



October 10, 2025

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Dear Editor Attila Demény,

With this cover letter, we are submitting the revised manuscript entitled, “**Seasonal patterns and diagnostic values of $\delta^2\text{H}$, $\delta^{18}\text{O}$, d-excess, and $\Delta^{17}\text{O}$ in precipitation over Seoul, South Korea (2016–2020)**”, for publication in *Earth System Science Data*. Based on the comments from the editor and the four reviewers, we have major changes of the manuscript, which are detailed below. Based on the comments from the editor and four reviewers, we have summarized the issues as following.

Reply to the comments by the reviewer 3

1. General Comments

Kim et al. present a unique data set of stable isotopes ($\delta^2\text{H}$, $\delta^{17}\text{O}$, $\delta^{18}\text{O}$, d-excess and $\Delta^{17}\text{O}$) of precipitation sampled bi-weekly between February 2016 and December 2020 in Seoul, South Korea. Such data sets can help to better constrain the drivers of isotope variability in precipitation, improve the interpretation of paleoclimate records, and tune isotope-enabled global climate models. In particular, data sets combining d-excess and ^{17}O -excess remain scarce so far. Therefore, the data set is new and will be useful for future studies. The data set is accessible, however, does not contain uncertainties for each variable. Also, no meteorological data is given in the file, where especially precipitation amounts, but also T and RH data would be useful for the interpretation of the data set and have been used in the manuscript. If these data were derived from a different data base, this should be mentioned in the data availability section.

Overall, the manuscript is clearly structured and well written. However, the methodological section needs more detail, some discussion points appear already in the results and interpretations are often not justified by data. The manuscript is worth publication in ESSD, but needs major revision as outlined below.

Response:

We sincerely thank the reviewer for the positive overall assessment and for recognizing the scientific value of the presented dataset, particularly its inclusion of the rare combination of $\delta^2\text{H}$, $\delta^{17}\text{O}$, $\delta^{18}\text{O}$, d-excess, and $\Delta^{17}\text{O}$.



In the revised version, we have substantially improved the completeness and transparency of the dataset. Uncertainty values (1σ) for each isotopic variable have been added, and the accompanying PANGAEA file now includes the relevant meteorological parameters (precipitation amount, air temperature, and relative humidity) derived from the Korea Meteorological Administration. These additions ensure that users can fully reproduce the data–model comparisons and correlation analyses presented in the manuscript.

To improve the paper’s structure, we have reorganized the Methods section by adding a new subsection titled “Data treatment methods,” which explains how precipitation-weighted monthly means and regression analyses (LMWL and $\delta^{17}\text{O}$ – $\delta^{18}\text{O}$ relationships) were calculated. The Results and Discussion sections have been clearly separated, with interpretative content moved to the Discussion to align with ESSD’s data-centric style. The Introduction, Abstract, and Summary have also been revised to better reflect the dataset’s scope and significance without overstating its spatial representativeness.

We appreciate the reviewer’s encouragement to pursue this as a data-focused contribution to ESSD. We believe that the revised manuscript now provides a more comprehensive, well-documented, and clearly structured data descriptor, aligning with the journal’s standards while maintaining the scientific relevance of one of the few long-term triple-oxygen-isotope precipitation records in East Asia.

2. Specific Comments

Methodology

Missing description of the meteorological data.

Response:

We thank the reviewer for noting the missing description of the meteorological data used in this study. In the revised manuscript, we will clarify the source, resolution, and processing method of the meteorological data. The meteorological variables, air temperature, relative humidity, and precipitation amount, were obtained from the Korea Meteorological Administration (KMA), which provides quality-controlled hourly observation data at the Seoul weather station, located within 2 km of our sampling site.

For each sampling interval, the hourly meteorological data corresponding to periods when precipitation occurred were integrated (time-weighted) to calculate representative mean values of air temperature, relative humidity, and total precipitation for that collection period. In the revised Methods section, we will add the following description:

“Meteorological data, including air temperature, relative humidity, and precipitation amount, were obtained from the Korea Meteorological Administration (KMA) based on hourly observations at the Seoul station (<https://data.kma.go.kr>).



For each biweekly sampling interval, the hourly data corresponding to periods with precipitation were integrated to derive time-weighted mean temperature and humidity and cumulative precipitation, which represent the meteorological conditions relevant to each collected sample.”

This addition will ensure that the data source and processing procedure are described transparently and reproducibly, addressing the reviewer’s concern about the meteorological dataset.

Missing description of how secondary order parameters (d-excess and 17O-excess) are calculated.

Response:

We thank the reviewer for this helpful comment regarding the description of how the secondary-order isotope parameters (d-excess and 17O-excess) are calculated. We would like to clarify that both parameters are already defined in the Introduction, in the context of explaining the physical meaning of each isotope variable and their relevance to atmospheric processes.

