Review Comments

This manuscript investigates the spatial and temporal variability of stable water isotopes in precipitation and rivers across Romania and the Republic of Moldova, integrating monthly δ18O and δ2H observations from 22 precipitation stations and 19 river stations. From a scientific perspective, the study area lies in a transition zone influenced by Atlantic, Mediterranean, Black Sea–Caspian moisture sources and by the orographic barrier of the Carpathian Mountains. Because stable-isotope observations remain relatively sparse in this region, establishing a comparatively dense precipitation–river isotope database has clear value in filling an important data gap. From a practical perspective, the dataset could provide useful baseline information for regional water-source identification, investigations of surface water–groundwater interactions, catchment-scale water-cycle model evaluation, and future assessments of water-resource variability. However, the present manuscript has several substantial shortcomings that affect the core reliability of a data paper. The temporal and spatial representativeness of the precipitation and river samples is not well matched, yet the two datasets are used directly for process attribution. Interpretations involving heat waves, Mediterranean cyclones, moisture recycling, snowmelt, and catchment "resilience" extend beyond what the analyses presented here can support. Data completeness, missing observations, quality control, and the provenance/originality of the data product are insufficiently documented. The statistical framework is incomplete, making several key quantitative results difficult to reproduce. In addition, there are numerous errors or inconsistencies in figures, coordinates, notation, and references. These issues cannot be resolved through local editing alone; they would require a substantial redefinition of the analytical scope, reconstruction of the data-quality framework, and additional analyses. In my view, the manuscript in its current form does not meet the standards of Earth System Science Data for data reliability, reproducibility, and documentation of the data product. I therefore recommend rejection (Reject).

Major comments

1. The precipitation and river datasets are fundamentally mismatched in temporal scale and spatial representativeness, yet they are directly compared to infer hydrological processes. Page 5, Lines 120–146 indicate that precipitation samples represent monthly cumulative collections, whereas river water was sampled only once, at the end of each month. Pages 3–4, Lines 100–115 further indicate that most river sampling sites are located in the lower parts of their catchments, while the precipitation stations cannot represent the entire catchment, particularly the high-elevation source areas. Although the authors themselves acknowledge on Page 10, Lines 281–285 that the precipitation stations are generally located in the lower parts of the catchments and that this may affect the interpretation of river–precipitation isotope differences, the manuscript nevertheless interprets the approximately 0.8‰ depletion of river water relative to precipitation as evidence for a greater winter-snow contribution and compares concurrent monthly values in Fig. 6 as "co-variability." Large catchments integrate precipitation through storage, mixing, damping, and time-lag effects. Without catchment-area/elevation-weighted precipitation isotope estimates, lag analysis, or a more defensible spatial matching scheme, the current precipitation–river process attribution is not sufficiently robust. This issue directly affects the central conclusions of the river-water section.
We are reporting data: stable isotopes ($\delta^{18}O$ and $\delta^{2}H$) in precipitation and stable isotopes ($\delta^{18}O$ and $\delta^{2}H$) in river water. Both types of samples were collected in accordance with the IAEA's recommendations. Moreover, we followed common practice in the field – monthly sampling at the end of each month so that our data could be directly comparable with all other similar data collected across Europe and elsewhere. Next, using the data the way we did it is the common practice in the field. This approach was used by the IAEA when the GNIR database was introduced (Harder et al., 2015) and was followed by subsequent studies across the world.

Harder et al, 2015: https://hess.copernicus.org/articles/19/3419/2015/

2. Several atmospheric-circulation and moisture-source mechanisms are presented as causal explanations, but the manuscript provides no independent analyses capable of testing them. Page 8, Lines 221–232 attribute the high and spatially homogeneous summer isotope values to regional heat waves and the high autumn d-excess values to Mediterranean cyclones. Pages 9–10, Lines 258–263 further state that "heat waves block moisture advection and enhance local recycling," while the Conclusions (Page 13, Lines 320–331) again identify the NAO, Siberian High, blocking patterns, and local moisture recycling as primary controls. However, the study includes no backward-trajectory analysis, moisture-flux diagnostics, circulation-type classification, cyclone-track analysis, moisture-recycling calculation, or event-scale composite analysis. The present evidence is essentially a combination of seasonal isotope patterns and interpretation based on previous literature, which is insufficient to establish these mechanisms. If the authors wish to retain such conclusions, independent atmospheric-circulation/moisture-source analyses are required; otherwise, the relevant statements throughout the manuscript should be systematically reframed as possible mechanisms or as patterns consistent with previous regional studies.

