Response to Reviewers

We appreciate your insightful comments and suggestions. These have greatly helped us refine our manuscript and improve the clarity of our key points. We also thank you for your positive feedback.

In this response letter, we have addressed each comment in detail below. Our responses are in blue, with specific changes to the text highlighted in *blue italics*. All line numbers in this document correspond to the line numbers in the updated version of the manuscript.

With best regards,
Di WANG
On behalf of all authors

AC1: 'Comment on acp-2022-223', 27 Jun 2022

Review of "Vehicle-based in-situ observations of the water vapour isotopic composition across China: spatial and seasonal distributions and controls" by Di Wang et al. submitted to ACP

This paper presents a very interesting and impressive dataset of vehicle-based stable water isotope measurements in China with several new results and interpretations about the drivers of the variability observed in different regions. The observations cover two seasons: the pre-monsoon season 2019 and the monsoon season 2018, which are compared in terms of their different dynamics and isotope signals recorded along the route. I recommend publication of the paper after two major and several minor comments have been addressed:

We appreciate your positive comment. We also appreciate your detailed suggestions.

Major comments:

A) Synoptic vs. seasonal variations: This analysis is very interesting. - However, it comes very late in the manuscript, even though the whole interpretation of the observations evolves along the main finding that the spatial (seasonal) variations dominate the variability of various isotope and meteorological variables. The results section even starts with referring to this analysis but asks the readers to postpone their curiosity to a much later section. Why not starting the results with what you show in Section 4.7?

We agree that the analysis of the seasonal vs synoptic should be earlier in the manuscript. We put it as section 3.3, just after the description of the raw data in section 3.1 and 3.2, and before the seasonal variations in section 3.4 and analyzing the factors controlling the spatial and seasonal distributions in section 4.

- What happens if you do the same regression analysis as presented in Section 4.7 using the actual observations for estimating the synoptic and seasonal components? Looking at the comparison of the simulation and the observations (Fig. 11), it strikes me that the daily model output follows the “temporal-mean” output much more closely than the actual measurements in both the premonsoon and the monsoon seasons. To me this shows that the Iso-GSM simulation on a relatively coarse grid is not capable of reproducing the observed mesoscale to large-scale variability in the water vapour isotope and meteorological fields. Therefore, in my view the finding that the isotope variability across China in the pre-monsoon and monsoon periods is mainly a result of seasonal/spatial variations and only marginally affected by synoptic-scale systems is biased towards what the Iso-GSM shows.

We accepted your suggestion and assessed the relative contribution using multi-year averages from 2010 to 2020 and a new estimation method that takes into account model bias and gives upper and lower bounds on the value of the contribution.

We modified (lines 327-361):

“2.6 Method to decompose the observed daily variations

The temporal variations observed along the route for a given period represent a mixture of synoptic-scale perturbations, and of seasonal-mean spatial distribution:

$$\delta^{18}O_{\_daily} = \delta^{18}O_{\_seaso} + \delta^{18}O_{\_synoptic} \qquad (4)$$

The first term represents the contribution of seasonal-mean spatial variations, whereas the second term represents the contribution of synoptic-scale variations. Since these relative contributions are unknown, we use outputs from Iso-GSM. The daily variations of $\delta^{18}O$ simulated by Iso-GSM also represent a mixture of synoptic-scale perturbations and seasonal-mean spatial distribution, but with some errors relative to reality:

$$\delta^{18}O_{\_daily\_Iso\text{-}GSM} = \delta^{18}O_{\_daily\_Iso\text{-}GSM} + \in = (\delta^{18}O_{\_seaso\_Iso\text{-}GSM} + \in_{\_seaso}) + (\delta^{18}O_{\_synoptic\_Iso\text{-}GSM} + \in_{\_synoptic}) \qquad (5)$$

where $\delta^{18}O_{\_daily\_Iso\text{-}GSM}$ is the daily outputs of $\delta^{18}O$ for each location, $\delta^{18}O_{\_seaso\_Iso\text{-}GSM}$ is the multi-year monthly outputs of $\delta^{18}O$ for each location, and $\delta^{18}O_{\_synoptic\_Iso\text{-}GSM} = \delta^{18}O_{\_daily\_Iso\text{-}GSM} - \delta^{18}O_{\_seaso\_Iso\text{-}GSM}$, $\in_{\_seaso}$ and $\in_{\_synoptic}$ are the errors on $\delta^{18}O_{\_seao\_Iso\text{-}GSM}$ and $\delta^{18}O_{\_synoptic\_Iso\text{-}GSM}$ relative to reality, respectively, $\in$ is the sum of $\in_{\_seaso}$ and $\in_{\_synoptic}$.

