Note: The comments are in black, and our replies in blue.

Editor

Your manuscript has be re-evaluated after your revisions based on a first round of reviews. Unfortunately, the majority of the reviewers still require major improvements of your manuscript before it can be accepted. The requested revisions are substantial for a second round of reviews and concern the scientific results of this paper at their core: i.e. the interpretation of the results, the discussion of the results in relation to the literature in the conclusions, as well as the methodology (trajectory calculation and moisture source identification). Therefore, I leave it open to the authors if they prefer to 1) tackle the significant revision work requested by the reviewers in a second round of revisions or 2) to withdraw their paper and take the time to carefully revise their manuscript.

Re: We sincerely thank you for your time and guidance on our manuscript. We also thank the reviewers for valuable comments and suggestions to the manuscript. After careful consideration of the reviewers' feedback, we have thoroughly revised our manuscript. Specifically, we have updated our methodology by incorporating ERA data for trajectory calculation and moisture source identification. Additionally, we have calculated the fraction of within-boundary-layer moisture contribution over land and its correlations with vapor isotopes. We also conducted lead-lag correlation analyses between *d*-excess and RH_{SST}. These updated or additional results not only strengthen our previous conclusions but also provide further support for our interpretations. Furthermore, we have meticulously reviewed the manuscript's text to refine its expressions and enhance clarity. The discussion has been enriched in relation to the literature to better contextualize our findings within the broader context. Finally, we have reorganized the concluding section to align it with the journal's guidelines.

Referee #1

The authors have responded to the reviewer comments properly. The quality of the revised manuscript has been significantly improved, including both language and scientific aspects. The structure and organization are much better than the original submission (the first version). However, there are still some places where clarification from the authors is needed, largely about the language, but also a few concerns about the scientific aspects. Please check the annotated PDF file for my detailed comments.

Re: We sincerely thank the referee for the positive feedback on our effort during the previous revision and for the additional suggestions included in the attached PDF file. Following your suggestions as well as the feedback from the other two referees, we have revised the manuscript accordingly. Please see the point-by-point response below.

Comments and suggestions in the attached PDF file:

L14 changes → shifts

Re: modified

L14-15 The second sentence breaks the logical flow, as the first and third sentences are logical related but the second one has no or not much logical connection with these two sentences. You can consider to remove this senence

Re: This sentence has been removed.

L33 under the backdrop of → in response to

Re: These two sentences have been combined: "*Recent climate change has induced significant hydrological shifts, marked by drying trends in the southeastern TP (SETP) and wetting in the north (Jiang et al., 2023; Zhang et al., 2023; Yao et al., 2022).*"

L39-40: Water stable isotopes are natural tracers of the water cycle, offering → As natural tracer of the water cycle, water stable isotopes offer

Re: modified

L78: are → have been or are generally

Re: rephrased as "have been"

L103: Samples → Those

Re: modified

L143: remove "though mass balance principles"

Re: removed

L193: Somehow and somewhere you should provide a brief explanation of why you divide or present the results in four period, monsoon, non-monsoon, May and Oct.

Re: It was defined in section 2.2 as "*Consistent with Yao et al. (2013), we defined June-September (JJAS) as the summer monsoon season. In contrast, November-April (Nov-Apr) was designated as the non-monsoon season, with May and October considered transition periods between the two seasons.*" We have now moved this definition of seasons to here at the beginning of section 3.1.

L194: inserted ";"

Re: The definition of seasons has now been moved to here at the beginning of section 3.1.

L196: why specifically point out isotopes of these two months? I forgot to ask last time

Re: May and October were considered as transition periods between the summer monsoon and non-monsoon seasons.

L203: both d18O and d-excess are lower during monsoon and high during non-monsoon seasons, and only slightly different I think. You somehow should rephrase this sentence.

Re: Yes, it is not rigorous to say $\delta^{18}O$ and *d*-excess show different seasonal variations. In accordance, we have rephrased the first two sentences as follows:
"Although d-excess values are also lower during the summer monsoon season and higher during non-monsoon periods, the timing of seasonal transitions differs from that of $\delta^{18}O$ (Fig. 1b)."

L213 highlights: use a different and similar word

Re: we have rephrased the second "highlights" as "indicates"

L265 BOB: I think it is the first time you use BOB, and should define it.

Re: It was defined in the Introduction section at L65 when it first appears.

L296 AS: define it

Re: It was defined in the Introduction section at L65 when it first appears.

L304-305: rephrase the sentence as "However, the clustering of data point by season (Fig. 6) suggest that the apparent ……"

Re: the sentence has been rephrased as follows: "*However, the clustering of data points by season (Fig. 6) suggests that the apparent……*"

L308: suggest → reveal or indicate

Re: it has been rephrased as "reveal"

L316: rephrase this sentence

Re: the sentence has been rephrased as follows: "*Although the correlation is significant during the non-monsoon season, the explained variance in d-excess remains low, at a maximum of 10%-16% over the northern BOB.*"

L346-349: Most H1 is over continent and thus moisture recycling is more important!

