EGUsphere:

Isotopic insights into the dynamics of soil water pools along an elevation gradient

Jiří Kocum et al.

AUTHORS' RESPONSE

REVIEWER 1

Reviewer's Comments:

In this work, the authors compared isotopic dynamics and seasonal origins of different soil water pools at four study areas at various elevations. The novelty of this study lies in the use of a new extraction method to determine tightly bound soil water. This new technique appears promising, but no spike-experiment results and no comparisons with other soil-water-extraction methods are presented. Furthermore, the uncertainty associated with the mass-balance mixing model was not estimated. A detailed description of the technique and its critical evaluation is necessary to assess the quality of the results. In addition to this major comment, other descriptions of the methods should be improved (e.g., the determination of soil water content) and/or moved to other sections (e.g., the use of RMA-based regression).

Authors' Response:

AR:

We thank the reviewer for the detailed comments. In response, we have added the results of spike experiments and explicitly included uncertainty propagation for the mass-balance mixing model. A complete description and critical evaluation of the new extraction technique, along with all test results, have been added to the Supplement. Additionally, the main text has been revised according to the reviewer's suggestions, including improvements to the description of soil water content and the relocation of method details (e.g., RMA-based regression) to the Methods section.

Specific Comments:

RC:

Line 65: Maybe I missed that information, but I do not see a definitive protocol proposed in Ceperley et al. (2024).

AR:

This part of the text has been rewritten and the reference removed.

RC:

Table 2: Please group soil cores and mobile soil water by sampling depth, and please indicate the number of locations used for each water source in each study area.

AR:

We took this comment into account and modified the table based on the recommendation.

RC:

Section 3.2: The main findings of this research depend on the application of this method to extract tightly bound soil water (TBW). Conventional spike experiments were performed (lines 156-157), but the manuscript does not present evidence of these results or a critical evaluation of the method (including comparisons with other approaches). The use of glass balls for air removal (are they effective?), and the choice of the labelled water seem fundamental for the method and to determine the isotopic composition of TBW based on a mass balance mixing model (and to minimize the uncertainty in the estimations of TBW). Uncertainties arising from the application of the mixing model (e.g., calculations can be based on the error propagation method) should also be presented and discussed. Finally, it would have been helpful to compare the obtained results with those derived from soil water extracted by other methods (e.g., by cryogenic vacuum distillation).

AR:

Extraction method

We thank the reviewer for this important comment. We agree that the robustness of the TBW extraction method needs to be clearly demonstrated, as it underpins the main findings of the study. We have therefore added the results of the conventional spike experiments to the Supplementary Material (Table S5) as well as to the end of this document. These experiments demonstrate that the applied extraction and mixing approach is able to reliably recover the isotopic composition of tightly bound soil water. While the isotope mixing equation itself is a well-established concept in hydrology and commonly used (Haig et al., 2020; Zhao and Wang, 2021; Qiu et al., 2025), we now explicitly discuss the assumptions and potential sources of uncertainty associated with its application to tightly bound soil water.

Furthermore, we acknowledge that cryogenic vacuum distilation (CVD) is currently the most widely used method for soil water extraction and is often considered a reference approach. However, we consider a direct comparison between our method and CVD to be methodologically problematic, as individual CVD setups differ substantially among laboratories in terms of design, operational parameters, and achievable accuracy, as repeatedly documented in the literature (e.g., Orlowski et al., 2018; Kocum et al., 2025).

Such a comparison would therefore not allow for an unambiguous separation of differences arising from the methodological principles themselves from those caused by specific laboratory implementations, and could lead to misleading conclusions. For this reason, we adopted an alternative approach based on quantifying the systematic offset of the method and its variability using controlled spike experiments. These parameters provide a transparent and transferable measure of method performance, allowing subsequent comparison with specific CVD setups (or other extraction methods) validated in individual laboratories, without confounding the interpretation by uncertainties associated with the heterogeneity of CVD approaches. To clarify this rationale, we have expanded the discussion section of the manuscript to explicitly address the chosen validation strategy. In our opinion, the use of different extraction methods across laboratories, each associated with varying degrees of systematic errors, does not necessarily constitute a critical limitation, if these errors are quantified and subsequently used for the data correction.

