Reviewer #1

This is a well-written paper (albeit using ChatGPT) that addresses important deficiencies of the Richards' equation, which is widely used for soil water simulations at scales from global to local. The paper is well referenced and written with clarity. It does a good job in explaining some of the issues and highlighting the fact that discrepancies in behaviour arise in slow as well as fast flow processes.

However, the paper does not consider several other common complications in the use of the Richards' equation. One is the effects of phase change, e.g. freeze-thaw processes; a second is the effect of geochemical interactions between the solid soil and the liquid solutes. A third is the case of fracture flow with solid phase interactions. These effects are often neglected in Richards' equation applications.

In my opinion, the paper makes a useful contribution to the discussion of soil water flow processes and their modelling, but I suggest the authors clarify the scope of the comparison and spend a little space to note the issues that have not been considered, and which also need to be addressed.

Points of detail:

Line 190 mentions increasing or decreasing pressure head, but line 198 only mentions water leaving the soil column. Clarification is needed. Maybe edit line 192, replacing change by decrease.

The figures are so small that it is very difficult to distinguish the symbols. In fig 2b I can't see a green line, which is referred to in the caption or the symbols, and what is the dotted blue line? Fig 5 green dashed line is undefined. Fig 6c is confusing. Why is the equilibrium retention line shown in red, which denotes the non-equilibrium model?

l353 apparent reference error

Thank you very much for your positive evaluation. We will address all your comments and redo all the figures as requested. We wish to keep the focus on variably saturated water flow and do not intend to broaden the scope by including chemical transport, as requested ("geochemical interactions" and "solid-phase interactions"). Fracture flow is a special case of preferential flow, and we will highlight this fact in the revised manuscript. Since Reviewers 2 and 3 raised overlapping concerns, please see our replies to their comments for the full set of intended changes.

Reviewer #2

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.

Thank you very much for the thorough review and the numerous comments. We agree with many of them and will address them accordingly. Where we disagree, we explain our reasoning below

.**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.

Yes, we largely agree with this comment. The paper was intended as a mini-review because we believe the insufficiency of the Richards equation to describe these experiments is already well known and discussed in other papers (at least for the fast experiments). However, we wanted to synthesize this across all the observation types we identified.

We believe that mini-review/synthesis papers like this one are very useful for the community, as they bring together results from different studies — which runs somewhat counter to the current trend of highly focused, isolated modeling and experimental work.

Regarding the transparency of our data, we have included Table 1, where we provide references indicating where the data have been published before. We never claimed that the data (except the transpiration experiments) are a unique contribution.

To be honest, we ourselves struggled to identify the right format for this paper. We believe our results are strong and that we do not need to invest additional effort in re-demonstrating that the Richards equation, as described in our paper, cannot describe non-equilibrium water flow in monotonic (non-hysteretic) experimental observations. On the contrary, after receiving two very similar reviews, we think that including the description of all the experimental data together with the Dual-NE approach and its model application to all datasets may have added too many new elements to this mini-review paper, leading to a loss of focus.

Taking your comments into account, along with the comments from the other reviewers, we would like to revise our manuscript as follows:

1. We will keep the mini-review format, but we will remove (i) the modeling results obtained with the Dual-NE model, (ii) the discussion of which model can describe the observations, and (iii) any discussion of the Dual-NE model. In this way, the focus will be on the broad evidence that Richards' equation cannot describe simple monotonic, non-hysteretic observations like those provided in this paper.
2. We will focus on these systematic discrepancies and the potential challenges of using the Richards equation in water flow simulations.

3. We will clearly distinguish, throughout the text and in each figure caption, which results are newly generated for this manuscript and which are reproduced or adapted from our previous publications, including the specific source for each.

4. For the simulations using the Richards equation only, we will add all the details in the supplementary materials. This also applies to any newly presented data, such as the data from the transpiration experiments, as well as to reproduction and permission-to-reproduce information.

