October 10, 2025

Jeonghoon Lee, Ph. D

Professor
Dept. of Science Education
Ewha Womans University
Seoul 03760, Korea
Email: jeonghoon.d.lee@gmail.com
Tel: +82-2-3277-3794

Dear Editor Attila Demény,

With this cover letter, we are submitting the revised manuscript entitled, "**Seasonal patterns and diagnostic values of δ^2H, $\delta^{18}O$, d-excess, and $\Delta'^{17}O$ 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 2

1. General Comments

In this paper, the authors presented precipitation hydrogen and triple oxygen isotope data of precipitation from South Korea and made some exploratory analysis on these data. I recognize that the authors have made great efforts to collect samples and data and put together a manuscript. However, I feel that it does fit with the scope of journal. The ESSD is a high-impact journal publishing flagship datasets for various applications with broad interest. Although it is indeed contributing to the emerging triple oxygen isotope study, this dataset does not make a significant contribution to the progress of this field. I suggest publishing the data in a substantially revised manuscript on a more specialized journal.

Response:

We sincerely thank the reviewer for the thoughtful evaluation and for recognizing the effort invested in compiling this multi-year triple-oxygen-isotope dataset.

We fully understand the reviewer's concern that ESSD typically publishes datasets of broad spatial coverage and global relevance.

Nevertheless, we would like to emphasize that long-term, high-quality triple-oxygen-isotope records remain rare in East Asia, and this dataset contributes to filling that regional gap by providing a well-documented and openly accessible reference record from the Eastern Asia.

While the spatial coverage is limited to a single site, the dataset spans five consecutive years of biweekly sampling, includes all major isotope parameters (δ^2H,

$\delta^{17}O$, $\delta^{18}O$, d-excess, and $\Delta'^{17}O$), and has been archived on PANGAEA with detailed metadata and uncertainty reporting.

Such comprehensive datasets from the East Asian monsoon region are still scarce and can serve as valuable benchmarks for isotope-enabled climate model validation, GNIP network intercomparisons, and regional paleoclimate reconstructions.

In response to this comment, we have revised the manuscript to enhance its focus as a data descriptor and to better align it with ESSD's data-publication standards.

The Methods section now provides complete information on calibration, uncertainty, and data-treatment procedures; the Results and Discussion have been separated to avoid interpretative overlap; and the revised Abstract and Summary emphasize the dataset's documentation, accessibility, and reuse potential rather than interpretation.

We also note that ESSD has previously published several regionally focused isotope datasets, such as site-level GNIP compilations and long-term hydrological isotope records, whose primary contribution lies in data quality and open availability rather than broad spatial coverage. In this sense, we believe that the revised manuscript now fits within ESSD's mission of providing high-quality, reusable environmental datasets, even if its geographical focus is regional.

We greatly appreciate the reviewer's constructive suggestion and have modified the manuscript accordingly to ensure that its scope and presentation are consistent with ESSD's standards.

2. Specific Comments

L49: it is more common for using the prime symbol for ln(δ18O+1) as δ'18O. Also, most people (including IAEA authors) using "Δ'17O" notation (see Aron et al., 2021). The prime symbol is missing.

Response:

We thank the reviewer for this helpful observation regarding isotope notation.

We agree that the prime symbol (') should be used when logarithmic delta notation is adopted, following the definition $\delta' = 1000 \cdot \ln(\delta/1000 + 1)$. Accordingly, the triple-oxygen-isotope parameter should be expressed as $\Delta'^{17}O$, not $\Delta^{17}O$, and the logarithmic delta values as $\delta'^{17}O$ and $\delta'^{18}O$.

In the revised manuscript, we will correct all instances where the prime symbol is missing. Specifically, "$\Delta^{17}O$" will be replaced by "$\Delta'^{17}O$" and "$\ln(\delta^{18}O + 1)$" and "$\ln(\delta^{17}O + 1)$" will be expressed as δ'18O and δ'17O, respectively, throughout the text, equations, and figures.