We believe that these additions strengthen the transparency and reproducibility of the method while preserving the primary focus of the manuscript on soil water dynamics rather than on the development or benchmarking of a new extraction method.

Glass balls

Glass balls, typically 10 to 100 per sample, serve two complementary purposes within the applied methodology. First, they help to minimize or completely eliminate air bubbles within the sample. The presence of dissolved oxygen in the added isotopically labelled water is further reduced by using freshly prepared distilled water.

Second, due to the design of the experimental setup (see Fig. A1) and the applied mixing principle, the glass beads are repeatedly displaced within the sample under the influence of gravity. This mechanical movement facilitates the disruption of larger soil aggregates and promotes effective mixing of the resulting solution.

These methodological details have been added to the revised manuscript to improve clarity and reproducibility.

Figure A1: Custom-built laboratory apparatus designed to ensure homogeneous mixing of soil and added water through controlled mechanical motion of sealed sample vials prior to analysis.

Labelled water

We agree with the reviewer that the isotopic composition of the labelled water is a critical parameter of the mixing approach. While a strong isotopic contrast between the labelled water and soil water can in principle facilitate source discrimination, it also amplifies the propagation of mass-balance uncertainties when the contribution of tightly bound water represents a relatively small fraction of the total water pool.

In our approach, distilled tap water was therefore deliberately selected as the labelled water because its isotopic composition lies approximately in the middle of the seasonal range of soil water isotopic variability at the study site. This choice represents a compromise that minimizes error propagation in the mixing equation: even very small uncertainties in the weighed masses of the individual components (on the order of hundredths of grams) can lead to disproportionately large uncertainties in the estimated TBW isotopic composition when the isotopic contrast between the labelled and target water is large (Fig. A2).

By using labelled water with an intermediate isotopic signature, we reduce the sensitivity of the mass-balance model to small weighing errors, thereby improving the overall robustness of the TBW estimation. This rationale has now been clarified in the revised manuscript.

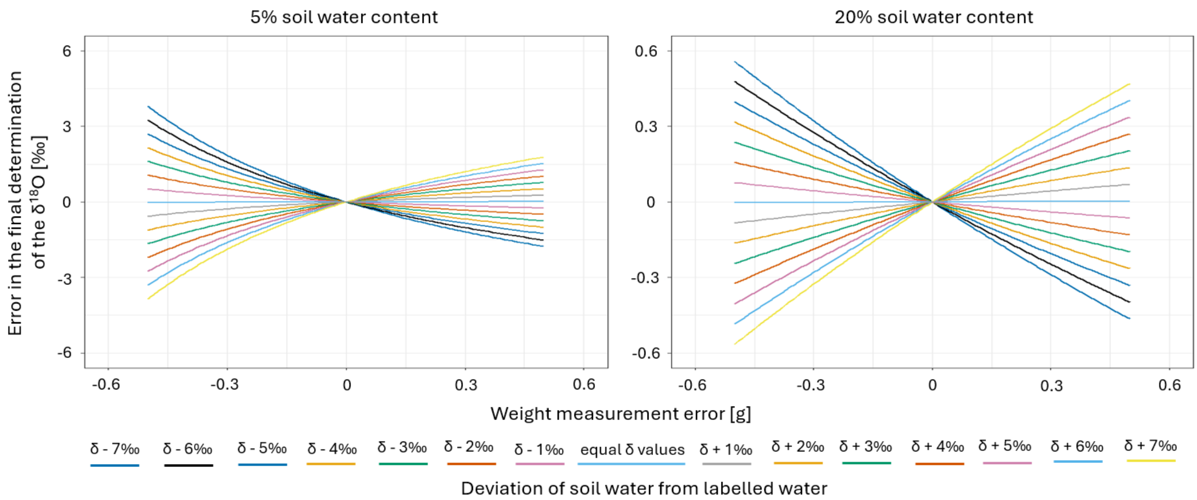

Figure A2: Sensitivity of the calculated isotopic composition of tightly bound water to weighing uncertainties at different soil moisture contents. The figures show the resulting error in $\delta^{18}O$ (Y axis) as a function of cumulative weighing error (X axis) for different isotopic contrasts between soil water and labelled water ($\Delta\delta$, coloured lines), illustrated for soil water contents of 20% and 5%, respectively.

