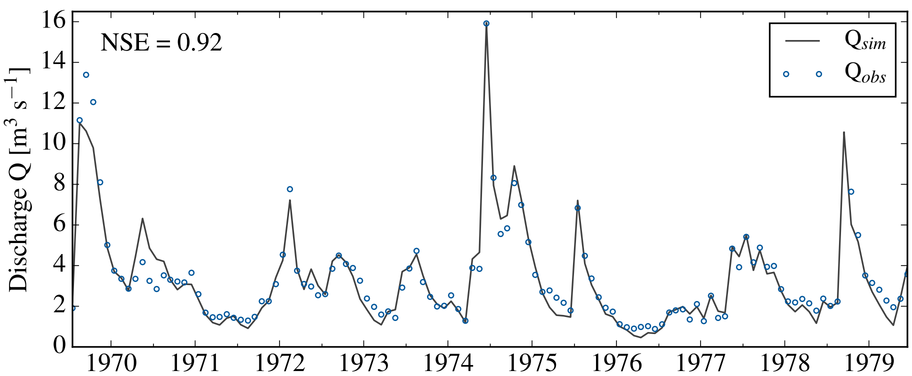

Fig. 1.

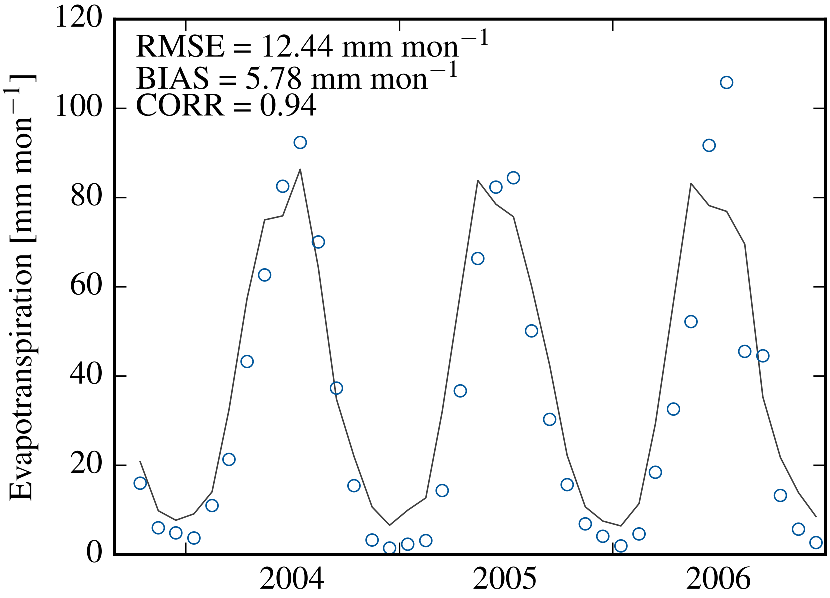
RMSE = 12.44 mm mon^{-1}
BIAS = 5.78 mm mon^{-1}
CORR = 0.94
Evapotranspiration [mm mon^{-1}]
0
20
40
60
80
100
120
2004
2005
2006

Fig. 2.