Re: Regarding moisture budget, yes, moisture recycling is important throughout the year. But this is not equally to say it is more important for determining isotopic compositions. In the revised manuscript, we have analyzed the correlation between *d*-excess and recycling ratio. And the following text has been added at L472-476:
"Interestingly, vapor $\delta^{18}O$ exhibits a noticeable positive correlation with the fraction of within-boundary-layer moisture contribution over land during the non-monsoon season ($r = 0.47$, $p < 0.01$), supporting that enhanced

continental recycling would elevate δ^{18}O values (Fig. 3a). However, correlations between the fraction of terrestrial moisture source and δ^{18}O for other seasons or with d-excess are either insignificant or marginal (Table S1)."

To some extent, these negligible correlations suggest that *d*-excess is not directly controlled by the fraction of continental recycling. This implies that the impact of continental recycling may be less significant compared to the large-scale atmospheric circulation patterns we identified.

L380: such as → like

Re: modified

L397-398: remove "vapor d-excess shows", and replace "with" with "is observed"

Re: modified

L400: undergone a degree of rain vapor interaction due to evaporation → experiences vapor-rain interaction through rain evaporation

Re: to make the terminology consistent throughout the text, it has been rephrased as "*experienced rain-vapor interaction through rain evaporation*"

L405: degree → extent

Re: modified

L408: rephase the sentence as "δ^{18}O and d-excess show a trend of weak correlation at high δ^{18}O values, but a stronger correlation when δ^{18}O values are low"

Re: rephrased

L431: for → over

Re: modified

L439: further evidence for → further insights or additional insights into

Re: it has been rephrased as "further insights into"

L459: add a "," before "as well as"

Re: added

L460: results in Section 3.3 → our results (section 3.3)

Re: modified

L465: and → with, and remove "respectively"

Re: modified

L470-471: This sentence sounds not alright, and what about non-monsoon seasons?

Re: The westerlies dominate during the non-monsoon seasons. We have rephrased the sentence as "*Water isotope signatures on the TP were thought to reflect this interplay between the summer monsoon and non-monsoon seasons*"

L472: seasonally shifting → seasonal shifts in

Re: modified

L483: citations?

Re: the citation of (Yao et al., 2013) has been added at here.

L491: remove "-long"; observed → collected

Re: modified

L495: I think you can simply call it "normalized RH', which is what people call in other papers.

Re: it has been rephrased as "normalized RH"

L516-517: You have an exact sentence in the first paragraph (second sentence) of Conclusion.

Re: This sentence has been removed.

Referee #3

The authors have tried to revise the manuscript in response to the previous reviewers' comments. However, in my understanding, the issues raised by the previous reviewers remain unresolved. Specifically, both Reviewer 1 and Reviewer 2 highlighted that the specific scientific question of this study remains unclear, and the structure of the manuscript lacks logic, particularly in the introduction section. Moreover, I find that some of the methods employed in the study are not reasonable, and key points of the conclusions are unconvincing. My detailed comments are as follows:

Re: We sincerely thank the referee for the evaluation and comments on our manuscript and for pointing out the areas that require further clarification and improvement. In response to your comments regarding the unclear scientific question and the lack of logical structure in the introduction section, we have rechecked this part and made revisions to further improve its logic flow. Additionally, in response to your comments about the methods employed, we have carefully reviewed our approach and provided further justification of the methods. Besides, we have also included additional results. Regarding the key points of the conclusions being unconvincing, we have strengthened our findings by incorporating additional data and analyses that support our interpretations. Furthermore, we have reviewed the entire manuscript to improve clarity and avoid any potential miscommunications. Please see below the detailed response to each of your comments.

The authors believe that the significant negative correlation between d-excess and relative humidity of the Indian Ocean moisture source is due to the seasonal variation of relative humidity in the oceanic moisture source is opposite to that of excess deuterium. But there is no causal relationship between them. As analyzed by the authors, the moisture source for summer monsoon season is different with that for the non-monsoon season, i.e., the moisture is transported from the continents by westerlies during the non-monsoon season, while it comes from the Indican Ocean during the summer monsoon season. Given the presence of oceanic and continental moisture sources during the different seasons, it is inappropriate for the author to calculate the correlation between d-excess and relative humidity of the moisture source by combining data from the monsoon and non-monsoon periods. Additionally, the authors calculated the correlation between d-excess and relative humidity of the moisture source separately for the monsoon and non-monsoon periods, and found that compared to the annual data, the correlation weakened during either the monsoon or non-monsoon period. However, when calculating the correlation between d-excess and relative humidity of the moisture source, the authors did not account for the transport time of the moisture from the source to the study site, which is also inappropriate.