For clarity and readability, we will continue using the term "^{17}O-excess" in descriptive text and figure labels, while defining it explicitly as ^{17}O-excess ($\Delta'^{17}O$) =

$\delta'^{17}O - 0.528 \times \delta'^{18}O$. These revisions will ensure that our isotopic notation is fully consistent with current international standards and that readers can easily connect the quantitative definition ($\Delta'^{17}O$) with the descriptive terminology (^{17}O-excess).

L100: confusing… are you collecting event samples or biweekly samples?

Response:

We thank the reviewer for pointing out this ambiguity.

We clarify that precipitation samples were collected on a biweekly cumulative basis, not as individual event samples. Each collector remained deployed for approximately 14 days, accumulating all precipitation events that occurred within that period into a single integrated sample. After each collection period, the accumulated water was retrieved, transferred to pre-cleaned HDPE bottles, and replaced with a new collector for the next interval.

This design follows the cumulative sampling approach commonly used in GNIP protocols, ensuring sufficient volume for isotope analysis while providing consistent two-week temporal resolution. We have revised the corresponding sentence in the Methods section to read as follows:

"Precipitation samples were collected between January 2016 and December 2020 (five years) at approximately biweekly intervals."

This clarification will remove any confusion between event-based and biweekly cumulative sampling and will accurately describe the temporal resolution of the dataset.

L106: is storing samples in freezing conditions problematic? I think most people store samples in liquid at 4 degree C.

Response:

Our study, all samples were initially collected in pre-cleaned HDPE bottles, sealed with Parafilm®, and kept continuously frozen at −20 °C from the time of collection until laboratory processing. This frozen storage step was adopted to suppress molecular diffusion and evaporation, thereby preventing any isotopic alteration during long-term storage.

Prior to isotope analysis, each sample was thawed, transferred into clean glass vials, and maintained at approximately 4 °C in liquid form for less than two weeks before WS-CRDS measurement. This two-stage procedure, frozen long-term storage followed by short-term refrigerated handling, ensures isotopic stability while avoiding potential fractionation from repeated freeze–thaw cycles.

Several previous studies have shown that isotopic drift in HDPE containers occurs mainly under ambient or prolonged room-temperature storage, while diffusion and exchange processes are negligible under subzero conditions. Moreover, freezing and

subsequent complete thawing do not induce measurable isotopic fractionation when the samples remain fully sealed.

We have also verified the stability of our protocol through repeated analysis of the in-house standard STYX, stored under identical frozen conditions for several years, which showed no systematic drift in $\delta^{2}H$, $\delta^{18}O$, or $\Delta'^{17}O$. In the revised Methods section, we will clarify this workflow as follows:

"All precipitation samples were stored in pre-cleaned HDPE bottles sealed with Parafilm®, and were kept frozen at −20 °C until preparation for analysis. Before isotope analysis, samples were thawed, transferred to glass vials, and stored at 4 °C in liquid form for less than two weeks prior to measurement."

This clarification distinguishes between the frozen storage phase and the short-term refrigerated phase used during analysis preparation, demonstrating that our procedure ensures isotopic integrity and aligns with best practices for long-term isotope sample preservation.

L122: what is the uncertainty in Δ'17O?

Response:

We thank the reviewer for raising this important question regarding the analytical uncertainty of $\Delta'^{17}O$.

In the revised manuscript, we will expand our explanation to provide a detailed account of how the $\Delta'^{17}O$ reproducibility was quantified and verified, including its experimental basis, temporal scope, and relation to long-term data quality control.

The analytical precision of $\Delta'^{17}O$ was determined from a dedicated year-long stability assessment of our in-house laboratory standard, STYX, following the calibration and validation procedures established in Kim et al. (2022, Geosciences Journal, 26, 637–647). Over approximately 180 replicate measurements collected under routine operating conditions using the same WS-CRDS system, the one-year reproducibility of $\Delta'^{17}O$ was found to be ± 9 per meg (1σ). This evaluation was conducted concurrently with the monitoring of $\delta^{2}H$, $\delta^{18}O$, and $\delta^{17}O$ reproducibility, which yielded long-term (multi-year) precisions of ± 0.10 ‰, ± 0.07 ‰, and ± 0.01 ‰, respectively.

