**Replies to Reviewer 1 (typeset in red)**

Reviewer 1: I would like to be clear from the outset. This article represents an important viewpoint for the readers of HESS, which has virtually not been represented in analogous publications, and the few publications that are relevant have not been cited in this forum. Further, I use the word major only to make sure that the revised manuscript gets re-reviewed. I think that the revisions needed may be substantial, but it is mainly due to a lack of discussion of two closely related works that appear to have been overlooked, which does not indicate that there are major defects in the manuscript that require a complete rewriting.

We thank the reviewer for the positive evaluation of the work, and thank the reviewer for directing us to these interesting reading materials.

One paper that needs to be cited is: Hunt, A. G., 2004, Continuum percolation theory for water retention and hydraulic conductivity of fractal soils: 2. Extension to non-equilibrium, Advances in Water Resources, 27, 245-257. I have provide this paper as an attachment. We added, in line (35), a mentioning of this paper "Hunt (2004) used percolation theory while addressing only the connection to the infinite cluster as given by the accessibility function to analyze non-equilibrium effects on hysteresis in fractal soils, which showed close agreement with experimental data (despite overlooking air trapping). These dynamic effect are beyond the scope of the present study that focuses on capillary equilibrium conditions."

In particular, Figure 2 of this paper looks quite similar to Figure 5 and, I think, for the same combination of ink-bottle and connectivity contributions to the hysteresis. However, the basis for Figure 2 of Hunt (2004) is somewhat simpler; instead of addressing all the effects of percolation at once, it only addressed the connection to the infinite cluster, as given by the accessibility function. Thus, there is no air trapping at the wet end of the curve in that paper. Other distinctions are: the 2004 paper addresses a real medium, not a simulation, and it thus does not have the capacity to resolve different effects. On the other hand, it does produce an exceptional agreement with experiment, effectively without adjustable parameters as to the shapes of the curves, only the characteristic pressures - and these were adjusted to make them agree with the experiment at the wet end. There is a lot of detailed material in there that the authors may or may not find useful; I think that the discussion of the time scales involved that might not allow maximum hysteresis (which was modeled using Eq. (8)).

In this context, we added in line (66): "The time scale in which dynamic effects can be ignored, and its dependency on the water content and lattice properties are discussed in Hunt (2004)."

The other paper that is worth mentioning is Hunt, A. G., and Gee, G. W., 2003, Wet-end deviations from scaling of the water retention characteristics of fractal porous media, Vadose Zone Journal, 2, 759-765. In Hunt and Gee (2003), it is pointed out that it may be a frequent occurrence (and was so in a set of around 50 Hanford site soils) that the wet end "air-entry pressure" is not the "bubbling pressure" for the same reason that there is hysteresis, namely air can enter beyond just the edges only when all the air-allowable pores actually percolate. By analyzing the data from this perspective, it was possible to show that the critical air fraction for percolation, at least in coarser media with small clay contents, was nearly the same as the water fraction for percolation at the dry end. Thus, the overshoot in drying at the wet end was governed by the same accessibility function with the same critical volume fraction as then inability to wet the medium at the dry end. It is possible that the scaling of invasion percolation and the use of the accessibility function in random percolation give very similar understanding - but it may be that the similarities in results are not indicative of the same understanding. This topic also needs further discussion.

We added this interesting observation in line 531: "This conclusion is in accordance with the results of Hunt and Gee (2003) in coarse media, with a probably more uniform pore size distribution (low CV), the saturation degree at percolation was found to be closer to the complementary disconnection saturation degree. The same can be observed here in Fig. 8a when comparing the $S_w$ of ISRT to $1 - S_w$ of DBIN at their percolation point ($\Delta$)."

Next, the authors use different sized domains to check for possible limitations of finite-sizes. But there is some potential problem here. As pointed out by the inventors of invasion percolation, Wilkinson and Willemsen, whom the authors cite, there are two percolation transitions in any transit from wet to dry in porous media. These transitions imply that the correlation length at these to moisture contents must diverge. Indeed, in a third paper, Steenhuis, T., Hunt, A. G., Parlange, J.-Y., and Ewing, R. P., 2005, Assessment of the application of percolation theory to water-repellent soils, Australian Journal of Soil Research, 43: 357-360, we show that this divergence makes it possible for as incorporation of as little as 5% water repellent particles into a sand will cause the entire medium to be water repellent. Thus, I recommend that the authors consider the use of finite-size scaling in their analyses, which would allow the finite-size effects to be eliminated completely without the necessity of estimating the magnitude of the effect.

The discussion regarding the Lattice size effects in the limitation section and the analysis in the appendix is meant to only validate that large enough lattices were used for analyzing the hysteresis mechanisms and not explore correlation lengths and fine-size effects. Our analysis shows that the effects of the lattice size on the hysteresis loops are indeed small. This is in accordance with others' results (cited in the text). Therefore, scaling our numerically obtained curves (using mostly empirical relations) will probably not change our conclusions.

The last important link between the present manuscript and Hunt (2004) is the latter's use of an analogy to the glass transition in viscous liquids to establish the relevance of non-equilibrium effects, which can be directly evaluated from the pressure saturation curve and the use of critical path analysis to find the hydraulic conductivity as a function of the tension. This equation is Eq. (24). The analogy to the cooling rate in a glass is the rate at which the tension h is increased.

This link is not obvious, in our manuscript we focus specifically on (limit the scope to) **quasi-static conditions**. Discussing the (limited) analogy of this case to the non-equilibrium glass transition or overshoot might confuse the readers. We are now in the preparation of a following study dealing with different dynamic hysteresis mechanism which is more suited to include this interesting discussion about the analogy between different rate limiting hysteretic processes.

I have one last important comment, but it casts no aspersion on the authors. When I submitted the manuscript to Advances in Water Resources in 2003, it went to Rink van Dijke as a referee, at that time in Scotland. He pointed out that Muhammad Sahimi with his co-author Heiba had already published something similar in 1982. I was unable to find the reference, but luckily, Robert P. (Toby) Ewing knew of it. It was in the petroleum engineering literature. Indeed, those authors had developed the language "allowable" and "accessible" to describe pores with radii that would accept the one type of fluid in question and those that were also connected to the infinite cluster. They applied this formalism to address hysteresis on Bethe lattices, but without producing specific formulas that could be compared with experiment. Given that 22 years later, the porous media community was unaware of their work and now, another 22 years later, it is unaware of what I contributed in 2004, it might be time for the porous media community to come to grips with the fact that there has been a successful application of percolation theory to predict hysteresis in a real system. That. however, does not take anything away from what the authors are doing, just as the work of Sahimi and Heiba did not invalidate my work as new. But it would be to the community's advantage not to have to wait another 22 years. All of these works support Irwin Fatt's argument from the 1950's that it is time to use network models, rather than bundles of capillary tube models. That is now 70 years ago.

Thank you for this insight and historical perspective. We hope that in the (revised) article we cited the main contributions to the topic and that in the current study we have "placed one more small brick in this building.".

I thank the authors for moving forward again on this critical problem and the editor Bo Guo for giving me the opportunity to review it.

We thank the reviewer for carefully reading our manuscript and for the insightful comments.