Author Response to the Referee Comments to the manuscript "Water vapour isotopes over West Africa as observed from space: which processes control tropospheric H2O/HDO pair distributions?" [EGUSPHERE-2024-1613] submitted to Atmospheric Chemistry and Physics.

We would kindly thank the anonymous referee for providing a review of the manuscript. The individual comments are listed below (shown in red) including our responses (shown in black).

"This article documents variations in the tropospheric water vapor isotopic composition over West Africa at the convective, seasonal and inter-annual scales, using 3 products of satellite observations: IASI, AIRS and TROPOMI. Processes respconsible for the isotopic variations are analyzed. This paper is consistent with many previous papers showing the impact of convective processes and air mass mixing on the isotopic composition of water vapor. The article is not exceptionally novel, but contributes to a more confident understanding of processes controlling the isotopic compositon of water vapor.

The article is well written and well illustrated. My comments are minor."

Thank you very much for the valuable and insightful feedback!

General Comment

"What is the added value of {H2O, δD} pairs? Can they teach us anything new? The paper explains well how {H2O, δD} are consistent with processes that we already know. But it doesn't show how they could improve the konwledge. It's fine, maybe they just cannot improve the knowledge. But then maybe some sentences would need to b e toned down. e.g. l3: "quantify the atmospheric branches"; l20: "underlines the overall value…"; l 471-473: "underlines the strong potential"."

As has been demonstrated in previous studies, the added value of {H2O, δD} pairs is that it can shed light onto the impact of isotopic processes in a way that would not be possible by individual H2O or δD distributions alone. The comment is correctly assuming that our study does not increase our knowledge about the isopotic processes per se. Instead, it shows that various processes can be detected on different time scales using space-based datasets of H2O and δD, and, hence, fosters our general understanding of {H2O, δD} pair variability from an observational point of view.

Therefore, we understand that we will need to tone down some phrases and focus more on the actual scope and outcomes of this study.

Detailed Comments

"l 3: "quantify the atmospheric branches": unclear. We can quantify some fluxes or reservoirs, or the contribution of some processes, but not clear what it means to quantify a branch."

Thank you for pointing out this inaccurate wording. The intended meaning of this phrase was to refer to the challenge of assessing the individual impact of atmospheric processes within the hydrological cycle in observations and models. We will apply the necessary adjustments to this phrase.

"l 8-9: "from a convective as well as seasonal perspective and with respect to" -> "at the convective and seasonal scales and""

Ok, we will rephrase it.

"l 12: "δD depletion -> δD depletion in the vapor""

Ok, will be corrected.

"l 13: remove "without showing significant δD depletion": obvious after "enriched signals""

Ok, will be removed.

"l 15: "anti-correlation": at what time scale? How different is (3) different from (1)?"

(3) refers to the anti-correlation between precipitation amount and δD, while (1) refers to the anti-correlation between H2O and δD. For (3), we observe the described effect on monthly averages compared for different years. We will adjust the description of (3) accordingly to underline the considered time scale.

"l 30: what does "degree of dessication" means? Could this be clarified for people from the isotopic community who don't know about it?"

In the context of Africa, the term of desiccation refers to the progressive drying of soil and the subsequent desertification, e.g. as result of decreasing rainfalls and intensifying droughts. We will adjust this phrase in the manuscript accordingly to clarify its meaning.

"l 39 and around: is convection really the main source of uncertainty in climate projections over the Sahel? Re ent studies suggest a key role of Atlantic SSTs as well, e.g. [Monerie et al., 2023]"

Thanks for pointing this out. We agree that indeed there are further important factors driving the uncairtenty in climate predictions over the Sahel. We will tone down the term "major" to "one of the main" and refer to Atlantic SSTs as example for further factors.

"l 63: "in both liquid hydrometeors and ambiant vapor" -> "in ambiant vapor". Evaporation rather enriches the droplets"

Ok, we will rephrase it.

"l 87: "interannual seasonal -> "interanual"."

