The manuscript addresses an important topic: the limitations of the classical local-equilibrium formulation of Richards' equation for describing dynamic water flow in soils. This topic is relevant to soil hydrology and to the broader HESS readership.

The manuscript is presented as a mini-review, but it also makes strong modeling and originality claims. In particular, the authors argue that Richards' equation cannot describe six classes of observations and that the Dual-NE model can provide a unified description of these behaviors. These are potentially important claims, but they are not supported by sufficient theoretical clarification, methodological detail, quantitative evaluation, or transparent documentation of data and figure provenance. In my view, the manuscript would require substantial reconceptualization before it could be considered further.

Major comments

1. The main contribution and originality are not sufficiently clear.

The manuscript is described as a mini-review, but much of the paper is structured as a model-comparison study. The authors present simulations using Richards' equation and the Dual-NE model and use these comparisons to support broad conclusions about the validity of Richards' equation. However, it is not clear what is new in the present manuscript relative to previous work by the authors and others.

Several figures and descriptions appear to be reproduced from, or closely adapted from, previously published studies, particularly the authors' earlier work on Dual-NE modeling under various boundary conditions. The figure captions do not clearly state whether the data and model curves are newly generated, reproduced, adapted, or replotted from previous publications. This is a serious transparency issue. For each figure and panel, the authors should explicitly state the source of the data, whether the simulations are new or reproduced from earlier studies, and whether permission is required for reused material. The current manuscript should also clearly distinguish previously published demonstrations from genuinely new analyses.

This issue is particularly important because the manuscript claims that it provides the first demonstration of a non-equilibrium model across all six types of observations. If the new contribution is only the addition of capillary rise, evaporation, and transpiration examples to an already published Dual-NE framework, this should be stated much more clearly and the novelty should be framed accordingly.

2. The manuscript contains a fundamental conceptual ambiguity in its treatment of water content, pressure head, hysteresis, and non-equilibrium.

A central problem is the repeated statement that Richards' equation assumes "equilibration between water content and pressure head." This wording is physically imprecise. Water content and pressure head are not two physical quantities that equilibrate with each other. Pressure head reflects the energy state of water, whereas water content is a volume-averaged measure of saturation. The water-retention curve is a constitutive relationship that maps pore-scale fluid configurations to averaged variables under specified equilibrium or quasi-equilibrium conditions.

Therefore, the local-equilibrium assumption should be described more carefully as the assumption that the capillary pressure-saturation state can be represented by an appropriate equilibrium constitutive relation, possibly including hysteretic scanning curves. It should not be described as a literal equilibration between theta and h.

The manuscript also does not adequately distinguish hysteresis from dynamic non-equilibrium. Hysteresis can occur under equilibrium or metastable conditions and reflects path-dependent pore occupancy, contact-angle hysteresis, ink-bottle effects, and related mechanisms. Dynamic non-equilibrium, by contrast, refers to rate-dependent or transient deviations from an equilibrium or quasi-equilibrium capillary pressure-saturation relation. These are related but not interchangeable phenomena.

The current manuscript tends to collapse these mechanisms into a general "decoupling" between water content and pressure head, which weakens the theoretical basis of the paper.

3. The title and framing overstate what the manuscript actually demonstrates.

The title, "On the validity of Richards' equation under dynamic flow conditions," is too broad. The manuscript does not evaluate the validity of Richards' equation in a general sense. Rather, it evaluates the limitations of a specific classical implementation: a single-domain Richards equation closed with a unique equilibrium water-retention curve and a standard hydraulic conductivity function.

A failure of this restricted formulation does not necessarily imply failure of Richards' equation more broadly. Richards-type models can include hysteresis, dual-permeability structure, effective hydraulic properties, dynamic capillary pressure closures, root water uptake formulations, or scale-dependent parameterizations. The manuscript should therefore avoid broad statements that Richards' equation is invalid and instead specify the exact formulation and assumptions being tested.

The title should be revised to reflect the actual focus. For example, the manuscript appears to be more about the validity of equilibrium water-retention closures under dynamic or rate-dependent conditions than about Richards' equation as a whole.

4. The comparison between Richards' equation and Dual-NE is not sufficiently rigorous.

The manuscript repeatedly states that Richards' equation cannot describe the observations, whereas Dual-NE can. However, the comparison is mostly qualitative and figure-based. The authors do not provide objective goodness-of-fit metrics, parameter uncertainty, calibration procedures, objective functions, or validation tests.

This is especially problematic because Dual-NE introduces two additional fitted parameters, f_ne and tau. A model with additional degrees of freedom can naturally reproduce more behavior than a simpler model. The authors need to demonstrate that the improvement is not merely due to added flexibility. At a minimum, the manuscript should report fitted hydraulic and non-equilibrium parameters, calibration targets, objective functions, parameter uncertainty, identifiability, and whether parameters are transferable across experiments or must be independently adjusted for each case.

The paper should also compare Dual-NE not only against the most restrictive form of Richards' equation, but against reasonable alternatives, or at least discuss why such alternatives are not considered.

