Dear editor and reviewers, please find below a point-by-point response to the raised comments. Comments from reviewers are highlighted in blue. The format of the colored version of the revised manuscript is as follows: Text that has been added, such as content moved from one section to another, is highlighted in blue, while text that has been removed is highlighted in red.

Before addressing the individual points, we would like to thank both reviewers for their valuable comments during the review process. We believe their comments greatly contributed to improving the manuscript.

Please note that we have updated the references for the simulation data uploaded on Zenodo. Specifically, we have added data for simulations without intrinsic permeability distribution and updated the README file accordingly.

Reviewer 3

I read this new version of the article and the Authors replies to my previous notes. The Authors satisfactorily detailed all the issues I raised and I think that the article can be now recommended for publication.

The section 4.4 and the new Figure A5 are useful because provide a clear discussion on which is the relative effect of the distribution of intrinsic permeability, hysteresis and semicontinuum model. It is interesting the fact that the formation of a saturated wetting front is reached, in the semicontinuum model, for q smaller than the conductvity at saturation Ks = 15 cm / min (Figure A2), possibly as a consequence of the saturation overshoot which is not represented by a classical Richards framework.

- We have added a brief comment in section *Dependence of flow on infiltration rate* regarding the formation of a saturated wetting front for infiltration lower than the saturated conductivity, as we find this detail also relevant.

Reviewer 2

In the following, line numbers refer to the manuscript with tracked changes.

The new edits introduce considerable numbers of citations after the Introduction (case in point: l. 200-201). This indicates that the Introduction is not well-aligned with the rest of the paper. This is illustrated for the same case by l. 201-202, where you argue why you did not use other hysteresis models. The Methods section is there to detail the methods you used, not to discuss alternatives, that should have been clarified before. There are other examples of this that can be easily fixed by repositioning some of the text in the appropriate section, but I cannot go through all of them. For instance, I believe that the final paragraph of 2.4 contains hardly any methodology at all, but discussed the literature. Therefore, most of it belongs in the Introduction. In the Results and Discussion, more citations occur because you compare your results to those of others. But if you introduce many new papers that were not treated in the Introduction, the Introduction probably can be improved. I did not check this.

- We agree that a considerable number of new citations have been added to Sect. 2.4, particularly in the first and last three paragraphs. To address this, we have moved these parts before the Methods into a new section, Sect. 2, titled *Retention curve and its sample size dependence.* The corresponding text has been slightly modified for clarity.
- We agree that a few citations have been introduced in the Results and Discussion sections. However, we believe that maintaining the flow of the text in these sections is essential for clarity and readability. Therefore, no changes have been made in these sections.

In a few sections of the discussion, you rely heavily on figures in the appendix. Should some elements of the appendix therefore perhaps be moved to the main text?

- We did not want to extend the manuscript too much, however, we agree with this comment. For better continuity, we have moved two figures A5 and A6 to the main text. Consequently, sections A4 and A5 have been removed.

The name ‘Rooij’ should be replaced by ‘de Rooij’ throughout.

- We have fixed this issue.

l. 24-25: it is either ‘the Richards equation’ or ‘Richards’ equation’.

- We have fixed this issue.

l. 45: Glass et al. (1989c) built a two-dimensional...

- We have clarified the sentence.

l. 120: insert space after the 'for all' operator

- We have fixed this issue.

l. 141, Table 1, possibly elsewhere: Pas → Pa

- Unit of dynamic viscosity is Pa s, not Pa. For clarity, we have added a small space between Pa and s and elsewhere.

l. 144: The gas pressure is not assumed to be zero, but rather is this pressure used as the reference with respect to which the liquid pressure Is expressed.

- We have clarified the sentence.

l. 260: ... 10^5 Pa, so we set K_{PS} to this value.

- We have clarified the sentence.

Table 1, possibly elsewhere: there need to be spaces between units: $m\,s^{-2}$ instead of ms^{-2}

- We have added a small space between units everywhere.

l. 266-267: the boundary condition essentially switches from a no-flow boundary condition (BC) when the matric potential at the bottom is negative to a prescribed flux BC when the pressure potential >= 0. In the first case, the BC necessarily affects the region above he boundary because water piles up until the matrix potential at the bottom reaches zero. But why is that a problem?

- Although in the reality, the bottom boundary condition can affect the region above this boundary, we have chosen the implementation given by Eq. (8). This allows us to isolate the behavior of the model from the influence of the bottom boundary condition. Simply put, we want to ensure that the behavior we observe in simulations comes from the model itself and is not caused by a boundary condition. We have clarified this in the corresponding paragraph.

l. 275: What is a small distribution? What are the properties of this distribution?

- We have clarified the corresponding paragraph in the manuscript. For more details, we also refer to [1], specifically pages 75–76.

l. 280 and elsewhere: add a space between a value and its unit(s).

- There actually is a small space between a value and its unit. We have also checked other values in the manuscript.

Fig. 9 and the paragraph above it: There seems to be a discrepancy between the cases shown in the graph and those discussed in the text, or am I overlooking something?

- Thank you for your notice, as it is relevant for the main text of the manuscript. There were two cases where we wrote $q_{\text{top}} = 0.0005\,\text{cm}\,\text{min}^{-1}$ instead of $q_{\text{top}} = 0.005\,\text{cm}\,\text{min}^{-1}$. We have fixed this issue. Moreover, we have slightly changed the paragraph above Fig. 9.

l. 453: The contact angle of air-water interface with the solid phase is also important. This angle is flux-dependent.

- In a static case, the contact angle of air-water interface is independent of the flux. Since the semi-continuum model uses the retention curve in an equilibrium state, we do not consider this factor relevant for our study. However, it is worth noting that other models directly incorporate the role of the contact angle into the pressure-saturation relationship, such as [2], which is referenced in the manuscript.

l. 494-495: repeats earlier text.

- We have removed the repetition of the earlier text.

References

[1] J. Kmec. *Analysis of the mathematical models for unsaturated porous media flow, Ph.D. thesis.* Palacký University in Olomouc, Czech Republic, 2021.

[2] N. Brindt and R. Wallach. The moving-boundary approach for modeling 2D gravity-driven stable and unstable flow in partially wettable soils. *Water Resour. Res.*, 56(5):e2019WR025772, 2020.