These individual error components $\in_{\_seaso}$ and $\in_{\_synoptic}$ are unknown, but we know the sum of them ($\in$), i.e. the difference between daily outputs and observations. For the decomposition, we made two extreme assumptions to estimate upper and lower bounds on the contribution values:

1) *We assume that the error is purely seasonal-mean, i.e. $\in = \in_{\_seaso}$, and $\in_{\_synoptic} = 0$:*

$$\delta^{18}O_{\_daily} = \delta^{18}O_{\_seaso\_Iso\text{-}GSM} + (\delta^{18}O_{\_synoptic\_Iso\text{-}GSM} + \in) \qquad (6)$$

To evaluate the contribution of these two terms, we calculate the slopes of $\delta^{18}O_{\_daily}$ as a function of $\delta^{18}O_{\_seaso\_ Iso\text{-}GSM}$ ($a_{\_seaso}$), and of $\delta^{18}O_{\_daily} - \delta^{18}O_{\_seaso\_ Iso\text{-}GSM}$ ($a_{\_synoptic}$). The relative contributions of spatial and synoptic variations correspond to $a_{\_seaso}$ and $a_{\_synoptic}$ respectively. This will be the upper bound for the contribution of synoptic-scale variations, since some of the systematic errors of Iso-GSM will be included in the synoptic component. This is equivalent to using the seasonal-mean of Iso-GSM and the *raw time series of observations.*

2) We assume that the error is purely synoptic, i.e. $\in = \in_{\_synoptic}$, *and* $\in_{\_seaso} = 0$ *:*

Then $\delta^{18}O_{\_daily} = (\delta^{18}O_{\_seaso\_Iso\text{-}GSM} + \in) + \delta^{18}O_{\_synoptic\_Iso\text{-}GSM}$. *(7)*

To evaluate the contribution of these two terms, we calculate the slopes of $\delta^{18}O_{\_daily\_Iso\text{-}GSM}$ *as a function of* $\delta^{18}O_{\_seaso\_Iso\text{-}GSM}$ ($a_{\_seaso}$ *), and of* $\delta^{18}O_{\_daily\_Iso\text{-}GSM}$ *-* $\delta^{18}O_{\_seaso\_Iso\text{-}GSM}$ ($a_{\_synoptic}$ *). This will be the lower bound for the contribution of synoptic-scale variations, since we expect Iso-GSM to underestimate the synoptic variations.*

The same analysis is also performed for the Iso-GSM simulation q (Table 2) and reanalysis q (Table 3).”

We updated the discussion on disentangling seasonal-mean and synoptic variations in section 3.3 (lines 446-466):

*“ **Table 2** The relative contribution (in fraction) of spatial variations for a given season* ($a_{\_seaso}$) *and of synoptic-scale variations* ($a_{\_synoptic}$) *to the daily variations of q and* $\delta^{18}O$ *simulated by Iso-GSM. We checked that the sum of* $a_{\_seaso}$ *and* $a_{\_synoptic}$ *is always 1.*

Period	*variables*	*Controbutions*	
		$a_{\_seaso}$	$a_{\_synoptic}$
Pre-monsoon	*q*	*0.73~1.05*	*0.27~-0.05*
	$\delta^{18}O$	*0.54~0.70*	*0.46~0.30*
Monsoon	*q*	*0.63~0.71*	*0.37~0.29*
	$\delta^{18}O$	*-0.50~0.10*	*1.5~0.9*

***Table 3** The same as Table 2, but for reanalysis q.*

Period	*variables*	*Controbutions*	
		$a_{\_seaso}$	$a_{\_synoptic}$
Pre-monsoon	*q*	*0.77~0.88*	*0.23~0.12*
Monsoon	*q*	*0.69~0.88*	*0.31~0.12*

