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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 δ^2H, δ^{18}O, d-excess, and Δ'^{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 4

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. The manuscript presents a new precipitation isotope record for δ2H, δ18O, d-excess and 17O-excess for Seoul spanning four years and discusses the seasonality in the context of the regional-scale circulation. It further investigates the asynchronous seasonality of d-excess and 17O-excess and discusses possible causes.

As for the discussions that emerged regarding the dataset's/manuscript's fit into the scope of ESSD, I must admit it's a delicate trade-off between the novelty and rarity of precipitation 17O/18O LMWL datasets, and the limited spatiotemporal coverage of the dataset (4 years, and applicability of an LMWL at best regional).

I am sorry to say that the structure of the manuscript merits improvement. The description of the analytical method is unclear to me (see detailed comments), and results and conclusions are a bit too tightly intertwined. The chapter 4.3 reads like an "encapsulated mini manuscript"; to me it is not adequately introduced at the beginning and includes methods and results which should be in chapters 2 and 3, *respectively. A fair bit of the text unfortunately reads very generic or top-level but this is not supported by the granularity and/or spatiotemporal resolution of the data.*

My recommendation is, and I am writing this before the background of my own struggles with getting datasets of a similar kind published, that the authors take a step back, review the hypotheses that can be addressed with the already-existing dataset, and then make a renewed attempt to publish an upgraded version of the manuscript.

Response:

We sincerely thank the reviewer for the detailed and constructive overall evaluation of our manuscript. We deeply appreciate the acknowledgement of our efforts to collect, maintain, and analyze this multi-year precipitation isotope dataset. We fully understand the reviewer's concerns regarding the fit of the manuscript within the scope of Earth System Science Data and the need for clearer structure and focus. ESSD indeed prioritizes the publication of flagship, high-impact datasets with broad applicability.

While our dataset is regional in scope, we believe that it nonetheless holds substantial value as a long-term, high-resolution precipitation isotope record that includes δ^2H, $\delta^{18}O$, $\delta^{17}O$, d-excess, and $\Delta'^{17}O$, parameters that remain rare in East Asia. Such datasets are critical for isotope-enabled model benchmarking and for regional paleoclimate calibration efforts, and we have therefore carefully revised the manuscript to better emphasize its contribution as a reusable, well-documented observational dataset rather than as an interpretative study. In response to the reviewer's comments, we have implemented several major revisions:

(i) The Methods section has been expanded and reorganized to clearly describe analytical procedures, calibration standards, and reproducibility, addressing the earlier lack of clarity.

(ii) The Results and Discussion have been streamlined and combined into a single coherent section, improving readability and separating factual observations from interpretative discussion.

(iii) The Iso-GSM comparison (Section 4.3) has been reframed as an illustrative example of potential dataset applications rather than a stand-alone modeling analysis; additional technical details were intentionally omitted to retain the data-focused nature of the paper.

(iv) The Summary has been rewritten to highlight the dataset's accessibility, long-term stability, and potential for reuse in model–data intercomparisons and East Asian climate research.

We also acknowledge the reviewer's observation that some parts of the previous manuscript read too generically relative to the spatial and temporal resolution of the data. The revised text now focuses on empirical results, quantitative ranges, and physically based explanations, avoiding speculative interpretations that are beyond the scope of the dataset. Overall, this revision aims to make the paper more concise, transparent, and aligned with ESSD's data-descriptor style.

The revised manuscript now emphasizes the dataset's role as a regional benchmark that fills a gap in East Asian triple-oxygen-isotope records and provides a solid foundation for future collaborations with modeling groups to extend this work. We sincerely thank the reviewer once again for the constructive critique and for sharing personal insights from similar publishing experiences. This reviewer's comments were invaluable in guiding the restructuring and refocusing of the manuscript, which we believe has greatly improved its clarity and alignment with ESSD's publication standards.

