

Comment on hess-2021-430

Anonymous Referee #1

Referee comment on "Quantifying time-variant travel time distribution by multi-fidelity model in hillslope under nonstationary hydrologic conditions" by Rong Mao et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-430-RC1>, 2021

Comments on "Quantifying time-variant travel time distribution by multi-fidelity model in hillslope under nonstationary hydrologic conditions" by Mao et al.

The authors presented a computationally efficient method for estimating travel time distributions (TTDs), residence time distributions (RTDs), and the StorAge Selection(SAS) functions using process-based models that account for the diffusion (or hydrodynamic dispersion) process. They argued that there are two ways of estimating those distributions and functions, but those methods are computationally burdensome. One way is solving the advection-diffusion (or advection-dispersion) equation (ADE) for each rainfall event, and another way is using the particle tracking model considering the random walk. Their proposed method (called multi-fidelity model) estimates the TTDs, RTDs, and the SAS functions by combining the result of the particle tracking model *without* the random walk component (so-called low-fidelity model) and the result of the ADE simulations for *selected* rainfall events (so-called high-fidelity model). They argued that the multi-fidelity model reproduces the result of the ADE model closely (especially when about 10% of all rainfall events are simulated using the ADE model) with a potential reduction of computational time.

My recommendation for the current manuscript is "rejection" due to the following reasons. First, the need for this method is not convincing especially because the proposed method reduces the accuracy of TTDs, RDTs, and the SAS function estimation (see major comment 1). Second, the test is not well designed, and the method's performance is questionable (see my major comment 2). Third, some model results seem to be inaccurate (see major comment 3). I will also provide other comments in the hope that those comments will be helpful to the authors.

Major comments

1. Not convincing motivation and unclear computational efficiency

The manuscript does not provide a sufficient explanation of the motivation for developing the multi-fidelity model. Reducing computation time seems to be the primary motivation (e.g., L84-86), but there are many other ways to reduce the computation time (especially when sacrificing accuracy for reduced computation time is allowed). We can run a full model (e.g., a model that solves the ADE for each rainfall event or the particle tracking model with the random walk component) in a computationally efficient manner. For example, utilizing a parallelized code (e.g., ParFlow), using a coarser grid, or using a coarser time step is an option. Utilizing parallelized codes to solve Richards' equation or tracking particles would not reduce the accuracy of the results but significantly reduce the computation time. Using a coarser grid or a coarser time step could reduce the model's accuracy, but the proposed method, using the multi-fidelity model, could also reduce the accuracy. We can also estimate a BTC conditional to a set of consecutive events rather than to a single event which then can be used to estimate the TTD. We can probably define the injection time of the set as the center of mass of those consecutive events (and, if needed, perform a bit of correction to consider negative travel time). This method would not be able to estimate fast time variabilities of the TTDs, RTDs, and the SAS functions, but there is also no guarantee that the proposed method, the multi-fidelity model, could estimate such fast time variability (see also my major point 2). The result generated using this method, estimating TTDs condition to a set of events, would actually be easier to interpret since we know what information is averaged out, while interpreting the result of the multi-fidelity model would not be very easy. How should a user of the multi-fidelity model interpret the result of the multi-fidelity model? In other words, what should a user expect to be the difference between the high-fidelity model result and the multi-fidelity model result?

Also, when running the ADE model for every single rainfall event (without the simplifications discussed above) or running the particle tracking model with the random walk component is practically feasible, the proposed method would not be useful. For example, let's say running a full model (e.g., running the ADE model for every single rainfall event) takes ten days. Running the multi-fidelity model would take one day + additional time to run a low fidelity model, e.g., the particle tracking model without the random walk component. In this case, do we need to use the multi-fidelity model to save less than nine days even though the method reduces the accuracy (since the multi-fidelity model result is not the same as the high-fidelity model result)? In what case do we really need this multi-fidelity model?

In addition, computational efficiency is not discussed in detail in this manuscript. Since the method was developed for computational efficiency, the efficiency must be discussed in more detail.

2. Performance of the multi-fidelity model and design of the test

Figure 4 shows that the model error does not converge to zero as the rainfall interval decrease. When the rainfall interval of 5 is used, the mean error is a bit larger than the mean error for the case with the rainfall interval of 10. If the mean error is not expected to converge to zero as the rainfall interval decreases, it needs to be discussed in more detail so that readers know what to expect from the multi-fidelity model.