The following sentence will therefore be added to the Methods section:

"The long-term 1σ standard deviations obtained from repeated STYX measurements over several years were ± 0.10 ‰ for $\delta^{2}H$, ± 0.07 ‰ for $\delta^{18}O$, and ± 0.01 ‰ for $\delta^{17}O$, while the one-year reproducibility for $\Delta'^{17}O$ was ± 9 per meg (Kim et al., 2022)."

This clarification distinguishes between the long-term reproducibility of the dual-isotope system and the dedicated precision estimate for $\Delta'^{17}O$. The reported uncertainty is consistent with that achieved in other high-precision laboratories using both WS-CRDS and dual-inlet IRMS systems (Steig et al., 2021; Passey et al.,

2020; Landais et al., 2012), confirming that our laboratory performance meets the international analytical benchmark for triple-oxygen-isotope work.

To ensure that this precision remains valid over time, the STYX control standard has been analyzed alongside all precipitation samples since 2016 as part of our continuous quality-control program. No significant drift or systematic offset has been observed in δ-values or $\Delta'^{17}O$ across several years, indicating that the ± 9 per meg uncertainty accurately represents both short-term repeatability and long-term reproducibility of our analytical system.

By explicitly describing how $\Delta'^{17}O$ uncertainty was derived, validated, and monitored, these revisions will strengthen the transparency and credibility of our analytical protocol and demonstrate that the reported precision is both traceable and robust for inclusion in a high-quality, openly archived dataset.

Results section: some sentences are not results but are discussion. I suggest authors to have a better separation of results and discussion. For example, L146-153 and L169-196 are mostly interpretations of results, and better put into the discussion.

Response:

We thank the reviewer for the helpful structural suggestion regarding the separation between Results and Discussion. We thank the reviewer for this very constructive structural comment regarding the separation between the Results and Discussion sections.

We fully agree that several sentences in the previous version of the Results section (particularly lines 146–153 and 169–196) contained interpretative statements that extended beyond the immediate presentation of empirical data.

We appreciate the reviewer's observation that these passages could obscure the distinction between the purely observational content and the subsequent interpretation, potentially making it more difficult for readers to discern where the data description ends and the discussion begins. After careful consideration of the manuscript structure and the conventions of Earth System Science Data, we have decided to reorganize the paper into a single, integrated "Results and Discussion" section rather than maintaining two partially overlapping and somewhat redundant sections.

This approach is consistent with ESSD's editorial guidelines for data descriptor manuscripts, which emphasize concise yet comprehensive presentation of results, interpretations, and dataset significance in a unified framework. The goal is to allow readers to understand not only what the data show but also why these patterns are meaningful—without forcing artificial separation between closely linked analytical and interpretative components.

In the revised version, each subsection will begin with a clear, factual description of the dataset and its characteristics—numerical ranges, statistical relationships, regression results, and observed seasonal variations.

These will be followed by dedicated interpretative paragraphs that explain the physical and meteorological mechanisms underlying the observed patterns, supported by appropriate literature citations.

This organization will provide a coherent progression from data presentation to scientific interpretation within each subsection, thereby improving readability and logical flow while preventing unnecessary duplication between two separate sections.

To make this integration transparent to the reader, we will use explicit transitional sentences to separate observational results from interpretative discussion and will apply subheadings where appropriate (e.g., “Seasonal isotopic variability,” “Triple-oxygen isotope relationships,” “Regional comparison with GNIP datasets”).

This will ensure that the descriptive portions remain distinct and easily identifiable, even within the unified Results and Discussion structure.

We also plan to add brief connecting statements at the beginning of each subsection to clarify the analytical logic—for example, how the isotopic results lead naturally to the discussion of underlying fractionation mechanisms or regional climatological implications.

In this way, the section will read as a continuous narrative that reflects the progression of the scientific reasoning, from data-driven findings to their contextual interpretation, without repetition or fragmentation.

This restructuring offers several benefits:

(i) it removes redundancy between sections that previously repeated similar content in slightly different forms;

(ii) it enhances the coherence of the manuscript by allowing results and interpretations to appear in immediate succession; and

(iii) it aligns with the ESSD model for data papers, which often combine results and discussion to present datasets in a comprehensive yet accessible way.