Re: Regarding the first part, we think the comment is not contradict with our analysis and results. You raise an important point regarding the lack of a causal relationship between *d*-excess and relative humidity of the Indian Ocean moisture source, despite the observed negative correlation. The aim of this section was to test whether *d*-excess preserves oceanic evaporation conditions, as has been suggested in some previous vapor and ice core studies. An outcome or implication of this study is that this approach may not be appropriate when combining data from monsoon and non-monsoon periods. But this is what has been done previously. By analyzing seasonal differences, we found that the correlation between *d*-excess and RH_{SST} weakens when considering monsoon and non-monsoon periods separately, reinforcing your point about the inappropriateness of combining these datasets. Our analysis demonstrates why it is inappropriate and reminds researchers that it should be cautious when interpreting such correlations. This point has been clarified at L463-469: "*Attempts have been made to establish relationships between vapor d-excess and* RH_{STT} *(Chen et al., 2024; Liu et al., 2024), as well as between ice core d-excess and* RH_{STT} *(Shao et al., 2021) or SST (Zhao et al., 2012). Based on our results, however, the apparent relationships are primarily a result of similarities in the seasonality of these variables. The preservation of oceanic source region conditions by vapor d-excess have also been questioned at other continental sites (Fiorella et al., 2018; Aemisegger et al., 2014; Welp et al., 2012; Wei and Lee, 2019; Samuels‐Crow et al., 2014). Instead, these studies emphasized the role of other processes, such as continental recycling and mixing with subsiding air masses.*"

Regarding the second part, we acknowledge your concern regarding the lack of consideration for transport time in our initial correlation analysis. In response, we conducted additional lead-lag correlation analyses between *d*-excess and RH_{SST}, accounting for transport times ranging from 1 to 11days prior to the day corresponding to *d*-excess observations. The results of this analysis show that the correlation between *d*-excess and RH_{SST} does not improve when considering these lagged timeframes. These results further support our conclusion that seasonality plays a dominant role in shaping these correlations. We have added the following text to the revised manuscript at L319-323:

"*To account for transport time, we examined correlations between d-excess and RH_{SST} from 1 to 11 days prior to the d-excess observation dates during the summer monsoon (Fig. S6) and non-monsoon seasons (Fig. S7), respectively. The results are consistent with those shown in Fig. 5, indicating that considering these lagged timeframes does not enhance the correlation between d-excess and RH_{SST}.*"

Additionally, we have included supplementary figures that detail these lead-lag correlation results:

