EGUsphere:

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

Jiří Kocum et al.

AUTHORS' RESPONSE

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 evaluation of our manuscript. We greatly appreciate the reviewer's detailed comments and the identification of specific weaknesses, which have been very helpful. Below, we provide a point-by-point response to individual comments (in red), and we will do our utmost to address them 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 hypotheses will be added corresponding to our study objectives. In addition, the last objective will be 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 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 will therefore add the results of the conventional spike experiments to the Supplementary section. These experiments will 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, we will 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 will adopt 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 will expand the discussion section of the manuscript to explicitly address the limitations of direct CVD comparisons and to justify the chosen validation strategy.

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 agree that the dataset is ifluenced by the sampling period. There were two main reasons for this limitation. First, 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. Second, access to the higher-elevation sites is strongly limited, as they are located in a strictly protected zone of the National Park, making year-round sampling logistically infeasible. This rationale and the resulting temporal coverage will now be explicitly clarified in the revised manuscript.

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 will supplement the dataset with snow isotope samples collected from adjacent areas with comparable elevation and climatic conditions. These additional data will then be used to better constrain δ_WinterP and, in turn, improve the robustness of the SOI calculations.

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:

Thank you for this concern. The historical dataset lacks measurements of tightly bound soil water, which limits a consistent comparison with the main dataset used in this study. For this reason, these data were initially used only to provide contextual support in the Discussion section. In the revised manuscript, we will now explicitly justify this limitation and relocate the presentation of the historical data to the Results section as supplementary results, clarifying their role and scope.

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 thank the reviewer for pointing out this oversight. In the revised version, this correction will be applied, and the SOI calculations and related interpretations will be updated accordingly.

RC:

Figures have little information content and questionable choice of visualization

AR:

In the revised manuscript, the figures will be redesigned to increase clarity and information content (reflecting also comments from Reviewer 1). In particular, we will present the data as time series, a more commonly used and accessible format. Where appropriate, we will use alternative visualizations to better convey the temporal dynamics of soil water isotopes.

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:

We agree that the reviewed literature in the original manuscript was limited. In the revised manuscript, we will expand the literature review to include additional relevant studies on mobile and bulk soil water isotope composition, providing a broader context for our findings.

RC:

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

AR:

As mentionedin one of the previous comments, the graphical outputs will be redesigned to improve clarity and information content. In particular, we plan to replace the monthly bar plots with time series or alternative visualizations that better convey the temporal dynamics of the data.

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 will replace 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:

The text will be rephrased as suggested, and clear definitions of TBW and MW will be provided in the Introduction.

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 will revise the Introduction accordingly by removing methodological details and sharpening the focus on the 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 will be 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 will now be stated as: "To determine whether replacing bulk soil water with tightly bound water can lead to different 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 of Orlowski et al. (2016), it was not possible to completely prevent evaporation during the two-week extraction period. To minimize evaporative effects, we extracted the core of the soil sample, removing the upper and lower parts, which are most susceptible to evaporation or potential contamination from the ceramic plate. The residual error introduced by this unavoidable evaporation was quantified using a classical spike experiment and was accounted for in the results. These procedures and their potential impacts will now be described in more detail in the revised manuscript.

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:

As mentioned in one of the above responses, we will add the results of the conventional spike experiments and more detailed description of the method to the Supplementary Material.

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 represents a limitation of the study. To partially address this, we will incorporate available historical precipitation and snow isotope data for all study locations to better constrain the seasonal isotopic signal. Precipitation isotope monitoring is ongoing at the mountainous sites; however, lowland sites are no longer actively monitored.

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” are misleading in this context. In the revised manuscript, samples will be referred to using depth-based labels (D20 and D40), corresponding to 20 cm and 40 cm depth, respectively. This nomenclature will be clearly defined in the Methods section (Section 3.1) and used consistently throughout the manuscript.

RC:

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

AR:

Samples affected by contamination were characterized by high lc-excess values (≥ 10‰; up to 32‰ in some cases), whereas the remaining tightly bound water samples exhibited lc-excess values close to zero and followed the isotopic characteristics of precipitation and mobile water.

RC:

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

AR:

We thank the reviewer for pointing this out. The citation will be corrected to Kirchner, and the remaining incorrect citations will be removed.

RC:

204: I think you should provide the standard deviation here

AR:

Thank you for this suggestion. The standard deviation will be added to the revised manuscript.

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:

We thank the reviewer for this important comment. We agree that a correction for evaporative enrichment following the approach of Allen et al. (2019) and Benettin et al. (2018) is needed. This correction will be applied in the revised manuscript, and the SOI calculations and interpretations will be updated accordingly.

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 will be removed and will no longer be used. In the revised manuscript, bootstrap will be applied solely to assess RMA slope differences and will be 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:

We thank the reviewer for this suggestion. The figure will be revised to display the data as time series.

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:

In the revised Figure 4, precipitation isotope time series will be added alongside the soil water data.