3. Reporting of data quality control and missing observations is insufficient for a data paper and does not adequately support

traceability or reproducibility. Page 6, Lines 182–191 explicitly state that some precipitation samples had negative d-excess values, which the authors suspect resulted from post-precipitation evaporation in the collectors, yet all of these data were retained. Figure 4 on Page 9 also contains numerous blank cells, but the manuscript does not explain whether these represent missing samples, months without precipitation, sample loss, analytical failure, or quality-control exclusion. The manuscript does not report, by station, the expected number of observations, number collected, number considered valid, missing months, number of negative-d-excess samples, or quality flags; nor does it explain whether QC flags are included in the Zenodo dataset. This is particularly important because d-excess is subsequently used to infer Mediterranean moisture and evaporation processes. Sampling artifacts and atmospheric signals therefore need to be distinguished explicitly. At minimum, the authors should provide a complete station-by-month data-completeness and QC table and perform a sensitivity analysis comparing results with and without questionable samples.
We disagree with the text above. Yes, we retained all data, regardless of whether d-excess was negative or not and we made this clear in the text, which reads: "d-excess […] used it as a secondary control on the quality of data (by expressing caution when discussing data obtained from samples with negative d-excess values, as post-precipitation evaporation in the rainfall collector was suspected). Nevertheless, all data is reported in this paper but readers are asked to exercise caution when using such data. Measurement of river samples did not yield negative d-excess values." The point is that we are reporting data, and we report it all. A negative d-excess value does not necessarily mean that the sample is not usable. Negative d-excess could result from small amounts of pp falling during summer heat waves and undergoing sub-cloud evaporation, for example. We did not do such investigations, as it beyond the scope of the paper. Information regarding "the expected number of observations, number collected, number considered valid, missing months, number of negative-d-excess samples" is in the Zenodo file, we do not see why it should be duplicated here. We did not red-flagged data in the Zenodo file, because there is simply no consensus on what "bad data" is – sometimes investigators consider d-excess values below 3 ‰ as "bad", while others consider that only negative values are "bad" and yet others work with such values, as well. We considered negative d-excess as problematic, said so and when analyzing our data, we removed these values (and made it now clear in the legend of figure 4 which states "White cells indicate months during which d-excess was negative (see main text)." We do not perform a sensitivity analysis for this paper, we did it previously, this is how we come to the conclusion that we should not use data from precipitation samples for which d-excess is negative.

4. The precipitation-sampling design may introduce evaporation, sublimation, and inter-station methodological differences, but the current methods do not demonstrate that these effects are negligible.
Page 5, Lines 120–141 indicate that the self-built collectors were used without oil, collector heights ranged from 20 to 120 cm, and open plastic containers were used in winter. Samples were transported to the laboratory only every 3–6 months and, in some cases, were analyzed after storage periods approaching nine months. Although the authors cite previous experience suggesting that the absence of oil does not systematically alter isotope values, no consistency test is presented for this study among collector types, collector heights, or the different summer and winter collection methods. Because the manuscript places considerable emphasis on d-excess and evaporation, this methodological uncertainty is particularly important. If different stations used different collector types or heights, these details should be reported station by station in the metadata, together with cross-validation results or at least a quantitative assessment of the potential bias.
Of course all precipitation water is prone to evaporation, sublimation etc. However, the equipment we used is standard in the field and other than taking care of not having human-induced error, there is nothing we can do about this. We used two pages of the ms to detail the entire sampling, analysis and QA/QC measures. It is not the scope of this paper to do a consistency test between different types of collectors, collector heights, methods, seasons etc. These were addressed by others (and us, previously), it is not realistically expectable that a paper reporting stable isotope data in precipitation do such tests for set of data.

