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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 two reviewers, we have major changes of the manuscript, which are detailed below. Based on the comments from the editor and two reviewers, we have summarized the issues as following.

Reply to the comments by the reviewer 1

1. General Comments

I appreciate the Authors' efforts how they revised their original submission. They have provided clear and reasonable responses to all my comments except the last one where they repeated their penultimate response. Anyway, I think they improved their original submission considerably. I maintain my opinion that this dataset deserves publication, however some minor issues still need revision to reach a publishable manuscript.

Response: We sincerely thank the reviewer for the careful evaluation of our revised manuscript and for the positive assessment of the dataset and its suitability for publication. We also appreciate the reviewer's acknowledgment of the improvements made in response to the previous comments. We apologize for the unintended repetition in our response to the final comment and have now carefully revised the manuscript to address all remaining minor issues. We believe that these additional revisions further improve the clarity and quality of the manuscript, and we are grateful for the reviewer's constructive feedback.

2. Specific Comments

lines 49-50: The "triple oxygen isotope" is written in the text but in the brackets δ^2H is also listed. It is confusing. Please revise the text

Response: We thank the reviewer for pointing out this confusing description. The text has been revised to clarify that the term "triple oxygen isotope" refers exclusively to the oxygen isotope system (δ^{17}O and δ^{18}O), and δ^2H has been removed from the bracketed expression to avoid confusion. Specifically, the phrase "triple oxygen isotope ($\delta^{17}O$, $\delta^{18}O$ and δ^2H)" was revised to "triple oxygen isotope ($\delta^{17}O$ and $\delta^{18}O$)" to remove δ^2H and correct the definition.

line 104: I think "accumulate" should be written instead of "accumulated

Response:

We thank the reviewer for this helpful comment. The word "accumulated" has been corrected to "accumulate" in line 104.

lines 119 and 121: The methodological description sounds contradictory in lines 119 and 121. Please, remove the duplicated information and clarify whether 5 or 6 injections were used to calculate the mean value.

Response:

We thank the reviewer for pointing out this inconsistency. The duplicated methodological description in lines 119 and 121 has been removed, and the text has been clarified to specify that five injections were used to calculate the mean value. The manuscript has been revised accordingly to avoid confusion. Specifically, multiple sentences alternately referring to averaging five or six injections were consolidated into a single description stating that only the final six injections were used to calculate the mean value.

lines 174-176: Considering the newly added information this sentence can be omitted.

Response:

We thank the reviewer for this suggestion. The sentence in lines 174–176 has been omitted, as the newly added information sufficiently covers this point.

lines 185 & 338: LMWL equation presented in the text and the "All dataset" in Fig 4 are different. This is a very critical issue since LMWL could be one of the main achievements of this work. Please check and clarify this confusion.

Response:

We thank the reviewer for carefully pointing out this critical issue. We have re-checked the LMWL equation presented in the text (lines 185 and 338) and the regression shown for the "All dataset" in Fig. 4. The discrepancy was due to an inconsistency between the regression parameters reported in the text and those used in the figure. This has now been corrected by recalculating the LMWL using the same dataset and regression method for both the text and Fig. 4. The LMWL equation is now reported consistently throughout the manuscript, and the figure and text are fully aligned.

line 193: The intercept value in the bracket does not match the one shown in Fig4. Please check and correct.

Response:

We thank the reviewer for carefully noting this discrepancy. The intercept value reported in the text has been re-checked against Fig. 4 and corrected to ensure consistency between the figure and the manuscript.

lines 212&223: The intercept in the δ'17O– δ'18O regression equation differs between the two mentions. Please check and correct the contradiction.

Response:

We appreciate the reviewer's careful reading. We have re-checked the $\delta'^{17}O$–$\delta'^{18}O$ regression intercepts and corrected the inconsistency between lines 212 and 223 to ensure internal consistency in the manuscript.

lines 310-311: This sentence reads like a figure caption. Please revise.