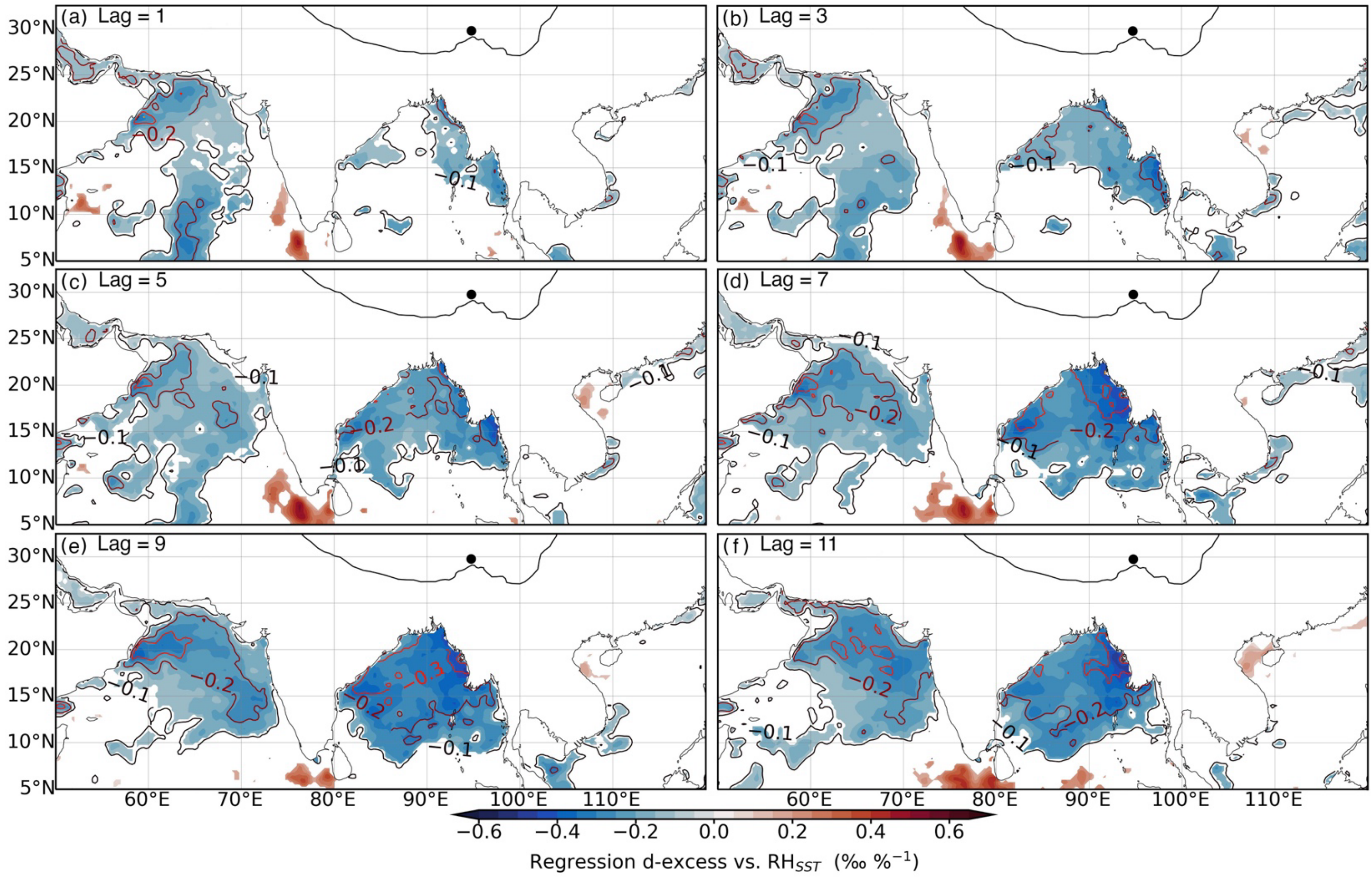

Figure S6. Relationships between vapor d-excess and relative humidity scaled to sea surface temperature (RH_{SST}) during (a) 1, (b) 3, (c) 5, (d) 7, (e) 9, and (f) 11 days before the day corresponding to d-excess observation dates for the data within the summer monsoon season. Shading represents regressions of d-excess against RH_{SST} (only values significant at the 95% significance level are shown). Contours at an interval of 0.1 indicate correlation coefficients between them (only negative correlations are shown). The black dots mark the location of the SETP station. The black solid lines denote the Tibetan Plateau with altitude contour at 3000 m.