2. Specific Comments

As for the annotation of 17O-excess, please unify your annotation. Commonly, 17O-excess is expressed as Δ'17O = δ'17O - 0.528 δ'18O, with δ' = ln (δ+1), an equation which traces back to Angert et al. 2004. The usage of the Δ' is very much encouraged to distinguish the Δ'17O from other excess calculations in isotope geochemistry that do not log-normalize the deltas (e.g. Aron et al. 2021 or other reviews on the topic). The annotation should be either in per meg, or in ‰ with three decimal places. It is not correct to use ‰ but express in per meg, as it is often done throughout the manuscript. The authors may also consider to consolidate the "equations" part into a "definitions" section either in the methods or in the introduction. Right now, the 17O-excess equation is not numbered and in line with introduction text, while the isotope ratio equation is numbered, in the methods, and after the 17O-excess equation.

Response:

We thank the reviewer for this helpful observation regarding isotope notation. We sincerely thank the reviewer for this detailed and constructive comment regarding the notation, unit, and placement of the $\Delta'^{17}O$ definition. We fully acknowledge the importance of adopting a consistent and standardized formulation, particularly for readers who may not be familiar with the differences between $\Delta'^{17}O$ and other non-logarithmic "excess" parameters. In the revised manuscript, we have carefully unified all $\Delta'^{17}O$ notations following the formulation originally introduced by Angert et al. (2004):

Δ'17O=δ'17O−0.528·δ'18O,

where δ' = 1000·ln(δ/1000 + 1).

The use of the prime symbol and the Δ' notation has been standardized throughout the entire manuscript (text, tables, and figures) in accordance with the widely accepted conventions summarized by Aron et al. (2021) and Luz and Barkan (2010). Regarding the units, all $\Delta'^{17}O$ values are now consistently expressed in per meg (10^{-6}).

We have thoroughly checked the entire manuscript and figure captions to ensure that no instance remains where the unit "‰" was incorrectly used for values expressed in per meg. In addition, we have reorganized and consolidated the isotope-notation equations into a new “Definitions” subsection within the Methods section, where all relevant equations, δ, δ′, d-excess, and $\Delta'^{17}O$, are presented together in a numbered and clearly formatted manner.

This revision ensures that all key parameters are defined in one place, improving accessibility for readers and methodological transparency. We greatly appreciate the reviewer’s detailed suggestions, which have significantly improved the clarity, consistency, and professional presentation of our manuscript. We believe that these changes now align our notation and units fully with current standards in isotope geochemistry and triple-oxygen-isotope research (Angert et al., 2004; Luz and Barkan, 2010; Aron et al., 2021).

Concerning the listing of isotope effects (line 40), please take into consideration that the “amount effect” is one of the most debated empirical relationships in isotope hydrology, and that the modern-day discourse is cautious of a unanimous endorsement of it. While the cited Conroy et al. (2016) detected it, later publications (e.g. Konecky et al. 2019) have a much more differentiated approach. I acknowledge that the “amount effect” is still widely taught, but the data reality is often much more complicated than the initial concept. Please also note that your manuscript claims to “analyse […] in mid-latitude precipitation”, which I think is a bit of an overstatement since it would suggest a global analysis.

Response:

We fully acknowledge that the “amount effect” is one of the most debated empirical relationships in isotope hydrology and that its expression varies across climatic and geographical contexts. While Conroy et al. (2016) reported a clear amount effect in certain regions, more recent studies such as Konecky et al. (2019) have indeed emphasized the complexity and regional dependence of this relationship. In the revised manuscript, we will revise the corresponding paragraph to reflect this nuance more accurately. The updated text will note that the “amount effect” represents an empirical relationship that can vary in magnitude and even sign depending on atmospheric circulation, convective dynamics, and moisture recycling. The revised sentence will read:

“Two widely discussed empirical relationships—the temperature effect, where colder temperatures lead to lower $\delta^{18}O$ and $\delta^{2}H$ values, and the amount effect, describing isotope depletion that often accompanies increased rainfall—have been observed in many, but not all, climatic settings (Dansgaard, 1964; Conroy et al., 2016; Konecky et al., 2019).”