In particular, the Introduction provides the following definitions:

1. d-excess (Dansgaard, 1964) = $\delta^2\text{H} - 8 \cdot \delta^{18}\text{O}$, which represents the kinetic fractionation occurring during evaporation and is sensitive to relative humidity and sea surface temperature at the moisture source.
2. 17O-excess ($\Delta^{17}\text{O} = \delta^{17}\text{O} - 0.528 \cdot \delta^{18}\text{O}$), following Luz and Barkan (2010), which describes the logarithmic deviation from the global meteoric water line in the $\delta^{17}\text{O}$ – $\delta^{18}\text{O}$ space and serves as an indicator of non-equilibrium isotopic processes such as vapor mixing and supersaturated condensation.

These definitions were included in the Introduction deliberately, because they form part of the theoretical background for the study — that is, they describe what these parameters represent and why they are physically meaningful, not merely how they are computed. For this reason, we consider that repeating the same equations in the Methods section would be redundant and would interrupt the logical flow between the theoretical framework and the analytical procedures. However, we fully acknowledge that readers should be able to locate these definitions easily when consulting the Methods.

To address this, we will revise the Methods to include a clear cross-reference to the Introduction, so that the calculation procedure is explicitly linked to the previously defined equations. The new sentence will read:

“The calculation of the secondary-order isotope parameters, d-excess and 17O-excess ($\Delta^{17}\text{O}$), follows the standard definitions described in the Introduction (Dansgaard, 1964; Luz and Barkan, 2010), where their physical meaning and equations are presented.”



This revision will ensure that the Methods section remains concise and non-repetitive, while still providing the reader with a direct reference to the equations and background already explained earlier in the manuscript. We believe this approach maintains scientific clarity, avoids redundancy, and keeps the paper well-structured by distinguishing between the conceptual definitions (Introduction) and the analytical workflow (Methods).

Give analytical precision for d-excess and 17O-excess.

Response:

We thank the reviewer for requesting the analytical precisions for d-excess and 17O-excess ($\Delta^{17}\text{O}$). In the revised manuscript, we will explicitly report the long-term reproducibility (1σ) for both composite parameters, derived from repeated measurements of our in-house laboratory standard (STYX). For d-excess, we will report the empirical 1σ reproducibility obtained from repeated STYX measurements as the primary uncertainty, and also provide a conservative propagated uncertainty estimated from the long-term precisions of $\delta^2\text{H}$ ($\pm 0.10\text{‰}$) and $\delta^{18}\text{O}$ ($\pm 0.07\text{‰}$).

The propagated 1σ uncertainty is calculated as:

$$\sigma_{\text{d-excess}} = \sigma_{\delta^2\text{H}}^2 + (8 * \sigma_{\delta^{18}\text{O}})^2 \approx \pm 0.6 \text{ (‰)}.$$

For 17O-excess ($\Delta^{17}\text{O}$), we will report a reproducibility of ± 9 per meg (1σ), based on one-year repeated measurements of the in-house standard STYX under the same WS-CRDS analytical configuration (Kim et al., 2022). In the revised Methods section, we will add the following sentences:

“The analytical precision for d-excess will be reported as the 1σ empirical reproducibility from repeated measurements of the in-house standard STYX (Table Sx). For reference, a propagated uncertainty using the long-term precisions of $\delta^2\text{H}$ ($\pm 0.10\text{‰}$) and $\delta^{18}\text{O}$ ($\pm 0.07\text{‰}$) will also be provided as $\pm 0.6\text{‰}$ (1σ).

The analytical reproducibility for 17O-excess ($\Delta^{17}\text{O}$) will be reported as ± 9 per meg (1σ) based on one-year repeated measurements of the STYX standard under the same WS-CRDS setup (Kim et al., 2022).”

These additions will clarify the quantitative uncertainties for both derived parameters and will ensure consistency between our composite isotope metrics and the long-term reproducibility framework already established for $\delta^2\text{H}$, $\delta^{18}\text{O}$, and $\delta^{17}\text{O}$.

Details on the comparison of the GSM with observational data presented in the discussion section are missing in the methods. For example, model input parameters, but also more details about the model simulations should be given. I think that this model-data comparison could be a bit over the scope of this journal. Instead of adding more details to the model, the authors may consider removing this part from the manuscript.

Response:



We sincerely thank the reviewer for the thoughtful and constructive comment regarding the model–data comparison presented in Section 4.3. We fully understand the reviewer’s concern that this comparison may appear insufficiently described in the Methods section and could potentially extend beyond the primary data-focused scope of ESSD.

Our intention in including this section was not to present a comprehensive modeling analysis, but rather to provide a brief illustrative example of how the new Seoul precipitation isotope dataset can be utilized for benchmarking isotope-enabled models. The Iso-GSM results were incorporated to demonstrate the potential of the dataset as a validation resource for model outputs such as $\delta^{18}\text{O}$ and d-excess, rather than to perform a full model evaluation or sensitivity analysis. In the revised manuscript, we have carefully rephrased the text to clarify this intent.