Furthermore, Figures 4 and 5 show that the multi-fidelity model has a large variance of error, meaning that the time variability of hydrologic transport is still not well captured. Even if I assume that the error is bearable for this test case (i.e., the test using the homogeneous 2D hillslope domain), the error could become larger if the method is applied to a hydrologic system with larger time variability of hydrologic transport dynamics.

The temporal variability of hydrologic transport dynamics in the homogeneous 2D hillslope domain is not large, resulting in the SAS function with a limited time variability (Figure 9b). In many real-world studies, the SAS function time variability is much larger than what is presented in Figure 9b (see many studies that applied the SAS Function). Could this multi-fidelity method be useful when the time variability of hydrologic transport dynamics is large?

3. Model results

Some results do not seem to be accurate. Figure 9 shows the SAS function, and the shape of the SAS function is very spiky especially for the low fidelity model. The SAS function, in general, is expected to have a much smoother shape (and that is one reason why the SAS function becomes popular). Also, see Kim et al. (2020), where they showed that the SAS functions derived for a similar hydrologic domain are smooth.

This incorrect result is perhaps due to the insufficient number of particles used in the model for the low-fidelity model. Or maybe there is a problem in the estimation of the RTDs and the SAS functions. If the former is the case, this incorrect result may have resulted in the non-zero bias I pointed out in major comment 2.

[Reference]

Kim, M., T. H. M. Volkmann, Y. Wang, C. J. Harman, & P. A. Troch (2020), Direct observation of hillslope scale StorAge Selection functions in an experimental hydrologic system: Geomorphologic structure and the preferential discharge of old water, <https://doi.org/10.1002/essoar.10504485.1>

4. Section 2.3

It is unclear what "correlation" means here, and how (15) leads to the argument in L177-178 ("Theoretically, a mapping is capable of being setting up between ... if the diffusion parameter D is known. This is the theoretical foundation of the multi-fidelity model") and L198-199 is also unclear. What authors show in this section using (10) – (15) is trivial---As the diffusion coefficient decreases, the transport is dominated by advection. How does it support the argument in L177-178? Also, the 1-D configuration used in this section is not enough to support the argument. There is no consideration of transversal diffusion or dispersion. Furthermore, spatial and temporal variations of velocity are not considered.

Minor comments

Needs less focus on the hillslope scale: In many places throughout the manuscript, the authors talked about hillslope (e.g., in the title, L3, L18, and so on). However, I am not sure if the multi-fidelity model could be useful at the hillslope scale, which is relatively small. For a small hydrological domain like a hillslope, running the full ADE simulations or the particle tracking model with the random component will be feasible.

Diffusion process: It is unclear what the diffusion process simulated in the 2D hillslope model is. The authors may have assigned a specific concentration for the rainfall event of interest while the concentration of all other water is set to zero. Since the diffusion is controlled by the concentration gradient and the diffusion coefficient, it needs to be more precise what the gradient in the model (which depends on the assigned value for the rainfall event of interest) means and what the diffusion coefficient means.

L5: "transient ground water flow" is not the only process that could result in time-variant TTDs.

L11-13: It is not clear how significantly the number of particles can be reduced.

L29: "yearly averaged" does not seem to describe the time-invariant TTD correctly.

L67-69 and L146-148: While I agree, this sentence is not enough to motivate this study. I think the authors need to cite studies that show that considering diffusion or dispersion requires significantly more particles (if such a study exists) or need to show it clearly somewhere in the manuscript.

L76: Not clear what message the authors are trying to convey.

L83: Not clear if the authors solved the full 5D equation or just combine the results of the high fidelity model and the low fidelity model.

L152: No evidence is provided to support the "drastic" decrease of the number of particles

L175: While I agree with the "no matter what boundary conditions" argument, what the authors show in this section is for a specific boundary condition used to get (14).

L265: There are many studies where RTDs are estimated using BTCs. Most of the experimental studies of the SAS function and many the SAS function studies that utilize process-based model estimated RTDs using BTCs.

L280-281: This sentence is unnecessary for the purpose of this paper.

L291: Lower concentration of which solute?

L305: "quasi-steady state" means something different.

L313: Each single rainfall event at daily time step?

L320-321: Richards' equation-based model has many assumptions. Also, perhaps better to avoid the term "real physical process".

Figure 3: Please clarify what the particles at $x = 100$ m are.

L373-376: A model result of solute concentration cannot be lower during the whole time than the concentration estimated using another model.