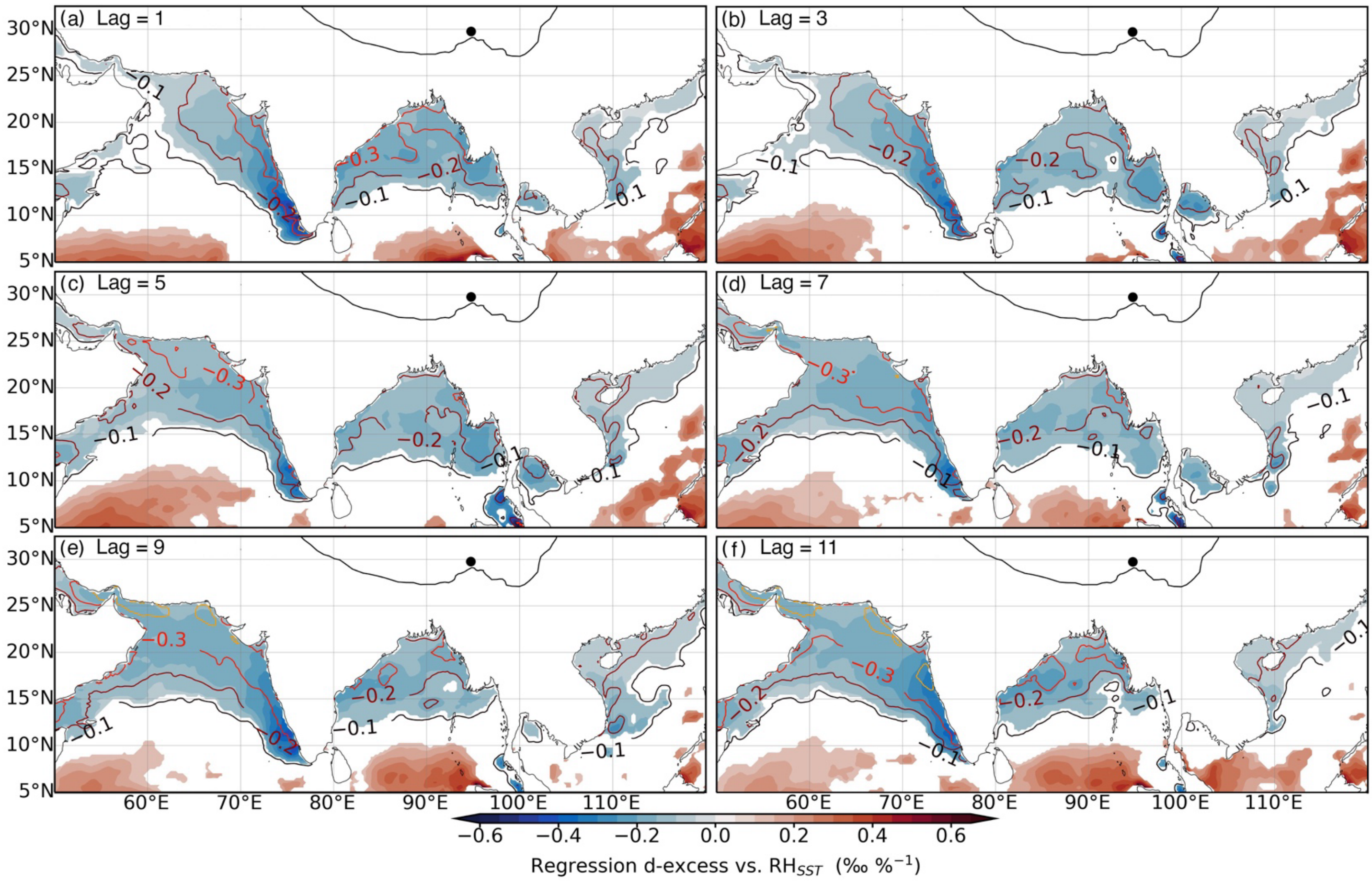

Figure S7. Same as Fig. S6 but for the non-monsoon season.

Furthermore, one of the key conclusions of this paper is that d-excess and $\delta^{18}O$ during the monsoon season are influenced by the evaporation effect of raindrops. Although this is possible, the authors also need to consider other possible factors. For example, during the summer monsoon period, a strong rainout effect along the moisture transport pathway could also potentially account for the decrease in $\delta^{18}O$ and the increase in d-excess.

Re: Yes, the rainout effect should not be ruled out. While raindrop evaporation is emphasized in our study as s significant process leading to a "super Rayleigh" trajectory, we did not rule out the role of rainout in our conclusion. In our conclusion, we considered both possibilities: rainout, which typically results in a Rayleigh distillation process, and raindrop evaporation, which introduces an additional effect and leads to the "super Rayleigh" trajectory. Our conclusion is that the isotopic compositions during the monsoon season are not solely the results of simple Rayleigh processes but involve the added influence of raindrop evaporation. This dual-process mechanism helps explain the observed trends in *d*-excess and $\delta^{18}O$. To ensure clarity, we have refined our terminology throughout the manuscript. The term "super Rayleigh" refers to isotopic trajectory beyond what is expected from classical Rayleigh distillation, often indicating additional processes such as rain evaporation. We have checked our expressions on this point throughout the manuscript and revised the text accordingly to avoid any potential miscommunications.

Referee #4

Review of Cai et al ACP

Cai et al provide an analysis of a series of water vapor isotope measurements taken over three years from the South-East Tibetan Plateau Station to try to investigate the hydrological processes driving variations in water vapor isotope ratios in the region. They conclude that: a) d-excess may not be a reliable indicator of oceanic evaporation conditions, b) high d-excess values in the non-monsoon season are driven by dry air intrusions and westerly winds, and c) summer d18O and d-excess reflect "super-Rayleigh" processes and partial rain evaporation.

Re: We sincerely thank the referee for the thorough and constructive comments on our manuscript. We have carefully addressed all the scientific issues by refining the text of the manuscript and by providing additional data and results, including re-run the trajectory calculations and related analysis using the ERA5 data, the fraction of within-boundary-layer moisture contribution over land and its relationship with vapor isotopes. Please see below the point-by-point response to the specific comments.

The analysis of the isotopic data seems sound, but the analysis seems incomplete and many of the conclusions are not novel. For example, several studies have investigated and questioned the preservation of d-excess in water vapor from the evaporative source, as the authors note in L. 71-74. This thread isn't really followed up on in the current manuscript, and the results for this site would be stronger if they were put into better context with this existing literature.