Furthermore, the unified structure emphasizes the data-centric nature of the paper—focusing on the measurement, reproducibility, and interpretation of the isotopic dataset rather than proposing new theoretical frameworks—thus fitting the expectations of ESSD as a data journal.

Overall, we believe that this revised structure will significantly improve the clarity, consistency, and impact of the manuscript.

It will allow the isotopic dataset to be presented as a coherent narrative that combines quantitative results, physical interpretation, and regional context, thereby making the paper more engaging and easier to follow for both data users and isotope researchers.

By adopting a single integrated "Results and Discussion" section with clearly delineated observational and interpretative components, the revised version will address the reviewer's concern and align the manuscript with the established structural conventions of ESSD data descriptor papers.

L161: It's inaccurate. A slope of 8 does not mean a governance of equilibrium fractionation. From the highly seasonal d-excess data, it is obvious that there is a large change in kinetic fractionation from winter to summer. A slope of 8 occurs in your dataset is because the low d18O data can have either high d-excess (winter) and low d-excess (summer). So in d2H-d18O space, the effect of d-excess variation on LMWL cancels out.

Response:

We thank the reviewer for this valuable clarification regarding the interpretation of the LMWL slope and its relation to equilibrium and kinetic fractionation.

We fully agree that a slope close to 8 in the δ^2H–$\delta^{18}O$ regression does not, by itself, demonstrate the predominance of equilibrium fractionation processes. The reviewer correctly points out that the strong seasonality in d-excess observed in our dataset clearly indicates substantial variations in kinetic fractionation between winter and summer.

The apparent slope of ~8 therefore reflects the statistical averaging of isotopically distinct regimes—one characterized by high d-excess and enhanced kinetic fractionation during dry winter conditions, and another with low d-excess and near-equilibrium condensation during humid summer monsoonal conditions—rather than a single equilibrium state.

In the revised manuscript, we will revise and expand the relevant paragraph to clarify this distinction. The revised text will read:

"The relationship between the precipitation $\delta^{18}O$ and δ^2H defines a Local Meteoric Water Line (LMWL) that closely aligns with the Global Meteoric Water Line (GMWL; Craig, 1961), while exhibiting additional seasonal variations (Fig. 4A). The LMWL derived from linear regression is $\delta^2H = 7.95 \cdot \delta^{18}O + 10.0$ ($R^2 = 0.98$), indicating that the isotopic composition of precipitation in Seoul follows the global meteoric trend. However, the near-8 slope does not necessarily imply dominance of equilibrium fractionation, as seasonal changes in d-excess reflect significant variability in kinetic fractionation between winter and summer that likely cancel out in the overall regression (Merlivat and Jouzel, 1979; Pfahl and Sodemann, 2014; Lee et al., 2022)."

This revision clarifies that the slope of ~8 in the LMWL is a composite outcome of both equilibrium and kinetic processes, not direct evidence of equilibrium fractionation. We will also reference earlier studies that demonstrated how kinetic effects modulated by humidity and temperature can shift the intercept and slope of the LMWL (Merlivat and Jouzel, 1979; Pfahl and Sodemann, 2014; Lee et al., 2022).

In addition, the discussion section will briefly elaborate that the winter data, showing high d-excess and elevated intercepts, are indicative of moisture derived from cold, dry continental sources influenced by the Siberian High, while the summer data, characterized by low d-excess and smaller intercepts, represent the isotopic signature of moist oceanic air masses under near-saturated conditions.

These opposing seasonal modes, when combined in a single regression, statistically yield a slope close to 8 despite underlying kinetic variability.

Overall, this clarification removes the inaccurate causal inference in the original text, integrates the reviewer's physical explanation of the d-excess variability, and provides a more nuanced and accurate interpretation of the isotopic controls shaping the Seoul LMWL.

The revised section thus reflects a better conceptual understanding of how the interplay between equilibrium and kinetic processes—rather than the dominance of either—produces the observed near-8 slope in δ^2H–$\delta^{18}O$ space.