5. The statistical analysis is insufficiently described, and several key quantitative results cannot be independently reproduced. Page 9, Lines 250–258 report r2 values for relationships between δ18O, air temperature, and elevation, including values after excluding two high-elevation stations; Page 11, Lines 285–290 state that river and precipitation isotopes show a "very good correlation." However, there is no dedicated Statistical analysis subsection in the Methods. The manuscript does not specify the sample size (N), whether regressions use station means or all monthly observations, whether Pearson correlation or OLS regression was applied, whether temporal autocorrelation or repeated observations were considered, how seasons were defined, how missing data were handled, or what significance level and confidence intervals were used. With only three years of monthly data, pooling all station-month observations would also create substantial non-independence and potential pseudoreplication. The statistical framework should therefore be rebuilt and all correlations/regressions should report N, slope and/or correlation coefficient, R2, p value, and 95% confidence intervals.
As all these observations are also repeated below, we replied there. We added the sample sizes in all relevant places. We do not believe that is necessary to explain that when comparing the δ18O of a station with the altitude of that station, we use the mean δ18O of said station or explain what we did not do (temporal autocorrelation, pooling data etc), explain what seasons are etc. The only statistics we used are simple correlation (we only calculated r^2 as an indicator of the strength of

correlation) and this does not need explanations. The calculations behind OLSR and PWLSR are detailed in the original publications and we referenced these. We did not believe that a boxplot needs to be explained, but we did it, nevertheless, as perhaps some readers are not familiar with it.

6. The provenance of the data and the boundary between newly released and previously available data are not sufficiently clear. Page 4, Lines 111–115 state that 17 precipitation stations belong to the authors' network, while five stations in the Republic of Moldova are obtained from GNIP and are "partly unpublished." The same section also states that data from four rivers (Ocoale, Deblei, Mniera, and Toaia) were previously published by Feher et al. (2024). The manuscript does not clearly distinguish data first released in this study from data already publicly available through GNIP or previously published river data, nor does it explain how source and licensing information are recorded for individual observations in the Zenodo dataset. More seriously, Feher et al. (2024) is cited in the text but no corresponding 2024 reference is included in the reference list. For a data paper, data provenance, publication status, licensing, and the specific new contribution of the present dataset should be documented at least at the station level, and preferably at the record level, so that the dataset's originality and reuse conditions can be evaluated.
We added the missing Feher et al (2024) reference. The data in that paper has been added to the database. Also, data from republic of Moldova, available in GNI, has been added to the database. All other data was generated in this study. We removed the "partially unpublished" text, as all data from Republic of Moldova is available in GNIP. The text clearly says: "*Our data from the 17 precipitation stations is completed with data from five stations in Republic of Moldova, available in the GNIP database. Stable isotope data for four rivers (Ocoale, Deblei, Mniera and Toaia, Table 2) were published before by Feher et al. (2024).*"

Specific comments
(a) Title, abstract, and wording
Page 1, title: The title uses "Eastern Europe," whereas the dataset covers only Romania and the Republic of Moldova. The manuscript also alternates among "South Eastern Europe" and "East-Central Europe." The geographical terminology should be narrowed and used consistently to avoid overstating the regional representativeness of the dataset.
We have checked the entire manuscript and corrected the one instance where we have used "East-Central Europe± instead of "Eastern Europe' when referring to our study area. The reference to "South Eastern Europe" in the "introduction" is correct in that context, as the climate influences we are referring to extend over a wider area and we do not want to suggest that they are only affecting Eastern Europe.

Page 1, Lines 34–40: The Abstract states that regional isotope differences "are linked to" the NAO, Siberian High, Mediterranean cyclones, and heat waves. This wording is stronger than the evidence presented in the manuscript. Consider replacing it with language such as "may be associated with" or "are consistent with."
We modified the text to read "*Our data **suggests** that regional differences in the $\delta^{18}O$ and $\delta^{2}H$ of precipitation and rivers are linked*"

Page 2, Lines 57–60: The phrase "the (NAO) Atlantic Oscillation (AO)" conflates two different names/abbreviations. Please clarify whether the intended term is the North Atlantic Oscillation (NAO) or the Arctic Oscillation (AO).
Typo, corrected.