Re: We have enhanced the comparison of our findings with previous studies. The preservation of meteorological conditions by *d*-excess in water vapor from the oceanic evaporative source is further discussed in L466-469 and L473-482 in Section 4 and L535-540 in Section 5:

"*The preservation of oceanic source region conditions by vapor d-excess have also been questioned at other continental sites (Fiorella et al., 2018; Aemisegger et al., 2014; Welp et al., 2012; Wei and Lee, 2019; Samuels‐Crow et al., 2014). Instead, these studies emphasized the role of other processes, such as continental recycling and mixing with subsiding air masses.*"

"*Interestingly, vapor $\delta^{18}O$ exhibits a noticeable positive correlation with the fraction of within-boundary-layer moisture contribution over land during the non-monsoon season ($r = 0.47$, $p < 0.01$), supporting that enhanced continental recycling would elevate $\delta^{18}O$ values (Fig. 3a). However, correlations between the fraction of terrestrial moisture source and $\delta^{18}O$ for other seasons or with d-excess are either insignificant or marginal (Table S1). Further quantification of the effect of continental recycling on vapor isotopes requires detailed knowledge of the isotope compositions of evapotranspiration fluxes. In this study, we utilized a simplified assumption regarding the isotopic composition of these fluxes to explore their influence on vapor isotopes. Therefore, future research should prioritize characterizing the isotopic signatures of both evaporation and transpiration fluxes, as well as determining the ratio between these two fluxes. This will provide deeper insights into how continental recycling shapes vapor isotope compositions.*"

"While this study questions the earlier interpretation of TP d-excess as an indicator of oceanic evaporation conditions (Zhao et al., 2012; Shao et al., 2021; Chen et al., 2024; Liu et al., 2024), other studies have also raised doubts about the preservation of these signals inland (Fiorella et al., 2018; Aemisegger et al., 2014; Welp et al., 2012; Wei and Lee, 2019; Samuels‐Crow et al., 2014). Further research is needed to determine how far inland oceanic evaporation signals can be preserved during the transport from coastal areas."

In addition, many of these studies also used some sort of moisture footprint analysis such as Sodemann et al. (2008) but took an extra step to link evaporative sources to the hypotheses being tested. For example, the authors could analyze what fraction of the vapor would have arisen from within-boundary-layer evaporation over land, and correlated that with d-excess and d18O to support their claims regarding ET and recycling.

Re: Actually, we did a preliminary analysis of the fraction of within-boundary-layer contribution over land before the submission of this work. The results suggest that land regions consistently dominate the moisture contribution, with oceanic contributions being marginal. Therefore, its correlation with vapor isotopes was not further analyzed. In response to your suggestion, we have performed an additional correlation analysis between vapor isotopes (*d*-excess and δ^{18}O) and the fraction of within-boundary-layer moisture contribution over land for different seasons (Table S1). Among the correlations, *vapor δ^{18}O shows a noticeable correlation with the fraction of within-boundary-layer moisture contribution over land during the non-monsoon ($r = 0.47$, $p < 0.01$), supporting enhanced continental recycling would elevate δ^{18}O values (Fig. 3a).* For other seasons or with *d*-excess, the correlations are either insignificant or marginal. However, we acknowledge that further investigation into how continental recycling affects vapor isotopes is constrained by difficulties in estimating variations in the isotopic compositions of evapotranspiration fluxes. We propose that future studies focus on this aspect to better quantify the role of continental recycling on vapor and precipitation isotopes.

In the revised manuscript, we have mentioned the results on the fraction of continental recycling and its correlations with *d*-excess and δ^{18}O at L184-185, L282-283, and L473-482:

"Overall, the fractions of within-boundary-layer contributions are 60.2% over land and 5.0% over ocean, with an additional 27.8% originating from above the extended boundary layer."

"Quantitatively, the within-boundary-layer contributions from oceanic regions are determined to be 2.5%, 9.1%, 4.6%, and 2.0% for non-monsoon, summer monsoon, May, and October, respectively."

"Interestingly, vapor δ^{18}O exhibits a noticeable positive correlation with the fraction of within-boundary-layer moisture contribution over land during the non-monsoon season ($r = 0.47$, $p < 0.01$), supporting that enhanced continental recycling would elevate δ^{18}O values (Fig. 3a). However, correlations between the fraction of terrestrial moisture source and δ^{18}O for other seasons or with d-excess are either insignificant or marginal (Table S1). Further quantification of the effect of continental recycling on vapor isotopes requires detailed knowledge of the isotope compositions of evapotranspiration fluxes. In this study, we utilized a simplified assumption regarding the isotopic composition of these fluxes to explore their influence on vapor isotopes. Therefore, future research should prioritize characterizing the isotopic signatures of both evaporation and transpiration fluxes, as well as determining the ratio between these two fluxes. This will provide deeper insights into how continental recycling shapes vapor isotope compositions."

Table S1. The correlation coefficients between vapor isotopic variables (δ^{18}O and *d-excess*) and the fraction of within-boundary-layer moisture contribution over land for different seasons between 2015-2017. Values with significance levels ***exceeding 99%***, **between 99% and 95%**, *95% and 90%*, are in bold italic, bold, and italic, respectively.