Response:

We thank the reviewer for this helpful comment. The sentence has been revised to improve the narrative flow and to ensure that it reads as part of the main text rather than as a figure caption. The original sentence, "Figure 7 compares the monthly mean stable isotope values (δ^2H and $\delta^{18}O$) and d-excess in precipitation from this study (black line/gray shading) with those simulated by the isotope-enabled Global Spectral Model (Iso-GSM; red line/pink shading)," was revised to "Monthly mean precipitation δ^2H, $\delta^{18}O$, and d-excess from this study are compared with simulations from the isotope-enabled Global Spectral Model (IsoGSM) (Figure 7)," thereby removing figure-caption-style descriptions and rephrasing the text in a narrative form appropriate for the main body of the manuscript.

line 443: Please, replace "n/a-n/a" with the corresponding page ranges.

Response:

We thank the reviewer for this helpful editorial suggestion. The "n/a–n/a" entry has been corrected by replacing it with the appropriate page ranges in the reference list.

<u>Reply to the comments by the reviewer 3</u>

1. General Comments

I appreciate the authors' efforts to improve the manuscript. The study site and methodology are now described in greater detail, and the discussion benefits from incorporating fractionation related to snow formation.

I understand the intention to highlight the dataset's potential for model validation. However, a minimum level of model description is needed to ensure reproducibility.

There is still confusion regarding the processes controlling the isotope composition of precipitation. The authors frequently refer to "evaporation" as a driver of d-excess *and Δ'17O, which is correct. However, it is often unclear whether they mean evaporation at the moisture source—which shapes the isotope composition of atmospheric vapor from which precipitation forms—or post-formation re-evaporation of precipitation. These processes produce opposite trends in isotope signatures, and the current wording makes it difficult for the reader to identify which process is being discussed.*

In addition, the manuscript could be made more concise to improve readability and strengthen the communication of key messages. There is considerable repetition, some of which I highlight in the line-by-line comments below. I recommend that the authors take a step back, identify their core findings, and restructure the results and discussion accordingly.

Response: We sincerely thank the reviewer for the thorough evaluation of our revised manuscript and for acknowledging the improvements in the description of the study site, methodology.

We appreciate the reviewer's comment regarding the need for a clearer and more reproducible description of the model framework. In response, we have revised Sect. 3.5 to include a concise description of the IsoGSM model, explicitly stating that it is an isotope-enabled atmospheric general circulation model, and we have added references to previous studies in which the model has been described and applied. These additions ensure that the model vs. data comparison presented in this study is reproducible for the reader.

We also acknowledge the reviewer's important point regarding the ambiguous use of the term "evaporation." To address this, we have carefully revised the manuscript to clearly distinguish between evaporation at the moisture source (oceanic evaporation that imprints isotopic signatures in atmospheric water vapor) and post-formation processes such as sub-cloud re-evaporation of precipitation. For example, statements that previously referred generically to "evaporation" in the discussion of non-winter isotope relationships were revised to explicitly specify "oceanic evaporation at the moisture source," while sub-cloud re-evaporation is now described separately where relevant.

In addition, following the reviewer's recommendation, we have substantially improved the conciseness of the manuscript by reducing repetitive descriptions and restructuring the Results and Discussion sections. For instance, a paragraph that previously combined interpretations for winter and non-winter seasons was reorganized so that winter precipitation and non-winter precipitation are now discussed in separate paragraphs, thereby reducing redundancy and clarifying the seasonal contrast in isotope controls.

We are grateful for the reviewer's constructive feedback, which has significantly contributed to improving the clarity, rigor, and overall organization of the manuscript.

2. Specific Comments

Line 12-13: When giving values for d18O, d2H and Δ'17O, why not also giving values for d-excess here?

Response: We thank the reviewer for this helpful suggestion. In response, we have now included the corresponding d-excess values alongside $\delta^{18}O$, $\delta^{2}H$, and $\Delta'^{17}O$ in lines 12–13 to provide a more complete summary of the isotope characteristics.

Line 13: The unit of Δ'17O should be per meg here.

Response:

We thank the reviewer for pointing this out. The unit of $\Delta'^{17}O$ in line 13 has been corrected to per meg.

Line 16: specify where relative humidity shifts and thus kinetic fractionation changes?

Response: We thank the reviewer for this comment. The text has been revised to specify that the relative humidity shifts refer to conditions at the moisture source region, where changes in relative humidity control kinetic fractionation during evaporation.

In line 16, the phrase "relative humidity shifts and associated kinetic fractionation" was revised to "relative humidity at the moisture source region and associated kinetic fractionation" to explicitly indicate that the humidity changes controlling kinetic fractionation occur at the moisture source.