Ok, we will rephrase it.

"l 182: clarify the average: average over the previous day?

Yes, the term "average" refers to the averaged precipitation of the previous day.

l 184: why do we need half-hourly IMERG observations rather than just daily? Are air masses followed along back-tra je tories to calculate average rainfall? Or is the rainfall local?"

We agree with the reviewers comment that the description of the clustering method is somewhat confusing.
We have to add the comment that we actually utilize daily IMERG observations instead of hourly to run the precipitation clustering method. Following the IMERG data description, the daily IMERG observations are derived from averaging all corresponding half-hourly IMERG observations at the considered 0.1 x 0.1 degree grid. Based on this, the daily IMERG observations are distributed via the data provider as daily precipitation (i.e. mm per day). As we make direct use of these daily precipitation products, no further averaging is required for identifying post-rain events with the considered clustering method.

Further, we agree the consideration of air mass backward-trajectories would be indeed interesting to consider for improving the detection mechanism of non-rain and post-rain events. As this exceeds the scope of this study, we will add it as perspective.

"Fig 2: give some more context for these lines, e.g. in the caption. What difference between the 2 mixing lines? How are initial conditions and end members chosen?"

We will add following descriptions based on Diekmann et al. 2021a into the caption of Fig. 2:

- The air mass mixing curves are representative for dry and moist mixing processes over West Africa, with considering the mixing members x1 = (5e1 ppmv, -700 ‰,) and x2 = (1.53e4 ppmv, -120 ‰,) for the dry mixing curve (upper mixing curve) and x1 = (1e3 ppmv, -450 ‰,) and x2 = (2.2e4 ppmv, -120 ‰,) for the moist mixing curve (lower mixing curve).
- The Rayleigh curve is computed as Rayleigh process with initial conditions of δD_0 = - 80 ‰, relative humidity of 90 % and T_0 = 30 deg. The Super-Raleigh curve is branching off of the Rayleigh curve following predominant signals observed along the backward trajectories in Diekmann et al. 2021a.

"l 252: you may cite [Risi et al., 2021] to support the impact of snow melt on the water vapor composition"

Thanks for pointing out this study, which we will add as reference here.

"l 273-276: If I understand well, you interpret the absence of isotopic difference between non-rain and post-rain events by the compensation at lowest levels of the enrichment by surface evaporation and depletion by convection? If the case, how an you explain the depletion of the vapor observed after rain events in surface observations [Tremoy et al., 2012, Tremoy et al., 2014]?
l 278 and around: I'm not sure about this rationale. The depleting effect actually accumulates along the descent in unsaturated and mesoscale downdrafts of convective systems, as shown by cloud-resolving simulation [Torri, 2022, Risi et al., 2023]. So we do expect, and generally observe, depletion near the surface after convective systems in the Sahel.
Actually, Fig 6 shows that the humidity is not even larger for post-rain events. This questions whether the clustering methods applied to TROPOMI observation is really comparing non-rain and post-rain events. Is it possible that near the surface, the water vapor recovers more quickly after the event due to surface evaporation, and so the clustering methods based on average rain over the previous days might not properly capture post-rain vapor?

As these three comments point to the same issue, we will hereafter provide a combined answer.

We appreciate this insightful comment and the constructive suggestions. We understand that in the discussion for the TROPOMI data we have underestimated the near-surface depleting effect associated with convection. In addition to the studies referred in the comment, it can be seen in Fig. 3.9 of Lafore et al. (2017) that warming and drying behing squall lines is very strong due to the dry downdrafts in the rear of the squall line.

Therefore, we believe that the proposed explanation of a rapidly recovering near-surface water vapor after the passing of convection sounds reasonable. This would be supported by Fig. 5 and 6, where H_2O and δD are overall similar for non-rain and post-rain conditions. A more sophisticated clustering method with higher temporal frequency would be beneficial in order to create better links between individual convective events and collocated TROPOMI data. Therefore, we will adjust the interpretation of these data accordingly and add the potential improvements for the clustering method as perspective.