During the pre-monsoon period, based on both the Iso-GSM simulation and NCEP/NCAR reanalysis, we can find that the seasonal-mean contribution to q is higher than the synoptic-scale contribution: $a_{\_seaso}$ *is 73%-105% for Iso-GSM and 77%-88% for reanalysis, whereas* $a_{\_synoptic}$ *is 27%~ -5% for Iso-GSM and 23%~12% for reanalysis (Table 2 and Table3). The relative contribution of seasonal-mean spatial variations to the total simulated variations of* $\delta^{18}O$ *(54%-70%) is also much higher than that of synoptic-scale variations (27%- -5%). This suggests that the observed variability in q and* $\delta^{18}O$ *is mainly due to spatial variability, and marginally due to synoptic-scale variability. During the monsoon, seasonal-mean spatial variations is also the main contribution to the observed q variations. But it is different for* $\delta^{18}O$*: the relative contribution of synoptic-scale variations (90%-150%) dominates the total simulated variations of* $\delta^{18}O$*.”*

- What exactly is the “temporal-mean” output? A multi-year seasonal mean? Or the mean over the considered 2018 and 2019 seasons? And over which spatial scale did you average?

We have added a description and application of the "temporal-mean outputs" in the manuscript to make it clearer (lines 319-322):

"We also use the multi-year monthly-mean outputs from Iso-GSM (March monthly for the pre-monsoon period and August monthly for the monsoon period) for each observation location from 2011 to 2020 to quantify the relative contributions of seasonal-mean and synoptic-scale variations."

- I am not convinced that you can conduct this analysis in a robust way, given the few observations you have for a given region and season. A discussion on this sampling issue of the seasonal signal from only few synoptic events would be beneficial. Still, it is a valuable analysis especially if it is done with the measurements and the model, comparing and discussing the two results.

We carried out this analysis on a spatial scale for a dense set of observations using the method in the response to comment 1. Comparison with multi-year averages reveals that during the pre-monsoon, spatial variability dominates for both q and $\delta^{18}O$; during the monsoon, q still mainly reflects seasonally averaged spatial variability, however, $\delta^{18}O$ is more influenced by synoptic-scale processes that may include different sources of water vapor, convective processes, etc., as discussed in the manuscript.

We added more discussion on the impact of possible synoptic-scale processes (lines 468-485):

"3.3.3 Diagnosing the reasons for the GCMs performance

"Since typhoons are known to be associated with depleted rain and vapor (Gedzelman et al 2003, Xu et al 2019, Battacharya et al 2022), during our monsoon observations, landfall of tropical cyclones Jongdari and Yagi correspond to the low values of δ^{18}O we observed in the eastern China (Fig.S7a), Bebinca corresponds to the low values of δ^{18}O we observed in the southwestern China (Fig.S7a). Iso-GSM captures the large-scale circulation associated with a tropical cyclone, but given its coarse resolution, it underestimates the depletion associated with the meso-scale structure of the cyclone (Fig.7). The northward propagation of the Northern Summer Intra-Seasonal Oscillation (BSISO) during July-August 2018 allowed for strong convection over large areas of China (Susskind et al., 2011;Kikuchi, 2021) (Fig. S8). Moreover, short-lived convective events that frequently occurred during our observation period (Jing Wang et al., 2018). It is possible that these rapid high-frequency synoptic events are not fully captured by Iso-GSM. Iso-GSM underestimates the observed variability, especially synoptic timescale variability. Thus, Iso-GSM performs well during the pre-monsoon season, when seasonal mean spatial variability dominates q and δ^{18}O. In contrast, it performs less well during the monsoon season, when δ^{18}O variations are mainly due to the synoptic-scale variability. This could explain the different performances of Iso-GSM during pre-monsoon and monsoon periods."

B) Organization of the manuscript: The reading of the manuscript would profit from a re-organization with a clearer separation of methodological aspects and results section.

Many methodological aspects are in the results and interrupt the flow of the reader (regional analysis in Table 1, Urban emissions, methodological aspects on Eq. 4)

We accepted your suggestion, we have moved regional analysis in Table 1 to section 2.4, moved section "4.7 urban emissions" to section 2.2.6, and moved methodological aspects on Eq. 4 to section 2.5.

Minor comments:

1) L. 25: "large-scale (order 10000 km) continuous observations of near-surface vapor isotopes": can be misunderstood, reformulate. You made in-situ observations over a large area. Not continuous observations at multiple locations.

   We have reworded as " in-situ observations of near-surface vapor isotopes over a large region (order 10000 km) across China" to avoid misunderstandings.

