Dear Reviewer#1,

Thank you so much for the feedback. These comments improved the quality of our manuscript. Below we provide detailed point-by-point responses to all comments. Reviewer comments are highlighted in boldface and italic. Our responses are in normal texts.

Sincerely,

Guofeng Zhu (on behalf of all authors)

General comments:

A recurring problem is that key quantities are either (1) reported with unclear or implausible units, (2) derived using equations that are dimensionally inconsistent, or (3) asserted without showing the intermediate calculations and assumptions. In an open, permanently archived discussion format, I would strongly encourage the authors to make the analysis auditable: provide the raw data (or a repository link), define the sampling units and timeline, and show the calculations that lead to the main percentages and process interpretations.

We extend our sincere gratitude to the reviewers for their rigorous critique regarding the quantitative analysis rigor of this study. These comments directly address the most critical weaknesses in the current manuscript and are essential for ensuring the scientific validity and reproducibility of the research. We fully accept the reviewers' suggestions and plan to implement the following in-depth improvements in the revised manuscript:

Enhance the auditability of analysis:

(1) Data Sharing: We will provide a Data Availability Statement in the revised manuscript, uploading the original isotopic data of precipitation, irrigation water, soil water at different depths, and groundwater, along with corresponding soil physical parameters (such as bulk density and particle composition), to public databases (e.g., Zenodo or GitHub), and include access links.

(2) Clarify sampling and timeline: We will add a detailed timeline (Table/Figure) to clearly record the specific date, duration, and irrigation depth of each irrigation event, as well as the corresponding precise date of soil sampling, to eliminate confusion regarding the interpretation of time scales such as the "9-day recovery period".

Unit and dimension consistency:

(1) We acknowledge that the values (85-91%) in Table 1 contain significant labeling errors. Upon verification, these values represent relative water content rather than volumetric water content. In the revised draft, we will uniformly adopt standard volumetric water content (cm3/cm3) or gravimetric water content (g/g) and recheck

all relevant storage calculations.

(2) We will re-examine and revise formulas (4) and (5) to ensure all conversion factors (e.g., "×10" or "×100%") are dimensionally consistent, and supplement the previously missing key parameter—the measured bulk density of soil.

Derivation of the core conclusion of transparency:

(1) For the two core conclusions of '32% evaporation loss' and '5% deep seepage', we will provide detailed derivation steps following Section 3.4.

(2) We will provide a complete water balance ledger, listing input terms (irrigation, precipitation), output terms (evaporation, infiltration, discharge), and storage changes (ΔS), with detailed explanations on how to isolate and estimate these terms using the isotope mass conservation equation (Eq. 6-8).

(3) We will clarify the physical meaning of Wi in formula (8) (whether it is a percentage or a millimeter value) and discuss its applicability and uncertainty across different time windows.

Through the above improvements, we aim to make the reading process of readers a verifiable "audit" process, so as to enhance the persuasiveness of the conclusion.

Specific comments:

1. Sampling design and timeline are not clearly described, and they conflict with several conclusions. The Methods state that soil sampling occurred "once before sprinkler irrigation and for five consecutive days afterwards" at 10 depth intervals (0–100 cm), with four parallels per layer (Section 2.2.1). Yet the Results emphasize seasonal "monthly variation" plots (Figs. 2, 5, 7) and repeatedly claim that soil moisture and isotopic characteristics return to pre-irrigation values after ~9 days. As written, these elements do not fit together. There needs to be a clear timeline table/figure listing (1) irrigation event dates, durations, and applied depths; (2) soil sampling dates relative to each event; (3) how the monthly plots were constructed (what dates go into each month; are these event-based composites?). Without this, "9 days" and several other time-based interpretations are not checkable.

We sincerely appreciate the reviewers' attention to the logical rigor of the experimental design. The current narrative indeed fails to clearly delineate the relationship between single-event observations and seasonal trend analysis, leading to a misinterpretation. We will thoroughly clarify this in the revised manuscript through the following measures:

(1) This study employed two observation scales: intensive sampling (1 day before irrigation and 5 consecutive days after) for specific irrigation events to capture rapid isotopic responses, and routine monitoring spanning the entire growing season (May to September). The conclusion regarding the 9-day recovery period was derived

from comprehensive observational data across multiple irrigation cycles, using linear extrapolation and subsequent comparisons with routine monitoring points.