"Fig 6: this compares non-rain and post-rain events. It would have been interesting to document the impact of the intensity of rain events, e.g. through the rainfall rate. If this is too much work for this article, this could be mentionned as a perspective."

Thank you for this suggestion. The current design of the considered clustering method only foresees to detect samples for the discrete groups of non-rain and post-rain. The consideration of the impact of rainfall intensity on the post-rain event would be a very interesting analysis, which would require a further evolution of the clustering method, e.g. to define further post-rain events with different rainfall selection criteria. Thus, we will add this as perspective to the discussion of Fig. 6.

“l 286 and next lines: “types of convection”: the impact of convection type was not addressed in this study. Only non-rain and post-rain onditions are compared. To analyze the role of convection type, a more sophisticated clustering method would be useful, e.g. squall line vs isolated systems. This paragraph up to l 296 needs to be completely revised.
It would have been interesting to link the δD to convection type. If this is too much work for this arti le, this could be mentionned as a perspective

We appreciate this constructive comment and understand that the clustering method for identifying non-rain and post-rain conditions actually does not provide conclusions on δD as result of different types of convection. We observe regional differences in the isotopic signature of the post-rain events between the Guinea Coast and the Sahel, and we agree that further work would be needed in order to investigate to the reasons for these differences and to which extent different types of convection may account for these findings. Therefore, we will revise the corresponding paragraph and add the analysis with respect to convection types as further perspective.

l 295: “unaggregated convection (as is the case in Sahelian squall lines)”: this is the contrary! Squall lines are highly aggregated convective systems e.g. [Abramian et al., 2022]. Aggregated means that convection is gathered into one big system, whereas unaggregated means that convection is scattered into several isolated systems, e.g. [Bretherton et al., 2005, Tobin et al., 2012]”

Thank you for the clarification and correction. We will remove the interpretation with respect to the study of Galewsky et al. (2023) and instead add the link to “aggregated and unaggregated convection” to the perspective as described in the response of the previous comment.

“l 311: I can see only a few permil drop in fig 7”

The average value for IASI δD in August, as given as difference to the median over all years and as shown in Fig 7, are as follows

YEAR	**δD (‰)**
2015	5.96
2016	7.02
2017	14.72
2018	-3.97
2019	-4.17
2020	-11.48

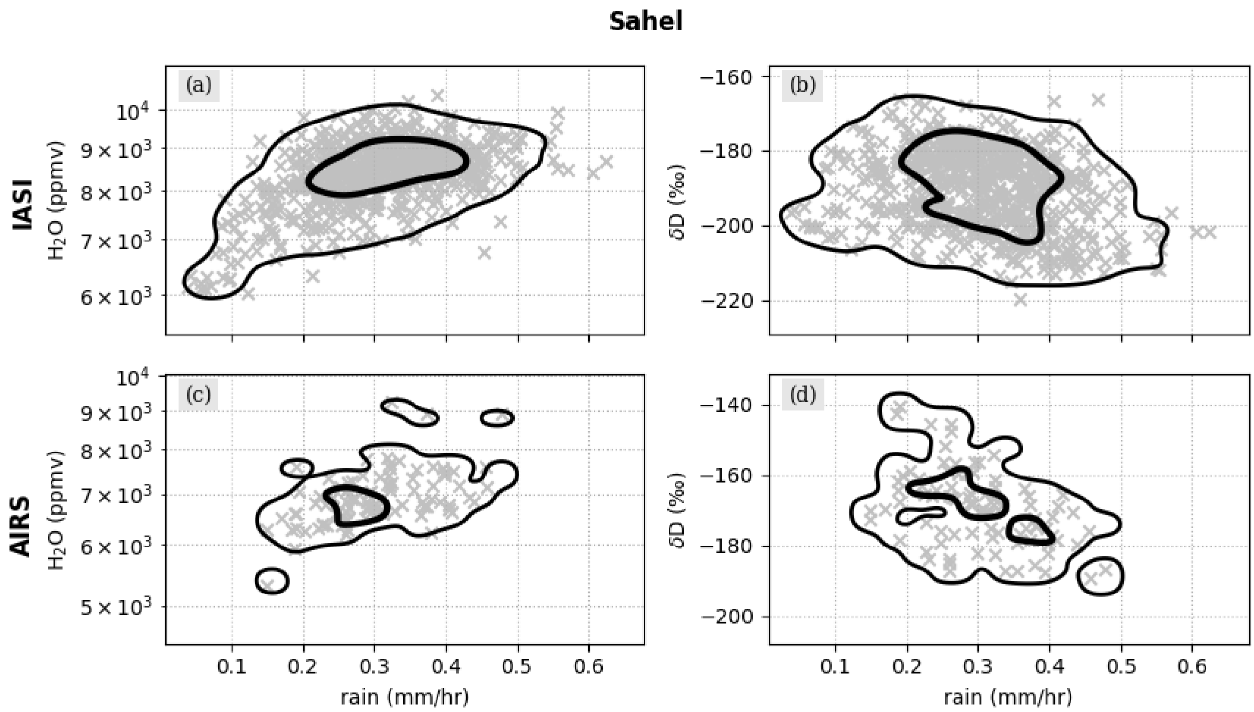
l. 311 refers to the drop of δD value in 2017 compared to the values between 2018 – 2020, which lies between -19 and -25 ‰. We will correct the referred δD drop from “-25 ‰” to “down to -25 ‰”.