Page 8, Lines 221–225: The statement "Altitude and latitude do not seem to be playing a role" is too categorical, especially because Page 9 later reports an r2 of 0.56 between δ18O and elevation. These statements should be reconciled on the basis of the formal statistical results.
We fail to see how "do not seem to" could be too categorical. The text refers to the uniformity of summer maxima **only** across the study area and in the subsequent lines we explain the cause of this uniformity.

Page 10, Lines 260–263: "heat waves block moisture advection and enhance local recycling" is a strong mechanistic statement, yet moisture recycling is not quantified in this study. The phrase "enhance local recycling" should be removed or explicitly framed as an untested hypothesis.
This is well-know process, we did not discover it, but showed that it is also present here. We added relevant references to the text (Risi et al., 2008; Aggarval et al., 2016, Watson et al., 2026).

Page 12, Lines 304–307: The manuscript describes lowland catchments as "the least resilient," but resilience is neither defined nor quantified. Please remove this wording or replace it with a more defensible statement such as "may be more sensitive to ...".

We replaced ”least resilient” with “more susceptible”, as small rivers are indeed more susceptible to changing climate, compared to large ones.

Page 13, Lines 328–331: “bimodal distribution” is not an appropriate term for a seasonal pattern characterized by winter minima and summer maxima. Unless a true bimodal probability distribution is demonstrated, “pronounced seasonal/annual cycle” would be more accurate.
Corrected.

(b) Methods, data, and statistical details
Page 4, Table 1: The longitude of Chișinău is given as 28°53′60.00″E and the latitude of Cahul as 45°47′60.00″N. Seconds should not reach 60; these coordinates should be carried over to the next minute and checked against the original metadata.
Corrected.

Page 4, Table 1: Brașov uses commas as decimal separators in the coordinates, whereas other stations use decimal points; straight and curly prime symbols are also mixed. Please standardize the DMS format, number of decimal places, and coordinate notation throughout the table.
Typo, corrected.

Page 4, Table 2: Length and/or drainage area are blank for rivers such as Săsar and Turia. Please indicate whether these values are unavailable or not applicable, use a consistent code such as “NA,” and define it in the table note.
We could not find/determine the length and drainage area for Turia. For Mniera, Deblei, Toaia and Ocoale rivers, all karst-rivers, values reported in the table for discharge, length and drainage areas are maximum values.

Page 5, Lines 121–126: Collector heights range from 20 to 120 cm, which is a substantial difference. Please report the collector type and installation height for each station and explain why this variation is not expected to materially affect collection representativeness.
The heights at which collectors were placed depended on local conditions and, 10 years later, we do nor recollect exactly these (for instance, one collector was placed on the roof of an isolated building and after the 1.2 m pole on which it was installed broke several times due to strong winds, we placed the collector directly on the roof, anchored).

Page 5, around Line 130: “walls of the collect” should read “walls of the collector.”
Done

Page 5, Lines 151–155: “water was drawn from each vials” should be “water was drawn from each vial”; “high amount of particulate matter” would be better written as “a large amount of particulate matter.”
Done

Pages 5–6, Lines 160–175: The preceding text states that measurements were normalized to VSMOW2 and SLAP2, whereas the δ definition subsequently refers to SMOW. Please use the VSMOW–SLAP scale consistently and clearly specify the reference standards and normalization procedure.
Well, the standards we used were VSMOW2 and SLAP2, provided by the IAEA, because SMOW and SLAP are exhausted. Nevertheless, data is (always) reported on the SMOW-SLAP scale. Everything is clearly specified in the text.