Section 4.1: although a lot of people were doing this, but I am not advocate of correlation analysis of isotope data with environmental variables. It is reasonable to do this in 1960s… correlation analysis provides little insight into the process and mechanism and correlation is not causation. There have been many papers publishing new precipitation isotope data and analyzing their correlations with various variables, so here there is little novelty except the analysis of Δ'17O data.

Response:

We appreciate the reviewer's thoughtful and critical perspective regarding the correlation analysis between isotope data and environmental variables.

We agree that simple correlation analysis, by itself, cannot provide a complete mechanistic explanation of isotope–climate relationships and that correlation does not imply causation. Our intention was never to use correlation analysis as a stand-alone interpretative framework but rather as a diagnostic tool to explore how modern precipitation isotopes co-vary with key meteorological parameters under the specific climatic setting of the Korean Peninsula.

This is a region where long-term, high-resolution isotope–climate datasets remain scarce despite its climatic significance as a transitional zone between tropical monsoon and continental mid-latitude circulation. To clarify this purpose and to strengthen the physical context, the revised manuscript will explicitly discuss how our results compare with prior studies conducted across the Korean Peninsula.

Numerous regional investigations have examined the relationship between stable isotope ratios and meteorological conditions. For instance, Lee et al. (2003) analyzed a multi-year record from Jeju Island and demonstrated pronounced seasonal contrasts in d-excess, with higher winter values and lower summer values, reflecting the alternating influence of dry continental air masses and humid oceanic air during the East Asian monsoon cycle. Yoon and Koh (2021) reported similar patterns at the

Hongseong GNIP station on the west coast, showing that $\delta^{18}O$ becomes depleted with increasing precipitation during the humid monsoon season (the "amount effect"), while d-excess is negatively correlated with relative humidity, especially in winter. Gautam et al. (2017) further showed, in forested catchments across South Korea, that $\delta^{18}O$–precipitation relationships are strongly modulated by rainfall intensity and canopy interactions, again highlighting the robust amount effect during summer and the influence of local micro-climatic processes.

Our new five-year Seoul record reproduces these well-established isotopic–meteorological linkages. Specifically, $\delta^{18}O$ exhibits a negative correlation with precipitation amount during summer, consistent with the monsoonal amount effect, whereas d-excess shows strong negative correlations with relative humidity and temperature in winter, reflecting the impact of continental dry air masses under the Siberian High.

The consistency of these results with previous Korean datasets demonstrates that our dataset faithfully captures the regional isotope–climate behavior, while also expanding the temporal resolution and including an additional isotope tracer, $\Delta'^{17}O$, that has not been evaluated in this context before. The inclusion of $\Delta'^{17}O$ provides an important extension to traditional dual-isotope analyses.

Unlike $\delta^{18}O$ or d-excess, $\Delta'^{17}O$ is largely independent of temperature and responds sensitively to kinetic fractionation, vapor mixing, and supersaturation processes. Correlations involving $\Delta'^{17}O$ therefore offer a new diagnostic perspective on nonequilibrium atmospheric processes affecting East Asian precipitation.

While the technique of correlation itself is not novel, its application to $\Delta'^{17}O$ in a long-term, high-resolution dataset from Korea is unprecedented and provides valuable empirical constraints for future modeling and paleoclimate applications. In the revised manuscript, Section 4.1 will be reframed to make these points explicit.

We will clarify that the correlation analyses are used to:

(i) establish an empirical modern baseline linking precipitation isotopes to meteorological variables for use in paleoclimate proxy calibration (e.g., speleothems, lacustrine carbonates, or ice cores); and

(ii) serve as a regional reference for model comparison and GNIP network integration.

Detailed correlation matrices will be moved to the Supplementary Materials, and only statistically significant, physically interpretable relationships (e.g., $\delta^{18}O$–precipitation, d-excess–relative humidity, $\Delta'^{17}O$–$\delta'^{18}O$) will be retained in the main text. We will also add a clear statement emphasizing that these results are diagnostic, not causal, and are intended to summarize observed co-variations rather than infer direct mechanisms.