2) L. 28-29: "mainly due to spatial variations and marginally influenced by synoptic-scale variations": This is interesting! I was very curious about how you came to this conclusion, when starting to read your manuscript. When just reading the abstract, I found this statement and the following ones about the importance of Rayleigh distillation (cloud formation during large-scale ascent?), different moisture sources (variability induced by large-scale weather systems?), continental moisture recycling (transport regimes favouring oceanic vs. continental sources) and convection (mesoscale circulation) contradicting. First you write that the "spatial variations" dominate and then you mention different processes that are relevant at the synoptic- to meso- scale. The synoptic systems are very different in southern vs. northern China and these weather systems shape the spatial contrasts. It would be helpful if you could concisely state in the abstract how you come to the conclusion that "seasonal"/"spatial" variations dominate the synoptic variability and think carefully about the best terminology to use.

   We have reworded the abstract to clarify what processes play a role at the seasonal-mean and synoptic-scale:

   "The spatial variations during the pre-monsoon period represent mainly seasonal-mean variations, but significantly influenced by synoptic-scale variations during the monsoon period. During the pre-monsoon period, the spatial variations of vapor $\delta^{18}O$ are mainly controlled by Rayleigh distillation along air mass trajectories. The North-South gradient observed during the pre-monsoon period is counteracted by different moisture sources, continental recycling processes and convection during moisture transport during the monsoon period."

3) L. 38-39: Why is the performance of the Iso-GSM model weaker over the monsoon period?

   We point out the main reasons for the different performance of Iso-GSM in the pre-monsoon and monsoon periods (lines 462-466):

   "3.3.3 Diagnosing the reasons for the GCMs performance

During our monsoon observations, Super Typhoon Jundari, Super Tropical Storms Yagi and Bebinca, and Super Tropical Storm Lumbia occurred in the China *region. Since typhoons are known to be associated with depleted rain (Gedzelman et al 2003, Xu et al 2019, Battacharya et al 2022), they probably contributed to the low values of $\delta^{18}O$ we observed during the corresponding period (Fig. S7). In addition, the northward propagation of the Northern Summer Intra-Seasonal Oscillation (BSISO) during July-August 2018 allowed for strong convection over large areas of China (Susskind et al., 2011;Kikuchi, 2021), particularly in the south (from latitudes 30N to 45N) (Fig. S8). It is possible that these rapid high-frequency synoptic events are not fully captured by Iso-GSM. Iso-GSM underestimates the observed variability, especially synoptic timescale variability. Thus, Iso-GSM performs well during the pre-monsoon season, when seasonal mean spatial variability dominates q and $\delta^{18}O$. In contrast, it performs less well during the monsoon season, when $\delta^{18}O$ variations are mainly due to the synoptic-scale variability. This could explain the different performances of Iso-GSM during pre-monsoon and monsoon periods."*

4) L. 46: The first sentence of the introduction is a bit heavy, could you think of a more general motivation for your study? And think about how to guide a non-isotope specialist reader into your subject.

   We have reworded the beginning of the introduction (lines 49-52):

   "Stable water isotopes have been applied to study a wide range of hydrological and climatic processes (Gat, 1996;Bowen et al., 2019;West et al., 2009). This is because water isotopes vary with changes in the water phases (e.g., evaporation, condensation), and therefore produce a natural labeling effect within the global water cycle."

5) L. 54: Are only Tibetan plateau ice cores questioned in terms of their use as past temperature records?

   We modified "isotopic records in Tibetan ice cores" to "isotopic records in ice cores from low and middle latitudes regions".

6) L. 56: "significant role of large-scale"

   We have modified "the significant role of large regional atmospheric circulation" to "the significant role of large-scale atmospheric circulation".

7) L. 57: is a specific teleconnection mode meant here?

   Based on suggestions from the other reviewer, we have reorganized the introduction and removed this sentence.

8) L. 74-77: Isn't the Bailey et al. 2013 study from an oceanic environment? Aemisegger et al. 2014 ACP discusses the isotope variability of continental evaporation induced by synoptic-scale variability.

   We modified as (104-105):

   "One study made vehicle-based in-situ observations to document spatial variations, but this was restricted to the Hawaii island (Bailey et al., 2013)."

9) L. 97: Dry intrusions bring… cold and dry upper tropospheric airmasses

We have modified "Dry intrusions" to "Westerlies".

10) Fig. 1: the route is a bit difficult to make out from the otherwise very nice figure

Thank you for your suggestion, we highlighted the observation routes.

11) L. 122: is computed

We have modified "are computed" to "is computed".

12) L. 164: no drift in the standards: could you indicate the standard deviation of your series of standard measurements?