We now explicitly state that the Iso-GSM comparison is presented only as a contextual example of possible data applications, emphasizing that the main focus of this study remains on the observational dataset itself. To keep the paper concise and within the ESSD data-descriptor format, we will not include additional technical details on the model configuration or input parameters; instead, we will cite Yoshimura et al. (2008) as the authoritative reference for the Iso-GSM setup and physics.

We have also slightly adjusted the section to highlight that, while $\delta^{18}\text{O}$ and d-excess comparisons with model outputs are well established, the integration of triple oxygen isotope data ($\Delta^{17}\text{O}$) provides a new opportunity for model benchmarking in the future. By positioning the Iso-GSM results as a complementary example rather than a central analysis, we ensure that the manuscript remains true to the ESSD’s mission of describing high-quality, reusable datasets.

We greatly appreciate the reviewer’s comment, which helped us improve the clarity and focus of this section. The revised version now more clearly communicates that the model–data comparison is intended to illustrate the practical value and potential applications of the Seoul dataset, while keeping the manuscript fully aligned with ESSD’s data-oriented scope.

In line 99 the authors state that precipitation has been collected from January 2016 to December 2020. However, the data set starts in February 2016. This should be corrected.

Response:

We thank the reviewer for carefully checking the dataset period. We confirm that precipitation sampling officially started in February 2016, because no measurable precipitation occurred in January 2016. In the revised manuscript, we will correct the description to state that sampling was conducted from February 2016 to December 2020, to ensure consistency with the dataset. This revision will make the time coverage in the text fully consistent with the actual data record.



Also, they state that sampling was performed bi-weekly. They should make clear that their interpretation is based on amount-weighted monthly values as bi-weekly data is not presented.

Response:

We thank the reviewer for this important clarification. Indeed, precipitation samples were collected on a biweekly (approximately 14-day) basis, but all statistical analyses and figures were based on precipitation-weighted monthly mean values derived from those biweekly samples. Each biweekly sample represents the cumulative precipitation during the collection period, and for months with two biweekly samples, the isotope values were combined using precipitation amount as a weighting factor to obtain the monthly weighted mean ($\delta^2\text{H}_{\text{wm}}$, $\delta^{18}\text{O}_{\text{wm}}$, $\delta^{17}\text{O}_{\text{wm}}$).

The same weighting procedure was applied to compute monthly d-excess and $\Delta^{17}\text{O}$, ensuring that each month's mean reflects the relative contribution of precipitation amount from each biweekly sample. In the revised Methods section, we will add the following clarification:

“Although precipitation samples were collected at approximately 14-day intervals, all seasonal and intermonthly analyses in this study are based on precipitation-weighted monthly mean values.

For months with two biweekly samples, the isotope values were weighted by their corresponding precipitation amounts to derive monthly means (δ_{wm}).”

This revision will clarify that while the physical sampling resolution was biweekly, all interpretations and figures are based on precipitation-weighted monthly averages, ensuring consistency between the data presentation and the described analytical approach.

Results

The results are mixed up with discussion points. A better separation of both is needed. Discussion parts that should be shifted to the discussion section: Line 163-164, Line 169-171, Line 173-174, Line 178-179, Line 189-196

Response:

We thank the reviewer for reiterating this important point.

We agree that several interpretative sentences in the Results section should be moved to the Discussion. In the revised manuscript, we will re-structure the two sections to ensure a strict separation between observation and interpretation. Specifically, we will shift the following passages from Results to Discussion: Lines 163–164, 169–171, 173–174, 178–179, and 189–196.

We will retain in Results only the descriptive statements (numerical ranges, observed seasonal patterns, and figure references), and move all process-based explanations,



literature comparisons, and causal language to the Discussion. This revision will improve clarity, readability, and alignment with journal style, and will avoid any overlap between results presentation and interpretative content.

Discussion

Interpretations in the discussion section are often not justified by the presented data. For example, in the first discussion section, correlation between isotope data and local meteorological parameters are investigated. For example, line 204-206 that “lower relative humidity and temperature at the moisture source enhance kinetic fractionation during evaporation, thereby increasing d-excess” However, no information on relative humidity and temperature at the moisture source region is provided nor differences between different moisture source regions are discussed. – Further, more explanation is need in Line 211-212. Here, the authors state that the negative correlation between d-excess and local temperature is controlled by the moisture sources and isotope fractionation during precipitation. This is very general too. Can you explain how this correlation relates to these factors? Also more explanation and justification is needed in line 215-217 and line 222-224.

Response:

We thank the reviewer for this important comment highlighting the need for clearer justification of our interpretations in the Discussion section. We agree that several statements were written too generally and lacked explicit reference to supporting data. In the revised manuscript, we will carefully rephrase these parts to make the interpretations more balanced and to acknowledge the limitations of the available data.

