

Dear reviewer,

Thank you very much for your constructive comments. We have provided our point-by-point responses below. For clarity, your comments are shown in black, bold, and italic, while our responses and revisions are shown in light and dark blue texts, respectively.

Regards,

Han Fu (on behalf of all authors)

1. MOIST description: The paper could use a little more emphasis and effort in describing the key properties of RWU and drainage quantification methodology that make it robust to the experimental constraints composed here. See specific comments.

Thank you for this important suggestion. We have expanded the description of MOIST in the Methods section (2.2.1), with particular emphasis on how root water uptake (RWU), interlayer water fluxes, and drainage are quantified: “Specifically, soil water movement in MOIST is simulated by numerically solving the Richards equation, with vertical water fluxes calculated from soil hydraulic properties and hydraulic gradients using the Darcy-Buckingham law. For the forward simulations used here, the proportion of total RWU assigned to each depth interval is prescribed according to either the shallow- or deep-dominated RWU pattern. The prescribed proportions are then multiplied by the given transpiration rate to obtain the RWU flux for each depth interval. These layer-specific RWU fluxes are imposed as sink terms that remove water from the corresponding soil layers, and MOIST subsequently simulates the resulting evolution of soil water content and vertical water fluxes. Thus, unlike the SWSB approach evaluated here, the benchmark RWU profile is prescribed as an input to the forward simulation and is independent of finite changes in layer water storage and estimated interlayer water fluxes.”

2. More depiction of the soil profile dynamics for the model cases (I am pasting a “specific comment” here which captures the point): “Lines 436-437; “dry periods” (here and throughout): I would strongly advise giving more detail exactly how dry this scenario was (water content range; potential etc.--ideally in a figure) to increase transferability and make sure that “dry” is not equated with “minimal precipitation.”

Thank you for this important comment. We agree that the terms “dry” and “wet” used in the original manuscript could misleadingly imply prescribed or spatially uniform soil-moisture states. In fact, the two scenarios were defined by precipitation forcing (0 and 10 mm d⁻¹), while soil water content evolved dynamically during the simulations and also depended on the prescribed RWU profile. We have therefore replaced “dry” and “wet” throughout the manuscript with P₀ and P₁₀, respectively, and explicitly defined these terms when they first appear in the Methods. The revised text reads: “Initial soil water content profiles were prescribed, and subsequent soil water dynamics were simulated under two precipitation scenarios: a no-rainfall scenario (P = 0 mm d⁻¹, P₀) and a rainfall scenario (P = 10 mm d⁻¹, P₁₀).”

We have also added a supplementary figure (Figure S1 below) showing the evolution of the complete soil water content profile over the 7-day simulations for both shallow- and deep-dominated RWU. The figure shows that under P₀, pronounced drying developed in the upper soil profile, whereas deeper soil layers remained comparatively moist, with the exact drying pattern depending on the prescribed RWU distribution. This addition provides a more explicit description of the hydraulic conditions represented by each model scenario and avoids equating zero precipitation with a uniformly dry soil profile.

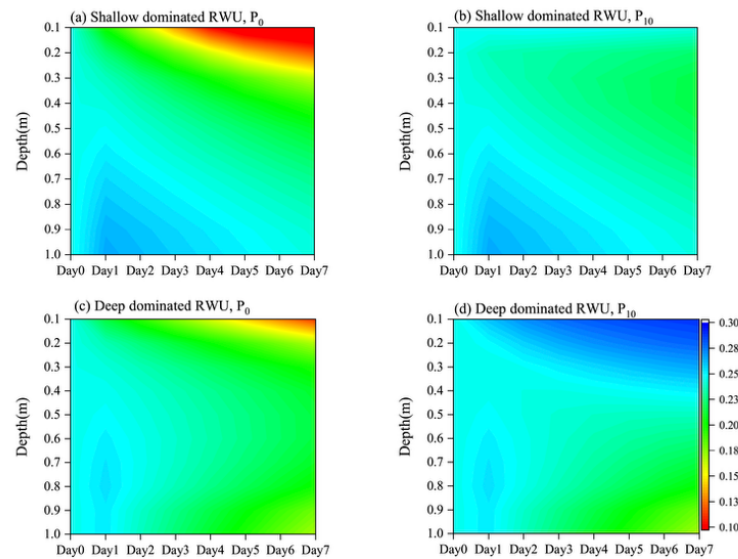


Figure S1. Temporal evolution of simulated soil water content profiles under different precipitation forcings and prescribed RWU patterns during the 7-day simulation period. Panels show (a) shallow-dominated RWU under no-rainfall forcing (P₀, P = 0 mm d⁻¹), (b) shallow-dominated RWU under rainfall forcing (P₁₀, P=10 mm d⁻¹), (c) deep-dominated RWU under P₀, and (d) deep-dominated RWU under P₁₀. All simulations started from the same initial soil water content profile, whereas subsequent soil water dynamics evolved in response to precipitation forcing and the prescribed RWU distribution.