Additionally, we agree that the phrase “mid-latitude precipitation” in the Introduction may sound overly broad. In the revised manuscript, we will clarify that our focus is on mid-latitude precipitation over the Korean Peninsula, rather than implying a global-scale analysis. These revisions will make our framing of the “amount effect” more balanced and consistent with the modern understanding of isotope–climate relationships, while avoiding any overstatement of the study's geographical scope.

Kindly also work on your definitions of "long-term" and "high resolution" (line 69, 79, 85 etc.). For much of the triple O isotope work, the "long-term" discussion is complicated by absence of records as long as are available for "dual isotopes". (Leuenberger & Ranjan 2021 and Terzer-Wassmuth et al. 2023 have the longest records reaching back furthest in time, to my knowledge). What is your definition of "high resolution"? To me, it would imply any sampling that is at minimum daily if not sub-daily (like the typhoon records of Munksgaard et al. [2014], the hurricane studies of Sun et al. [2024] and similar). Also, hinting at extreme weather events (e.g. line 96) deems far-fetched in the context of a biweekly sampling.

Response:

We fully understand that the term "high-resolution" is relative and that, in the broader context of isotope hydrology, it may refer to daily or even sub-daily sampling, as achieved in event-based studies (e.g., Munksgaard et al., 2014; Sun et al., 2024). However, in the context of triple oxygen isotope observations in East Asia, most existing records have been collected at monthly intervals (e.g., Lee et al., 2013; Shin et al., 2021; Yoon and Koh, 2021). Our biweekly (~14-day) integrated sampling, maintained continuously for five years, therefore represents one of the most temporally resolved and regionally extensive datasets currently available for $\Delta'^{17}O$ measurements in the Korean Peninsula.

In the revised manuscript, we will clarify that the term "high-resolution" is used in this relative regional sense, indicating that our dataset provides twice the sampling frequency of most previous studies and sufficient temporal resolution to capture seasonal and interannual isotope variability, while acknowledging that it does not resolve individual precipitation events. This clarification will make clear that our usage of "high-resolution" reflects a comparative improvement over existing Korean and East Asian datasets, and will ensure that the term is interpreted appropriately within its regional and methodological context.

The sample collection is described as relating to the GNIP manual, but this is neither cited and, in several aspects, does not follow the manual. First, authors should consider referring to one of the 5 methods mentioned in the manual (additional sampler designs are in Michelsen et al. 2018). Furthermore, the GNIP manual nowhere recommends biweekly sampling (presumably because if its inherent difficulties to match the intervals with established monthly records). Also, freezing samples is not described in the GNIP manual. The authors should provide a sketch drawing of the sampler, or some detailed photos (all relevant aspects of sampler design are hidden behind bricks), plus a photo that shows the greater context of the sampling location in the SM for clarity.

Response:

We thank the reviewer for this important clarification regarding our description of the sampling procedure. We agree that the current wording may incorrectly suggest that our protocol followed the GNIP manual, whereas our approach was only inspired by the cumulative-sampling concept commonly used in isotope hydrology. In the revised manuscript, we will remove the explicit reference to the GNIP manual and instead describe our procedure as an independent biweekly cumulative sampling protocol developed to suit local logistical conditions.

Although our design follows the same general principle of collecting integrated precipitation over a defined interval, it differs from the standard GNIP setup in two key respects:

(i) the collection interval was approximately 14 days instead of monthly, and

(ii) samples were stored frozen (−20 °C) rather than refrigerated at 4 °C, in order to minimize evaporation and isotopic alteration during long-term storage.

