On the reliability of analytical models to predict solute transport in a fracture network

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General Comments:

This paper mainly compared the performances and reliabilities of classical Mobile-Immobile Model (MIM) and Explicit Network Model (ENM, or the so called discrete fracture network model) for describing transport behavior in a fracture system. The research is of general interest to scientists and engineers in hydrology and other Earth sciences. In general the methods are technically fine, but I have a few concerns about the explanations of the results (see the specific comments below). The fracture network in this study is very simple (only including two flow paths), so the authors should discuss the implication of their results for an in-situ fracture network (e.g. including hundreds of fractures). In addition, the writing should be further polished. Overall, I think this manuscript would be an interesting paper, if the authors could address the scientific matters that are listed below.

Specific Comments:

1. Introduction: Although many interesting works are listed in this section, but they are not arranged in a good order. This makes the Introduction too long and not easy to follow. The authors should rewrite this part in an order of background, scientific gap and aim, and in particular with emphasis on the poor understanding of transport behavior in fracture network under non-Darcian flow conditions. In addition, the authors should summarize and discuss the previous works in a compact way, rather than pasting their abstract here.
2. Page 14923 line 2: The sentence of 'a delay of solute transport for high flow rates' is confusing to this reviewer. A decreasing of travel time with the increasing injection flow rate means that high flow rates speed up the solute travel.
3. Page 14925: The mobile water fraction β varies between 0.41 and 0.95, and there is not obvious relationship between β and the injection flow rate. Even though MIM model can satisfactorily fit the experiments with different injection flow rates, the physical meaning of the best-fit values of β is not clear to this reviewer. The authors should explain why β could randomly change with increasing injection flow rates in the same fracture network, from a physical point of view.
4. Page 14926 lines 8-13: More explanation is need for Figures 7 and 8, because I still do not understand why P_Q is different between the flow model and ENM models.
5. Page 14926 lines 16-24: Is the secondary path (3-4-5) included in the immobile zone of MIM model? Actually the secondary path (3-4-5) becomes quite active under high injection flow rate, and in my opinion they should be mobile zone.
6. Page 14927 lines 1-5: In order to better show the nonlinear flow regime, why not plot the relation between flow velocity and water pressure?
7. Page 14927 lines 6-18: Why the dispersions obtained from MIM and EMM4 models are so different?
8. Figure 6: The observation curves are not necessary and the authors can just leave the measured data points.

Technical corrections:

1. Page 14922 line 2: Eq. (24) should be Eq. (23)? Eq. (31) should be Eq. (30)?
2. Page 14923 line 13: Eq. (16) should be Eq. (15)?
3. Page 14925 line 18: What does ‘soil’ mean?
4. Page 14926 line 5: why do you refer to ‘soil volume’?
5. There may be other mistakes, so the authors should have a carful check during revision.

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