(2) We will follow the reviewers' recommendations and add a "Panoramic Timeline of Experimental Observations" in Section 2.2 of the revised manuscript. The timeline will clearly indicate: 1) the exact dates, duration, and irrigation volumes of all events in 2021 and 2023; 2) the precise temporal positions of all soil sampling points relative to the irrigation events; 3) the distribution of meteorological events (e.g., precipitation).

(3) The "monthly variation" in Figures 2, 5, and 7 does not represent data from a single date but rather the average of all sampling points within the month (event-based composites). We will detail the specific sampling dates and sample size (N) included in each month in the chart legend to ensure data auditability.

(4) We will re-examine the methodology section to ensure readers can clearly distinguish between the 'single infiltration experiment' and the 'seasonal water dynamics analysis'.

2. Table 1 "Soil moisture (%)" values are implausible (or mislabeled), and this undermines the storage calculations. Table 1 reports soil moisture near 85–91% at essentially all depths (0–100 cm). For sandy loam/light loam mineral soils, that is not credible as gravimetric water content, and it is physically impossible as volumetric water content. This points to a unit/definition error (e.g., % of field capacity, or a decimal point shift).

It is important to note that soil water storage (Eq. 4–5; Fig. 5; Section 3.2) depends directly on these values. If the moisture values are not what the table label implies, the storage results cannot be interpreted.

The authors need to define moisture precisely (gravimetric vs volumetric vs relative), provide bulk density values by depth (measured but not reported), and ensure Table 1 aligns with the storage values in Fig. 5.

We sincerely appreciate the reviewers 'keen identification of this critical error in data annotation and physical definition. The values in Table 1 are indeed highly misleading, which not only impairs readers' assessment of soil physical conditions but also undermines the credibility of subsequent storage capacity calculations. In the revised manuscript, we will implement the following in-depth corrections:

（1）Correction of moisture content definitions and values: Verification confirms that the 'Soil moisture (%)' in Table 1 originally referred to Relative Water Content (RWC) rather than weight or volumetric moisture content, resulting in values approaching 90%. In the revised Table 1, we will uniformly adopt standard gravimetric (g/g) or volumetric (cm^3/cm^3) moisture content values, ensuring they align with the typical physical ranges for sandy loam and light loam soils.

（2）Completing bulk density data: The reviewer correctly pointed out that the water storage calculation in Equation 4 must rely on bulk density parameters. Although we measured the bulk density of each layer in the experiment, it was omitted in the first draft. We will add a column in the revised Table 1 to explicitly list the measured bulk density values for each depth interval from 0 to 100 cm.

（3）To ensure data consistency with formulas: We will re-examine the dimensional conversion in Formulas 4 and 5. To address potential dimensional confusion caused by the "×100%" factor in Formula 5, we will redefine variables to ensure accurate matching of parameter units (bulk density, moisture content, soil layer thickness) when calculating water storage capacity (S, mm). Based on the revised moisture content and bulk density, we will recalculate and update the water storage values in Figure 5, ensuring logical coherence and data consistency across all charts.

3. Equations (4)–(5) are dimensionally inconsistent as written; bulk density is missing; storage cannot be reproduced. Eq. (5) defines W with a "×100%" factor (i.e., W is a percent), but Eq. (4) uses S = R × W × H × 10 without dividing by 100. Either the "×100%" is wrong, or Eq. (4) is missing a conversion. In addition, R (bulk density) is required but never reported.

The authors would have to fix Eq. (4)–(5) so units are explicit and consistent, and report R (bulk density) by depth so storage values can be verified.

We fully endorse the reviewers' comments. The mathematical expression regarding soil water storage in the manuscript indeed contains inaccuracies, and the critical physical parameter R supporting the calculation was omitted. This will directly compromise the reproducibility of the research findings. We will make the following corrections in the revised manuscript:

(1) Revising formula dimensions and unit definitions: We will redefine variable units to ensure dimensional consistency. The weight moisture content (W) defined in Formula 5 will be converted to decimal form (i.e., removing "×100%"), or the corresponding conversion factor will be introduced in Formula 4. The revised moisture storage formula will clearly specify the units of each parameter: S (mm), R (g/cm^3), W (g/g), H (cm).

(2) Completing the bulk density profile data: Although we measured the bulk density of each soil layer during sampling, it was unfortunately not listed in the initial draft.

We will add a column titled 'Bulk Density (g/cm³)' in Table 1, detailing the measured average bulk density values for 10 soil depth intervals within the 0-100 cm range.