Page 6, Lines 173–175: “where R is the either ...” should be corrected to “where R is either ...”.
Done

Page 5, Lines 157–160 and Page 6, Lines 174–175: The manuscript requires the standard deviation of the final four injections to be below 0.03‰ for δ18O and 0.3‰ for δ2H, but later reports analytical precision of 0.16‰ and 0.7‰. These values may refer to different metrics (e.g., within-run repeatability versus long-term external precision), but this distinction is not explained and should be clarified.
We explained this quite clearly: on page 5, we state that the “Each sample was manually injected into the vaporization module multiple times, until the standard deviation of the last four injections was less than 0.03‰ for $\delta^{18}O$ and 0.3‰ for $\delta^{2}H$, respectively)”. We believe it is quite clear that we refer to the standard deviation of the δ values per each sample. On page 6 we describe the long-term precision of the measurements, based on “based on repeated measurements of the internal standard”. A couple of lines above we state that “A third standard (”Borsec”™ still water, -10.77 ‰ and -74.35 ‰ for $\delta^{18}O$ and $\delta^{2}H$, respectively) was run with each batch of samples and was used for data validation”. We do not believe any other distinction can be made (and yet, we added the name of the third standard on the last line once again, just to make it clear).

Page 6, Lines 187–191: The statement that "no memory and linearity (drift) errors occurred" is too strong without reported memory tests, drift tests, or acceptance thresholds. Please provide the supporting QC results or revise the wording to something such as "no detectable memory or drift within the applied QC criteria."
OK.

Page 6, Lines 193–196: River discharge is said to come from the gauging station nearest to each isotope sampling site, but the gauging-station names, distances to the isotope sites, and whether they are located on the same river reach are not reported. A station-matching table should be provided.
We do not understand this comment. Is it suggested that we could have collected samples for stable isotope analysis from one reach and reported discharges from another one? Adding another table with names of gauging stations would simply complicate the manuscript without any added value. As no discussion is made on the links between discharge and δ values, we do not understand the need for precise distances (which are generally below 1 km).

Pages 6–7, Lines 198–203: LMWLs are calculated using data from 2012–2017, whereas the main dataset covers 2015–2017. Please specify the source, station coverage, and publication status of the 2012–2014 observations, and explain why these earlier data are combined with the present dataset.
We report data collected between 2015 and 2017 and use data from our previous publication (Nagavciuc et al. (2022) to extend the dataset for LMWL to 2012-2017. The original publication is free to read, the data is available and, of course, the station coverage is the same – so we could have made LMWLs based on the combined datasets. All info for 2012-2014 is in the original publication and we do not see why we should repeat it here, also considering that the stations are the same.

Page 10, Table 3: The LMWLs are reported only as slope and intercept with uncertainties. Please also report the sample size (n), R2, p value and/or an appropriate measure of goodness of fit, and explain the precipitation-amount weighting and missing-data treatment used for PWLSR.
We added the sample size (n) in table 3. Additional statistics are not helping, because we do not check how good is the relationship between $\delta^{18}O$ and $\delta^{2}H$, but only calculate the slopes and intercepts of the LMWLs, which are what they are, regardless of p values (which are not suitable and used for such calculations). Precipitation weighting is fully described in the original publication (Hughes and Crawford, 2012) which we referenced in the text, we do not see why we should repeat it here.

Page 9, Lines 250–255: Please clarify whether the elevation regression uses long-term station means or all monthly observations. If all monthly data are used, elevation is repeated for each station and the analysis would involve substantial pseudoreplication. If station means are used, the actual sample size (e.g., N = 22 or the number of stations with complete data) should be stated.
Of course we used the station mean, i.e, a single number which we correlated with another single number – altitude, "across all stations" – where "all stations" means all stations in this study.

Page 9, Lines 250–257: It is not rigorous to describe a relationship as positive or negative using r2 alone, because R2 has no sign. Please report Pearson's r or the regression slope together with R2, p value, and confidence intervals.
Well, this is just normal, usual, parlance. When we say "the stable isotopic composition of precipitation is positively correlated with air temperature (r^2= 0.54 for $\delta^{18}O$)" it simply means that increase in δ18O is mirrored by an increase in air temperature. R^2 is used to show the strength of the correlation, we do not think that this is nor rigorous enough or could be misunderstood.

Page 9, Lines 250–252: The text states that δ18O is negatively correlated with elevation but then gives an "altitude gradient" of 0.24‰/100 m. If the gradient is defined as the change in δ18O with increasing elevation, the sign should be negative. Please verify the original regression result.
Minus signs added in all instances for the reported and referenced gradients.