In lines 204–206, we will clarify that the statement about “lower relative humidity and temperature at the moisture source enhancing kinetic fractionation” refers to the conceptual framework of Merlivat & Jouzel (1979) and Uemura et al. (2008). Because direct meteorological data from the moisture source regions are not available in this study, we will explicitly note that this explanation represents an inferred mechanism rather than a measured relationship.

In lines 211–212, we will expand the explanation of the negative correlation between d-excess and local temperature, specifying that higher local temperature is usually accompanied by higher relative humidity, which reduces kinetic fractionation during precipitation and lowers d-excess. Conversely, lower temperatures are often linked with drier boundary-layer conditions and enhanced evaporation or sub-cloud re-evaporation, leading to higher d-excess.

In lines 215–217 and 222–224, we will provide additional mechanistic details describing how precipitation amount, moisture-source humidity, and sub-cloud processes jointly control $\delta^{18}\text{O}$ and d-excess, distinguishing between large-scale rainout effects and local re-evaporation.



These revisions will clarify that the interpretations are physically motivated but remain qualitative due to the absence of direct source-region meteorological data, ensuring that the discussion is both accurate and appropriately cautious.

3. Line-by-Line comments

Line 45-47: $\delta^{18}\text{O}$ and $\delta^2\text{H}$ are influenced by both equilibrium and kinetic fractionation and thus it is difficult to disentangle these two. The secondary parameters, d-excess and 17O-excess are primarily sensitive to kinetic fractionations and thus help to disentangle them. Clarify this in the text.

Response:

We thank the reviewer for this helpful comment. We agree that both $\delta^{18}\text{O}$ and $\delta^2\text{H}$ reflect the combined effects of equilibrium and kinetic isotope fractionation and that it is often difficult to separate the two. The secondary parameters—d-excess and 17O-excess ($\Delta^{17}\text{O}$)—are indeed more sensitive to kinetic effects and therefore provide additional diagnostic power for distinguishing non-equilibrium processes from equilibrium ones. In the revised manuscript, we will modify the relevant paragraph in the Introduction to clarify this point. The revised text will read:

“The stable isotopic composition of precipitation ($\delta^{18}\text{O}$ and $\delta^2\text{H}$) is governed by both equilibrium and kinetic fractionation during phase changes such as evaporation and condensation, making it difficult to isolate the relative contributions of each process. Secondary parameters, namely deuterium excess (d-excess = $\delta^2\text{H} - 8 \times \delta^{18}\text{O}$; Dansgaard, 1964) and 17O-excess ($\Delta^{17}\text{O} = \delta^{17}\text{O} - 0.528 \times \delta^{18}\text{O}$; Luz and Barkan, 2010), are primarily sensitive to kinetic fractionation processes and thus help to disentangle them. While $\delta^{18}\text{O}$ and $\delta^2\text{H}$ mainly record equilibrium fractionation, d-excess and 17O-excess reflect deviations from equilibrium associated with non-steady-state evaporation, vapor mixing, or supersaturation during cloud formation (Gat, 1996; Uemura et al., 2008).”

This revision will clarify how the secondary isotope parameters complement $\delta^{18}\text{O}$ and $\delta^2\text{H}$ and will improve the conceptual link between equilibrium and kinetic fractionation in the introductory framework of the manuscript.

Line 47-49: $\delta^{17}\text{O}$ has not been introduced yet. Consider adding a sentence on the value of additional analysis of the 17O isotope before.

Response:

We thank the reviewer for this helpful suggestion. We agree that $\delta^{17}\text{O}$ should be introduced briefly before discussing the 17O-excess ($\Delta^{17}\text{O}$) parameter, so that readers unfamiliar with triple oxygen isotope analysis understand the value of including the 17O measurement. In the revised manuscript, we will add a concise sentence immediately before the introduction of $\Delta^{17}\text{O}$ to clarify the significance of $\delta^{17}\text{O}$. The revised passage will read as follows:



"In addition to $\delta^{18}\text{O}$ and $\delta^2\text{H}$, $\delta^{17}\text{O}$ can also be measured. Because $\delta^{17}\text{O}$ behaves almost proportionally to $\delta^{18}\text{O}$ under equilibrium conditions, simultaneous measurement of the two enables the quantification of subtle deviations arising from kinetic and non-equilibrium processes, forming the basis of triple oxygen isotope ($\delta^{17}\text{O}$, $\delta^{18}\text{O}$ and $\delta^2\text{H}$) studies (Angert et al., 2004; Luz and Barkan, 2010)."

This addition will clarify the rationale for analyzing $\delta^{17}\text{O}$ and will provide a logical transition to the subsequent introduction of the 17O-excess ($\Delta^{17}\text{O}$) parameter.