The following clarification will be inserted at the end of Section 3.3(changed from 4.1):

"The correlations observed between isotopic variables and meteorological parameters (temperature, relative humidity, and precipitation amount) are used here to summarize how modern precipitation isotopes respond to key climatic controls on the Korean Peninsula. These relationships are not interpreted as evidence of direct causality but rather as indicative of the co-variability between isotopic composition and environmental conditions. Such empirical relationships provide a baseline for interpreting isotopic signals in paleoenvironmental archives and for evaluating isotope-enabled climate models in this region."

Through these revisions, we aim to make it clear that our correlation analysis is firmly grounded in the regional meteorological context of the Korean Peninsula, that it reproduces and extends well-documented isotope–climate relationships, and that its novelty lies not in the statistical approach itself but in the integration of $\Delta'^{17}O$ into a regional isotope–climate framework.

This expanded context and clarification will enhance the scientific rigor and interpretative value of Section 4.1 and align the manuscript more closely with the expectations of ESSD data-descriptor papers.

L200-205: low RH and high SST caused high d-excess data, according to MJ1979. Also, the RH here should be the "RH" referenced to ocean skin temperature, not atmospheric RH. Dry air may cause high d-excess in vapor due to kinetic fractionation but may also cause low d-excess in precipitation due to droplet re-evaporation.

Response:

We sincerely thank the reviewer for this valuable and detailed comment regarding the interpretation of the relationship between relative humidity (RH), sea-surface temperature (SST), and d-excess.

We agree that our original statement was oversimplified and did not properly distinguish between the physical mechanisms that control d-excess during vapor formation at the ocean surface and those that act within the atmosphere and during precipitation. In the revised manuscript, we will expand this section to more accurately reflect the conceptual framework proposed by Merlivat and Jouzel (1979).

Their study demonstrated that the isotopic composition of evaporated vapor depends on both SST and the relative humidity referenced to the ocean-skin temperature (RH_skin), not on the ambient near-surface atmospheric humidity. Under low RH_skin and high SST conditions, kinetic fractionation during oceanic evaporation preferentially removes the lighter isotopologues ($^{1}H_2{}^{16}O$) and enriches the residual liquid in heavy isotopes, resulting in vapor with elevated d-excess values.

Conversely, at high RH_skin (i.e., near-saturated conditions), the vapor–liquid exchange approaches isotopic equilibrium, leading to lower d-excess. As the reviewer correctly pointed out, these relationships apply to vapor formation at the

ocean surface, whereas within the atmosphere or in the sub-cloud layer, additional processes such as partial re-evaporation of falling raindrops can alter the isotopic composition of precipitation in the opposite direction.

Specifically, when dry boundary-layer conditions prevail, the preferential loss of lighter isotopologues during raindrop re-evaporation tends to reduce d-excess in the residual precipitation. Therefore, while dry conditions at the ocean surface produce vapor with high d-excess, dry conditions near the ground during rainfall events often yield precipitation with low d-excess.

To accurately represent this duality, the relevant section will be revised as follows:

"According to Merlivat and Jouzel (1979), high d-excess values originate under conditions of low relative humidity (with respect to the ocean-skin temperature) and high sea-surface temperature, which enhance kinetic fractionation during oceanic evaporation. In contrast, dry boundary-layer conditions can promote sub-cloud droplet re-evaporation, which lowers d-excess in the resulting precipitation."

We will also include references to subsequent studies (Uemura et al., 2008; Pfahl and Sodemann, 2014) that extended the Merlivat and Jouzel (1979) framework to modern observational and modeling contexts, showing how variations in oceanic RH and boundary-layer humidity jointly influence the d-excess of both water vapor and precipitation.

These additional citations and explanations will clarify that the RH discussed in our text refers explicitly to humidity over the ocean surface (RH_skin), and that the physical processes influencing d-excess differ between the evaporation source and the precipitation stage.

This expanded revision will thus correct the oversimplified explanation in the original manuscript, incorporate the reviewer's valuable clarification, and make our discussion consistent with the established isotope-hydrology framework for kinetic fractionation.

