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Referee Comment on hess-2022-117

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Referee comment on "Subsurface flow paths in a chronosequence of calcareous soils: impact of soil age and rainfall intensities on preferential flow occurrence" by Anne Hartmann et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-117-RC2, 2022

Review by John Nimmo.

This paper provides an extensive and valuable set of field observations of the subsurface flow patterns generated by three different irrigation intensities over four members of a soil chronosequence. As in previous works using similar methods, this study offers quantitative analysis of unsaturated flow features that otherwise would be evaluated subjectively and without quantification.

The main value is in providing evidence to elucidate how factors including soil age, input intensity, vegetative cover, and others influence the depth and homogeneity of the distribution of the infiltrated water. In particular, a major issue is the distinction between preferential and homogeneous flow patterns, understanding of which has tremendous importance to water supply and water quality matters, as well as to agriculture and ecosystem health. The linkage to physical phenomena is primarily through classification into six categories based on a modified version of the scheme of Weiler and Flühler (2004). The paper provides useful documentation of soil developmental processes over 13500 years. Together with an earlier study of Hartmann et al. (2020a), it provides evidence of the differences resulting from calcareous-vs.-siliceous parent materials.

The data analysis is extremely thorough. A great variety of statistical methods are employed, perhaps more than necessary. I see little or no value in the P_{dye} analysis because the constraint of monotonicity is a serious shortcoming that could distort the interpretation of how water behaves in the profile.

Presentation of multifactor comparisons of many individual experiments is unavoidably complex, and is done here (figures 3-10) through an organization that requires the reader’s time and effort to understand and evaluate, but it does show the results in a way

that the effects of soil age, irrigation intensity, and spatial variability can be directly seen.

The main problem I find in the manuscript is confusion and inconsistency concerning the classification of types of subsurface flow. Much of this relates to the term "finger flow", for which I don't find a clear definition in this paper, and which seems to be used in different ways.

Some background from my own understanding: Three main categories of preferential flow are commonly used—funneled flow, which is directed into particular downward paths as a result of heterogeneities of the medium that provide faster flowpaths through the more conductive material; fingered flow, which is initiated at flow instabilities in the wetting front and sustained in downward preferential paths by the greatly enhanced hydraulic conductivity of the newly wetted material; and macropore flow, which proceeds through elongated continuous pores over significant distances within the medium.

I see these categories to be represented in the scheme of Weiler and Flühler (2004) (hereafter referred to as WF2004), which is designed specifically for use in interpreting dye-tracer results. Macropore flow needs matrix interaction to be visible, as acknowledged in the first three categories of WF2004. I see the term "matrix heterogeneous flow" as a synonym for funneled flow, and it is quite adequate in that usage. Instability-initiated fingered flow would be difficult or impossible to distinguish from matrix heterogeneous flow when the only evidence is from pictures of dye-tracer distribution. Thus it is appropriate to group both of these flow modes together as in the fourth WF2004 category, "Heterogeneous matrix flow and fingering". Absence of preferential flow is reasonably called homogeneous matrix flow in the fifth category. In the present study, the use of the WF2004 classification scheme is a suitable approach for evaluating dye-tracer patterns in terms of preferential flow. It is extended reasonably with the added sixth category to accommodate effects of large stones in the soil. The other modifications adopted here are poorly explained, and appear to deviate significantly from some widely understood general features of preferential flow, and from the evidence available from this study as I understand it. Below, I explain these issues further in relation to finger flow and macropore flow.

- Finger flow

Instability-initiated fingers are possible, though my expectation in such heterogeneous soil is that these are likely to be rapidly channeled into funneled flowpaths. Based on the images and other available information in the present study, I doubt that it is possible to discern whether instability-initiated fingering is an active process. In 18:24 (location noted as page:line) the term "finger flow" seems to mean any preferential flow that is identified by finger-like patterns of dye tracer, not limited to the downward-moving fingers of wetness generated at a wetting-front instability. The finger-like patterns in the dye could result from other modes of preferential flow. If what is meant is just that the patterns have a finger-like shape, without regard to specific process, "finger flow" would be better replaced by the general term "preferential flow". This issue occurs also in 1:14, 21:4,

22:3-6, 22:16, 23:21-22, 24:5-8, and 26:4-13. On the other hand, the specific mode of instability-initiated finger flow is the subject of 23:4-6 and 24:11—25:2. It also is strongly related to the effects of hydrophobicity in 22:6 – 23:9. These passages need clarification and consistency. Overall, finger flow must be explicitly defined and the term used consistently. If the paper actually does claim that instability-generated finger flow is detected in these experiments, there needs to be justification for how this can be determined.

- Macropore flow

There needs to be more discussion of the possible effects of macropores. The soils are likely rich in narrow macropores that result from growing and decaying roots (apparent in the images of both young and old soils), and other bioactive processes. If such macropores convey significant water that then has some degree of interaction with soil matrix material, they could create flow pattern features of the types observed. The statements in 18:15-22 are hard to understand and accept, where it is implied that finger flow can be distinguished from macropore flow, and stated that no macropore flow was found. If there are reasons to justify ruling out active macropore flow, they need to be carefully explained.

I cannot make sense of the statements in 7:30-33, which seem to imply that finger flow can be distinguished from macropore flow, but then contradict that in saying that no such differentiation is made. Then there is confusion in the statement that narrow finger flowpaths could somehow be misclassified as macropore flow with high (but not low or intermediate) interaction.

Section 3.2 (18:12-30) needs to be rewritten for consistency with other clarifications. The category "Macropore flow with high interaction/ Finger flow" is mentioned here and in Figure 10, but it is not mentioned in the definition of the categories on page 7 and is not in the scheme of WF2004.

Overall:

This paper is dense with useful information and provides insights into the development of preferential flow paths during landscape evolution and several other important facets of unsaturated flow in calcareous soils. It needs revision for consistency and adherence to evidence and general understanding of the different types of preferential flow paths. Because the basic experimental work and presentation of data are sound, I have classed these revisions as minor, though I see them as extremely important.

Minor comments:

6:16-18. Rewrite for clarity. Use of "below" in line 16 suggests that the excavation is downward to produce horizontal planes, but "vertical profiles" in 17 suggests otherwise. Does "below" mean "downslope of"? The operation suggests that a trench was first excavated off to the side of the plot to provide access for vertical profiling. More details on this would be helpful.

7:17. What is meant by "amount"? The number of flow paths?

7:28-29. Clarify—maybe make two sentences. Start with a clear description of the problem caused by rocks. Then the solution devised.

7:31-33 Why "misclassified"? What is unreasonable about "macropore flow with high interaction"?

7:34. Proportion in relation to what? PFF needs to be defined more clearly.

12:8-10. Split sentence into two, for clarity.

14:1. Replace "Whereas" with "In contrast," or similar expression.

17:4-5. It seems at best to be a very subtle effect for the middle portion of the profiles to be less significantly different. Maybe not worth mentioning.

21:30-31. Delete "influence the water transport and".

22:2-3. Word missing from sentence?

22:4-5. The point is not about the water transport in general but the *pattern* of the water transport that is affected. Insert "pattern" or some similar expression.

23:22. Word wrong or missing.

23:29. “Matric potential”, not “Matrix potential”.

23:32. The paragraph starting here, and also the next one, are all about the two older soils. This should be made clear to the reader in the first sentence at line 32. Consider rearranging discussion from this point through 25:13 in order to proceed in the logical order of young to old.

25:24-27. Confusing. Which of the plots were less affected by the direct application of water? Why is there consideration of the boundaries in this?