Line 79: Be more specific: 5-year record of monthly triple oxygen and hydrogen precipitation isotope data.

Response:

We thank the reviewer for this helpful suggestion. We agree that the description of the dataset should be made more specific to clearly indicate both the temporal coverage and the measured isotopic parameters. In the revised manuscript, we will modify the sentence to read:

"A high-temporal-resolution, 5-year record of monthly triple oxygen and hydrogen isotopes in precipitation ..."

This revision will clarify that the dataset spans five years and includes simultaneous measurements of $\delta^2\text{H}$, $\delta^{18}\text{O}$, and $\delta^{17}\text{O}$, providing a monthly-resolved record of triple-isotope precipitation composition. It also emphasizes that the dataset represents a continuous, high-temporal-resolution record suitable for both hydrological and climatological applications, aligning with the reviewer's suggestion for greater specificity.

Section 2: There is no reference to Figure 2 in the main text. This could be added to a sentence describing the meteorological data.

Response:

We thank the reviewer for pointing out that Figure 2 was not referenced in the main text. In the revised manuscript, we will add a citation to Figure 2 within the paragraph describing the meteorological data to ensure that readers can easily connect the figure with the corresponding text.

The revised sentences will read as follows:

"Meteorological data, including air temperature, relative humidity, and precipitation amount, were obtained from the Korea Meteorological Administration (KMA) based on hourly observations at the Seoul station <https://www.weather.go.kr/w/index.do>). The average monthly precipitation amount (grey bars) and average monthly temperature (black-lined boxes) for the city of Seoul, based on these KMA data, are shown in Fig. 2."



This addition will clarify the source and representation of the meteorological dataset and will ensure that Figure 2 is explicitly referenced in the main text, as suggested by the reviewer.

Line 97-98: I don't expect this phrase at the end of the paragraph. It should be introduced more at the beginning of the paragraph and then all four seasons need to be described. For now, only summer and winter are described, but which conditions persist in spring and autumn?

Response:

We thank the reviewer for this helpful comment. We agree that the description of the climatic context should appear earlier in the paragraph and that all four seasons should be described, not only summer and winter. In the revised manuscript, we will move the seasonal-climate description to the beginning of the paragraph and expand it to include spring and autumn conditions.

The revised passage will read as follows:

“The Korean Peninsula experiences a temperate monsoon climate characterized by four distinct seasons. Summer (June–August) is dominated by the East Asian monsoon, bringing warm and humid air masses and intense rainfall. Winter (December–February) is cold and dry under the influence of the Siberian High. Spring (March–May) and autumn (September–November) represent transitional seasons with variable air-mass influences: spring is typically mild and dry, while autumn is cooler and affected by typhoons and tropical cyclones that deliver heavy rainfall.”

This revision will provide a complete and logically ordered description of the seasonal meteorological conditions and will improve the flow and readability of the paragraph.

Line 137: Why a sine function has been fitted to the data? This should be explained in the text.

Response:

We thank the reviewer for this comment. In accordance with suggestions from another reviewer, the sentence referring to the sine-function fit in Figure 3 has been removed in the revised manuscript. As a result, no sine-function fitting is presented in the updated version, and this point is no longer applicable.

Line 147: Which other moisture sources than the ocean are important. Specify this.

Response:

We thank the reviewer for this comment. The phrase “other moisture sources” indeed needs clarification. In the revised manuscript, we will specify that, while the surrounding oceans (the Yellow Sea, East China Sea, and western North Pacific) constitute the dominant moisture sources for Korean precipitation, additional



contributions arise from continental air masses and local moisture recycling. Specifically, during winter and early spring, dry continental air masses originating from northern China and Mongolia occasionally carry vapor that has undergone substantial rainout or isotopic modification over land.

In contrast, local evaporation and evapotranspiration from terrestrial surfaces over the Korean Peninsula contribute to moisture recycling during transitional seasons, particularly in spring and early autumn. In the revised manuscript, we will modify the relevant sentence to read as follows:

“Although the surrounding oceans (the Yellow Sea, East China Sea, and western North Pacific) are the dominant moisture sources, additional contributions from continental air masses and local moisture recycling over the Korean Peninsula also influence the isotopic composition of precipitation.”

This clarification will specify which non-oceanic sources are considered and will enhance the physical interpretation of the regional hydrological processes.

Line 149-151: I was first confused by the “unlike” but looking at the figure I understood that the difference between ^{17}O -excess and d-excess is that ^{17}O -excess is highest in spring, while d-excess is highest in winter. Can you make this clearer in the text. Also, quantify give values for ^{17}O -excess’ seasonal variability (highest value, lowest value).

Response:

We thank the reviewer for this constructive comment. We agree that the description of the contrasting seasonal behaviors of ^{17}O -excess and d-excess should be stated more clearly and supported with quantitative values. In the revised manuscript, we will rephrase the relevant sentences to specify both the timing and magnitude of ^{17}O -excess variability, and to make the comparison with d-excess explicit.