5. The Methods section is far too limited for the claims being made.

The Methods section provides only a brief description of Richards' equation, the Dual-NE model, and a table of experimental datasets. This is not sufficient for a manuscript making model-comparison claims across six experiment types.

For each experiment, the authors should provide the source of the data; whether the data were previously published or newly collected; initial and boundary conditions; soil hydraulic parameters used in Richards' equation; whether hydraulic parameters were fixed, re-estimated, or transferred from previous studies; fitted values of f_ne and tau; calibration procedure and objective function; numerical implementation details; goodness-of-fit metrics; uncertainty or sensitivity analysis; and whether the same parameter set can describe multiple experiments on the same material.

Without this information, the reader cannot evaluate whether the model comparison is robust or reproducible.

6. The manuscript is unclear about whether it is a review article or an original research article.

The paper calls itself a mini-review, but it includes simulations and at least one unpublished dataset. Table 1 lists the transpiration experiment as unpublished data. If the manuscript includes unpublished experimental data, it should provide a full experimental description, including plant species, soil type, column geometry, root distribution, transpiration control, tensiometer locations, sensor calibration, temperature conditions, and uncertainty.

If the paper is intended to be a review, unpublished data should either be removed or clearly separated from the review synthesis. If the paper is intended to be an original model-comparison study, the methods and results must be expanded substantially.

7. The scale argument is not adequately supported.

The manuscript repeatedly refers to applications of Richards' equation from centimeter to kilometer scales and suggests implications for field-scale or large-scale hydrologic modeling. However, the actual evidence presented is based on laboratory column experiments. The manuscript does not perform upscaling, derive scale-dependent parameters, or demonstrate how laboratory dynamic non-equilibrium affects field-scale simulations.

The scale discussion should therefore be substantially revised. If scale translation is not a focus of the study, the authors should avoid making broad claims about scales. If they want to retain this framing, they need to discuss the upscaling problem much more carefully, including how laboratory-observed dynamic effects propagate, or fail to propagate, to field-scale effective hydraulic functions.

8. The physical mechanisms remain speculative.

The manuscript acknowledges that different mechanisms may be responsible for the observed non-equilibrium behavior, including water entrapment, air-entry effects, dynamic contact angle, wettability, heterogeneity, and scale averaging. However, it later suggests that all six observations can be explained by a general mechanism involving fast flow through accessible pore paths followed by slower equilibration between pores of different sizes.

This interpretation may be plausible in some cases, but it is not demonstrated. The mechanisms responsible for multistep outflow, multistep flux, capillary rise, evaporation, and transpiration may differ substantially. For example, water repellency, thermal effects, root water uptake, sensor response, and local redistribution could all influence some of the slow-flow examples. The authors should present the proposed mechanism as a hypothesis rather than a conclusion and should more carefully discuss alternative explanations for each experiment type.

9. Important model details are unclear.

Several technical aspects of the Dual-NE formulation need clarification. For example, the manuscript should explain how theta*_eq is defined and used, how K(theta) is evaluated in the Dual-NE model, and whether conductivity depends on equilibrium water content, non-equilibrium water content, total water content, pressure head, or another effective saturation. This is not a minor detail because conductivity controls the flux response and strongly affects the interpretation of the model.

The notation should also be made consistent. The manuscript uses theta_ne, theta_neq, and theta*_eq in ways that are not always clear. The limiting cases of the model should be checked carefully, and all equation references should be corrected.

10. Data and code availability are insufficient.

The current Data and Code Availability statements say that the materials will be made available upon acceptance. This is weak for a manuscript whose central claim depends on reproducing model-data comparisons across multiple experiments. The data, scripts, parameter files, or at least digitized datasets and simulation input files should be made available during review. This is particularly important because some figures appear to be adapted from earlier publications and one dataset is unpublished.

Other comments

The manuscript contains numerous typographical, formatting, and reference problems. Examples include "Riuchards," unresolved "Error! Bookmark not defined" fields, duplicated references, inconsistent terminology, and incomplete or incorrect DOI formatting. These issues are frequent enough to affect the credibility of the manuscript.

The figures are also too small and difficult to evaluate. Since the argument relies heavily on visual model-data comparison, the figures should be enlarged and made more legible. Each caption should include the data source, whether the figure is reproduced or adapted, and the key fitted parameters used in the simulations.

The abstract should be revised to be more specific. It currently does not clearly explain what was done, what was newly analyzed, what was reviewed from previous literature, and what specific limitation of Richards' equation is being challenged.

The conclusion should also be toned down. The statement that Richards' equation "systematically fails" should be replaced by a more precise statement about the failure of a single-domain, non-hysteretic, local-equilibrium constitutive closure under the specific laboratory conditions considered.

Overall, the topic is important, but the manuscript is not yet sufficiently rigorous, original, or transparent for publication. The main issues are conceptual ambiguity, overstatement of the failure of Richards' equation, insufficient distinction between hysteresis and dynamic non-equilibrium, unclear originality and figure provenance, inadequate methods, lack of quantitative model evaluation, and weak reproducibility. A substantially redesigned manuscript would be needed before the work could be reconsidered.