

## ***Interactive comment on* “Experimental study of fingered flow through initially dry sand” by F. Rezanezhad et al.**

**G. de Rooij (Editor)**

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Editorial comments.

The reviews for this paper are conflicting. The more critical reviewer (nr. 2), however, carefully supports hers/his criticisms. I agree with this reviewer in its critical appraisal. I too have some reservations about the novelty of the work, and its added value with respect to the existing literature on the subject. I suggest you considerable revisions to the present version. I will probably seek another review of the revised version.

The paper will certainly benefit if the literature review in the Introduction is expanded. For instance, the work of Selker et al (included in your reference list) is closely related

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to your work, but you present trends in soils moisture in fully developed, growing fingers that differ from theirs. Including the work in the Introduction and comparing your results to theirs could strengthen the paper. A more thorough literature review should also deal with the fact your Fig. 10 is only slightly different from Fig. 2 in Nieber, 1996 (Geoderma 70:2017-229). In the discussion, you pay attention to the water-air curvature at the finger tip, yet you have not addressed several recent papers on the dynamic water entry pressure head (see, for instance, Annaka and Hanayama, 2005 (Vadose Zone Journal 4:127-133)).

Improving the literature overview may help you to explicitly indicate the knowledge gaps you are trying to reduce/eliminate, thereby leading to a well-defined objective of the paper, which is entirely missing from the current Introduction. Having this clearly outlined, the purpose and value of your work is easier to assess. In the current version, the reader is left searching for the contribution of your work without much guidance. The paper addresses many aspects of fingered flow; some of which merely repeat existing literature. This is very apparent from the conclusions: in particular the latter two are well known from work at Cornell (Glass, Liu, Steenhuis, Parlange, among others) in the 1980s and 1990s. I urge you to limit or skip those sections that underpin these conclusions and focus on the new material. This will also lead to a more natural flow of thought throughout the paper. Reading the current text is a bumpy ride at times.

The reviewers and I like the improvement you made to the Light Transmission Technique. However, you can demonstrate this even better by following the more critical reviewer's suggestion and show that the sequence of drying and rewetting behind the finger tip cannot be demonstrated without deconvolution. If this is proven to be the case indeed, you also may have a (partial) explanation for the deviation between your results and those of Selker et al. In the case that you cannot provide an explanation for the observed wetting-drying-wetting sequence I would suggest you present it in the paper anyway, if you are positive that your observations are real, not some kind of artefact. You can then simply state that this behavior has not been observed before (it

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would help if you could explain why - perhaps because deconvolution was not applied before?), and that a theoretical explanation is lacking at this time.

You made quite an effort establishing a non-uniform layer in your setup, but you only seem to use the results to claim fingers disappear there and re-appear below. Isn't there anything more to say?

I agree with the more critical reviewer that the lack of matric potential measurements is a serious methodological limitation of the paper. It hampers the physical interpretation of the data. The paper would benefit greatly if you could repeat your experiments with matric potential sensors. Several types of rapid-response tensiometers have been reported in the literature, and my own experience suggests a design with small ceramics and sensitive pressure transducers should do the job. However, if this option is not realistically feasible, I am certainly willing to consider a revision without these additional data.

Detailed comments:

Please review the English: some sections are essentially flawless as far as this non-native speaker can see, but others have grammatical errors (e.g., p. 2599, l.12-14; p. 2600, l.7-9, l. 20; caption of Fig. 2).

Please give the dimensions when you explain variables (e.g., p.2597, l. 18-20).

The methodology section is rather brief here and there: the filling procedure is of interest, for instance; and how did you add the Brilliant Blue?

Eq. (3); Should z be x?

Eq. (6): This is not a convolution integral. Please explain.

p.2606, l. 3-15. I don't quite follow your line of argument; perhaps you should separately discuss the zones behind the finger tip with increasing, decreasing, and slowly increasing water contents to make it easier to follow the reasoning. As it is I am not

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convinced the rationale is correct, but that may be because I misunderstand. However, the fact that you have no matric potential data could be harmful to this section.

p.2607, l. 1: *Why the mean curvature, and not the curvature within individual pores?*

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 2595, 2006.

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