Response to comments of Anonymous Referee 1

We would like to thank the referee for the valuable comments and suggestions, which improved the quality of the paper. Below is our response to the comments and suggestions.

Comment on hess-2022-298

I have tried to read this paper multiple times now and every time find myself frustrated. I am highly litterate in terms of mathematically dense papers, but I found this paper next to impossible to make my way through. I do not usually write grumpy reviews, but this will be one. I have three major concerns that lead me to recommend that this paper be rejected.

(1) My first and likely biggest issue is going from equation (1) to (2). Any time you average and ADE equation like the one the authors have you will have a mean and fluctuation of the things that vary. In this case concentration, velocity and depending on the nature of the dispersion coefficient that also. Where are all of these gone? They don't just dissappear as it seems that they do in equation (2) - i.e. it's fine to say that the average of the fluctuation of concentration is zero, but the average of the product of concentration and velocity fluctuations is not. Indeed this is exactly what leads to things like macrodispersion and deviations from standard behaviors. Where have these gone here? There is no discussion of them and none of the assumptions I see in the problem setup suggest they do not exist or are negligible. This is the starting point of the paper and frankly makes me feel like the authors are departing from a faulty point from the getgo.

Response

a. The derivation of Eq. (1) to Eq. (2) was presented in Holly (1975). Equation (2) has been widely used to analyze problems related to solute transport by fluid flow (e.g., Zerihun et al. 2005, Baek et al. 2006, Chavez et al. 2014).

Zerihun, D., Furman, A., Warrick, A. W., and Sanchez, C. A.: Coupled surface-subsurface solute transport model for irrigation borders and basins. I. Model development, J. Irrig. Drain. Eng., ASCE 131(5), 396-406, 2005.

Baek, K. O., Seo, I. W., and Jeong, S. J.: Evaluation of dispersion coefficients in meandering channels from transient tracer tests. J. Hydraul. Eng., ASCE 132 (10), 1021-1032, 2006.

Chavez, C., Fuentes, C., Brambila, F., and Castañeda, A.: Numerical solution of the advection-dispersion equation: Application to the agricultural drainage, J. Agric. Sci. Technol., 16(6), 1357-1388, 2014.

b. We apologize for not mentioning Holly's (1975) idea in developing Eq. (2) regarding the average of the product of concentration and velocity fluctuations. Holly (1975) considered the mixing of the contaminant plume over depth in natural channels to be complete, so that the fluctuations around the depth-averaged concentration are relatively small. Then the average of the product of concentration and velocity fluctuations can be considered to be absorbed into the gradient transport terms in Eq. (2).

A note is added to the manuscript mentioning this as follows:

"In developing Eq. (2), it is assumed that the contaminant plume in confined aquifers is well mixed over depth, so that variations around the depth-averaged concentration are relatively small. Then the average of the product of concentration and velocity fluctuations can be assumed to be absorbed in the gradient transport terms in Eq. (2)"

c. It can be clearly seen (or verified) that Eq. (2) for flow in aquifers of uniform thickness (i.e., $B(x_1,x_2)$ = constant) reduces to the traditional two-dimensional advection-dispersion equation for solute transport in confined aquifers, with the flow fields characterized by the aquifer transmissivity fields instead of the hydraulic conductivity fields.

(2) As I noted I am someone who writes and reads a lot of papers with pretty dense and complex mathematics in it, but I found a lot of what the authors present extremely hard to follow, where in some places there is abundant detail and in others serious gaps.

Response

Please clarify. We will do our best to change it.

(3) Last but not least, even if everything is right (which I cannot verify) I struggle to see the real importance of this paper and thus am hesitant to see it published in such a high level journal such as HESS which is one of the top journals in our field. Much of the paper feels a little archaic in nature and while I love theoretical papers with full mathematics I also feel that something clear should be gained by ellaborating it and I just do not see that here.

Response

a. Natural confined aquifers at the regional scale often exhibit nonuniform thickness. In the traditional approach to regional groundwater flow problems, the effects of aquifer thickness variations are implicit in aquifer transmissivity term. Therefore, it is very difficult to use the traditional approach to assess the influence of thickness on the flow field and thus the displacement of solutes.

b. In this work, the relationship between the two-dimensional depth-averaged solute conservation equation and the Fokker-Planck equation is used to relate the effects of aquifer thickness variations to flow field variations. In this way, a definite relationship can be established between the thickness variation and the solute displacement variation. This work shows that variability in aquifer thickness can lead to nonstationarity in hydraulic head fields and thus to nonstationary flow velocity fields and anomalous longitudinal dispersion. The work also shows that neglecting the variability of aquifer thickness when predicting the longitudinal displacement of solutes at large times can lead to a significant underestimation of longitudinal dispersion. To our knowledge, the analysis of the influence of the variability of the thickness of the aquifer on the longitudinal displacement of the solute within the framework of stochastics has not yet been presented in the literature.