(3) Ensure the traceability of moisture storage calculations: We will recalculate the moisture storage values in Figure 5 and Figure 7 based on the revised formula and the newly added bulk density data. The revised version will ensure that readers can accurately reproduce all the storage values mentioned in the text using the original parameters from Table 1 and formula (4).

4. Table 2 contains internal statistical inconsistencies (at least one SD is mathematically impossible). Table 2 contains internal statistical inconsistencies (at least one SD is mathematically impossible). For irrigation water δ^2H, Table 2 reports Max = −53.47, Min = −72.80, Mean = −66.87, SD = 16.80. Given the stated min–max range (19.33‰), an SD of 16.80‰ is not just "large"; it is inconsistent with the range (i.e., it cannot occur for any dataset bounded by those min/max values under standard SD definitions).

The authors should re-check Table 2 calculations and transcription; report N for each water type and for each soil depth bin; and clarify whether statistics pool both years, multiple events, etc. Endmember statistics must be correct because they propagate into the mixing/infiltration calculations.

We are deeply grateful for the reviewers' meticulous and professional review. The reviewers correctly identified the mathematical logical contradiction between standard deviation (SD) and range. In any bounded dataset, the standard deviation cannot approach or exceed half of the range (for a range of 19.33‰, the theoretical maximum SD is only 9.67‰). This reflects a serious oversight in our data processing or manuscript transcription, for which we sincerely apologize. We will make the following in-depth revisions in the revised manuscript:

We will recheck the original isotope records for all water bodies (precipitation, irrigation water, soil water, groundwater) in Table 2. Preliminary verification indicates that the SD value of δ^2H for irrigation water was a transcription error, which we will update to the accurate values verified by statistical software. As requested by the reviewers, we will add a column in Table 2 explicitly labeling the sample size (N).

We will report N values for each water sample category (Rainwater, Irrigation water, Groundwater) and for 10 soil depth gradients (0-100 cm). The source of the statistical data will be clearly stated in the table notes: the current statistical values are compiled from all observation events during the two growing seasons (May-September) in 2021 and 2023. We will explain whether the average value was used as the endmember or whether dynamic changes from different irrigation events were considered when calculating the mixing/leakage ratio. Since the δ^{18}O statistical values of endmembers are directly involved in the calculations of formulas 7 and 8, we will rerun all computational programs to ensure that any minor changes resulting from the revision of statistical values are accurately reflected in the final leakage ratio results.

5. Headline loss fractions (32% evaporation; 5% "excess irrigation" below 60 cm) are asserted but not derived transparently. The Abstract and Conclusions state evaporation losses of 32% and deep losses of 5%. The manuscript defines PET (Eq. 2) and an isotope mass-balance approach for infiltration (Eq. 6–8), but it does not show how those lead to the reported percentages, nor does it report the necessary bookkeeping terms (irrigation applied in mm, precipitation, Δ storage, ET or evaporation estimates, drainage).

The authors need to present an explicit water balance (with units and uncertainty) that produces these percentages. If the percentages are isotope-derived, show the full calculation chain and assumptions (including how "loss" is defined).

We are deeply grateful for the reviewers' suggestions. The derivation of core quantitative results (32% vs. 5%) indeed appeared overly abrupt in the original manuscript, lacking necessary "accounting" data support, which undermined the scientific rigor of the research conclusions. In the revised manuscript, we will make the following improvements: We will supplement complete observation-period water balance terms (unit: mm), including: total irrigation (Irrigation), total precipitation (Precipitation), change in soil water storage (ΔStorage), estimated evapotranspiration (ET), and deep infiltration (Drainage). We will elaborate on how the 32% evaporation loss was determined through the isotope mass balance model. This involves coupling the infiltration ratio (Wi/Wa) derived from the modified formula (8) with the total water supply (irrigation + precipitation).

We will clarify the physical definition of "loss": We define "evaporation loss" as the proportion of water that is directly lost to the atmosphere during and shortly after irrigation, without entering the soil matrix; we define "deep loss" as infiltration exceeding the bottom of the 60 cm root-active layer. For the 5% deep infiltration (over-irrigation), we will combine the conceptual model in Figure 6 to demonstrate the changes in isotope characteristics of the 60-100 cm soil layer and the increase in water storage at this depth range.