Error propagation

We agree with the reviewer that uncertainties associated with the application of the mixing model should be explicitly quantified and discussed. In the revised manuscript, we therefore account for both (i) the analytical uncertainty of the isotopic measurements and (ii) the uncertainty arising from the mass-balance mixing procedure. The total uncertainty of the estimated isotopic composition of tightly bound water was quantified using standard Gaussian error propagation (see Supplement section B).

RC:

Line 147: Please provide the isotopic composition of the traced water. I also suggest using 'isotopically labelled water' instead of 'traced water' to improve the clarity. Furthermore, I expect that the isotopic signature of the traced water is significantly different from that of the samples. Otherwise, it would be challenging to discriminate between TBW and the traced water.

AR:

Thank you for the suggestion. We have revised the terminology to "isotopically labelled water". The isotopic composition of the labelled water has been added to the manuscript. The stable isotopic composition of labelled water used in this study was ranging from -8.4 to -9.6 ‰ and -63.6 to -65.2 ‰ for $\delta^{18}O$ and $\delta^{2}H$, respectively. The choice of isotopically labelled water is explained in the previous answer.

RC:

Line 158: Do the values between parentheses represent isotopic shifts or isotopic compositions?

AR:

These are isotope shifts. The text has been rewritten for clarity.

RC:

Lines 195-196: Besides a better visual representation, is there a more quantitative purpose for the application of a 3rd-degree polynomial fit? Given that you cannot use it to obtain an amplitude for the specific water sources, I would suggest removing such analysis based on polynomial fits.

AR:

Given that the main graphical output (Figure 5) will be revised to simpler time series, the 3rd-degree polynomial analysis will be removed from this figure. We propose to retain it in Figure 8, however, where it provides a clear representation of the data and effectively highlights differences in the annual cycle dynamics between sites.

RC:

Lines 197-206: The description of RMA-based regression lines should be moved to section 3.3, before Equation 3. Please clarify whether RMA was applied to determine the $\delta^{2}H$-$\delta^{18}O$ regressions of all water sources.

AR:

The description of RMA-based regression lines has been moved to section 3.3. A statement was added to indicate that RMA was applied to determine the $\delta^{2}H$-$\delta^{18}O$ regressions for all water sources.

RC:

Section 3.5: Did you apply any isotopic correction for samples with an evaporative signature? If so, please describe it. If not, you should consider it for samples with a negative lc-excess.

AR:

Prior to SOI analysis, samples with negative lc-excess were additionaly corrected according to Benettin et al. (2019) to account for potential biases in isotope measurements.

RC:

Section 3.6: I do not understand very well the aim of this statistical analysis, and particularly of the 'differences in the annual courses of isotopic composition of individual waters'. Are you comparing δ^2H-$\delta^{18}O$ regression lines or time series of different water sources? The current text is unclear.

AR:

The intention was not to compare δ^2H–$\delta^{18}O$ regression lines, but to statistically evaluate differences between the seasonal time series of isotopic composition among individual water sources.

Specifically, sinusoidal functions were fitted to the isotope time series, and a robust bootstrap approach was used to assess whether the fitted seasonal patterns (i.e., their amplitudes and phases) differed significantly between water sources.

As Figure 5 has now been completely redesigned and the fitting of individual isotope signals omitted, this statistical analysis is no longer relevant and has therefore been removed from the revised manuscript.

RC:

Section 4.1 and Figure 3: Are the regression slopes significantly different or not? Many TBW samples fall on the left side of the LMWL; do they lie within the 95% prediction interval for precipitation? If not, why do they plot there? Were there any issues (e.g., organic contamination) observed during the isotopic analysis?

AR:

In the revised version of the manuscript, we will complement the regression analysis with a bootstrap-based assessment of RMA slope differences, including the calculation of 95% confidence intervals. The results of this analysis and their implications for the interpretation of the isotopic relationships among the investigated water types will be explicitly discussed in the revised manuscript.

RC:

Figure 4: I recommend showing the sample size associated with each boxplot, as well as which water sources are significantly different.

AR:

Thank you for this suggestion. We have completely redesigned Figure 4, which now includes all data points used in the analysis.