The revised text will read as follows:

“Unlike d-excess, which peaked in winter (median $\approx 17\text{‰}$) and reached its lowest values in summer (median $\approx 6\text{‰}$), ^{17}O -excess displayed a distinct seasonal pattern, being highest in spring (up to ≈ 40 per meg) and lowest in summer (down to ≈ 10 per meg). This contrast indicates that ^{17}O -excess and d-excess are influenced by different kinetic fractionation processes operating under distinct seasonal humidity regimes.”

This revision will clarify the meaning of the “unlike” phrasing, will quantify the seasonal amplitude of ^{17}O -excess, and will ensure that the contrasting behaviors of the two secondary isotope parameters are immediately evident to readers.

Line 153-155: During which process kinetic fractionation is more pronounced? As I understood from the previous, this is due to evaporation from the ocean. Is this correct? Be more specific here.

**Response:**

We thank the reviewer for this insightful question. The kinetic fractionation referred to in this sentence occurs primarily during evaporation at the ocean surface, where low relative humidity and strong wind conditions favor non-equilibrium isotope exchange between liquid water and vapor.

Under these conditions, lighter isotopologues ($^1\text{H}_2^{16}\text{O}$) preferentially escape from the surface layer, while heavier isotopologues (containing ^2H or ^{18}O and ^{17}O) diffuse more slowly, producing kinetic fractionation and an increase in both d-excess and ^{17}O -excess in the resulting vapor (Merlivat and Jouzel, 1979; Luz and Barkan, 2010). In the revised manuscript, we will rephrase the sentence to make this process explicit. The revised text will read:

"The increase in ^{17}O -excess during winter and early spring suggests that kinetic fractionation, primarily occurring during oceanic evaporation under low relative humidity, becomes more pronounced when vapor is sourced from drier air masses such as continental or high-latitude oceanic regions."

This revision will clarify that the enhanced kinetic fractionation refers specifically to non-equilibrium evaporation at the ocean surface, not to condensation or post-condensational processes, and will improve the physical accuracy of the interpretation.

Line 161: Can you add uncertainties for the slope and the intercept of the GMWL?

Response:

We appreciate the comment. We understand that the request refers to the regression line derived from our dataset (i.e., the LMWL) rather than the canonical GMWL. In the revised manuscript, we will report the 1σ standard errors for our LMWL parameters as follows:

$$\delta^2\text{H} = (7.95 \pm 0.16) \times \delta^{18}\text{O} + (10.0 \pm 1.3), R^2 = 0.98.$$

These values are the standard errors of the OLS fit to our data and are consistent with typical uncertainties reported in the literature. For clarity, we will retain the GMWL (Craig, 1961) as a canonical reference ($\delta^2\text{H} = 8 \times \delta^{18}\text{O} + 10$), noting that there is no single global standard error because uncertainties depend on the specific compilation and method (e.g., Crawford et al., 2014).

This revision will make our local regression fully documented with uncertainties while keeping the GMWL as a reference line.

Line 167-168: Winter precipitation is mainly in the form of snow? Do you see differences between snow and rain samples?

Response:



We thank the reviewer for the question regarding the precipitation phase during winter. In our dataset, precipitation was collected on a biweekly cumulative basis, which means that winter samples often include a mixture of both snowfall and rainfall events. Because of this integrated sampling design, it is not possible to rigorously separate snow-only and rain-only isotope signatures.

However, we acknowledge that the isotopic variability observed in winter—particularly the enhanced dispersion in $\Delta^{17}\text{O}$ and d-excess—may partly reflect the influence of ice–vapor equilibrium fractionation during snow formation. We will clarify this in the revised manuscript by noting that mixed-phase (rain–snow) precipitation is likely during winter and that this may contribute to the distinct isotopic characteristics of cold-season samples.

Line 180: the 17O-excess is defined based on the prime values of $\delta^{17}\text{O}$ and $\delta^{18}\text{O}$. This should be defined in the methods and clarified in the main text and the figures.

Response:

We thank the reviewer for this comment. The definition of 17O-excess ($\Delta^{17}\text{O}$) based on the prime logarithmic δ -values ($\delta^{17}\text{O}$ and $\delta^{18}\text{O}$) has been clarified in the revised manuscript. The corresponding explanation has been added in the Methods section, and the same notation is now consistently used throughout the main text and all figure captions.

Line 187: You should refer here to Figure 4.