6. The infiltration/mixing calculation (Eq. 6–8) rests on strong assumptions that are not defended, and the reported output ("infiltration into each layer") is not clearly defined. Eq. (6–8) assumes post-irrigation water storage in a layer is a two-component mixture of (i) pre-irrigation water in that layer and (ii) infiltrated irrigation water, diagnosed from $\delta^{18}O$. In reality, each layer is part of a through-flow system (water enters from above and exits below), and isotopes are also affected by evaporation and root water uptake over days. Without explicitly restricting the time window or modeling those processes, it is not clear how Eq. (6–8) could uniquely identify "infiltration amount" per layer.

Also, Eq. (8) appears to compute a fraction (Wi/Wa × 100%) rather than an amount (mm), but the text interprets it as a quantification of infiltrated water amount.

The authors need to define exactly what Wi represents (mm, fraction, or both), justify the two- endmember assumption over the sampling interval, and provide a sensitivity/uncertainty analysis (endmember variability, fractionation effects, etc.).

We sincerely appreciate the reviewers 'thorough examination of the model's physical foundations. The dynamic nature of the "through-flow system" and the impact of isotope fractionation on the mixing model remain central challenges in hydroisotope studies. In the revised manuscript, we will address these issues as follows: We acknowledge inconsistencies in the initial draft regarding the expression of Wi. Equation 8 calculates the proportion of infiltration water relative to total storage volume (Fraction,%). To derive the infiltration amount (Amount, mm), this proportion must be multiplied by the post-irrigation soil water storage Wa.

We will clearly distinguish between these two variables and standardize unit measurements throughout the text. At the moment of irrigation and in the immediate aftermath, changes in soil isotopic characteristics due to evaporation and root water uptake are secondary compared to the substantial irrigation flux. We will explicitly specify the model's application scenarios in the revised version and acknowledge potential long-term biases from neglecting fractionation effects. As noted in our responses to similar criticisms, treating measured soil isotopic values as a well-mixed control volume is a standard practice in isotope hydrology. Following established methodologies, we will quantitatively assess the uncertainty impact of evaporation-induced fractionation on mixing ratio estimation. A dedicated "Uncertainty Discussion" section will be added in Section 3.4 to report the error ranges of computational results.

7. Claims about horizontal transport are not identified by the data presented. The manuscript concludes that transport is "predominantly vertical" with "minimal" horizontal movement, but the sampling design described is vertical profiling at a single location/event set (no lateral transect, no spatial mapping). The discussion in Section 3.3 reads as a conceptual description (supported by a conceptual figure), not an inference constrained by measurements.

The authors need to either remove/soften the horizontal-transport conclusion, or provide spatial sampling/analysis capable of detecting lateral redistribution.

We are deeply grateful for the reviewers 'objective evaluations. Their critique regarding the logical disconnect between the sampling design (limited to vertical profiles) and the conclusions (weak lateral migration) is highly pertinent. We acknowledge that drawing quantitative conclusions about lateral migration intensity without lateral transects is not rigorous. The revised manuscript will undergo the following adjustments: We will revise the relevant statements in the Abstract and

Conclusions, replacing "minimal lateral movement" with a more cautious qualitative description, and explicitly state that the study's focus lies on vertical infiltration dynamics.

We will redefine the nature of the discussion in Section 3.3: We will clarify that Section 3.3 and Figure 6 constitute a conceptual mechanism derivation, aimed at demonstrating the potential fate of sprinkler irrigation water in an ideal homogeneous medium, rather than an inference entirely constrained by the current measured data. We will also expand the discussion on research limitations, highlighting the inadequacies of single-point vertical profile observations in characterizing complex lateral water migration in farmland.

8. Correlations involving lc-excess are partly tautological, and p-values likely overstate evidence due to non-independence. The manuscript reports significant correlations between SW lc-excess and δ2H/δ18O (Section 3.1). But lc-excess is defined as a linear combination of δ2H and δ18O (Eq. 3), so correlation with its components is expected and does not constitute independent process evidence. In addition, pooling depths and times creates strong non-independence (repeated measures along profiles and through time), so standard p-values are not meaningful unless the sampling unit and degrees of freedom are defined correctly.

The authors need to remove (or reframe) lc-excess vs δ correlations; relate lc-excess to independent drivers (RH, VPD, temperature, time since irrigation) using an analysis that respects the repeated-measures structure (e.g., mixed effects or event-level aggregation).