We will clarify this distinction in the revised Methods section as follows:

“Precipitation samples were collected every two weeks using a cumulative sampling protocol designed for this study. Although conceptually similar to cumulative collection methods used in isotope hydrology, the setup was adapted to local field conditions and does not strictly follow the GNIP manual.”

Regarding the sampling apparatus, we will add a schematic drawing and high-resolution photographs of the collector and its installation site in the Supplementary Materials to provide a clear visual reference of the design and field setting. The figure will show the funnel, collection bottle, sealing system, and surrounding structure to ensure transparency and reproducibility. These revisions will correct the inaccurate implication that our sampling followed the GNIP standard, provide a clearer description of our adapted design, and improve the methodological transparency through visual documentation of the sampler and site layout.

The sample analysis largely relies on a previously published methods paper and there are a couple of things that read inconsistent to me. First, you describe that the method determined the injection numbers, but then it’s a fixed number of 20 injections of which the last five are accepted. Second, the method claims to use VSMOW (exhausted – do you mean VSMOW2?), SLAP2 and GISP (also exhausted) for normalization of the data to the VSMOW-SLAP scale, which is acceptable in a methods testing setting but normally discouraged for routine analysis. If in-house standards are used, then their value should be provided and an eventual traceback to the primary reference materials should be given in the SM. Is this the ”laboratory standard” mentioned in line 122? What is the typical uncertainty of the method under routine analysis (e.g. expressed as a 1-sigma SD of the Δ’17O of a control sample)? It’s been three years since the original method by Kim et al. (2022) was published, hence a review of the method’s benchmark data deems merited.

Response:

We thank the reviewer for pointing out the ambiguity in our description of the injection protocol. In the revised manuscript, we will clarify that the method did not "determine the total number of injections", but rather the number of injections included in the average. Specifically, the instrument will perform 20 injections per vial, and, to mitigate memory effects, only the last five injections will be averaged to compute δ-values. Samples and reference waters will be prepared in duplicate vials (the first used as a buffer against carryover; the second used for evaluation).

We will also correct the calibration wording to state that the instrument will be calibrated to the VSMOW–SLAP scale using VSMOW2, SLAP2, and GISP2 reference waters (two-point calibration), and that in-house standards (STYX and KT), both traceable to VSMOW2/SLAP2, will be analyzed every ten samples as quality-control checks rather than for primary normalization. Finally, we will report our routine reproducibilities (1σ) from repeated STYX measurements, including ± 9 per meg for $\Delta'^{17}O$ (one-year). These changes will resolve the inconsistency and will make the procedure fully transparent.

In the "methods" chapter, the authors may also consider adding a paragraph "data treatment methods", i.e. not only about the weighted means but also how their LMWLs were calculated. (unweighted? Weighted? The 17O/18O one on the δ or δ'? With intercept, or 0-forced?).

Response:

We thank the reviewer for this important clarification. We sincerely thank the reviewer for this insightful suggestion. We fully agree that providing a detailed explanation of the data-processing workflow—including both the precipitation-weighted means and the regression methods used to derive the Local Meteoric Water Lines (LMWLs)—will improve the transparency and reproducibility of our analysis. In the revised manuscript, we will add a dedicated subsection entitled "Data treatment methods" in the Methods chapter. This new section will describe:

(i) how precipitation-weighted monthly means were calculated for δ^2H, $\delta^{18}O$, and $\delta^{17}O$;

(ii) how the LMWLs were derived; and

(iii) how the $\delta'^{17}O$–$\delta'^{18}O$ regressions were computed for the triple-oxygen-isotope relationships.

The LMWL will be calculated using the ordinary least squares (OLS) regression between δ^2H and $\delta^{18}O$ ($\delta^2H = a + b \cdot \delta^{18}O$), following Craig (1961). OLS was chosen to ensure direct comparability with the Global Meteoric Water Line (GMWL) and most regional studies across East Asia (Crawford et al., 2014; Lee et al., 2022). A supplementary total least squares (TLS) regression will also be performed to evaluate sensitivity to analytical uncertainties, and the results will be presented in Table S1.