RC:

Line 274: Please provide the name of the statistical test when you state that there are (or there are not) significant differences among samples.

AR:

The manuscript was revised to explicitly specify the statistical tests used when reporting significant differences.

RC:

Section 4.3: Soil water content is mentioned many times here, but details about the measurements (sensors, locations, spatial and temporal resolution) were not reported in earlier sections of the manuscript.

AR:

The original Section 4.3 was removed from the manuscript. Information on soil water content measurements has been added in Section 3.3 and in Supplement B, specifying that soil water content was determined gravimetrically.

RC:

Figure 5: In this figure, there are too many dots, lines, and vertical bars that make the interpretation very difficult. For instance, vertical bars for precipitation amount are not very visible, and symbols or lines for soil water content are not clearly represented. If soil water content is indicated by the vertical bars for TBW and MW, I do not think these values are representative of soil water content at the monthly scale (or of the temporal variability of wetness conditions) in the study areas. Moreover, it seems that the fitted lines do not capture well the temporal dynamics of the various water sources, but their efficiency needs to be quantified and deserves an explanation.

AR:

Thank you for pointing this out. This figure has been completely redesigned to enhance clarity.

RC:

Lines 317-320 and Table 3: The comparison between the fitted lines does not make much sense if the sine function can model a small fraction of the total variability. I also do not think this analysis is particularly informative in supporting the key messages of this work; therefore, this method and these results can be removed.

AR:

Along with the revision of Figure 5, this section of the text has been completely rewritten, and the method mentioned has been removed.

RC:

Figure 6: How many SOI values are there for each month? Please report the sample size inside each cell.

AR:

Thank you for this suggestion. The figure representing SOI results has been completely redesigned and now includes the sample size used in each sampling date.

RC:

Lines 351-355: This text belongs to the discussion. Please move it.

AR:

The text has been moved to the Discussion section.

RC:

Line 397: The previous text and the figures do not present the estimation of the transit times. Please revise the text and refer only to the phase shift.

AR:

We have revised this part of the text so it does not contain the transit times.

RC:

Line 18: Please replace 'intimate' with another term (e.g., 'comprehensive', 'detailed').

AR:

We replaced the word "intimate" with the word "comprehensive."

RC:

Line 115: 'lower sampled soil layer' instead of 'lower soil layer samples'.

AR:

This part of the text has been rewritten and, based on the suggestion of the second reviewer, the terms "lower" and "deeper" have been replaced with numerical values indicating depth in centimeters.

RC:

Line 142: 'and' instead of 'with'.

AR:

Thank you for pointing this out. The suggested stylistic change has been implemented.

RC:

Equations 1 and 2: Please use ‘TBW’ instead of ‘S’.

AR:

Thank you for this suggestion. We have revised the indexing in Eqs. (1) and (2) and replaced the symbol S with TBW as recommended. In addition, to maintain consistent terminology throughout the manuscript and in response to a previous comment, we replaced the symbol T (traced water) with L (labelled water).

RC:

Line 189: Zuecco et al. (2024) is not present in the list of references.

AR:

Thank you for pointing this out. The correct reference is Xia et al. (2024), which includes Zuecco as a coauthor. This has now been corrected in the manuscript.

RC:

Line 274: The term ‘absolute quantities’ is unclear (not very precise); please replace it with another term.

AR:

The term ‘absolute quantities’ was replaced with ‘relative contributions to soil water’ to improve clarity and precision.

References

Benettin, P., Volkmann, T. H. M., von Freyberg, J., Frentress, J., Penna, D., Dawson, T, E., and Kirchner, J. W.: Effects of climatic seasonality on the isotopic composition of evaporating soil waters, Hydrology and Earth System Sciences, 22, 288–2890, https://doi.org/10.5194/hess-22-2881-2018, 2018.

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Qiu, X., Zhang, M., Wang, S., Meng, H., and Che, C.: Comparison of stable isotope mixing models for examining plant root water uptake, PLOS ONE, 20, e0318771, https://doi.org/10.1371/journal.pone.0318771, 2025.

Zhao, Y., and Wang, L.: Insights into the isotopic mismatch between bulk soil water and Salix matsudana Koidz trunk water from root water stable isotope measurements, Hydrology and Earth System Sciences, 25, 3975–3989, https://doi.org/10.5194/hess-25-3975-2021, 2021.