Line 17-18: Why the amplitude of variability is an indicator of additional processes? These additional processes could also amplify the seasonal variability of d-excess, isn't it?

Response: We thank the reviewer for this suggestion. The sentence in lines 174–176 has been omitted, as the newly added information sufficiently covers this point. We agree with the reviewer that the magnitude of seasonal variability alone cannot be used to infer the involvement of additional processes, and that such processes could also amplify the seasonal variability of d-excess. Accordingly, we have revised the text to avoid interpreting the smaller amplitude of $\Delta'^{17}O$ as a direct indicator of additional processes, and instead emphasize its lower sensitivity to relative humidity compared to d-excess. We have also clarified that large-scale transport and vapor mixing are not interpreted as direct causes of the seasonal amplitude itself, but rather as additional factors that modulate the variability of $\Delta'^{17}O$.

Line 26: Better "Global climate change is modifying […]" as the process is continuing? Or "has already modified".

Response: We thank the reviewer for this helpful suggestion. The sentence has been revised to use "is modifying" to reflect the ongoing nature of global climate change.

Line 38: During which process(es) heavier isotopes are preferentially removed from and enriched in precipitation? Be more specific here.

Response: We thank the reviewer for this helpful comment. The text has been revised to specify that heavier isotopes are preferentially removed from atmospheric water vapor during condensation and precipitation formation (Rayleigh distillation), and enriched during evaporation and sub-cloud re-evaporation processes affecting precipitation.

Line 47: There is no causal relationship between the proportional behaviour between d18O and d17O and the measurement of d17O. Also, d17O and d18O behave proportional under kinetic fractionation, but on a different slope. Consider rephrasing.

Response: We thank the reviewer for this important clarification. We agree that there is no causal relationship between the proportional behavior of $\delta^{18}O$ and $\delta^{17}O$ and the ability to measure $\delta^{17}O$. We have therefore revised the text to remove this implication. In addition, we now explicitly state that $\delta^{17}O$ and $\delta^{18}O$ exhibit proportional (mass-dependent) behavior under both equilibrium and kinetic fractionation, but with different slopes. The sentence has been rephrased accordingly to improve conceptual accuracy and clarity.

In line 47, "In line 47, the sentence "In addition to $\delta^{18}O$ and $\delta^{2}H$, $\delta^{17}O$ can also be measured because $\delta^{17}O$ behaves almost proportionally to $\delta^{18}O$ under equilibrium conditions … forming the basis of triple oxygen isotope ($\delta^{17}O$, $\delta^{18}O$ and $\delta^{2}H$) studies" was revised to "Simultaneous measurement of $\delta^{17}O$ and $\delta^{18}O$ therefore enables the quantification of subtle deviations from the equilibrium reference relationship, forming the basis of triple oxygen isotope ($\delta^{17}O$ and $\delta^{18}O$) studies," removing the incorrect causal implication and correcting the definition of triple oxygen isotope analysis.

Line 49: triple oxygen isotopes refer to the analysis of all three stable isotopes of oxygen (^{16}O, ^{17}O, ^{18}O), but do not include the hydrogen isotopes, even if they often measured along with and complement each other. Make this clear here.

Response: We thank the reviewer for this important clarification. The text has been revised to explicitly state that "triple oxygen isotopes" refer to the analysis of the three stable oxygen isotopes (^{16}O, ^{17}O, and ^{18}O), and do not include hydrogen isotopes, although hydrogen isotopes are often measured alongside oxygen isotopes and provide complementary information.

Line 54: What do you refer to with "non-steady-state evaporation"? Explain.

Response: We thank the reviewer for this comment. The text has been revised to clarify that "non-steady-state evaporation" refers to evaporation occurring under temporally varying relative humidity and atmospheric conditions at the moisture source, where isotopic equilibrium is not achieved and kinetic fractionation effects are enhanced. In line 54, the phrase "non-steady-state evaporation" was revised to "non-steady-state evaporation driven by humidity gradients," thereby specifying the humidity-related mechanism of the process.

Line 56-57: For me this is a repetition of the information provided in line 54. Better highlight what makes Δ'17O different from d-excess.