For the triple-oxygen-isotope relationships, the regressions will be based on logarithmic delta notation ($\delta' = 1000 \cdot \ln(\delta/1000 + 1)$) with both slope and intercept freely fitted (not 0-forced). This ensures that the $\Delta'^{17}O$ values are consistent with standard practice and reflect true mass-dependent fractionation. These additions will clearly document how the isotopic datasets were processed, from event integration to regression analysis, and will make the treatment of δ- and δ′-based data fully transparent. We believe this revision will substantially enhance methodological clarity and align our work with best practices in isotope hydrology.

In the variations chapter (3.1), I found the description of 130 samples (which I translate as data points) a bit in contrast to the supplementary data file on Pangaea, which is roughly monthly and has less data points than described here. Without extensive calculations, some of the sine functions in Fig. 3 do seem bimodal while others don't. Whilst I agree with the comparison of the regional patterns with Jeju and mainland China, I miss a comparison with the data from GNIP/Cheongju (IAEA, 2025; admittedly a continental mountain station), as also highlighted by one of the other reviewers. The array of LMWL combinations (Seoul/Cheongju/Hongseung vs. weighted/unweighted) is huge and, to me, poses more questions than "similarities" as described in lines 174-179. It is commonly known that, due to the complex interplay between the Siberian High and the summer monsoon as drivers, the LMWL interpretation is complex, and the data is highly scattered, often causing unusually low R2. Note that the slope/intercept reported here are different to those reported in the summary (intercept of 10 here, 11.2 in the summary).

Response:

We sincerely thank the reviewer for these detailed and valuable comments. We agree that the apparent discrepancy between the total number of samples (130) described in the text and the number of data points in the PANGAEA dataset required clarification. Precipitation was collected biweekly throughout the study period, but for consistency with regional isotope records and for statistical robustness, most of the analyses were performed on precipitation-weighted monthly mean values. The dataset archived on PANGAEA therefore contains the monthly weighted means used for analysis, while the raw biweekly data were used only for internal quality control and one supplementary comparison. In the revised manuscript, we will make this workflow explicit in Section 3.1 by adding the following clarification:

"Although precipitation was collected at approximately 14-day intervals, the isotopic results presented here are primarily based on precipitation-weighted monthly means derived from these samples."

This will ensure that the sampling–analysis relationship is transparent and consistent with the archived dataset.

We also appreciate the reviewer's insightful comments regarding the complexity of LMWL interpretation and the comparison among regional datasets. The LMWL parameters in East Asia indeed show considerable scatter due to the strong interplay between the Siberian High and the East Asian summer monsoon, as noted by the reviewer. To provide better context, we will include a quantitative comparison with the Cheongju GNIP dataset from Terzer-Wassmuth et al. (2023), as well as discuss the slope and intercept differences among Seoul, Cheongju, and Hongseong in relation to their distinct climatic settings (coastal vs. inland, monsoon vs. continental influence). The intercept inconsistency noted between the main text and the summary will also be corrected to ensure internal consistency (intercept = 10.0). These revisions will make the dataset description more transparent, strengthen the regional comparison, and improve the clarity of the LMWL interpretation.

For the LMWL results, I agree that "seasonal disentangling" improves the LMWLs in this context. The R2=1 for the 17O/18O MWL is not surprising; similar has been observed by Terzer-Wassmuth et al. (2023) and many others. The authors should, ideally already in the "data treatment" section of the methods' chapter, outline how the 17O/18O MWL was calculated. The slope is very similar to that reported for Cheongju by Terzer-Wassmuth et al. (2023), but the intercept isn't (0.0105 vs. 0.0216). A comparison of a weighted LMWL intercept with the mean Δ'17O for Seoul would be helpful (they should be similar for a weighted MWL). I recommend removing Figure 4B; without scale it adds very limited value to the presentation of results. A table of MWLs would be more representative. Much of this section however overlaps with the discussion.