5. We will redo all figures.

In this way, we believe that our manuscript will serve its initial goal of summarizing existing observations that cannot be described by the Richards equation, as outlined in our manuscript. We hope that the editors and reviewers recognize the importance of a mini-review paper on this topic.

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.

We thank the reviewer for this constructive comment and agree with the concern raised. Rereading our paper, we realize that our terminology was mainly focused on the assumptions of the Richards equation, rather than on a formal definition of the local equilibrium assumption. We will revise the manuscript as follows:

1. Reformulate the description of the local-equilibrium assumption to avoid implying a direct equilibration between θ and h, framing it instead in terms of the underlying constitutive relation.

2. Clarify the role of the water-retention curve as a constitutive relationship.

3. Revise the discussion to clearly distinguish hysteresis from dynamic non-equilibrium, rather than grouping them under a general "decoupling."

4. Adjust terminology throughout the manuscript so that each mechanism is identified specifically where relevant.

We would also like to note, however, that part of the ambiguity in the original text stems from the fact that, experimentally, scanning curves are typically measured while water is actively moving through the sample. Under these conditions, hysteresis and dynamic non-equilibrium effects occur simultaneously and are difficult to disentangle from one another, since both manifest as deviations from the equilibrium retention curve. We will clarify this point in the revised manuscript to explain why the two mechanisms are sometimes conflated, while still maintaining the conceptual distinction between them.

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.

We thank the reviewer for this interesting comment, but we disagree with the assessment. The classic Richards equation is a single-domain model, and we define it correctly in Equation (1). The consideration of hysteresis, the use of effective properties, the inclusion of root water uptake via a sink term, and scale-dependent parameterizations do not change the equation, and it remains the RE. The RE must be distinguished from "Richards-type models," such as dual-permeability models (e.g., Gerke and van Genuchten, Water Resour. Res., 1993), which represent an extension (non-equilibrium-type models). Given this broader context, the title of the manuscript is not an overstatement.

We would also like to clarify a related point relevant to this discussion. Our experimental data are only for monotonic drainage or wetting conditions; hysteresis loops are not presented. Therefore, hysteresis associated with wetting-drying scanning curves and path-dependent pore occupancy is not encountered in our experimental data and analysis. This means that the deviations we observe between measured and predicted states cannot be attributed to hysteresis and are more directly interpretable as evidence of dynamic non-equilibrium effects. We will make this point explicit in the revised manuscript to avoid any ambiguity in this respect.

Action: In the revised manuscript, we will highlight the fact that our data can never be described by a hysteresis model incorporated into the Richards equation, as described in our paper.

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.

We thank the reviewer for this comment, but we somewhat disagree. Since we have decided to keep the mini-review character of our paper and will completely remove the model simulations with the Dual-NE model, we will not go into detail here. Very briefly, the inclusion of goodness-of-fit measures will not contribute much to the story but will instead distract readers from the fundamental evidence. For each experiment, we have explained in detail why the measurements contradict the assumptions of the (monotonic, non-hysteretic) RE. After removing the Dual-NE fits from the figures, the focus on this crucial aspect will be strengthened.

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.

We agree that the Methods section would have required substantial expansion to support the model-comparison (Richards vs. Dual-NE) claims made in the manuscript. However, since we have decided to remove the presentation of the Dual-NE model in a possible revised version, we will make the following changes:

- the source of the data, and whether it was newly collected or previously published (with citation);
- initial and boundary conditions;
- the soil hydraulic parameters used in Richards' equation, and whether these were fixed a priori, re-estimated, or transferred from previous studies.

Our intention is to present a summary table in the main text (data source, calibration methodology), with full procedural detail — numerical implementation, complete parameter tables — in the

Supplementary Material. This preserves a main text focused on the paper's central argument while giving readers full access to the information needed to evaluate reproducibility.

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.

We acknowledge that the manuscript's format sits between a conventional review and an original research article, and we agree that this needs to be stated explicitly rather than left for the reader to infer.