Xia, C., Zuecco, G., Marchina, C., Penna, D., and Borga, M.: Effects of Short-Term Climate Variations on Young Water Fraction in a Small Pre-Alpine Catchment, Water Resources Research, 60, e2023WR036245, https://doi.org/10.1029/2023WR036245, 2024.

REVIEWER 2

Reviewer's Comments:

The manuscript on "Isotopic insights into the dynamics of soil water pools along an elevation gradient" provides an interesting data set along an elevation gradient. The manuscript is mostly well structured, but has several weaknesses that need to be addressed. I am not sure if these can be addressed in a revision, but hope the authors can address the issues raised below:

Authors' Response:

We would like to sincerely thank the reviewer for their careful and highly professional evaluation of our manuscript. We greatly appreciate the reviewer's detailed comments and the identification of specific weaknesses, which have been extremely helpful. We will do our utmost to address all points raised in the revised manuscript.

RC:

No hypotheses provided, but a list of objectives, of which the last one is unclear to me what it could mean

AR:

In the revised version, clear hypothesis has now been added corresponding to our study objectives. In addition, the last objective has been reworded to improve clarity and better reflect the focus of the study.

RC:

The authors used a little (or not) known method for their isotope analysis and did not provide any evaluation of the method nor do they refer to a test presented in a previous manuscript. This is a major issue that will be difficult to address.

AR:

We agree that the robustness of the TBW extraction method needs to be clearly demonstrated, as it underpins the main findings of the study. We have therefore added the results of the conventional spike experiments to the Supplementary Material (Table S5) as well as to the end of this document. These experiments demonstrate that the applied extraction and mixing approach is able to reliably recover the isotopic composition of tightly bound soil water. While the isotope mixing equation itself is a well-established concept in hydrology and commonly used (Haig et al., 2020; Zhao and Wang, 2021; Qiu et al., 2025), we now explicitly discuss the assumptions and potential sources of uncertainty associated with its application to tightly bound soil water.

Furthermore, we acknowledge that cryogenic vacuum distilation (CVD) is currently the most widely used method for soil water extraction and is often considered a reference approach.

However, we consider a direct comparison between our method and CVD to be methodologically problematic, as individual CVD setups differ substantially among laboratories in terms of design, operational parameters, and achievable accuracy, as repeatedly documented in the literature (e.g., Orlowski et al., 2018; Kocum et al., 2025).

Such a comparison would therefore not allow for an unambiguous separation of differences arising from the methodological principles themselves from those caused by specific laboratory implementations, and could lead to misleading conclusions. For this reason, we adopted an alternative approach based on quantifying the systematic offset of the method and its variability using controlled spike experiments. These parameters provide a transparent and transferable measure of method performance, allowing subsequent comparison with specific CVD setups (or other extraction methods) validated in individual laboratories, without confounding the interpretation by uncertainties associated with the heterogeneity of CVD approaches. To clarify this rationale, we have expanded the discussion section of the manuscript to explicitly address the chosen validation strategy. In our opinion, the use of different extraction methods across laboratories, each associated with varying degrees of systematic errors, does not necessarily constitute a critical limitation, if these errors are quantified and subsequently used for the data correction.

We believe that these additions strengthen the transparency and reproducibility of the method while preserving the primary focus of the manuscript on soil water dynamics rather than on the development or benchmarking of a new extraction method.

RC:

It is unclear why the authors did not target to sample at least one entire year. I understand the logistical challenges for the mountainous snowy study site, but it seems all sites had only 10 months covered.

AR:

We acknowledge that the sampling period did not cover a full calendar year. There were two main reasons for this limitation. First, access to the higher-altitude sites is strongly restricted, with the highest-elevation site located in a strictly protected zone of the national park, making year-round sampling logistically infeasible. Second, the primary objective of the study was to investigate the retention of winter-derived soil water and its gradual replacement by isotopically heavier spring and summer precipitation, rather than to resolve complete annual cycles. This rationale and the resulting temporal coverage are now explicitly clarified in the revised manuscript. However, to support some of our claims, we used data from the previous year, 2022, although only mobile soil water data was available for that year.

