

## ***Interactive comment on “Preferential Flow Systems Amended with Biogeochemical Components: Imaging of a Two-Dimensional Study” by Ashley R. Pales et al.***

**Anonymous Referee #2**

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The manuscript presents experimental results of unstable flow patterns in sand boxes using a light transmission technique that allows identifying differences in flow patterns caused by the composition of the solution of the irrigation water. Plant exudates and soil solutions with different contact angles and surface tensions were tested and related to the effects on the flow finger development. Results demonstrate quite different patterns during the infiltration process. The test comparing various solution compositions is new and the experiments are highly sophisticated and carefully carried out, especially the combination with the light transmission method that allows determining local water contents is innovative. But the manuscript could be better structured, shortened, and more focussed on the analysis of these experiments.

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Claiming this manuscript to be on original research, my immediate impression was that authors should come to the main points more quickly; many references are not further used for the idea and results of this study. When continuing reading, the review part appears more and more excessive; in particular, the multiple referencing is changing the appearance towards a review article in which authors are trying to collect all relevant papers. Such an overview of the literature is quite nice and could be the basis for a separate manuscript. And despite the large number, referencing is still limited, for example, P5 L4: “. . . fronts has been studied primarily in two-dimensional tanks. . .”, recently also 3D patterns observed using geo-electrical imaging (e.g., Ganz et al., VZJ 2014, doi:10.2136/vzj2013.04.0074). Furthermore, the specific research hypotheses are not so explicitly stated in the introduction (more indirectly somehow within the review), so that the idea of the experiments and reasons for doing it as it was done remained unclear to me at the end of the introduction, where also the objectives were too general. Clear objectives statements are then found in the discussion and again in the conclusions. The methods are explained very detailed, for Tables and Figures, however, I found it very difficult to understand without having the abbreviations explained in headers and captions (e.g., Suwanee River Natural Organic Matter (SRNOM) acronym etc). One methodological problem that was probably discussed in earlier papers on the technique (?) was unclear to me. This is how to obtain repeatable uniformly compacted sand samples so that the packing effects are not influencing the effects of the solution composition. The stated accuracy of bulk density value (1.5043) with 4 digits is quite ambitious. Wetting and especially the partial wetting during the infiltration may change the arrangement of sand particles such that the pore structure may not be always constant. Although results of all three replicate infiltration experiments are provided, the question whether each of the finger pattern is characteristic for each solution and comparable for the replicate is not clear to me. I like the detailed explanation of results but data analysis seems still a bit limited. The hypotheses and how the results could be applied to soils remained unclear. Detailed comments 1. The abstract reads well, I only wondered if the conclusions here correspond to those in the

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conclusion chapter. 2. Page 7, Lines 15-25: not necessary and unclear 3. Discussion: Starts with the objectives, first paragraph contains hypothesis and should appear as part of the introduction. I was wondering how was equation 2 used? 4. Page 17, Lines 22-30: This is more or less an introduction to the closer topic and the results seem to confirm existing knowledge. 5. Page 19, Lines about 5-11: This is doubling introduction 6. Conclusions chapter gives more a summary of results than conclusions.

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