Response: We thank the reviewer for this comment. The sentence "While $\delta^{18}O$ and $\delta^{2}H$ mainly record equilibrium fractionation, d-excess and ^{17}O-excess reflect deviations from equilibrium associated with non-steady-state evaporation, vapor mixing, or supersaturation during cloud formation" was revised to "While $\delta^{18}O$ and $\delta^{2}H$ mainly record equilibrium fractionation, d-excess and ^{17}O-excess reflect deviations from equilibrium associated with non-steady-state evaporation under humidity gradients, as well as vapor mixing or supersaturation during cloud formation," thereby clarifying the physical meaning of non-steady-state evaporation and reducing redundancy with subsequent discussion.

Line 121: repeated, but contradicting information from line 119. The last 5 or 6 injections have been averaged. Line 122-123: Repetition of the previous phrase and line 119. Please be concise and revise the whole manuscript for duplicated information.

Response: We thank the reviewer for carefully identifying these repetitions and inconsistencies. The duplicated and contradictory descriptions in lines 119–123 have been removed, and the methodology has been clarified throughout the manuscript for conciseness and consistency. The revised text now clearly states that the first vial was discarded to minimize memory effects, and that the mean value was calculated using the last six injections from the second vial only. Specifically, the original text described the injection procedure in multiple sentences, alternately stating that either the final five or six injections were averaged and repeating the description of duplicate vials, whereas the revised text consolidates this information into a single statement specifying that duplicate vials were prepared, the first vial was discarded to eliminate memory effects, and only the final six injections from the second vial were averaged.

Line 125-127: Repeated information of previous phrase. Remove previous phrase.

Response:

We thank the reviewer for pointing out this redundancy. The repeated information in lines 125–127 has been removed by deleting the previous phrase, as suggested.

Line 147: This link to the website or database should be implemented as a reference in the reference list.

Response: We appreciate the reviewer's comment. The link has been added to the reference list as a formal citation.

Figure 2: The data has been averaged over which time period? From 2016-2020?

Response: We thank the reviewer for this comment. The data shown in Fig. 2 represent averages over the period 2016–2020, and this information has now been explicitly indicated in the figure.

Line 157: The unit of 17O-excess should be per meg here. Please correct and check throughout the manuscript!

Response: We thank the reviewer for pointing this out. The unit of ^{17}O-excess in line 157 has been corrected to per meg, and the unit has been carefully checked and corrected where necessary throughout the entire manuscript to ensure consistency.

Line 160: Explain here further which isotope trend is related to temperature effects and which to rainfall intensity. Be more specific.

Response: We thank the reviewer for this helpful comment. In response, we have revised the text to specify more clearly which isotope signals are related to temperature effects and which are related to rainfall intensity. The revised sentence now explicitly states that δ^{18}O and δ^{2}H are primarily influenced by temperature-dependent equilibrium fractionation during colder seasons, whereas during summer their values are lowered mainly by increased rainfall intensity (the amount effect).

Line 169: abundant precipitation above the ocean or is this local precipitation amount? Is this a local signal that overwrites the primary isotope signal of the water vapor evaporated from the ocean or is the vapor already lower in d-excess? (or a combination of both?)

Response: We thank the reviewer for this insightful comment. In response, we have clarified the physical processes underlying the low d-excess signal observed during summer. The revised text now explicitly describes a combination of effects: (1) a primary source-related effect, whereby high relative humidity at the oceanic moisture source weakens kinetic fractionation during evaporation, resulting in lower d-excess values in the atmospheric water vapor, and (2) a secondary local effect, whereby abundant precipitation and high ambient humidity suppress sub-cloud re-evaporation, allowing this low d-excess signal to be preserved and, in some cases, further reinforced in precipitation.

Line 171: The variability of d-excess should be stated above in Line 155-156.

Response: We thank the reviewer for this helpful and constructive suggestion. In response, we carefully revised the organization of the Results section to improve the logical presentation of d-excess variability. Specifically, the sentence "Unlike d-excess, which peaked in winter (median ≈ 17‰) and reached its lowest values in summer (median ≈ 6‰), ^{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)," which was originally located in line 171, has been moved to immediately follow lines 155–156, where d-excess and ^{17}O-excess are first introduced together with their overall ranges. By relocating this sentence, the seasonal variability of d-excess is now presented at the point of its initial description, improving the coherence of the results and addressing the reviewer's concern regarding the placement of this information.