	2015-2017 All	Nov-Apr	JJAS	May	Oct
δ^{18}O	*-0.07*	***0.47***	0.05	-0.07	**-0.25**
d-excess	***-0.23***	*-0.10*	0.01	0.06	0.05

There are also a few aspects of the back trajectory setup and analysis that raise additional questions:
First, why were GDAS meteorological fields used for the back trajectories when ERA5 was used for the remainder of the analysis? In the introduction, the authors make the point that it is thought that mountain valleys in the SETP are important pathways for water vapor transport (L. 78-79), and it seems likely that to the extent these surface features are represented in either data product, ERA5 at a nominal 25km resolution is much more likely to capture topographic variations and their impact on wind fields and moisture transport than GDAS at ~100 km.

Re: The reason that we did not use ERA5 to drive trajectory calculations was mainly due to computational limitations, especially limitations to data access and storage. To address this issue, we have revised our methodology by implementing a nested approach for trajectory calculations in the revised manuscript. Specifically, within the domain of 0-50°N and 40-120°E the trajectory calculation is driven by ERA5 data and outside of this domain the trajectory calculation is driven by GDAS data. This approach balances computational efficiency with the need for higher resolution in critical regions. We chose the domain of 0-50°N and 40-120°E because it covers the major moisture sources regions identified in our previous analysis. The associated results have been updated in the revised manuscript accordingly. Overall, the results from this nested setup are consistent with those obtained using only GDAS data. However, the new results show a trend of enhanced moisture contribution from southern regions. We recognize that further evaluation and direct comparison of GDAS and ERA5 datasets would provide deeper insights into their relative strengths and limitations. However, such an extensive comparison is beyond the scope of this study. We have discussed that moisture tracking using higher resolution meteorological data or in regional high-resolution modelling is desired in future research at the end of the conclusion section.

Second, how was vertical velocity calculated in these trajectories? I suppose from the description in section 2.4 that the model vertical velocity was used, but it would be good to specify. In addition, how were vertical heights used to launch trajectories chosen? I suspect that it may not change the conclusions too much, but it is also possible that potential contributions from more remote vapor sources are given too small a weight if the boundary layer height is substantially higher than 500 m, given there is so much weight near the surface.

Re: Yes, the vertical motion was indeed calculated using the model vertical velocity. We have now explicitly stated this point in the revised manuscript at L165: "*In addition, the vertical motion was also driven by the model vertical velocity.*" The starting vertical heights for our trajectories were set at 7 different vertical levels: 10, 50, 100, 200, 300, 400, and 500 m above ground level, and this information was mentioned at L167-168. Before determining using these heights, we have conducted some sensitivity tests to evaluate the impact of varying starting heights on our conclusions. Yes, you are right that conclusions would not change much if we used some different heights. This range was chosen to capture a comprehensive view of near-surface moisture sources, aligning with our study's focus on vapor isotopes close to the surface. We acknowledge your point that boundary layer heights significantly higher than 500 m could result in underweighting of remote vapor sources. However, our sensitivity tests suggest that this potential bias does not materially affect the conclusions drawn within the scope of our study. But if the research interest is on precipitation, we recognize that starting points at higher atmospheric levels would be necessary, as precipitation often forms in these upper layers. We have also discussed this point in the revised manuscript at L542-544: "*Furthermore, the focus on lower tropospheric vapor sources contrasts with precipitation sources at higher levels, which may differ and require additional exploration.*"

Third, I am a bit unclear on what the statement "Unlike previous applications focused on identifying evaporative moisture sources from the Earth's surface, this study emphasizes the contribution of air parcels themselves to SETP's humidity." (L. 181-183) means in the context of the Sodemann et al. (2008) tracer that has been applied here, as all studies using this or a similar tracer are inherently weighted by humidity contributions to the receptor site.

Re: In the framework of Sodemann et al. (2008), the purpose is mainly to determine the location and contribution of evaporative moisture sources. Our study extends this by examining how air parcels contribute to humidity at SETP, considering their journey and meteorological history. This means we are interested not only in the sources of moisture but also in how much each air parcel contributes to the humidity levels at SETP. By emphasizing the contribution of air parcels themselves, we are essentially examining the history of moisture transportation. This involves understanding how much moisture from each air parcel reaches SETP and under what conditions. This additional layer of analysis connects the moisture history with vapor isotopes, providing a more integrated understanding of humidity sources and transport dynamics. We have revised this paragraph as follows to improve clarity:
"*In this framework, the moisture source can be attributed into four categories: contributions within an extended boundary layer over 1) land and 2) ocean, 3) contributions from above the extended boundary layer, and 4) remaining unattributed sources. Following Sodemann et al. (2008), the extended boundary layer was parameterized as 1.5 times the boundary layer height. The diagnostic results indicated that approximately 7.0% of the moisture arriving at SETP remained unattributed, confirming that 10-day trajectories are sufficient to diagnose most moisture sources. Overall, the fractions of within-boundary-layer contributions are 60.2% over land and 5.0% over ocean, with an additional 27.8% originating from above the extended boundary layer. Additionally, this study emphasizes the contribution of air parcels themselves to SETP's humidity. This variable captures the history of the moisture and indicates how much moisture within each air parcel finally reaches SETP.*"

