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Interactive comment on "Aged streams: Time lags of nitrate, chloride and tritium assessed by Dynamic Groundwater Flow Tracking" *by* Vince P. Kaandorp et al.

Anonymous Referee #2

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This manuscript uses a distributed transient groundwater flow model with forward particle tracking to derive travel time distributions (TTDs) that are then coupled with input curves for tritium, chloride and nitrate to estimate how groundwaters transport these inputs into surface waters and to identify which characteristics of the groundwater system control the rate at which transport occurs.

The manuscript is generally well-written and provides a potentially interesting case study. However, I have major concerns about the modeling approach (see below) and I do not feel the authors have clearly articulated the novelty of the work, nor have they provided a strong argument as to how their study provides a clear contribution that is

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Discussion paper

of broad interest to the international scientific hydrology community.

Overall, I do not think this manuscript is suitable for publication without substantial additional work. Aside from the question of the study's overall novelty, the construction, application and scrutiny of the model is highly questionable. The model structure introduces very different levels of process representation (and therefore complexity) between unsaturated and saturated zone elements and invokes the use of "sensitivity analysis" as a means to use an existing model calibrated for water flows to apply as a solute transport model. But the sensitivity analysis was particularly thorough and seemed to confuse sensitivity analysis with uncertainty analysis. It is therefore unclear as to what this evaluation actually provides in terms of confidence in the model structure, parameterisation or predictive capacity.

In terms of the question of novelty and relevance to an international audience, I was unable to determine this from the abstract and conclusions. For instance, the abstract sets out what was done but doesn't really articulate any major contribution to wider hydrological understanding in terms of enhanced model development or physical processes. The main conclusions are:

1. That the presence of an "unsaturated zone, increased mean travel time and a longer mean distance between agricultural fields and stream cause a lag in the breakthrough of agricultural solutes"; 2. That the "recovery of concentrations after measures that aim to reduce the solute inputs is determined by these parameters"; 3. The "groundwater contributing area was shown to increase and shrink based on wetness conditions within the catchment"; 4. "Especially the location of agricultural fields in the groundwater contributing area in relation to the catchments' drainage network was found to be an important factor that largely governs the travel times of the agricultural pollutants"; and, ultimately 5. "We conclude that groundwater functions as a buffer on the effect of agricultural pollution, by distributing water in time and space and making it possible for different waters to mix.".

None of these findings are particularly insightful – rather they demonstrate that the model produces "as expected" behaviour. Which leads to the question: how such conclusions provide a meaningful contribution to the international literature?

In terms of the model application, an existing groundwater flow model was pre-designed for a different purpose (the representation of water levels and flows rather than to represent solute transport: lines 180-185) and this was then applied to the case of solute transport and subject to a sensitivity analysis to consider potential uncertainty in the resulting outputs. This seems rather odd. Surely a study of solute transport needs to rely on a model that is built and calibrated to do just that? There are many such models available and there isn't a sensible justification why this was not attempted. It would be interesting to see a comparison of the approach taken in this manuscript with a bespoke model (calibrated for the purpose) to demonstrate how sensitivity analysis may be used for the purpose described here, but that hasn't been presented.

A second major issue here is the function of the unsaturated zone. A large body of work in the international literature has outlined the complexity of unsaturated zone processes in some detail, and particularly their impact on solute transport. It therefore seems that a useful model linking land-based solute inputs to stream solute responses would require a detailed representation of the unsaturated zone processes in order to provide the sort of management decision-support that this manuscript purports to give. The unsaturated zone assumed in the model was of uniform thickness and did not account for any macropore flow or chemical processes. This would seem to have essential processes omitted and, therefore, automatically limits the utility of the model. I am therefore struggling to see how this can be used as a reasonable representation of the catchment processes.

The sensitivity analyses described seem to be very simplistic: the state of the art in model assessment for parameter, structural and data sensitivity are far more advanced than is being attempted here. Given the sensitivity analysis is being used to justify the validity of a model calibrated against water levels and flows as a way to represent solute transport, it would need to be much more comprehensive to provide confidence that the approach was appropriate and robust. I also found the framing of the sensitivity analysis rather confused (line 185). Is this a sensitivity analysis or is it an uncertainty analysis? From what is provided in the manuscript it is not at all clear how uncertainty has been accounted for.

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