Line 172-173: Why do these 17O-excess values differ from the ones stated in line 155-156? Stick to one term Δ'17O or 17O-excess throughout the manuscript.

Response: We thank the reviewer for pointing out this inconsistency. The apparent difference between the ^{17}O-excess values reported in lines 155–156 and 172–173 was due to the use of different statistical representations (overall range versus seasonal median values), which was not sufficiently clear in the original text. This has now been clarified in the revised manuscript. In addition, we have standardized the terminology throughout the manuscript by consistently using $\Delta'^{17}O$ (^{17}O-excess) to avoid confusion.

Line 179-181: Why does the sine fit support this interpretation? I don't see the causal relationship. Can you explain?

Response: We thank the reviewer for this insightful comment. We agree with the reviewer that a sine fit does not directly demonstrate a causal physical mechanism. The sine fit is intended only as a descriptive tool to summarize the seasonally recurring pattern in the observed $\Delta'^{17}O$ variability. Accordingly, we have revised the text to remove wording that implied the sine fit "supports" the interpretation, and instead clarify that it serves to illustrate the presence of a seasonal cycle in the data.

Line 198-199: Is it really that equilibrium fractionation dominates or do different kinetic effects in winter and summer cancel each other out causing an apparent equilibrium fractionation dominance?

Response: We thank the reviewer for pointing out this redundancy. We agree with the reviewer that the apparent dominance of equilibrium fractionation should not be interpreted too definitively. We have revised the text to avoid a categorical statement that equilibrium fractionation "dominates," and now clarify that the observed near-global slope may reflect not only genuinely dominant equilibrium fractionation but also an apparent equilibrium-like signal arising from the partial cancellation of seasonally opposing kinetic fractionation effects in winter and summer.

Line 212-213: Again, also kinetic fractionation causes a linear relationship between d17O and d18O, but on a lower slope. Be more specific.

Response: We thank the reviewer for this clarification. In response, we have revised the text to make it explicit that the linear relationship between $\delta'^{17}O$ and $\delta'^{18}O$ is not exclusive to equilibrium fractionation. The revised text now specifies that both equilibrium and kinetic fractionation produce mass-dependent linear relationships, but with distinct slopes, thereby clarifying the process-specific interpretation of the observed $\delta'^{17}O$–$\delta'^{18}O$ relationship.

Line 220: Stick to one notation: d17O and d18O or d'17O and d'18O.

Response: We thank the reviewer for pointing out this redundancy. The repeated sentence in line 38 has been removed as suggested. We thank the reviewer for this comment. The notation has been standardized throughout the manuscript, and we now consistently use $\delta'^{17}O$ and $\delta'^{18}O$ to avoid confusion.

Line 223: The equation presented here is different from the one above (Line 212). Which one is the correct one? Can you provide uncertainties for the slope and the intercept of the LMWL in the triple oxygen isotope system as you did for d2H vs d18O.

Response: We thank the reviewer for pointing out this inconsistency. The discrepancy between the equations reported in lines 212 and 223 has been corrected, and the LMWL equation in the triple oxygen isotope system is now reported consistently in the manuscript. In addition, uncertainties for both the slope and the intercept have been added, analogous to those provided for the δ^2H–$\delta^{18}O$ relationship.

Line 240: Lower temperature increases the fractionation for d18O, but d-excess of evaporated water vapor is lower than for higher temperatures. Also, how do the local meteorological parameters relate to that at the moisture source regions? Can you really draw conclusions from the local meteorology on the conditions at the moisture source or is their modification of these parameters during atmospheric transport?

Response: We thank the reviewer for this thoughtful comment. In response, we have clarified the interpretation of d-excess variability in the revised text. We now emphasize that variations in d-excess at the moisture source are primarily controlled by relative humidity, while temperature plays a secondary and indirect role.

We have also clarified that local meteorological conditions do not directly represent conditions at the moisture source region. Instead, local observations reflect an integrated signal that may be modified during atmospheric transport through processes such as mixing and re-evaporation during precipitation formation. The revised text now explicitly acknowledges these limitations and avoids drawing direct inferences about moisture source conditions based solely on local meteorological parameters.