The manuscript's purpose is to synthesize evidence for dynamic non-equilibrium (DNE) across a range of boundary conditions (restricted to monotonic, non-hysteretic processes), in order to show that Richards' equation cannot capture these observations. In the revised manuscript, we will remove the results obtained with the Dual-NE model. In this way, we believe that the mini-review format is the right format and that it serves our original goal.

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.

We think part of this comment stems from a lack of clarity in how the manuscript frames scale, and we agree that this needs to be tightened, though we respectfully disagree with the underlying suggestion that the manuscript claims or requires upscaling.

The reference to Richards' equation being applied from the centimeter to the kilometer scale, in the Introduction, is intended solely to illustrate the widespread use of the equation across the discipline, not to propose or imply any scale-translation exercise within this study. We agree that applying Richards' equation directly at the kilometer scale is conceptually problematic, as has been shown in prior theoretical work (Vogel and Ippisch; Dani Or; and others), and this is not a position we hold or advance.

The scale-related implication we do wish to retain, and consider a relevant outcome of this work, is different in nature: we argue that sub-grid-scale processes, such as air entrapment and fine-scale heterogeneity, affect the effective soil hydraulic properties (SHPs) that would be assigned at the grid scale in water-flow modeling studies. This is not an upscaling procedure, nor a claim about how laboratory-observed dynamic effects propagate to field-scale effective functions; rather, it is a statement about the potential errors involved in measuring SHPs under dynamic conditions with fast-changing boundary conditions and then applying those curves for field-scale simulations and above. We also note that pedotransfer functions (PTFs), typically derived from small-scale (often laboratory) datasets, are commonly used to inform SHPs at larger spatial scales, which provides a direct and immediate channel through which our findings are relevant beyond the laboratory scale.

Since we are going to introduce major changes to the manuscript (removal of the Dual-NE results), we will revise the manuscript to state this distinction explicitly: we do not perform, and are not proposing, upscaling of dynamic non-equilibrium effects to field-scale simulations; rather, we highlight that sub-grid processes affecting SHPs have implications for how such properties are parameterized at larger scales, including through PTFs. We will remove or rephrase any language that could be read as implying a scale-translation analysis within this study.

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.

Again, we somewhat disagree with the comment — we presented an effective description, or a hypothesis, if you prefer. However, since we are proposing to remove all the Dual-NE results in the revised manuscript, this part will be changed significantly, and a large part of it will be removed. The physical mechanisms for the observed effects remain unknown in most current applications; it is the reviewer's choice to replace the word "unknown" with "speculative." We present the evidence, nothing more. The criticism that we suggest a "general mechanism" is indeed justified. We will rephrase the relevant paragraph and will not attribute the observed effects to specific causes.

Our position is that the exact pore-scale mechanism(s) responsible for the observed non-equilibrium behavior are not the central claim of this work, and we did not intend to present the fast-flow/slow-equilibration picture as a demonstrated, unified mechanism. We are aware, and the manuscript already acknowledges, that the literature attributes non-equilibrium behavior to a range of distinct and potentially co-occurring processes, including water entrapment, air-entry effects, dynamic contact angle, wettability, heterogeneity, and scale averaging, and that the relative importance of these processes likely differs across experiment types and scales. For example, water repellency, thermal effects, root water uptake, sensor response, and local redistribution may all contribute to the slow-flow examples, in ways that differ from mechanism to mechanism.

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.

Thank you, very sharp reading. Since we will remove all the details about the Dual-NE, a detailed reply to the justified comment is no longer required.

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.

We do not think it is appropriate to make our data and code publicly available before the manuscript has been accepted. At this stage, we cannot be certain that the manuscript will be accepted in its revised form, and public release of the data in advance of acceptance would mean relinquishing control over previously unpublished material (including the transpiration dataset) without the corresponding publication being guaranteed. We believe this is a reasonable and common position for authors to take, and standard practice at most journals is to require open data and code upon acceptance, not during review. We are happy to update the Data and Code Availability statements to commit to full public data availability upon acceptance.