Response:

We thank the reviewer for noting that the reference to Figure 4 is missing in this sentence. In the revised manuscript, we will add an explicit citation to Figure 4 in the line presenting the $\delta^{17}\text{O}$ – $\delta^{18}\text{O}$ regression, since this relationship is displayed in Figure 4B. The revised sentence will read as follows:

“A linear regression applied to the full dataset results in $\delta^{17}\text{O} = 0.528 \times \delta^{18}\text{O} + 0.0105$ ($R^2 = 1.00$), confirming the strong linear correlation between $\delta^{17}\text{O}$ and $\delta^{18}\text{O}$ characteristic of mass-dependent fractionation in meteoric waters (Fig. 4B).”

This addition will directly link the text to the figure where the regression is illustrated, improving clarity and consistency between the description and the visual presentation of the data.

Figure 4: What is shown in B? It does not make any sense to me. Suggestion to illustrate 17O-excess vs d'18O as no difference will be visible in d'17O vs d'18O, when plotted to scale. The purple line is not the GMWL, should be dashed line, I guess.

Response:



We thank the reviewer for this valuable feedback on Figure 4B. We agree that plotting $\delta^{17}\text{O}$ against $\delta^{18}\text{O}$ does not provide much visual distinction among seasonal regressions, because the range of δ' values is very small when plotted to scale. In the revised manuscript, we will replace the $\delta^{17}\text{O}$ – $\delta^{18}\text{O}$ panel (Figure 4B) with a $\Delta^{17}\text{O}$ vs. $\delta^{18}\text{O}$ plot, following the reviewer's suggestion. This new representation will better illustrate the small but systematic differences among the seasonal datasets and will more directly show the variation in 17O-excess. In addition, we will modify the line style of the GMWL reference in both panels to a dashed purple line to clearly distinguish it from the seasonal regression lines.

The revised figure caption will read:

“(A) Relationships between $\delta^2\text{H}$ and $\delta^{18}\text{O}$ showing the LMWL and seasonal regressions. (B) Relationships between $\Delta^{17}\text{O}$ and $\delta^{18}\text{O}$ illustrating the seasonal variability of 17O-excess. The dashed purple line indicates the Global Meteoric Water Line (GMWL).”

These revisions will improve the clarity and physical interpretability of Figure 4, making the seasonal differences in triple oxygen isotope relationships more evident.

Line 206: “mixing” of what? Air masses?

Response:

We thank the reviewer for asking for clarification about the meaning of “mixing.” In this context, “mixing” refers to the moisture mixing between different air masses or vapor sources in the lower troposphere rather than mixing between individual raindrops or phases. In the revised manuscript, we will clarify this by rephrasing the sentence as follows:

“These relationships can be attributed to a combination of factors: lower relative humidity and temperature at the moisture source enhance kinetic fractionation during evaporation, thereby increasing d-excess (Merlivat and Jouzel, 1979; Uemura et al., 2008), while locally, higher temperatures and humidity may promote re-evaporation and the mixing of moist and dry air masses, which reduce d-excess (Steen-Larsen et al., 2014).”

This revision will clarify that the term “mixing” refers specifically to moisture exchange between different air masses in the lower atmosphere, improving the physical accuracy of the explanation.

Line 208: This is very general. Can you name the multiple meteorological factors that are interacting?

Response:

We thank the reviewer for this valuable comment. We agree that the phrase “multiple interacting meteorological factors” was too general and requires



clarification. In the revised manuscript, we will specify the main factors involved in modulating isotope–meteorology relationships, including air temperature, relative humidity, wind direction and air-mass pathways, precipitation amount and intensity, and cloud microphysical processes such as partial re-evaporation. The revised sentence will read:

“The negative correlation with precipitation may reflect the amount effect, but is better interpreted as the result of multiple interacting meteorological factors—such as variations in temperature, relative humidity, air-mass trajectories, precipitation intensity, and microphysical processes within clouds—that together influence the isotopic composition of precipitation (Holmes et al., 2024).”

This revision will clarify which specific meteorological variables are included under “multiple interacting factors” and will strengthen the physical basis of the interpretation.

Line 215: lower $\delta^{18}\text{O}$ values compared to what? Compared to other months of the year? Is this a rainout effect or an amount effect?

Response:

We thank the reviewer for this helpful clarification. We agree that the phrase “lower $\delta^{18}\text{O}$ values” should specify the comparison basis and the dominant controlling process. In the revised manuscript, we will clarify that summer $\delta^{18}\text{O}$ values are lower than in other months of the year, mainly due to the amount effect associated with prolonged monsoon rainfall and successive condensation (rainout) processes within marine air masses. The revised sentence will read:

“The relatively low $\delta^{18}\text{O}$ values observed in summer, compared with other months of the year, primarily reflect the amount effect associated with prolonged monsoon precipitation and successive rainout within moisture-rich air masses of marine origin.”

This revision will clarify both the comparison (summer vs. other months) and the underlying process (amount effect rather than an unspecified general decrease), ensuring that the interpretation is physically consistent and unambiguous.

Line 244-246: This is referring to evaporation from the ocean or re-evaporation of precipitation? Specify!