3. -Line 55; "inversion": "inverse modeling" is probably a more universal term here.

Thank you for this comment. We have replaced “inversion” with “inverse modeling” throughout the revised manuscript.

4. "differencing operations": not clear what is meant

Thank you for pointing this out. We have revised the sentence to: “Moreover, SWSB-based inverse modeling of RWU relies on changes in soil water storage between consecutive observations and on interlayer water fluxes derived from soil hydraulic parameters via Darcy-Buckingham law.”

5. Lines 55-57: It's not clear exactly, as written, why these approaches amplify uncertainty-- particularly in comparison to other listed methods

Thank you for this comment. We have revised it to: “Moreover, SWSB-based inverse modelling of RWU relies on changes in soil water storage between consecutive observations and on interlayer water fluxes estimated using the Darcy-Buckingham law. Measurement errors propagate into estimates of storage change, while uncertainties in hydraulic gradients and soil hydraulic parameters affect the estimation of interlayer water fluxes. Because RWU is obtained as a residual of these water-balance components, these uncertainties propagate directly into the estimated RWU, particularly when RWU is small relative to the other components of the water balance.”

6. Equation 8: Please list pressure and conductivity variables, explicitly

Thank you for pointing this out. In response to this comment and the related suggestions from Reviewer 1, we have carefully revised the equations and variable definitions in the Materials and Methods section. Specifically, the pressure-head and hydraulic-conductivity variables in Eq. (8) are now explicitly defined, together with their units.

7. Lines 126-127; small technical point:

1) you can certainly estimate fluxes using various lysimeters, etc. if carefully oriented
2) the authors just described a way to infer these under discrete, deterministic calculations (maybe just be extra be specific in the meaning of "numerical" in this case)-- solving diff eq. etc.

Thank you for this clarification. We agree that our original statement was too absolute. We have therefore revised the sentence to read: “By contrast, interlayer water fluxes are difficult to quantify directly under field conditions and are commonly estimated from soil hydraulic gradients and hydraulic conductivity using the Darcy-Buckingham equation.”

8 - Line 159: "... benchmark fluxes obtained directly from MOIST"

The authors are really comparing the applicability of the water balance trapezoidal method vs MOIST derived drainage and RWU fluxes. The water balance method is nicely articulated in prose, but the MOIST model is essentially buried in citations until now. The authors should put a much greater emphasis on explicit methodology used to produce these fluxes in MOIST and any apparent biases or advantages.

Thank you for this important comment. As also noted in our response to Comment 1, we have substantially expanded the description of the MOIST forward simulations in the revised Methods.

We have also clarified the rationale and limitation of using these simulations as benchmarks. “The prescribed layer-specific RWU fluxes therefore provide a known benchmark against which SWSB-reconstructed RWU can be evaluated, while the internally consistent interlayer and lower-boundary fluxes simulated by MOIST allow the effects of flux approximation on the SWSB reconstruction to be examined. Nevertheless, the simulated soil water dynamics and water fluxes remain model-generated quantities and should not be interpreted as error-free representations of field conditions.”

9. - Line 167: "MOIST-derived RWU profiles" or similar, not "true"

Thank you for this suggestion. We have replaced “true RWU profiles” with “benchmark RWU profiles” and have revised similar terminology throughout the manuscript accordingly.

10. - Line 204: Which observational data? Seems like the authors are providing observation error distributions, not observational forcing data per se.

Thank you for pointing out this ambiguity. Here, “observational data” referred specifically to the soil water content data required by the SWSB calculation and generally measured (observed) from field. In this numerical experiment, these data were generated by MOIST and then perturbed

according to prescribed measurement-error distributions to represent uncertainty in soil water content measurements. We have revised the text to avoid the ambiguous term “observational data” and to explicitly identify the variables to which uncertainty was introduced.