Response:

We thank the reviewer for these very thoughtful and constructive comments on the presentation of the LMWL and 17O/18O relationships. We appreciate the reviewer's positive assessment that seasonal disentangling improves the LMWL representation in our dataset, as seasonal partitioning indeed helps to reduce scatter caused by contrasting air-mass sources. In the revised manuscript, we will clarify in the Methods section (under the new Data treatment methods subsection) how the 17O/18O meteoric water line (MWL) was calculated.

Specifically, the $\delta'^{17}O$–$\delta'^{18}O$ regression will be described as being based on the logarithmic δ' notation ($\delta' = 1000 \cdot \ln(\delta/1000 + 1)$) with both slope and intercept freely fitted (not 0-forced). The LMWL between δ^2H and $\delta^{18}O$ will be calculated using an ordinary least-squares (OLS) regression for consistency with the GMWL definition, while total least-squares (TLS) fits will be provided in Table Sx for comparison. Following the reviewer's suggestion, we will also add a short quantitative comparison with the GNIP Cheongju dataset (Terzer-Wassmuth et al., 2023).

Our $\delta'^{17}O$–$\delta'^{18}O$ slope (~0.528) agrees closely with their reported value, whereas the intercept differs slightly (0.0105 vs. 0.0216). We will note that this difference likely reflects contrasting environmental conditions—the Cheongju station being a more continental, drier site—whereas Seoul experiences stronger marine moisture influence.

In addition, as suggested, we will compare the weighted LMWL intercept for Seoul with the mean $\Delta'^{17}O$ value, which are indeed of similar magnitude, demonstrating internal consistency between the weighted regression and the average 17O-excess. We agree that the $\delta'^{17}O$–$\delta'^{18}O$ plot (previous Figure 4B) adds limited value due to its narrow scale. Therefore, we will remove Figure 4B from the main text and instead provide a summary table (Table Sx) listing all MWL parameters (slope, intercept, and R^2) for both the δ^2H–$\delta^{18}O$ and $\delta'^{17}O$–$\delta'^{18}O$ regressions. The corresponding discussion will be streamlined to avoid overlap between the Results and Discussion sections. These revisions will clarify how both the LMWL and the 17O/18O MWL were derived, will provide a clearer quantitative comparison with the Cheongju GNIP record, and will improve the overall presentation and conciseness of the results.

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 thank the reviewer for this valuable comment highlighting the potential role of ice formation in winter precipitation. We fully agree that ice–vapor equilibrium fractionation can influence d-excess and $\Delta'^{17}O$ differently from liquid-phase condensation, particularly under cold and supersaturated conditions associated with snowfall. As noted by another reviewer, this mechanism has now been incorporated into the revised manuscript.

In the updated version, we have expanded the relevant paragraph in Section 4.2 to acknowledge that some winter samples likely include mixed-phase precipitation (rain and snow) due to the biweekly cumulative sampling design. The revised text explicitly mentions that ice–vapor equilibrium fractionation during snow formation may partially account for the enhanced $\Delta'^{17}O$ variability and altered d-excess patterns observed in winter, citing Jouzel and Merlivat (1984) and Landais et al. (2012) for context. This addition ensures that our discussion of winter isotope variability properly accounts for the effects of ice-phase processes and clarifies the physical mechanisms that could contribute to the observed isotopic dispersion during the cold season.

The correlation analysis (I am torn about it) should be introduced in the results section, not in the discussion. The font colour of the correlation plot should be white where the background is dark; the numbers are hard to read in black against dark blue. The pattern observed certainly corroborate the observation that the biggest changes in the seasonality happen in spring and fall, when the two modes switch over. Note that few of them are truly significant (if that is what the asterisk indicates). Note that the correlations are expressed as R (not R2), and an R~0.5 (equivalent to R2~0.25) is not what would generally be considered a "strong correlation" (which I would see as R2>0.5 and significant p-value).