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 reproduce

We thank the reviewer for these additional observations and will address each. We will correct all typographical errors and resolve the unresolved bookmark references, both of which should point to Ross and Smettem (2000). We will remove the duplicated reference entries and correct the DOI formatting throughout. Figures will be enlarged, and each caption will state the data source and whether the figure is new, reproduced, or adapted, as described in our response to Comment 1. The abstract will be revised to specify explicitly which observations and analyses are newly presented and which synthesize previously published results. Finally, we will revise the conclusion (and the corresponding statement in the abstract) and replace "systematically fails" with a more precise statement about the RE's failure to adequately match the measurements. However, we insist that the RE, in its classic form, is a single-domain model assuming local equilibrium between macroscopic water content and matric potential (see above).

Reviewer #3

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.

Thank you very much for your review. Your review aligns well with the comments from Reviewer 2. We will respond to each of your points below and, at the end, provide a summary of the intended changes. Please refer also to our reply to reviewers 1&2 comments

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.

Thank you. The same point was raised by Reviewer 2. The classic Richards equation is a single-domain model, and we define it correctly in Equation (1). The consideration of hysteresis, the use of effective properties, the inclusion of root water uptake via a sink term, and scale-dependent parameterizations do not change the equation, and it remains the RE. The RE must be distinguished from "Richards-type models," such as dual-permeability models (e.g., Gerke and van Genuchten, Water Resour. Res., 1993), which represent an extension (non-equilibrium-type models). Given this broader context, the title of the manuscript is not an overstatement.

In general, we are open to changing the title, but not for the part referring to "Richards equation." Perhaps we could specify it as follows: "On the Validity of Richards' Equation under Dynamic, Monotonic, and Non-Hysteretic Flow Conditions."

Perhaps this is better for Reviewers 1 and 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.

Thank you. This is almost identical to a comment from Reviewer 2. We will address both in the revised manuscript, as summarized at the end of our reply to Reviewer 2 and to your comments further below.

2) 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.

As we replied to Reviewer 2, we would like to keep the character of a mini-review paper, if the editor agrees to that. The reason is that we believe the intended message is as follows: it is well known that non-equilibrium effects appear under fast drainage and wetting experiments, but it is less well known (although observations exist from the 1960s–1970s) that this also happens in slow experiments. These data cannot be described by the Richards equation, and this will be the end point of the revised manuscript, as explained to Reviewer 2. With these changes, we believe that the mini-review character of the paper is now justified.

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.

Thank you. Again, a comment similar to one from Reviewer 2. We will redo the figures and make sure to describe everything clearly.

3) 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.

Thank you. As we replied to Reviewer 2, we will now provide those details in a table format in the main text, with all other details in the SI.

4) 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.

Thank you. We double-checked this, and the temperature effect on surface tension cannot fully explain these results. We had this in a previous version but removed it for the sake of brevity. We will add those details back in.

5) 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.

Yes, you are right, thank you for the constructive comment. This is a macroscopic observation, and in the revised manuscript we will present it as such, not as a genuine explanation.

To summarize, we will address all the major comments from Reviewers 1–3. Since we have to agree on what to change, we summarize the planned revisions below:

- Remove the presentation of Dual-NE simulation results, as well as any comparison of the Richards equation with Dual-NE.
- Reformulate the local equilibrium assumption, as Reviewers 1 and 2 suggest.
- Explicitly separate hysteresis from DNE.
- Revise the title to reflect a single-domain, non-hysteretic, monotonic Richards equation.
- State explicitly that no upscaling is performed, while retaining the implication of DNE for larger-scale simulations.
- Present the fast-flow/slow-equilibration macroscopic mechanism as a hypothesis to be tested in future experiments, not as an explanation.
- Expand the Methods section: experiments, modeling, and inverse modeling.
- Provide a full experimental description for the transpiration dataset in the Supplementary Material, flagged clearly as new/unpublished in the text, tables, and figure captions.
- Copyediting, figure sourcing, and abstract revision.