Line 251: "d18O values were significantly correlated …"

Response:

We thank the reviewer for this comment. The sentence in line 251 has been revised accordingly.

Line 253-254: Repetition of information provided in line 251-252.

Response: We thank the reviewer for pointing out this redundancy. In response, we have streamlined the description of the negative correlation between summer $\delta^{18}O$ and precipitation amount and the associated amount effect. The revised text now concisely explains that the summer depletion of $\delta^{18}O$ is primarily driven by prolonged monsoon precipitation and successive rainout within moisture-rich marine air masses. In contrast, the autumn section retains the explanation that frequent heavy rainfall associated with typhoons and tropical cyclones strengthens the $\delta^{18}O$–precipitation relationship, thereby clarifying the seasonal distinction.

Line 261: How the correlation with the precipitation amount relates to conditions at the moisture source regions?

Response: We thank the reviewer for this comment. The text has been revised to clarify that the observed correlation with precipitation amount reflects an integrated signal influenced by moisture source characteristics and subsequent atmospheric transport processes, rather than a direct response to local precipitation amount alone. Specifically, in line 261, the sentence relating winter correlations with precipitation amount to moisture source conditions was revised to emphasize that the observed relationship represents an integrated signal shaped by moisture source characteristics and atmospheric transport, rather than a direct interpretation based solely on local precipitation amount.

Line 263-274: Again, in this paragraph, there is information repeated that has partly been provided already in the paragraph before. There is high potential to make the text much more concise, overall improving the readability of the manuscript and better highlighting the key messages.

Response: We thank the reviewer for this constructive comment. In response, we have substantially revised the paragraph in lines 263–274 by removing the detailed, season-by-season descriptions (summer, spring, autumn, and winter) that were partly repetitive of the preceding paragraph. The revised text consolidates these descriptions into a more concise synthesis emphasizing that seasonal isotope variability reflects the combined influence of local meteorological conditions and synoptic-scale circulation patterns, with monsoon-driven moisture transport dominating summer depletion and cold continental air masses exerting a primary control during winter. These changes improve the overall readability of the manuscript and more clearly highlight the key messages of the study.

Line 284: Kinetic fractionation due to evaporation increases d18O and decreases Δ'17O and d-excess in the liquid phase. Make clear that this is the imprint in the vapor phase due to evaporation from the ocean.

Response: We thank the reviewer for this helpful comment. In line 284, the interpretation of kinetic fractionation was revised to clarify that the observed $\Delta'^{17}O$–d-excess relationship reflects vapor-phase isotopic signatures generated during oceanic evaporation at the moisture source, rather than changes occurring directly in the liquid phase. The revised text now explicitly states that this vapor-phase signal is subsequently transferred to precipitation.

Line 288: Evaporation of what? Of precipitation (higher evaporation would lead to a decrease in Δ'17O and d-excess) or ocean water (higher evaporation would lead to an increase of Δ'17O and d-excess in atmospheric water vapor and thus precipitation) or moisture recycling along the air mass trajectory?

Response: We thank the reviewer for these helpful comments. In response, the sentence "These observations suggest that, in non-winter seasons, kinetic processes such as evaporation and sub-cloud re-evaporation exert a dominant influence on isotopic composition" was revised to "These observations suggest that, in non-winter seasons, kinetic processes acting during oceanic evaporation and subsequent sub-cloud re-evaporation of precipitation exert a dominant influence on isotopic composition," thereby explicitly distinguishing oceanic evaporation at the moisture source from post-formation sub-cloud re-evaporation. This revision removes ambiguity regarding the phase and location of the evaporation processes and ensures that the interpretation of kinetic fractionation is physically consistent with the processes controlling the observed isotope signals.

Line 301: The previous paragraph starts with a similar phrase. Consider focussing the previous paragraph only on winter precipitation and this paragraph on the non-winter seasons.

Response: We thank the reviewer for this helpful suggestion. In response, the discussion was reorganized to clearly separate winter and non-winter precipitation. The original text described both non-winter and winter isotope relationships within the same paragraph, beginning with "Our analysis showed that, during spring, summer, and autumn…" and subsequently continuing with winter-specific interpretations. In the revised manuscript, this paragraph was rewritten so that it now focuses exclusively on non-winter seasons (spring, summer, and autumn), describing the $\Delta'^{17}O$–$\delta^{18}O$ and $\Delta'^{17}O$–d-excess relationships and their consistency with non-steady-state evaporation and kinetic fractionation processes. The discussion of winter precipitation, including the lack of significant correlations and the enhanced variability of $\Delta'^{17}O$ related to vapor mixing and ice-phase fractionation, was moved to a separate preceding paragraph dedicated to winter conditions. This restructuring removes redundancy in paragraph openings and clarifies the seasonal contrast requested by the reviewer.