RC:

No snow sampling is a problem, because this is likely to impact the delta_WinterP in the calculations of SOI

AR:

We agree with the reviewer that the absence of direct snow sampling could affect the estimation of δ_WinterP and, consequently, the calculation of the SOI. To address this limitation, we supplemented the dataset with precipitation samples dating back to November of the previous year, as well as snow isotope samples collected from adjacent areas, where possible, with comparable elevation and climatic conditions. These data were used to more accurately constrain δ_WinterP and thereby improve the robustness of the SOI calculations. All samples collected are now reported in Table 2.

RC:

Why is the "historical" data shown in Figure 8 ignored in this study? It appears that with Figure 8 results are introduced in the discussion section.

AR:

We thank the reviewer for this comment. The historical dataset does not include measurements of tightly bound soil water, which prevents a consistent comparison with the main dataset. However, we have included these data in the Discussion section to support our conclusions.

RC:

It seems that a correction of evaporation fractionation prior to SOI calculations is missing. This will affect the interpretation of the data.

AR:

We acknowledge that a correction for evaporative fractionation prior to SOI calculations was missing in the original manuscript. In the revised version, samples with negative lc-excess were additionaly corrected according to Benettin et al. (2019), and the SOI calculations and related interpretations have been updated accordingly.

RC:

Figures have little information content and questionable choice of visualization

AR:

We thank the reviewer for this comment. The vast majority of figures have been modified to improve readability and information content.

RC:

The reviewed literature is limited in the current manuscript. There have been several studies looking into mobile and bulk soil water isotope composition, while the authors discuss their results basically with two studies.

AR:

The Discussion section has been revised to include additional studies on mobile and bulk soil water isotope composition, providing a broader context for our findings. The section has also been shortened to improve clarity and readability.

RC:

The visualization (e.g., monthly bar plots) loses too much information

AR:

As noted previously, the graphical outputs have been redesigned to improve clarity and information content.

These major aspects are more outlined in the detailed comments below.

RC:

18: I don't think "intimate" is the right word here.

AR:

We replaced the word "intimate" with the word "comprehensive."

RC:

72: I suggest framing this as a definition. It seems that your definition of TBW is the water that remains in the pore space and cannot be extracted via suction lysimeter. I suggest rephrasing accordingly

AR:

We thank the reviewer for this suggestion. Following your recommendation, we have explicitly framed the definitions of the different soil water pools in our study. Mobile water (MW) is extracted at -60 kPa, tightly bound water (TBW) is defined as the water remaining in the soil after removal of MW, and bulk water (BW) represents the total soil water content. These definitions are now clearly stated in the manuscript.

RC:

76-86: This reads like a summary of methods. I don't think this is helpful or necessary in the introduction. I'd suggest to focus on research gaps, hypotheses, and objectives at the end of the introduction

AR:

We agree with the reviewer and have revised the Introduction accordingly to sharpen the focus on research gaps, hypotheses, and objectives at the end of the section.

RC:

91: unclear what this means.

AR:

We acknowledge that the original wording of this objective was unclear. The objective has been rephrased to explicitly clarify that the comparison refers to isotopic interpretations based on tightly bound soil water versus conventional bulk soil water, which integrates both mobile and tightly bound fractions.

The objective is now stated as: ‘Assess whether substituting bulk soil water with tightly bound soil water alters lead interpretations of soil water sources and their seasonal dynamics.‘

RC:

143: How was evaporative fractionation prevented over the 2 week period?

AR:

Following the procedure described by Orlowski et al. (2016), it was not possible to completely prevent evaporation during the two-week extraction period. To minimize evaporative effects, we extracted only the central part of each soil core, removing the upper and lower sections that are most susceptible to evaporation or potential contamination from contact with the ceramic plate. The residual error introduced by this unavoidable evaporation was quantified using a classical spike experiment and accounted for in the results (see Supplement B).

RC:

3.2: this seems to be a rather new or little used method. I think that a method evaluation is missing in this manuscript or there should be a reference to a study where it was done.

AR:

We thank the reviewer for this important comment. As this method is introduced here for the first time, we agree that its description and evaluation required clarification. To address this, we have added a detailed description of the method, including its assumptions, results of spike experiments, and uncertainty assessment, to Supplement B. In addition, the Discussion section now addresses the methodological limitations and the approach used to quantify and correct extraction-related errors. This allows the results obtained with the proposed method to be compared more consistently with those from other extraction approaches reported in the literature.