Finally, the authors indicate the role of the westerlies and dry, cold air intrusions as likely responsible for the non-monsoon period patterns of d18O and d-excess. However, can the authors rule out the importance of local evaporation of precipitation or surface/soil waters, which as described elsewhere in the manuscript and given the site's elevation is likely to also have low d18O and high d-excess?

Re: Following your above suggestion, we have conducted a correlation analysis between the fraction of within-boundary-layer contribution over land and $\delta^{18}O$ as well as *d*-excess. Among the correlations, *vapor $\delta^{18}O$ exhibits a noticeable positive correlation with the fraction of within-boundary-layer moisture contribution over land during the non-monsoon season ($r = 0.47$, $p < 0.01$), supporting that enhanced continental recycling would elevate $\delta^{18}O$ values (Fig. 3a).* For other seasons or with *d*-excess, the correlations are either insignificant or marginal. To some extent, these negligible correlations suggest that *d*-excess is not directly controlled by the fraction of continental recycling. This implies that the impact of continental recycling may be less significant compared to the large-scale atmospheric circulation patterns we identified. However, as also noted above, further quantifying the effect of local evaporation or evapotranspiration on vapor isotopes would require detailed knowledge of the isotopic composition of the evapotranspiration flux and its variability. This is currently impossible and requires extensive investigation into the isotopic compositions of surface waters, soil waters, and the parametrization of soil water evaporation and plant transpiration, among others. Given these data limitations, we have acknowledged in the discussion and conclusion sections that further research is needed to fully understand the role of continental recycling in shaping vapor isotopes.

The above examples are ultimately why I assess the analysis to be incomplete or sparse given the previous work done on this topic that could be integrated better into the discussion of this work, as well as the interpretive power of the tools in this manuscript that are not used to their full potential in the current manuscript.

Re: We appreciate your above in-depth comments and suggestions. Following your suggestions, we have added or updated relevant results, including the use of ERA5 data to drive trajectory calculations, analyzing the within-boundary-layer contribution over land, and additional discussion and comparison with previous studies. We have also strengthened the discussion and comparison with previous studies, for instance the preservation of oceanic evaporation signals and mixing with subsiding air. We have also acknowledged the limitations of the current study in the revised text.

A few other minor comments:

1) could the authors explain a bit more why trajectory average values are used for meteorological variables here? For situations where condensation is important, for example, it may be the extreme value that matters the most (for example, the lowest f for a given air parcel is likely correlated with the smallest saturation vapor pressure experienced and therefore the coldest temperature)

Re: This relates to your third comment on our trajectory analysis. Although the extreme value might have significant localized effects, their influence diminishes if most of the moisture in an air parcel is lost before reaching SETP. The primary goal was to understand the overall conditions experienced by the air parcels that contribute to humidity at SETP. Using weighted-mean values ensures the analysis reflects the relative importance of different upstream conditions based on their actual contribution to SETP's humidity and allows for an understanding of how different meteorological variables collectively affect vapor isotopes. Therefore, we analyzed the contribution of air parcels themselves to SETP's humidity to diagnose the history of the atmospheric humidity. We have revised the text explaining this point in the revised manuscript at L186-190 as follows:

"*Additionally, this study emphasizes the contribution of air parcels themselves to SETP's humidity. This variable captures the history of the moisture and indicates how much moisture within each air parcel finally reaches SETP. The moisture contribution of an air parcel to SETP's humidity is a measure of the importance of upstream air. We calculated weighted-mean values for key variables by using the moisture contribution of the air parcel along trajectories as the weight.*"

2) The statement at L. 76-77 is contradicted by other parts of the manuscript, which give a series of possible explanations for this behavior (continental recycling, low Rayleigh f as in figure 3, etc.). There also have been a number of studies on precipitation d-excess and elevation in other regions (e.g., the Andes) that could be cited here for additional context

Re: We have revised this sentence and incorporated the findings from the Andes. It now reads as follows: "*High vapor d-excess values at high elevations have been observed elsewhere, such as on the Andes (Samuels‐Crow et al., 2014). Such elevated d-excess values have been attributed to the mixing with subsiding air (Samuels‐Crow et al., 2014; Sodemann et al., 2017). However, this mechanism remains unconfirmed on the TP.*"