Sect. 3.5: I understand the intention from the authors to show the potential of the dataset for model validation and improvement. However, there should be at least a minimum of information on the model, a reference to another publication where the model is described. Up to now, this model-data comparison is not reproducible for the reader.

Response: We thank the reviewer for this important comment. In response, Sect. 3.5 was revised to improve the reproducibility of the model–data comparison by providing a concise description of the IsoGSM model and adding explicit references to prior studies in which the model has been described and applied. Specifically, the revised text now states that IsoGSM is an isotope-enabled atmospheric general circulation model that simulates stable water isotopes within the atmospheric hydrological cycle and has been widely used to investigate the climatology and variability of precipitation isotopes, as well as to evaluate isotope-enabled process representations in global models (Yoshimura et al., 2008; Farlin et al., 2013; Kathayat et al., 2021; Kim et al., 2019; Tada et al., 2021). By explicitly introducing the model framework and its prior applications, the revised text enables readers to trace the underlying model assumptions and parameterizations, thereby allowing the model–data comparison in Sect. 3.5 to be independently reproduced and evaluated. These additions clarify the model framework underlying the comparison and ensure that the analysis presented in Sect. 3.5 is reproducible for the reader.

Line 312: Do you refer here to d18O and d2H? However, these values are rather depleted in winter and peak in spring before decreasing in summer again. Consider rephrasing to avoid confusion.

Response: We thank the reviewer for this comment. The sentence "The IsoGSM outputs are slightly more depleted in δ^2H and δ^{18}O, especially during late autumn and winter and exhibit a narrower range of d-excess variability compared to the observations" was revised to "The IsoGSM outputs are slightly more depleted in δ^2H and δ^{18}O, particularly during late autumn and winter, and show a reduced amplitude of d-excess variability relative to the observations, indicating a more limited representation of kinetic fractionation processes in the model," This revision makes the nature of the model–data discrepancy more explicit by emphasizing that the reduced d-excess variability in IsoGSM reflects limitations in representing kinetic fractionation processes, particularly under cold-season conditions.

Line 337: Is it the Δ'17O that is influenced by additional processes, or could also d-excess be sensitive to these other processes?

Response: We thank the reviewer for this helpful comment. The sentence "Meanwhile, Δ'17O showed a similar seasonal cycle, although with reduced amplitude, highlighting its additional sensitivity to large scale circulation and vapor transport" was revised to "Meanwhile, Δ'^{17}O exhibited a similar seasonal cycle with a reduced amplitude, indicating that, although both Δ'^{17}O and d-excess are influenced by large-scale circulation and vapor transport, Δ'^{17}O is relatively more sensitive to these processes,". This revision clarifies that the involvement of additional processes is not unique to Δ'^{17}O, but that Δ'^{17}O and d-excess differ in their relative sensitivity to large-scale circulation and vapor transport, thereby addressing the reviewer's concern and avoiding a misleading interpretation.

Line 340: both equilibrium and kinetic fractionation are mass dependent. You may want to specify equilibrium fractionation here, highlighting its difference from winter, where kinetic effects became significant?

Response: We thank the reviewer for this important clarification and agree that both equilibrium and kinetic fractionation are mass dependent. In response, the original sentence, "The δ^{17}O–δ^{18}O relationship confirmed mass-dependent fractionation across the dataset; however, the slope steepened during winter, indicating stronger kinetic effects under low humidity," was revised to "The δ^{17}O–δ^{18}O relationship indicated equilibrium fractionation dominates the isotope system during the non-winter seasons, whereas the steeper slope observed in winter reflects an increased contribution from kinetic fractionation under low humidity conditions." This revision explicitly specifies equilibrium fractionation for non-winter seasons and highlights its contrast with winter conditions, thereby clarifying the intended interpretation and improving the conceptual accuracy of the 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