11. - Lines 226-228: citation, please

Thank you for pointing this out. We agree that the original wording could incorrectly imply that the EMD criterion was adopted from previous studies. Based on the benchmark simulations, an EMD of 0.01 m was adopted as a study-specific operational tolerance for successful RWU reconstruction. Reconstructions showing appreciable departures from the prescribed vertical RWU pattern, including substantial redistribution of uptake among soil layers and, in some cases, negative estimated uptake, were generally associated with EMD values exceeding 0.01 m (Figure 2). Accordingly, the success rate (SR) was defined as the proportion of realizations with EMD < 0.01 m. This criterion was used for comparisons among scenarios in this study and is not intended as a universal threshold for RWU reconstruction. We have revised the manuscript accordingly.

12. - Line 237: grammatical correction "...the trapezoidal rule assumptions of..."

Thank you for pointing this out. We have revised the sentence for grammatical clarity as follows: “This discrepancy arises because the trapezoidal rule assumes a linear variation in fluxes between the initial and final time points.”

13. - Figure 2: "1D vs 7D" is not easily inferred as one day vs 7 day timesteps from the figure alone. Please specify in caption; the right half of figure with faceted plots will need larger labels (very hard to see); "panels a and b... c and d ..." are not specified in figure;

Thank you for this helpful comment. We agree that the original Figure 2 did not sufficiently explain the “1D” and “7D” labels and that several labels in the faceted panels were too small. Reviewer 1 also raised concerns regarding the presentation and readability of Figure 2. Thus, we have comprehensively revised the figure. Specifically, we have clarified in the caption that “1D” and “7D” refer to the 1-day and 7-day SWSB integration intervals, respectively; enlarged the labels and other graphical elements in the faceted panels; and clearly added and identified the panel labels in both the figure and caption. The corresponding caption has also been revised to clearly describe each panel and the meaning of the integration intervals.