“Fig 7: clarify in the caption what the error bars mean. Is it the standard deviation of all instantaneous values?”

The bars denote the [2.5, 97.5] percentiles of the corresponding distributions. We will add this information into the caption of Fig. 7.

“Interpretation of fig 7: To better see the link between rainfall and δD, could a scatter plot of δD vs rainfall anomalies be added?”

We agree that such an analysis can provide interesting insights into the observed amount effect and hence support the interpretation of Fig. 7. Therefore, we will add a corresponding figure showing the correlation of rainfall vs. H2O and rainfall vs. δD using the monthly averaged data for Sahel for IASI and AIRS for the respectively available years (same data as used for Fig. 7):



Here, we observe the discussed anti-correlation between rainfall and δD with decreasing δD as rainfall increases, while H2O is increasing with intensifying rainfall. In this way, it fits well with the discussion of Fig. 7, where corresponding features were observed.

We will add this figure and its discussion in the context of Fig. 7 to Sec. 4 accordingly.

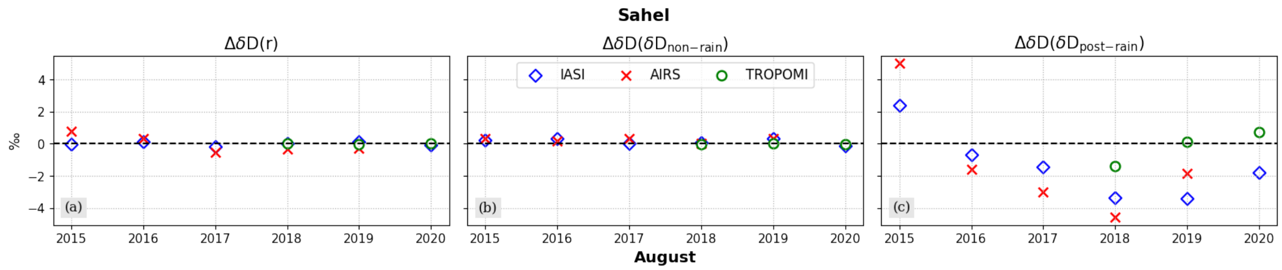
Interpretation of fig 7: Why are more rainy years more depleted? Is it b ecause there are more rainy events, which are more depleted (fig 6)? Or is it because non rainy events are more depleted? Or because rainy events are more depleted, e.g. because they are more intense? To answer this question, it could be easy to link Fig 7 to fig 6 with a decomposition method: $\Delta\delta D = \Delta r \cdot (\delta Drain - \delta Dnorain) + r \cdot \Delta\delta Drain + (1 - r) \cdot \Delta\delta Dnorain$, where ΔδD is the anomaly between high and low rainfall years and r is the fraction of rain samples in the yearly average"

We appreciate this suggestion of utilizing the clustering results for deriving a decomposition method that assesses the contribution of the different factors to the overall δD.