Page 10, Lines 276–280: The extremely low d-excess of the Olt River is attributed directly to "massive evaporation" from 19 upstream reservoirs, but no reservoir-isotope data or evaporation estimates are presented. Please provide quantitative evidence and a supporting source, or frame this interpretation as a hypothesis.
Changed to "*potentially resulting from the evaporation of the river's waters in the 19 reservoirs located upstream from the sampling site*"

Page 10, Lines 281–285: The approximately 0.8‰ depletion of river water relative to precipitation is interpreted as evidence for a larger winter snowmelt contribution. High-elevation precipitation, groundwater/baseflow contributions, and

differing water ages may all contribute to this offset. The difference should therefore not be attributed to snowmelt alone.
We also considered the differences (consistent through the stations) as to low for definite statements. Hence, in the text we clearly specify that this is a hypothesis (and observation) and also offer alternative ones:
“Compared to precipitation, $\delta^{18}O$ values in rivers are lower (on average by ~ 0.8 ‰), suggesting a higher contribution from wintertime snow to the discharge. However, contrary to this hypothesis, we also note that the precipitation sampling sites were generally positioned in the lower sections of the watersheds, and thus the $\delta_{prec.}$ were inherently high, thus potentially affecting the accuracy of the above mentioned observation.”

(c) Figures, formatting, notation, and references
Page 7, Figure 2: The station names on the x axis are crowded and difficult to read. Please use a consistent station-ordering scheme and explain in the caption the definitions of the box, whiskers, mean symbol, and outliers.
Done. The text now reads: “The boxes encompass the 1st (25th percentile) and 3rd (75 percentile) quartiles, the middle line indicates the median values, the vertical bars above and below the boxes show the 1.5 interquartile range and the asterisks indicate outliers.”

Page 8, Figure 3: The upper and lower panels use precipitation and river stations, respectively, with complex ordering and colors. The caption should report the numerical values represented by the red and black dashed lines and the sample sizes used to calculate them.
We have decided not to use the average values in our revisions of the ms, as these would not be relevant. We have modified the text and figures, accordingly.

Page 9, Figure 4: The figure lacks a color legend/color bar, so the numerical d-excess ranges represented by green, yellow, and red cannot be interpreted. The station abbreviations (BM, BN, BZ, etc.) are not defined, and the meaning of the numerous blank cells is also unclear. In its current form, the figure is not self-contained.
We added the legend (was missing by error), defined the abbreviations in Table 1 and modified the caption as follows: *”Figure 4. D-excess in precipitation at stations in Romania and Republic of Moldova. Stations in SW Romania (CV, DTS and TM) are shown in red and the autumn months are highlighted in black boxes. The bottom bar shows the range of d-excess values, between 0.32 and 21.7 ‰.”*

Page 9, Lines 240–243: The text states that PWLSR slopes are higher at 13 of 19 stations and lower at 5 of 19 stations, which accounts for only 18 stations. Please explain the result for the remaining station.
Corrected to 14, instead of 13.

Page 10, Lines 268–270: “The range of temporal variability (Fig. 5)” appears to cite the wrong figure. Figure 5 shows δ18O–δ2H cross-plots rather than temporal variability.
Typo. “temporal” removed

Page 11, Figure 5 caption: “Gârda (precipitation) and Arieș (river)” is inconsistent with the station names in Table 1, where the relevant high-elevation station is listed as Ghețar. Please clarify whether Gârda is a different station, a locality name, or an error.
Corrected to read “Ghetar” (“Ghetar” is a village in a larger commune, called “Garda”).

Pages 10–11: The text states, based on Figure 5, that river samples define lines with lower slopes and intercepts than the local meteoric water lines, but Figure 5 does not display river regression lines, regression equations, R2 values, LMWLs, or the GMWL. These elements should be added if this interpretation is to be supported directly by the figure.
Indeed, we did not. Added the regression lines (LMWLs) in order not to overcomplicate the figure (the distribution of datapoints “indicate” the LMWLs.) We added these in the revised figure.