We added the standard deviation of standard measurements during our campaigns (lines 197-200):

"For two standard waters, the standard deviation of standard measurements are 0.2‰ and 0.11‰ for $\delta^{18}O$, and 1.16‰ and 1.2‰ for $\delta^{2}H$ during the pre-monsoon period of 2019. During the monsoon period of 2018, the standard deviation of standard measurements are 0.09‰ and 0.06‰ for $\delta^{18}O$, and 0.6‰ and 0.33‰ for $\delta^{2}H$."

13) Section 2.2.3: should be humidity-dependent isotope bias correction

We have modified "Humidity calibration" to "Humidity-dependent isotope bias correction".

14) Was the specific humidity calibrated as well using an independent sensor?

Thank you for your suggestions, we added (lines 165-169):

"The specific humidity measured by Picarro is very close to that measured by an independent sensor installed in the vehicle (Fig.4). The correlation between the humidity measured by the Picarro and the independent sensor are over 0.99, the slopes are approximately 1 and the average deviation are less than 1 g/kg both during pre-monsoon and monsoon periods."

15) L. 212: why was only one air parcel started from 1000 m above the surface?

Tracing parcels starting from different altitudes would provide information on air mass mixing. However, due to the large number of our observation points, this would reduce the visibility of the backward trajectory results in Figure 2 and Figure 4. Therefore, we follow the conventional setup that people usually use in tracking near-surface water vapor sources, i.e., a tracking starting point of 1000 m from the ground (Guo et al., 2017;Bershaw et al., 2012),. We believe that this is representative of the backward trajectory of near-surface air, since most water vapor in the atmosphere is within 0–2 km above ground level (Wallace and Hobbs, 2006).

We added (lines 273-275):

"This is representative of the water vapor near the ground (Guo et al., 2017;Bershaw et al., 2012), since most water vapor in the atmosphere is within 0–2 km above ground level (Wallace and Hobbs, 2006)."

16) L. 229-234: This is a bit a difficult start of the results section. Could the analysis of the seasonal vs. synoptic drivers of isotope variability not come as a first result? It is otherwise very hard for the reader to follow the story.

We accepted your suggestion to place the analysis of seasonal versus synoptic drivers of isotopic variation earlier in the manuscript (section 3.3).

17) L. 230: refer to Fig. 2a,b

We have referred to these two figures in this sentence. As the order of the figures has changed, it is currently Figure 4 and Figure 5 (lines 364-365):

"Our survey of the vapor isotopes yields two snapshots of the isotopic distribution along the route (Fig.4 & Fig.5)."

18) L. 269: that could be the continental effect too… Should Dansgaard et al. 1964 be cited if you discuss the isotope effects? These different effects are not mutually exclusive, and in my opinion, they do not provide a direct mechanistic explanation for the correlation of the delta-values with temperature.

We cited Dansgaard et al. 1964 in the manuscript when we discuss the isotope effects:

Lines 513-514: "the altitude effect (the heavy isotope concentrations in fresh water decreasing with increasing altitude, Dansgaard 1954)"

Lines 690-691: "During the pre-monsoon period, all observations taken together exhibit a "temperature effect" (the δ's decreasing with temperature, Dansgaard 1964)"

We moved the discussion of the spatial variation of water vapor isotopes with temperature to Section 4. This discussion is further developed in the context of temperature data and correlation coefficients. In this section, we focus on the description of the observed data.

19) Section 3.2: I like the result about the fact that the monsoon counteracts the N-S gradient observed in the pre-Monsoon period very much. This could be included as a key result in the abstract.

We incorporated this great idea in the abstract, we modified lines 30-36 as:

"During the pre-monsoon period, the spatial variations of vapor $\delta^{18}O$ are mainly controlled by Rayleigh distillation along air mass trajectories. The North-South gradient observed during the pre-monsoon period is counteracted by different moisture sources, continental recycling processes and convection during moisture transport during the monsoon period. The seasonal variation of vapor $\delta^{18}O$ reflects the influence of the summer monsoon convective precipitation in southern China, and a dependence on temperature in the North."

20) L. 310-311: I don't understand this sentence. What is the separation line of the seasonal variation of dexcess? Reformulate.

We modified as (lines 532-534):

"The line separating the areas of positive and negative values of the d-excess$_{monsoon}$ - d-excess$_{pre\text{-}monsoon}$ differences coincides with the 120 mm P-mean line

(Fig.S2 f).”