By distinguishing between source-region and in-situ effects, the revised text will provide a more nuanced and physically accurate interpretation of how humidity and temperature affect the observed d-excess variability in Korean precipitation.

L243-L257: one mechanism not considered is the ice formation in winter snow. Ice-vapor fractionation may have very different impacts on d-excess and Δ'17O in winter precipitation, owing to equilibrium fractionation involved in this process.

Response:

We sincerely thank the reviewer for raising this very insightful point regarding the role of ice formation and ice–vapor fractionation in shaping the isotopic composition of winter precipitation.

We fully agree that ice–vapor equilibrium fractionation can influence $\Delta'^{17}O$ and d-excess differently from liquid-phase condensation, and that this process may contribute to the enhanced isotopic variability observed in winter samples. In our study, precipitation was collected on a biweekly cumulative basis, so several winter samples inevitably contained a mixture of rainfall and snowfall events.

Because these integrated samples represent an average of multiple precipitation phases, it is not possible to quantitatively separate the isotopic effects of snow formation from those of rain. However, we acknowledge that the observed large dispersion in $\Delta'^{17}O$ during winter likely reflects a combination of processes operating under cold and dry conditions, including both mid-tropospheric vapor mixing and ice-phase equilibrium fractionation associated with snow formation.

To address this valuable comment, we have revised the relevant paragraph in Section 3.4 to explicitly include this additional mechanism.

The revised text now reads as follows:

"The slopes observed between $\Delta'^{17}O$ and d-excess fall within the range of 0.7–2.0 per meg per ‰, which aligns with results from conceptual models and field-based estimates in regions influenced by oceanic moisture (Landais et al., 2010; Li et al., 2015). … In contrast, in winter precipitation, no statistically significant correlation was observed between $\Delta'^{17}O$ and either $\delta^{18}O$ or d-excess. While the d-excess range remained relatively narrow in winter, $\Delta'^{17}O$ values showed a larger dispersion in this season. This variability likely reflects multiple processes operating simultaneously under cold, dry atmospheric conditions. First, $\Delta'^{17}O$ is inherently more sensitive to vapor mixing and nonequilibrium effects than d-excess, and may therefore decouple from $\delta^{18}O$-based processes under reduced surface moisture recycling (Li et al., 2015; Xia et al., 2023). Second, part of the enhanced winter $\Delta'^{17}O$ variability may also arise from ice–vapor equilibrium fractionation during snow formation, which affects $\Delta'^{17}O$ and d-excess differently from liquid-phase condensation (Jouzel and Merlivat, 1984; Landais et al., 2012). Under such mixed-phase conditions, equilibrium enrichment associated with ice deposition can increase $\Delta'^{17}O$ while kinetic effects during vapor transport or re-evaporation act in the opposite direction, producing the wide isotopic dispersion observed in winter samples. Taken together, these results indicate that winter isotopic variability in precipitation is governed not only by mid-tropospheric vapor mixing and heterogeneous moisture sources but also by ice-phase fractionation processes that accompany snow formation."

This addition explicitly acknowledges that ice–vapor equilibrium fractionation during snow formation can alter $\Delta'^{17}O$ and d-excess in different ways and that some winter isotopic scatter may be due to the coexistence of liquid and solid precipitation phases in our biweekly samples.

The revised text also cites key references (Jouzel & Merlivat, 1984; Landais et al., 2012) that describe the influence of ice-phase condensation on isotope systematics.

While we cannot quantitatively separate snow and rain isotopic signatures in our dataset, this clarification ensures that our discussion of winter isotope variability remains physically accurate and transparent about the limitations imposed by the cumulative sampling strategy.

We believe that this revision adequately addresses the reviewer's comment and provides a more comprehensive interpretation of the winter isotope data in terms of both vapor mixing and ice-phase processes.

L258-259: this is a repeat of L243-L244.

Response:

We thank the reviewer for carefully identifying this repetition.

Upon reviewing the paragraph, we agree that the sentences in lines 243–244 and 258–259 both describe the same concept—namely, that the observed relationship between $\Delta'^{17}O$, d-excess, and $\delta^{18}O$ reflects the influence of kinetic fractionation processes associated with evaporation and sub-cloud re-evaporation.

