

Interactive comment on “Investigation of solute transport in nonstationary unsaturated flow fields” by C.-M. Chang and H.-D. Yeh

Anonymous Referee #2

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General comments

This manuscript presents a solution of the steady state transport equation in nonstationary unsaturated flow field. In the present form, it is neither a technical note nor a full paper. In the Introduction, the authors stated that the objectives of the manuscript are to address the impact of flow non-stationarity on macrodispersion in an unsaturated flow. Given these premises, I was expecting a full paper addressing at depth and with some generality the impact of flow unsteadiness on macrodispersion. Unfortunately this is not the case. The manuscript focuses on a particular solution of the steady state version of the transport equation with a very limited discussion and no justification of the strong hypotheses introduced at the beginning of the mathematical derivations. On

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the other hand, this manuscript can hardly be defined a technical note because it does not address a well defined technical issue. The authors, after introducing some relevant simplifications of the transport problem, present the solution followed by a very cursory discussion and a short conclusions section. The rather strong hypotheses introduced to simplify the problem limit the applicability of the solutions, but unfortunately the scanty justification of the simplifying hypotheses and discussion of the results make almost impossible to ascertain the value of the proposed solution. From a general point of view, to be useful a technical note should address a case of wide interest providing a solution easy to use. This is not the case of this manuscript, which provides a solution of a simplified (perhaps oversimplified) transport equation, which relevance is difficult to ascertain.

Specific comments

Flow is two-dimensional and nonuniformity is introduced by imposing a constant flux at the surface and a constant head at a given depth. In the horizontal direction the computational domain is unbounded. However, authors did not specify at which depth the head is imposed. I am puzzled by the relevance of this type of flow nonstationarity. As commented below the steady state hypothesis for the transport equation suggests that the authors are looking for the long time behavior of macrodispersion and in this case I am wondering if another type of nonstationarity emerges: the intermittence of precipitation. However, given for granted that a constant flux at the upper BC can be accepted in some situations, the only case I can figure out for which the lower BC is of constant head is when a water table is set at a given depth and the underlying aquifer is able to convey the entire recharge flux in order to keep the water table fixed in time. All these conditions are in my view quite unrealistic, in particular if we think that the domain is unbounded in the longitudinal direction.

Transport is solved in stationary conditions, i.e. concentration does not change with time. This is a very peculiar situation, which is often implemented in order to model cases in which mass flux entering the system, for example in an aquifer through a well,

is consumed by some reactions modeled as a sink term. This may occur, for example, when an organic contaminant is consumed by bacteria and the equilibrium between injected and consumed mass is typically reached at late time since the beginning of injection, which is supposed to be continuous. It is unclear to me what type of steadiness is reached in the case considered by the authors, considering that they do not specify the BCs for the transport equation. I am tempted to conclude that steady state in this case implies constant mass flux entering through the upper boundary and the same constant flux exiting from the lower boundary. If this occurs isn't that the solution is simply of constant concentration through the profile? However, if any steady state condition is attained it would require that the plume had traveled many length scales. This means that the correlation length of the saturated hydraulic conductivity cannot be increased freely, while keeping constant the depth of the computational domain as done in Figure 1. In fact, as the correlation length increases a larger distance is needed in order to reach steady state conditions and when this distance becomes larger than the formation depth the proposed solution cannot be applied. This is not investigated and discussed in the manuscript.

Furthermore, the effect of neglecting in the transport equation the term involving the spatial derivative of soil water content is unclear. In this case referring to existing papers is not enough. A more in depth analysis and justification of the assumption, specifically referred to the case at hand, is needed in my view.

Conclusions

For these reasons I regret to conclude that in my view this manuscript falls short both as a technical note and a full manuscript. Much more work is needed to make it suitable as a full paper, while I think that this topic, as presented, is not suitable for a technical note.

I will be happy to change my opinion expressed above if the authors are able to address coherently the following points, which in my view are the main drawbacks of this

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manuscript: a) a discussion of the relevance of the type of unsteadiness they consider in this manuscript. Is it possible to make the approach more general in this respect? b) transport equation: can the steady state hypothesis be removed? If not I would be glad to see an in depth discussion on the implications of this hypothesis; c) transport equation: what is the effect on the solutions of neglecting the term containing the spatial derivative of soil moisture? d) I would be happy to see a coherent and well structured discussion of the impact of flow unsteadiness on macrodispersion (possibly including early time behavior).

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