RC3

General comments:

This manuscript, presented as a mini review, asks whether Richards' equation (RE) remains adequate when flow is dynamic rather than static. The authors compile six experiment types and argue that RE, which ties water content to pressure head through a single retention curve, misses the lag between the two seen in the measurements, while the Dual-NE model tracks it across all six cases, which they put forward as a first.

The main strength is the breadth of examples: showing that the lag occurs in slow processes (capillary rise, evaporation, transpiration), not only in the fast experiments where it is already accepted. My comments concern positioning, presentation, and the strength of the mechanistic interpretation rather than the underlying observation, which I find convincing.

Specific comments:

1) The framing is broader than what the study tests. The title, "On the validity of Richards' equation under dynamic flow conditions," reads as a verdict on RE itself, but the manuscript examines one specific closure: a single-domain RE with a unique, non-hysteretic equilibrium retention curve. RE more broadly can include hysteretic closures, dual-permeability domains, dynamic-capillarity terms, and effective parameterizations, none of which are tested here. The title and framing could be narrowed to reflect this, for instance toward the limits of equilibrium retention closures under dynamic conditions.
2) The description of the local equilibrium assumption could be made more precise. The manuscript repeatedly frames RE as assuming "equilibrium between water content and pressure head" (e.g., abstract; lines 13, 130–131). Strictly, these are not two quantities that equilibrate with one another: pressure head is an energy state and water content a volume-averaged saturation, linked by a constitutive retention relation. The relevant assumption is that this constitutive relation holds instantaneously (a single-valued, rate-independent θ–h closure), rather than a literal mutual equilibration. Relatedly, the paper tends to merge hysteresis and dynamic non-equilibrium under one term "decoupling," although hysteresis is path-dependent and can occur at equilibrium, whereas DNE is rate-dependent. Distinguishing the two would sharpen the theoretical framing.
3) The "mini-review" label does not fit the body of the paper. Section 3 ("Evidence of dynamic non-equilibrium and simulations with DUAL-NE model") is built around the authors' experiments rather than a synthesis of the literature, and the abstract's first-of-its-kind claim (lines 24–26) is a research result, not a review outcome. The unpublished transpiration dataset (Table 1) points the same way, since reviews do not normally base their novelty on new primary data. The authors should clarify the category: either present this as the original modeling study it largely is, with expanded methods, or, if kept as a review, separate the new fits and unpublished experiment from the literature synthesis so reused and new material are distinguishable.
4) Figures are hard to assess as presented. In most figures, the scales and units are difficult to read, and where the RE curve, Dual-NE curve, and data overlap they cannot be told apart. Several

captions rely on color to convey the result, which does not work at this size. The figures should be enlarged and rendered at higher resolution, with line styles (not color alone) distinguishing measurements from each model and fully labeled axes.

5) Section 3 describes the agreement only qualitatively ("describes well," "accurately represents," "qualitatively describe"), with no quantitative measures. This matters because Dual-NE gains its closer match through two extra parameters that RE lacks, and a more flexible model reproducing more behavior is expected; the authors should show that the added mechanism, not just the extra freedom, accounts for the improvement. Reporting the calibrated equilibrium hydraulic parameters with the fitted f_ne and τ and at least one goodness-of-fit measure for each experiment would address this. It would also let readers verify the claim around lines 222–223 that a single parameter set covers both the slow and fast continuous-outflow runs.
6) In the evaporation experiment, a possible confounding effect is acknowledged but not ruled out. The authors note (lines 280–283) that soil temperature rises from 15 to 20 °C when evaporation is interrupted, and state that the resulting pressure-head change is too small to explain the observed relaxation. However, no supporting estimate or calculation is given for this claim. Since temperature affects surface tension and tensiometer readings directly, and the interruption coincides with the temperature change, a brief quantitative argument (or a controlled isothermal test) is needed before the relaxation can be attributed to non-equilibrium rather than to a thermal artifact.
7) More generally, the slow-flow examples may admit explanations other than a single common non-equilibrium mechanism. The manuscript proposes (lines 315–321) that all six observations reflect the same process — fast flow through accessible pore paths followed by slower inter-pore equilibration within the REV. This is plausible but not demonstrated, and the slow experiments involve effects that could act independently: water repellency in the capillary-rise case (which the authors themselves enhance with hydrophobic particles), root-water-uptake dynamics and sensor response in the transpiration case, and local redistribution in evaporation. It would be more defensible to present the unifying mechanism as a hypothesis and to discuss, per experiment, which alternative processes have or have not been excluded.