For this purpose, we have investigated the impact of the three factors to the δD anomaly as suggested in the comment:

- $\Delta r \cdot (\delta D_{post\text{-}rain} - \delta D_{non\text{-}rain})$ as the impact of yearly variations in the fraction of rainfall events
- $r_{post\text{-}rain} \cdot \Delta\delta D_{post\text{-}rain}$ as the impact of yearly variations in $\delta D_{post\text{-}rain}$
- $r_{non\text{-}rain} \cdot \Delta\delta D_{non\text{-}rain}$ as the impact of yearly variations in $\delta D_{non\text{-}rain}$

The following figure shows the results evaluated for IASI, AIRS and TROPOMI for the data used in Fig. 6 (i.e. for June – July of the respective years):



This figure shows that for the considered years the anomalies in $\delta D_{post\text{-}rain}$ exhibit high variability in their contribution to the overall δD anomaly, while respective anomalies in rainfall fractions as well as anomalies in $\delta D_{non\text{-}rain}$ show only low contribution to ΔδD. Since the latter implies that rainfall fractions have been overall stable during the considered time period, this lets us assume that the strong variations in $\delta D_{post\text{-}rain}$ are resulting from rainfalls with varying intensity.

Since Fig. 6 (and consequentially also the decomposition results) refer to June – July data and Fig. 7 to August data, their results cannot be linked directly to each other. However, we observe in Fig. 7 that the rainfall peaks have been stronger for the years 2018 – 2020 compared to 2015 – 2017, what matches with the overall results from the decomposition method, where e.g. for IASI the $\delta D_{post\text{-}rain}$ anomalies reach minimum values for 2018 and 2019 and are substantially lower compared to 2015 and 2016. This would let us assume that stronger rainfall events account for the negative $\delta D_{post\text{-}rain}$ anomalies and, hence, for the drop in δD for 2018 – 2020 as observed in Fig. 7.

This assumption would be supported by the correlation plot of δD vs rain (shown in the response for the previous comment), where stronger rainfall rates go along with decreased δD values.

"Around l 365: is it possible that the smaller sensitivity of AIRS could be due to the larger impact of the a-priori profile on AIRS than on IASI, i.e. smaller sensitivity?"

Thank you for pointing out this detail. We agree that the observed discrepancies in the {H2O, δD} pair data between IASI and AIRS in low H2O regimes might result from differences in the sensitivity. As is described in Sec. 2.1, the processing of the IASI data considers of a post-processing step that aims at increasing the sensitivity of the {H2O, δD} pairs at dry conditions, what however is not considered for AIRS. We will update this paragraph accordingly.

"l 384: "As result" -> "As a result"

Ok, will be corrected..

"Somewhere: the recent study by [Dahinden et al., 2023] would deserve to be cited."

Thank you for pointing out this study, which sounds indeed very interesting. We will add a reference to this study in Sec. 5.2 when describing the impact of Saharan air layers to the mid-tropospheric {H2O, δD} pair data.

Literature

Lafore, J. P., Chapelon, N., Diop, M., Gueye, B., Largeron, Y., Lepape, S., Ndiaye, O., Parker, D. J., Poan, E., Roca, R., Roehrig, R., Taylor, C., and Moncrieff, M.: Deep Convection, chap. 3, pp. 90–129, John Wiley & Sons, Ltd, https://doi.org/https://doi.org/10.1002/9781118391297.ch3, 2017.