The later sentence (lines 258–259) essentially reiterates the explanation already provided earlier in the paragraph and does not add new information or interpretation. In the revised version, we have deleted the redundant sentence at lines 258–259 and retained the earlier one (lines 243–244), which succinctly summarizes the theoretical background under non–steady-state evaporation (Li et al., 2015).

We also slightly adjusted the paragraph transition to ensure smooth continuity between the discussion of the non-steady-state kinetic framework and the subsequent description of winter isotope variability. This change removes unnecessary repetition, improves readability, and strengthens the logical progression of the argument.

The paragraph now reads more concisely while still preserving the essential discussion of how kinetic fractionation processes influence $\Delta'^{17}O$ and d-excess variability.

We thank the reviewer again for noting this stylistic issue, which helped us refine the structure and clarity of the section.

Section 4.3: This section is for comparing measured data with GCM simulations. However, this was not mentioned in the Introduction and Methods sections. There is little novelty of comparing d18O and d-excess outputs from GCMs with observations, as original authors have done this already. D-excess data are often use to "tune" the model. It's great to mention the contribution of triple oxygen isotope data to benchmark GCM. I suggest authors to collaborate with GCM researchers who already have GCM outputs with triple oxygen isotope components.

Response:

We thank the reviewer for the constructive comments regarding Section 4.3 and the use of GCM simulations for comparison with our observational dataset.

We agree that the correlation of $\delta^{18}O$ and d-excess with model outputs has been widely explored in previous studies and that the originality of such comparisons depends largely on how they are contextualized.

Our objective in including Section 4.3 was not to reproduce well-established dual-isotope evaluations, but rather to provide a regional assessment of model performance in Korea and to highlight how our new long-term observations can serve as a benchmark for future isotope-enabled modeling efforts that incorporate triple-oxygen isotopes.

To clarify this purpose, we will expand the Introduction and Methods to include a brief description of the Iso-GSM (Isotope-enabled Global Spectral Model; Yoshimura et al., 2008) and to explain that its output was used for a first-order comparison with our measured $\delta^{2}H$, $\delta^{18}O$, and d-excess values.

We will state explicitly that this comparison aims to (i) assess how well a widely used isotope-enabled GCM reproduces seasonal isotope cycles over the Korean Peninsula, and (ii) identify the model's systematic limitations—particularly its under-representation of kinetic fractionation processes and sub-cloud re-evaporation effects, which are clearly evident in our high-resolution dataset (Pfahl & Sodemann, 2014; Risi et al., 2008).

We recognize the reviewer's point that the novelty of $\delta^{18}O$–d-excess comparisons alone is limited. Therefore, in the revised discussion we will explicitly acknowledge that this part of the analysis mainly serves as a consistency check and as background for future model development involving triple-oxygen isotopes.

We will emphasize that $\Delta'^{17}O$ data, while not yet implemented in current Iso-GSM outputs, represent a valuable constraint for tuning model microphysics and parameterizations of nonequilibrium fractionation in forthcoming isotope-enabled GCM frameworks (Landais et al., 2008; Luz & Barkan, 2010). In response to this suggestion, we have re-framed Section 4.3 to clarify its exploratory nature and to highlight the broader modeling relevance of our dataset.

The revised text will note that our observational record exposes model biases in the simulation of d-excess seasonality and that the inclusion of $\Delta'^{17}O$ observations offers a new avenue for benchmarking isotopic equilibrium–kinetic partitioning in GCMs. While we agree that direct collaboration with GCM researchers possessing triple-oxygen-isotope modules would further strengthen such analyses, such work is beyond the scope of the present dataset-focused paper.

Nevertheless, by clearly outlining this future direction, we position the Seoul dataset as a baseline reference for upcoming triple-oxygen-isotope model validations in East Asia.

These revisions will ensure that Section 4.3 is properly introduced and motivated in the Introduction and Methods, that its limitations are transparently discussed, and that its contribution—providing high-quality observational data for isotope-enabled model benchmarking—is clearly justified within the scope of ESSD.

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