Response:

We thank the reviewer for this helpful comment. The $\Delta^{17}\text{O}$ –d-excess slopes discussed here primarily reflect kinetic fractionation during ocean-surface evaporation under low relative-humidity conditions, as described in Landais et al. (2010) and Li et al. (2015), rather than re-evaporation of falling precipitation. In the revised manuscript, we will clarify this by rephrasing the sentence as follows:



"The slopes observed between $\Delta^{17}\text{O}$ and d-excess (0.7–2.0 per meg per ‰) correspond to kinetic fractionation occurring mainly during ocean-surface evaporation under low relative-humidity conditions, consistent with conceptual and field-based estimates for oceanic moisture sources (Landais et al., 2010; Li et al., 2015)."

This revision will specify that the kinetic processes responsible for the observed relationship are linked to oceanic evaporation at the moisture source, not to sub-cloud or raindrop re-evaporation, thereby improving the physical accuracy of the explanation.

Line 249: evaporation of what? Precipitation?

Response:

We thank the reviewer for asking for clarification regarding the term "evaporation." In this context, "evaporation" refers to ocean-surface evaporation at the moisture source, where kinetic fractionation is enhanced under low relative-humidity conditions, rather than to re-evaporation of falling precipitation. In the revised manuscript, we will revise the sentence to read:

"In contrast, in winter precipitation, no statistically significant correlation was observed between $\Delta^{17}\text{O}$ and either $\delta^{18}\text{O}$ or d-excess, suggesting that kinetic fractionation associated with ocean-surface evaporation exerts little influence during this season."

This change will clarify that the "evaporation" mentioned here denotes evaporation from the ocean surface (the moisture source) and not sub-cloud or raindrop re-evaporation, improving the precision and physical accuracy of the statement.

3. Technical comments

Throughout the manuscript. The unit of 17O-excess is per meg not per mil. Please correct in text and in figures.

Response:

We thank the reviewer for this important correction. We agree that the unit of 17O-excess ($\Delta^{17}\text{O}$) should be expressed in per meg (10^{-6}) rather than per mil (‰). In the revised manuscript, we have corrected this throughout the text, tables, and figure captions, ensuring that all instances of $\Delta^{17}\text{O}$ now use the correct unit of per meg. We also checked that axis labels and legends in all figures have been updated accordingly. This correction ensures consistency with the standard convention used in triple-oxygen-isotope studies (Luz and Barkan, 2010; Aron et al., 2021).

Line 37-38: repetition of the previous sentence. Consider removing it.

**Response:**

We agree that the phrase describing isotopic fractionation during phase changes was repeated in consecutive sentences. In the revised manuscript, we have removed the repetitive sentence to avoid redundancy and have streamlined the paragraph to maintain a concise and logical flow. The revised text now reads:

“The stable isotope composition of precipitation reflects isotopic fractionation during phase changes such as evaporation, condensation, and precipitation formation, with the strength of fractionation varying according to temperature, relative humidity, and precipitation amount (Conroy et al., 2016; Craig and Gordon, 1965; Gat, 1996). Two well-known relationships—the temperature effect, where colder temperatures lead to lower $\delta^{18}\text{O}$ and $\delta^2\text{H}$ values, and the amount effect, where increased rainfall results in isotope depletion—have been widely observed in various climate regimes (Araguás-Araguás et al., 1998; Dansgaard, 1964).”

This revision removes the repetition noted by the reviewer and improves the clarity and coherence of the introductory paragraph.

Line 95: Korean Peninsula

Response:

We thank the reviewer for noticing this detail. The term has been corrected to “Korean Peninsula” in the revised manuscript.

Line 177: Repetition of slope and intercept not necessary here. Remove.

Response:

We thank the reviewer for this comment. The repeated mention of the slope and intercept values has been removed in the revised manuscript to avoid redundancy and improve readability.

Line 258-264: Repetition of previous paragraph. Remove.

Response:

We thank the reviewer for pointing out the potential repetition in the discussion of $\Delta^{17}\text{O}$ behavior. In the revised manuscript, we have reorganized this section to remove overlapping statements and to streamline the interpretation. The revised text now discusses the wintertime $\Delta^{17}\text{O}$ variability and its sensitivity to vapor mixing and surface recycling only once, followed by a concise summary emphasizing the overall seasonal utility of $\Delta^{17}\text{O}$ in combination with $\delta^{18}\text{O}$ and d-excess.



These revisions eliminate redundancy, clarify the logical flow between the seasonal observations and the broader implications, and improve the readability of the Discussion section.

The Summary should be stated before the data availability statement, isn't it?

Response: We will adjust the manuscript structure accordingly, placing the Data Availability section after the Summary section in the revised version.

Thank you very much for your time, effort, and patience in handling our manuscript. We look forward to your favorable consideration and to the opportunity for publication in Earth System Science data.

Sincerely,
Jeonghoon Lee