We are deeply grateful to the reviewers for their insightful critiques on statistical rigor. The reviewers accurately identified logical flaws in our statistical inference: the use of correlation between derived and original variables to justify physical processes, as well as the neglect of the statistical non-independence of spatiotemporal sampling data. In the revised manuscript, we will undertake the following substantial revisions: We acknowledge that the significant correlation between lc-excess and δ^2H and δ^{18}O largely stems from the mathematical definition in Equation 3. Following their recommendations, we will remove the argument regarding the correlation between lc-excess and its constituent isotopes, redefining it solely as an indicator of isotopic deviations from the Local Mean Water Level (LMWL).

Regarding the repeated measures issue raised by the reviewers, we will abandon simple correlation coefficients in favor of Linear Mixed-Effects Models (LMEMs)in the revised manuscript. Within these models, we will treat sampling depth and time as random effects and environmental drivers as fixed effects, thereby obtaining more reliable p-values and statistical interpretations while respecting the profile structure. The vertical heat map shown in Figure 2 will be reinterpreted to focus on the vertical gradient evolution of lc-excess as the irrigation signal diminishes, rather than merely numerical correlation.

9. Endmembers for "groundwater mixing" are not adequately reported, making the mixing interpretation unsupported. The text argues that 0–60 cm soil water is mainly irrigation water, while 60–100 cm reflects irrigation water mixing with groundwater (Section 3.4). However, groundwater isotope statistics are not presented in Table 2, and no quantitative mixing analysis with uncertainty is shown.

The authors need to report groundwater isotope values (N, mean, SD; and temporal variability if relevant), show them clearly in the isotope plots, and present a quantitative mixing framework if "mixing" is a key conclusion.

We are deeply grateful for the reviewers' suggestions. In the initial draft, the inference regarding the influence of groundwater on deep soil water indeed lacked sufficient quantitative data support. We will strengthen this core conclusion in the revised draft through the following measures: Although we collected and analyzed groundwater samples from agricultural wells near the experimental site during sampling, these data were unfortunately omitted in Table 2 of the initial draft.

We will add a "Groundwater" category in the revised Table 2, fully reporting its sample size (N), mean (Mean), standard deviation (SD), and seasonal fluctuations. We will update Figure 3 by using more prominent colors or symbols to mark the positions of groundwater (Groundwater) endmembers. This will visually demonstrate how moisture isotope values in the 60-100 cm soil layer are distributed between irrigation water and groundwater endmembers, providing preliminary visual evidence for "mixing." For the 60-100 cm soil layer, we will apply two-endmember or multi-endmember mixing models (e.g., linear mixing models based on δ2H and δ18O) to calculate the specific contribution ratios of groundwater.

10. Isotope laboratory methods and QA/QC are under-described for soil water extracted by vacuum distillation. The manuscript states soil water was extracted using a "low-temperature vacuum condensation extraction system" but then specifies 180°C (Section 2.3). This discrepancy needs clarification. More importantly, laser spectroscopy of extracted soil water can be affected by organics and matrix effects; the manuscript does not describe checks/corrections (standards bracketing, drift, VSMOW–SLAP normalization details beyond Eq. 1, memory correction, organic contamination screening).

The authors need to provide the QA/QC procedures sufficient for readers to trust that the reported shifts (often a few per mil in δ18O) are not methodological artifacts.

We are deeply grateful to the reviewers for their reminders regarding the rigor of the experimental methods. Precise isotope measurements form the foundation of all inferences in this study, and we fully endorse the necessity of detailed disclosure of QA/QC details. We will conduct in-depth supplementation and revisions to Section

2.3: The 180°C mentioned in Section 2.3 is indeed the set temperature of the heating jacket. This high-temperature setting is designed to ensure complete extraction of bound water (particularly clay particle adsorbed water) from the soil, thereby avoiding kinetic isotope fractionation due to incomplete extraction. Among the four parallel samples collected per layer, three were used for independent isotope measurements and averaged. These rigorous repeated measurements and calibration procedures ensure that the observed isotope evolution of soil water reflects genuine infiltration and mixing processes, rather than methodological artifacts.

Technical corrections (typos, formatting, clarity)

We sincerely appreciate the reviewers' patient and meticulous identification of these editorial and formatting deficiencies. These issues have significantly compromised the readability and academic rigor of the manuscript. We accept all proposed revisions and will implement the necessary corrections in the revised version.