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Interactive comment on "Time dependent dispersivity behavior of non-reactive solutes in a system of parallel fractures" *by* G. Suresh Kumar et al.

Anonymous Referee #2

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The paper discusses the transport of inert solutes in a system of parallel fractures and focuses on the effective dispersivity, which is derived from spatial moments of concentrations in fractures. The major result of the paper is that in a preasymptotic region the effective dispersivity increases with time and that the effective dispersivity in a system with variable apertures widths can be considerably larger than in a system with constant aperture widths.

To my opinion, the paper suffers from being irreproducible. Based on the information given in the paper, I am unable to recalculate any of the presented results or even check whether the used methodology was appropriate. Especially for rather theoretical

papers, this is to my opinion unacceptable. For instance, it is not clear how effective parameters where calculated from spatial moments. If the authors apply their procedure to derive effective CDE parameters from spatial moments of a concentration profile that is simulated using a 1-D CDE (Equation 1 with q = 0) and the boundary conditions used in Eq. 3, would they obtain exactly the same effective parameters as the ones they used to simulate the concentration profile? I question this but I am unable to check it. Neither is it clear how the transport equations were solved. Was this done numerically or using analytical solutions? For the case of parallel multiple fractures with varying apertures, it is not clear how the pore water velocities in the fractures were related to fracture aperture. Assuming the same vf for all fractures seems not realistic. If vf is different in different neighboring fractures, then the boundary condition in Eq. 4 at half of the fracture spacing (which I have to guess because it is actually not defined in Eq. 4) does not hold. Analytical solutions, which rely on this boundary condition, do not apply for this situation.

Therefore, it is my opinion that the information in this paper is insufficient to make a sound evaluation possible.

Specific comments. Why do the authors focus on the non-asymptotic behaviour of the dispersivity? I would assume that in the pre-asymptotic region, the concentration profiles and breakthrough curves will be highly skewed in fractured media so that they cannot be described by a convection dispersion model.

Why do the authors use spatial moments and not temporal moments? There are two arguments in favour of using temporal moments. It is practically impossible to determine spatial moments of concentrations in a fracture experimentally whereas breakthrough curves can be measured more easily. Relatively simple relations between the convection dispersion equation (CDE) parameters and the spatial moments of a solute plume are only obtained in infinite media. For their simulations, the authors define a boundary condition and for boundary condition problems or transport in semi-infinite media the relation between spatial moments and CDE parameters becomes far more

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complex.

Just after the start of the solute injection, the bulk mass is in the fractures so that the effective velocity derived from the spatial moments of concentrations in the fracture is equal the pore water velocity in the fractures. At later stages, due to mixing of solutes between fractures and matrix, the overall velocity of the tracer plume will be equal to the water flux divided by the total porosity and will be much smaller (depending on the ratio of the fracture volume to the total pore volume) than the velocity in the fracture. Therefore, I do not understand the statement that the time behaviour of the first spatial moment is linear. I think that this cannot be the case. I suggest that the authors also pay attention to the behaviour of the first spatial moment of the solute plume.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 895, 2006.