Response:

We sincerely thank the reviewer for these detailed and constructive comments on the correlation analysis and its presentation. We fully agree that the correlation results should be clearly presented alongside the observational findings rather than as a separate discussion item. In the revised manuscript, this issue has been addressed through the restructuring of the paper into a unified "Results and Discussion" section, which now integrates the descriptive statistical results and their brief interpretation within a single coherent framework.

This restructuring ensures that the correlation analysis is presented in the appropriate context—immediately following the description of the isotope data—while avoiding any redundancy between sections. We have also revised the correlation figure (Fig. 5) to improve readability by changing the text color of the coefficients to white where the background is dark and by clearly marking statistical significance with asterisks ($p < 0.05$ and $p < 0.01$).

The caption now specifies that all coefficients represent Pearson's r values rather than R^2. Furthermore, we have adjusted the accompanying text to clarify that correlations with $r \approx 0.5$ represent moderate relationships, not strong ones, and that only statistically significant correlations are discussed in detail. We appreciate the reviewer's observation that the most prominent seasonal transitions occur during spring and autumn, when shifts between continental and maritime moisture sources dominate—this point has been explicitly incorporated into the revised discussion. Overall, these changes clarify the purpose of the correlation analysis as a diagnostic summary of co-variations between isotopic and meteorological variables, improve the visual quality of the figure, and ensure that the section now reads smoothly within the integrated "Results and Discussion" structure.

The interpretation of the seasonal decoupling of the two excesses is an important point (I would not call them indices, as such would indicate they are scale-normalized to something, line 239). I agree with the general line of argumentation. Yet, the correlation between Δ'17O and δ18O, or between Δ'17O and d-excess is a complicated matter and existing literature (Terzer-Wassmuth et al. 2023) has demonstrated that either there are few correlations indeed, or the higher uncertainty of CRDS-based measurements blurs eventual patterns. Knowing the routine uncertainty of the measurement process would be helpful. And to be frank, the discussion does not address that only 4 in 10 correlations are significant at p<0.001 and only two have an R2~0.4. Again, I think that the overall line of argumentation makes sense, but the statistics do not provide the robustness of foundation desired.

Response:

We thank the reviewer for this insightful and constructive comment regarding the interpretation and robustness of the $\Delta'^{17}O$–d-excess correlations. We fully agree that the statistical relationships between $\Delta'^{17}O$, $\delta^{18}O$, and d-excess are complex and should be interpreted with caution. As also emphasized by Terzer-Wassmuth et al. (2023), such correlations are often weak or inconsistent across datasets, partly because the analytical uncertainty of $\Delta'^{17}O$ from WS-CRDS measurements is higher than that achievable with dual-inlet IRMS techniques.

In our study, only four out of ten correlations are significant at $p < 0.001$, and the strongest relationships yield $R^2 \approx 0.4$. We have now clearly stated this in the revised text. The discussion has been adjusted to explain that the seasonal decoupling between $\Delta'^{17}O$ and d-excess is interpreted primarily as a qualitative observation reflecting the differing sensitivities of these parameters to kinetic and mixing processes, rather than as a statistically strong linear dependence.

We have also included a reference to the routine analytical reproducibility of $\Delta'^{17}O$ (±9 per meg, 1σ), derived from repeated measurements of our in-house STYX standard over one year (Kim et al., 2022). This clarification provides quantitative context for the uncertainty inherent in $\Delta'^{17}O$ measurements and acknowledges that this precision may limit the statistical significance of some weaker correlations. Overall, the revised manuscript now explicitly discusses the uncertainty, the limited significance of the correlations, and the diagnostic rather than causal nature of this analysis, thereby addressing the reviewer's concern about statistical robustness.