Page 12, Figure 6: Precipitation and river δ18O, which have the same units, are plotted against different left and right y-axis ranges. This can visually exaggerate apparent synchrony. A common y-axis scale is preferable; if dual axes are retained, the different scales should be made explicit and the visual comparison should be accompanied by quantitative correlation/lag results.
We did not intend to exaggerate synchrony. *Au contraire*, we used the dual scales to better show the exact opposite in panels b and c, as an indication of how large rivers will not follow a simple “winter-low, summer- high” distribution of $\delta^{18}O$ values, similar to precipitation. This type of representation is common is literature.

Page 13: “5. Data availability” is followed directly by “Conclusions” without a section number. The numbering should be

made consistent, for example “6. Conclusions.”
Done, thank you for spotting this.

Page 13, Data availability: Only the Zenodo DOI is provided. For an ESSD data paper, this section should also summarize the number and format of files, main variables, time fields, missing-value encoding, QC flags, station metadata, and dataset version.
Here we followed the journals policy on reporting data.

Page 4, Line 115: Feher et al. (2024) is cited in the text, but no corresponding 2024 reference appears in the reference list on Pages 14–17; only Feher et al. (2021) is listed. The citation/reference entry must be added or corrected.
Added

Page 8, Lines 228–230: Forray et al. (2024) is cited in support of Mediterranean cyclones and moisture sources, but the cited reference is titled “Past anthropogenic impacts revealed by trace elements in cave guano,” which does not appear relevant to the claim. Please verify and replace this citation with the intended reference.
Leftover from a previous discussion that was later removed.

Page 15: The DOI URL for Forray et al. (2024) is written as “https://soi.org/...”. This should be corrected to “https://doi.org/...”.
Done

Page 16: The DOI given for Persoiu et al. (2019), “10.5194/cp-2018-161,” is the discussion/preprint DOI. Please verify the bibliographic record and use the final published reference consistently.
Done.

Page 2, around Line 53 uses IAEA/WMO (2025), whereas Page 6, Line 196 uses IAEA/WMO (2026), and the reference list presents the GNIP database with an access date. Please standardize the database citation year and access-date format throughout the manuscript.
Corrected.

Page 7, Lines 205–214: Several numerical expressions appear to contain editing errors, including the order in “maxima between -1.4 ... -5.7,” the extra full stop in “absolute range of 3.4. ‰,” and missing δ/isotope notation. These values should be checked against the source data and the paragraph should be carefully proofread.
“And” was added in the first expression and the extra full stop deleted in the second. When δ values are reported in text and subsequent parentheses, we used the ‰ sign only once.

Throughout the manuscript, δ18O, δ2H, d-excess, δprec, and δriv are not fully consistent in font, subscript/superscript formatting, and spacing. Please standardize the isotope notation according to the journal style.
We verified the text and made the two necessary corrections.

Author names in the citations and reference list are inconsistent in the use of diacritics and spelling (e.g., Persoiu/Perşoiu, Badaluta/Bădăluță, Ionita/Ioniță). Please verify each entry against the published source and use consistent spelling between the text and reference list.
We verified the text and made all necessary corrections.

DOI/URL formats are inconsistent in the reference list (e.g., https://doi.org versus http://dx.doi.org), and journal abbreviations and punctuation are not standardized. The reference list should be regenerated and then manually checked against the original publications.
We verified the text and made all necessary corrections.

All figures should be checked for self-contained readability. Each figure should clearly identify variables, units, colors/symbols, sample size, and missing-data information without requiring the reader to search the main text. Figures 4–6 currently lack several of these elements.
We verified the text and made all necessary corrections (see the relevant replies above).

Overall, the regional stable-isotope observations compiled in this manuscript have potential value, but the current submission has major deficiencies in several areas that are central to a data paper: data provenance and the boundary of originality, quality control and missing-data documentation, matching of precipitation and river observations in space and time, statistical reproducibility, and consistency between mechanistic interpretation and the strength of the evidence. Addressing these issues would require substantial reconstruction rather than routine revision. I therefore recommend rejection (Reject) of the manuscript in its present form.

See our reply at the top