As this represents an initial application of the method and we are aware of its methodological limitations, we additionally propose in Discussion Sect. 5.3 a modified procedure based on the same conceptual approach that avoids the use of pressure plate apparatus on intact soil samples. This alternative approach reduces the extraction-related uncertainty to that associated primarily with the mixing-based calculation itself and therefore represents a more robust implementation of the method.

RC:

128: it's a weakness that there was not even one full year of precipitation sampling for the isotope data. I hope the sampling continued and this manuscript can be updated with that data prior to publication.

AR:

We agree that the lack of a complete year of precipitation isotope data (especially precipitation from the preceding winter) represents a limitation of the study. To address this, we extended the precipitation isotope dataset back to November of the previous year (including snow sampling where possible) in order to better capture the winter precipitation signal relevant for the estimation of seasonal isotope inputs.

Precipitation isotope data from the period following the end of soil water sampling were not included, although they are available for three of the four sites. Because soil water isotopic composition typically responds to precipitation with a time lag, these later precipitation data would not meaningfully contribute to the interpretation of the sampled soil water, as the soil sampling campaign ended at the end of November.

RC:

132: please do not call these depth shallow and deep. 40 cm is arguably not deep. In times of LLMs scraping manuscripts such definitions will give a wrong assessment of "deep" processes. I ask you to simply use the depth and talk about "20 cm and 40 cm depth samples".

AR:

We agree with the reviewer that the terms “shallow” and “deep” may be misleading in this context. In the revised manuscript, the samples are now consistently referred to as D20 and D40, corresponding to sampling depths of 20 cm and 40 cm, respectively. This nomenclature has been clearly defined in the Methods section (Section 3.1) and applied throughout the manuscript.

RC:

180: what was the cut off in lc-excess?

AR:

An lc-excess value of 10 was used as a pragmatic data-screening threshold to identify potentially contaminated samples. However, this threshold was not applied strictly as a fixed cutoff. Values slightly exceeding 10 were retained when the overall distribution of the respective sample group remained close to this threshold and did not indicate systematic contamination. In contrast, samples showing clearly elevated lc-excess values (typically between 20 and 50) were excluded from further analysis. In such cases, all associated measurements from the affected sample set were removed, including occasional borderline values below 10 when they preceded consistently elevated lc-excess values within the same sequence. This methodological step has now been clarified in Section 3.3 of the manuscript.

RC:

189: Citation should be the manuscript that defined equation 4, which is Kirchner

AR:

We apologize for this error; the remaining citations have been removed.

RC:

204: I think you should provide the standard deviation here

AR:

We thank the reviewer for this suggestion. The standard deviations of the RMA regression parameters (slope and intercept) have now been added to the Results section (Sect. 4.1) and are reported in Table 3.

RC:

3.5: I believe that Allen et al. calculated for the SOI the "non-fractionated" water isotope ratio by back calculating where on the LMWL the water sample is located using Benettin et al. (2018). This would need to be done in this study here as well, because the soil water samples have been partly evaporated.

AR:

Prior to SOI analysis, samples with negative lc-excess were additionaly corrected according to Benettin et al. (2019) to account for potential biases in isotope measurements.

RC:

219: unclear what Y, A, and B represent. The "i" likely represents the bootstrap models, but I think its definition is missing

AR:

The original application of the bootstrap analysis, which was insufficiently explained, has been removed and will no longer be used. In the revised manuscript, bootstrap is applied only to assess RMA slope differences, and is described more clearly.

RC:

Figure 4: it's unclear why you would show your data as boxplots. You sample every two weeks to then bulk all the results into seasons? You lose so much information this way and I would highly recommend to show the data as time series.

AR:

The majority of the figures have been revised to improve readability and information content, and we believe that these changes enhance the clarity and interpretability of the results.

RC:

269: you are describing temporal dynamics between precipitation input and soil water isotopes. I think that a revised figrue 4 should show these temporal dynamics. Please add precipitation isotope time series to the new figure 4.