21) P. 11-12: this is a methodological part and should not be in the results section.

We have now moved pages 11-12 to section 2.4 untitled “Back-trajectory calculation and categorizing regions based on air mass origin”.

22) L. 439: bring -> brought

We have modified “bring” to “brought”.

23) L. 442: replace “strong” by “high”. Actually, the fraction of transpiration in evapotranspiration is the key factor determining d over the continent, when recycling is high (see Aemisegger et al. 2014, ACP).

We have modified “strong” to “high”, and also added the reference for discussion (lines 637-639):

“These properties are associated with a high d-excess, consistent with strong continental recycling and by evapotranspiration (Aemisegger et al., 2014).”

24) L. 369: reduction of humidity and water isotope ratios.

We have modified “reduction of vapor isotope ratios” to “reduction of humidity and water isotope ratios” (line 163).

25) Figure 6 is very interesting. Could it come earlier?

We have tried many ways to reorganize the paper, but since following reviewers’ comment we have moved the section disentangling seasonal/synoptic variations earlier, and the moisture source categorization to the method section, this figure actually comes a bit later. However, it comes as the first figure of the discussion section 4, untitled “Understanding the factors controlling the spatial and seasonal distributions”.

26) L. 391: this sounds a bit speculative: couldn’t it be another Rayleigh line or mixing?

In current Figure 9, we added the uncertainty range of the Rayleigh curve calculated for different initial conditions of key moisture source regions: during March 2019, light red and light blue Rayleigh curve are calculated for key moisture source regions of westerlies (δ^2H_0 = − 168.04‰, T=5°C) and BoB (δ^2H_0= -77.37‰, T=26.46°C) separately in (a); during July-August 2018, light red and light blue Rayleigh curve are calculated for key moisture source regions of westerlies (δ^2H_0= -149.64‰, T=6.16°C) and BoB (δ^2H_0= -82.75‰, T=27.69°C) separately in (b). These initial δ^2H are derived from iso-GSM, the initial temperature and RH are derived from NCAR/NCEP 2.5-deg global reanalysis data.

We can find that observations in the WR_3 region (Fig.3c) are truly located below the q-δ^2H Rayleigh distillation curve calculated for general values for pre-monsoon period and calculated for it’s key moisture source region of BoB, we modified as (lines 568-571) :

“Observations below the Rayleigh line, even when considering the most depleted initial vapor conditions (light blue Rayleigh curve in Fig 9b), indicate the influence of

rain evaporation from depleted precipitation (Noone, 2012;Worden et al., 2007).”

27) L. 493: “all observations taken together” because WR2 and WR3 do not show a “temperature effect”.

We modified “all observations” to “all observations taken together” (line 690).

28) L. 501: influenced

We have modified “influence” to “influenced”.

29) L. 509: degree of rain out

We modified “degrees along the Rayleigh distillation” to “degree of rain out” (line 707).

30) L. 517: why does the dexcess usually increase with altitude? Many recent studies show that it is more complicated than that (see, Salmon et al. 2019 ACP, Thurnherr et al. 2021 WCD). In many cases a decrease of dexcess towards the mid troposphere is observed.

We agree that our observations of altitude variation within 2km are not sufficient to discuss the relationship between d-excess and altitude. We deleted the sentence “Alternatively, this may reflect the fact that the d-excess generally increases with altitude (Galewsky et al., 2016)”.

We explain the positive correction between d-excess and altitude by the following sentence: “The vapor d-excess for all observations during the monsoon period (Fig.8d) is positively correlated with Alt ($r = 0.39$, $p<0.05$, Table S1). One possible reason is that the vapor d-excess is lower in coastal areas at lower altitudes, while at higher altitudes in the west, more recycling moisture leads to higher d-excess. The positive correlation between d-excess and altitude is consistent with previous studies in region (Acharya et al 2020).”

31) Fig. 9: I am not sure this analysis is very useful. Why is the correlation of a local observation with conditions of air parcels upstream relevant? To me only the correlation between dexcess and RH normalized to surface temperature upstream would make sense. This analysis could be left out and the corresponding text removed, it would make the paper more concise and highlight more the key findings.

We would like to keep this short discussion because many previous studies have shown that convection has a stronger impact on the vapor isotopic composition when integrated along trajectories than when considered locally in monsoon region. Therefore, we keep this analysis for two proxies of convection: precipitation (proxy for deep convection) and boundary layer mixing depth (proxy for both shallow and deep convection).