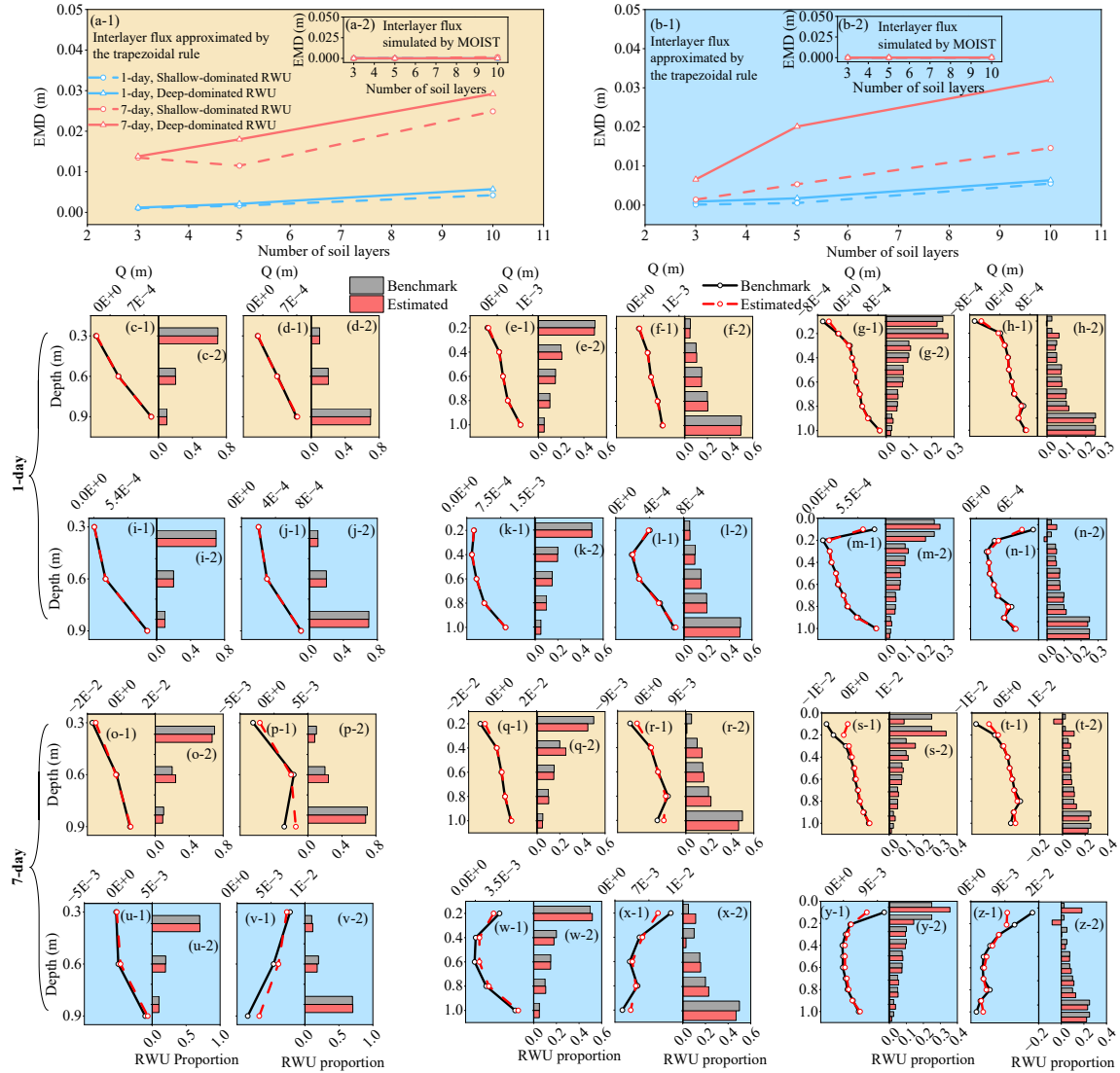


Figure 2. Effects of temporal integration interval, vertical discretization, and interlayer-flux approximation on SWSB-based RWU reconstruction. The upper panels show RWU reconstruction error, quantified by the Earth Mover's Distance (EMD), as a function of the number of soil layers under P_0 (0 mm d^{-1} , yellow background) and P_{10} (10 mm d^{-1} , blue background). Solid and dashed lines represent shallow- and deep-dominated prescribed RWU patterns, respectively, and blue and red lines represent 1-day and 7-day SWSB integration intervals, respectively. Insets show the corresponding EMD when interlayer water fluxes simulated by MOIST, rather than fluxes approximated using the trapezoidal rule, were used in the SWSB calculation. The lower panels compare the benchmark and reconstructed RWU profiles and the corresponding cumulative interlayer water fluxes for 3-, 5-, and 10-layer discretizations under the 1-day and 7-day integration intervals. Bars show the benchmark and reconstructed RWU proportions, while lines show the corresponding cumulative interlayer water fluxes.

14. -Lines 274-280; greater success in deeper layers etc.: How is this reconciled with the 7 d integration in deeper root systems showing worse performance based on EDM (Figure 2)?

Thank you for pointing out this apparent inconsistency. We agree that our original interpretation was too general. The higher SR of the deep-dominated RWU pattern occurred specifically under

the 1-day hydraulic-parameter uncertainty experiment (Case 1) and should not be interpreted as an inherent advantage of deep RWU reconstruction. Further examination showed that, under Case 1, the EMD distribution for the deep-dominated pattern was shifted toward lower values relative to that for the shallow-dominated pattern, indicating lower sensitivity of the reconstructed deep-dominated profiles to hydraulic-parameter perturbations under this particular configuration. This resulted in the higher SR observed for the deep-dominated cases.

Importantly, this difference largely disappeared once uncertainty in soil water content was introduced (Cases 2 and 3). The Sobol analysis showed that uncertainty in the initial and final soil water content profiles then became the dominant contributor to reconstruction error, whereas the relative contribution of hydraulic-parameter uncertainty decreased substantially. Because errors in soil water content directly affect the change in layer water storage used in the SWSB residual, this additional uncertainty affected both prescribed RWU patterns and largely overwhelmed the difference observed under hydraulic-parameter uncertainty alone.

The lack of a general advantage for the deep-dominated pattern is also evident in Figure 2. When the integration interval was extended to 7 days, reconstruction errors increased substantially, and the deep-dominated pattern did not consistently perform better. We have therefore removed the previous explanation attributing the higher SR to reduced influence of upper-boundary fluxes. The revised text now clarifies that the relative performance of shallow- and deep-dominated RWU reconstruction depends on the source of uncertainty and the temporal integration interval.

15. - Lines 338-342: interesting (and relevant)!

Thank you for this encouraging comment.

16. - Lines 436-437; "dry periods" (here and throughout): I would strongly advise giving more detail exactly how dry this scenario was (water content range; potential etc.--ideally in a figure) to increase transferability and make sure that "dry" is not equated with "minimal precipitation."

Thank you for this comment. Please see our response to Comment 2, where we have responded in detail.