RC:

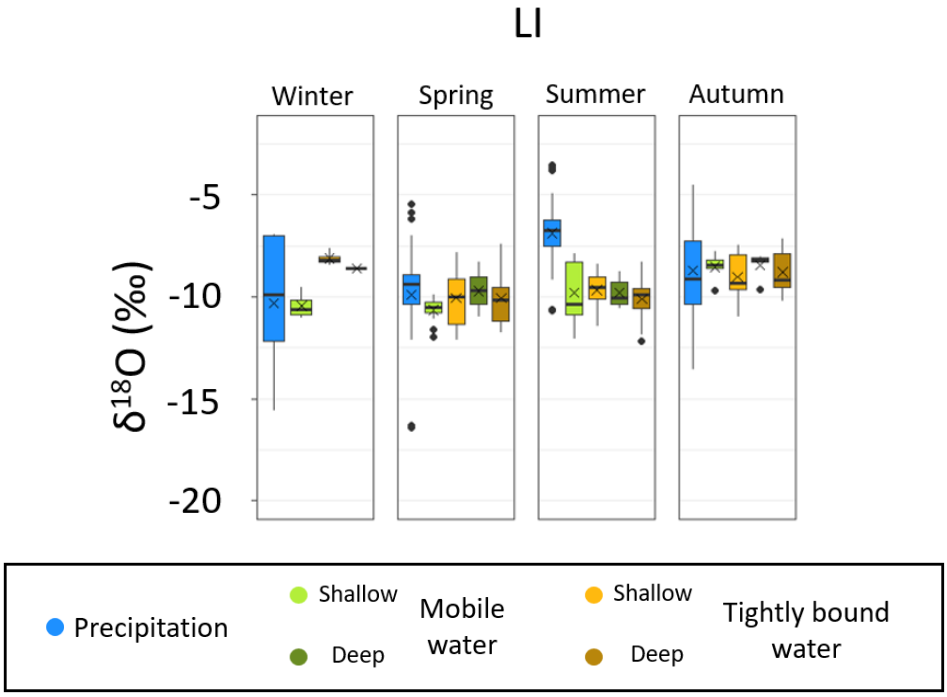
272: what does "stabilized" mean in this context? From figure 5, I would think that you mean that the values became all the same across 20 and 40 cm and for BW and TBW. If so, I don't think that stabilized is the right word.

AR:

By using the term “stabilized,” we intended to describe a summer period at the Liz site during which the isotopic composition of soil water remained relatively constant across depths (20 and 40 cm) and between mobile and tightly bound water (Figure 4; here, a section of Figure 4 is shown as Figure A1). This pattern resulted from a single extreme precipitation event

(approximately 100 mm within two hours, with an isotopic composition of about −10‰ for $\delta^{18}O$) that occurred under very dry conditions and led to substantial saturation of the soil profile. Subsequently, although isotopically heavier precipitation events, with significantly lower precipitation totals, had only a minor influence on soil water isotopic composition, which remained close to -10‰ value for $\delta^{18}O$ over the following period. However, we agree that the term "stabilized" was misleading in this context and will be therefore replaced by more precise wording in the revised manuscript, and the underlying process will be described in greater detail.

Figure A1: Cropped detail of Figure 4 showing the isotopic composition of precipitation and soil water at the Liz site.



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 agree with the reviewer that expressing water content as volumetric percentage is more appropriate. Accordingly, water content values will be converted from grams per 100 cm^3 to vol. % in the revised manuscript.

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. The original intent of Figure 5 was to show as many variables as possible in a single plot, because the displayed information complements each other and helps to interpret the seasonal dynamics of soil water. Trend lines were fitted to indicate the approximate course of the different soil water components throughout the year. We acknowledge, however, that this makes the figure overly complex and time-consuming to interpret. In the revised manuscript, Figure 5 will be completely redesigned using clearer time-series plots, which will better convey the temporal dynamics of the data. In addition, water content units will be adjusted as suggested.

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:

We thank the reviewer for this suggestion. We agree that grouping the data into monthly averages leads to a loss of information. In the revised manuscript, Figure 6 will be redesigned using time series with SOI on the y-axis, which will provide a clearer and more informative visualization of the data.

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:

We acknowledge that evaporative fractionation was not corrected in the original manuscript. In the revised version, this correction will be applied, and the SOI calculations and related interpretations will be updated accordingly.

RC:

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

AR:

Compared to Figure 6, which compares the SOI of mobile water (MW) and tightly bound water (TBW), Figure 7 illustrates what the SOI would look like if conventionally used bulk water (BW) were used instead of TBW, and whether any differences between them should be considered in future research. We thank the reviewer for this comment and will consider alternative graphical representations, as well as describe the purpose and meaning of Figure 7 more clearly in 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 will be removed from the Discussion in the revised manuscript.

RC:

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

AR:

We thank the reviewer for this comment. We agree that no transit times were explicitly calculated or reported in the Results section. 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 will revise 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 will be rephrased to more clearly describe the contrasting patterns in residence time between lowland and high-elevation sites.

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 will be revised accordingly, and the reference to meteoric origin will be 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 by the equilibration method, and this potential bias will now be explicitly discussed in the revised manuscript.

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