AR:

As mentioned above, we have revised the figures and we believe that these changes enhance the clarity and interpretability of the results.

RC:

272: what does "stabilized" mean in this context? From figure 5, I would think that you mean that the values became all the same across 20 and 40 cm and for BW and TBW. If so, I don't think that stabilized is the right word.

AR:

We thank the reviewer for this comment and apologize for the unclear wording. In the manuscript, the term “stabilized” was intended to describe a period at the Liz site during summer when the isotopic composition of soil water remained relatively constant across depths (20 and 40 cm) and between mobile and tightly bound water. This pattern resulted from a single extreme precipitation event (~100 mm in two hours, $\delta^{18}O \approx -10‰$) under very dry conditions, which led to substantial soil profile saturation. Subsequent precipitation events, although isotopically heavier and of lower magnitude, had only minor influence on the soil water $\delta^{18}O$, which remained close to −10‰ over the following period. The text ha been rewritten.

RC:

277: due to the know density of water, you should provide the water content as volume percentage. Grams per 100 cm3 is an uncommon unit.

AR:

We thank the reviewer for this suggestion and agree that expressing water content as volumetric percentage is generally more appropriate. However, following the shortening of the manuscript and the removal of certain sections, these water content values are no longer reported in the revised text.

RC:

Figure 5: this is a very busy figure and I do not know the benefit of the trend lines. Why are these amplitudes and sinusoidal fits done? I understand that these are usually used to infer Kirchner's young water fraction. However this is not done here and I do not see a benefit of these fitted lines. Again, unit of water content should be adjusted.

AR:

We thank the reviewer for this comment. In the revised manuscript, Figure 5 has been removed and replaced with a clearer figure that better conveys the relevant information.

RC:

Figure 6: again, there is quite a loss of information when the data gets grouped to monthly averages. I further think that a time series with SOI on the y-axis is a more informative visualization than using a heatmap.

AR:

The figuree has been completely redesigned.

RC:

346: I do not think that the isotope values were corrected for evaporative fractionation, which is why I don't think these interpretation necessarily hold in this paragraph.

AR:

As noted, a correction for evaporative fractionation following Benettin et al. (2019) has now been applied. Accordingly, the interpretation of the results in this paragraph has been revised to reflect the corrected isotope values.

RC:

Figure 7: What is the difference between this graph and Figure 6?

AR:

Figure 7 was originally intended to show the difference between TBW and BW. However, this figure has been removed from the revised manuscript.

RC:

388: I do not think that comparing the TBW with any of the xylem data from the referenced studies across the world in entirely different climates is meaningful at all.

AR:

Our original intent was to show that the isotopic patterns of soil water in our study are broadly consistent with xylem data reported in the literature, which could support the plausibility of our results. For example, the study area in Floriancic et al. (2024) is similar to our higher-elevation sites. However, we agree that, given that we did not extract xylem water in our study, a direct comparison with xylem data is not appropriate especially with data from different climates. Therefore, the corresponding text has been removed from the Discussion in the revised manuscript.

RC:

397: I have not seen any transit times reported in the results

AR:

We agree with the opponent that no transit times were explicitly calculated or reported in the Results. Kirchner (2016) shows that phase shifts of seasonal isotope cycles are governed by the transit time distribution and primarily reflect the younger fractions of water. As we did not estimate transit time distributions or calculate mean transit times, we have revised the wording to avoid reference to mean transit time and instead interpret the observed phase shifts qualitatively in terms of relative transit-time behavior and turnover.

RC:

522: Why “despite”?

AR:

We agree that the use of “despite” was unclear in this context. The sentence has been rephrased.

RC:

525: “meteoric origin” sounds awkward. Is not all water that you sampled of meteoric origin? What else would potentially be another origin?

AR:

We agree with the opponent that the term “meteoric origin” is redundant in this context. The sentence has been revised accordingly, and the reference to meteoric origin has been removed.

RC:

526: A bias could be that the evaporative signal is being diluted in the equilibration method, right?

AR:

We agree that evaporative signals could be partially diluted. However, our spike experiments indicate that this is not the case during the mixing method, which appears sufficient to preserve the original isotopic signal. In contrast, some dilution and greater uncertainty may occur in the pressure plate apparatus, as reflected by higher shifts in the results.

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