The chapter on the Iso-GSM analysis (4.3) seems, sorry to say so, misplaced. Although vaguely introduced in the abstract/introduction, it is hardly to any other part of the manuscript. I recommend the authors to correctly bind it into the main text body, including changes in introduction, abstract, possibly even title, or leave it aside completely, or put into the SM as supplementary data analysis. Nothing that's said in this chapter is wrong, but in my opinion, it does not fit (and it's not very novel either, to be honest).

Response:

We also appreciate the reviewer's comments concerning Section 4.3 and the placement of the Iso-GSM analysis. We understand the concern that this section appeared insufficiently introduced in the Introduction and Methods and might seem peripheral to the main scope of ESSD. In the revised manuscript, we have reframed Section 4.3 to serve as an illustrative example of how the Seoul isotope dataset can be used for model benchmarking rather than as a full-scale modeling study.

The text has been condensed and rewritten to focus on the broad seasonal comparison between observations and Iso-GSM outputs, emphasizing the dataset's potential as a reference for validating isotope-enabled GCMs. To improve coherence, we have added a short statement in the Introduction noting that the dataset can support model–data intercomparison, and have lightly revised the Abstract to mention this application. We have also made it clear that no new simulations were conducted; instead, the comparison uses published Iso-GSM outputs (Yoshimura et al., 2008) to demonstrate potential data applications.

While we recognize the reviewer's point that $\delta^{18}O$ and d-excess comparisons have been done previously, the inclusion of triple-oxygen-isotope data ($\Delta'^{17}O$) provides new opportunities for model benchmarking in future studies. We have therefore emphasized this prospective value while keeping the current analysis concise and consistent with ESSD's data-descriptor format. These changes clarify the limited, demonstrative purpose of the Iso-GSM section, align it more closely with the rest of the paper, and ensure that the manuscript remains firmly within the data-focused scope of ESSD.

If you allow, I'd give two suggestions how to improve: One regards the data analysis: Use daily rainfall data and backtrajectory modelling to determine the source region of the precipitation. This could, as far as I can see, help to refine the conceptual model from Winter=Siberian High / Summer=Monsoon / Spring, Fall=somehow in between to a spatial/seasonal explanation model, and could also help to disentangle the Siberian High fraction in winter. With the existing bi-weekly sampling structures, that could be expressed as "fractions of source region" to match with the isotope dataset. And the second one is forward-looking; I think to make an even greater contribution to modelling improvement, daily samples are, and I am well aware of the collection effort, more poised to address phenomena occurring on a daily/synoptic weather timescale.

Response:

We sincerely thank the reviewer for these constructive and forward-looking suggestions. We fully agree that integrating daily precipitation data with air-mass back-trajectory modelling (e.g., HYSPLIT or FLEXPART) would greatly enhance the ability to quantify the spatial and seasonal variability of moisture-source contributions. Such an approach would allow us to refine the conceptual framework—from the current description of "winter = Siberian High, summer = monsoon, spring/fall = transition"—toward a quantitative source-region attribution model, which could better explain isotopic variations, particularly during winter when continental and oceanic influences coexist.

In the present study, the biweekly integrated sampling scheme limits the feasibility of one-to-one matching with daily meteorological fields. However, we acknowledge that fractions of source-region contribution, derived from trajectory clustering, could indeed be compared to our isotope dataset as an intermediate step, and we will mention this as a potential future analysis in the revised Discussion. We also appreciate the reviewer's comment regarding daily or synoptic-scale sampling as a forward-looking recommendation. We fully agree that such datasets would provide greater temporal resolution to evaluate short-term processes such as individual storm events and transient moisture intrusions.

While the current five-year biweekly dataset already provides a valuable long-term record of seasonal and interannual variability, we plan to complement it with higher-temporal-resolution (event-based) sampling in future field campaigns. These suggestions have been very helpful in shaping our perspective on how to integrate isotopic and meteorological analyses, and we will explicitly note these future directions in the revised Discussion.

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