We added a few sentences to introduce the purpose of this section (lines 735-739):

“Past reconstructions of paleoclimate using ice core isotopes have relied on relationships with local temperatures, but many previous studies have suggested that water isotopes are driven by remote processes along air mass trajectories. In particular,

they emphasized the importance of upstream convection in controlling the isotopic composition of water vapor (Gao et al., 2013;He et al., 2015;Vimeux et al., 2005, Cai and Tian, 2016)"

We pointed out the relevance of the relationship between water vapor isotopes and temperature and humidity on water vapor transport pathways (lines 744-747) :

"The results show gradually increasing positive correlation coefficients as dtra changes from 10 to 3. This reflects the role of temperature and humidity along air mass trajectories and the large spatial and temporal coherence of T variations during the pre-monsoon period."

32) L. 552: I would rather say that convection transports moisture with high dexcess from lower altitudes to higher altitudes (see Thurnherr et al. 2021 WCD, Aemisegger et al. WCD).

Many studies documents an increase of d-excess with altitude in the free troposphere (Galewsky et al 2016, Salmon et al 2019), though the vertical profiles can be more complex in the boundary layer and just above it (Salmon et al 2010, Thurnerr et al 2021) or over oceanic regions in dry conditions (Aemisegger et al 2021). Vertical profiles measured in China (unpublished) confirm the increase of d-excess with altitude in our geographical setting.

33) L. 554: "deeper convection" instead of "more active convection" ?

We modified "more active convection" to "deeper convection".

34) L.. 593: non-equilibrium fractionation at the moisture source

We modified "kinetic fractionation" to "non-equilibrium fractionation at the moisture source".

35) L. 590-605: this is a bit repetitive, could be shortened

We have abbreviated this paragraph.

36) L. 597: it's mainly the decrease in T, RH can be very low at the sea ice edge during cold air outbreaks (see, Aemisegger and Papritz, 2018 JC, Thurnherr et al. 2020, ACP)

We add this reference (lines 795-799):

"This is consistent with the global-scale poleward decrease in T and increase in surface RH over the oceans (despite the occurrence of very low RH at the sea ice edge during cold air outbreaks (Aemisegger and Papritz, 2018, Thurnherr et al. 2020), resulting in global-scale poleward decrease in d-excess at mid-latitudes (Risi et al., 2013a;Bowen and Revenaugh, 2003)."

37) L. 610: what about the role of dew deposition during night over the continent in northern China? See Lee et al. 2021 HESS.

We agree that dew deposition during night could also contribute to the low d-excess over the continent in northern China. We add this reference (lines 799-800):

"Alternatively, the low-dexcess during the night over the continent in Northern China during the pre-monsoon could also contribute to the low-dexcess (Lee et al 2021)."

38) L. 621: To me it seems that Iso-GSM strongly underestimates the synoptic timescale variability. What happens to your synoptic vs. seasonal drivers of variability if you use the observations as a measure of synoptic variability?

As in the response to the first major comment, we now use observations to estimate the relative contributions, giving the upper and lower bounds on the values of the contribution accounting for errors in Iso-GSM.

39) L. 633: Could you speculate about the processes that are responsible for the model biases? Could it be linked to the representation of convection? Or evapotranspiration?

We added the interpterion for the bias (lines 500-503):

"The differences in $\delta^{18}O$ (Fig.7a) and q (Fig.7c) are spatially consistent. The overestimation of $\delta^{18}O$ therefore could be due to the overestimation of q, and vice versa. These biases could be associated with deviations in the representation of convection or in continental recycling."

40) L. 633/634: I was slightly confused by the writing here: "overestimation (respectively underestimation)…", could this be written more clearly? Do you mean biases affecting both ^{18}O and q?

What I would like to explain by this is that regions that overestimate q also overestimate $\delta^{18}O$, and conversely, regions that underestimate q also underestimate $\delta^{18}O$. I modified it (lines 500-501):

"The overestimation of $\delta^{18}O$ therefore could be due to the overestimation of q, and vice versa."

41) L. 649: is the strongly smoothed topography in Iso-GSM the reason behind the bad representation of the altitude effect?

We corrected for the altitude difference between observations and Iso-GSM using the method given in III. Supplementary Text. We have the same results. Therefore, we think that the smoothed topography is not the main reason.

42) L. 664: what is the temporal-mean output of ^{18}O from Iso-GSM? A multi-year seasonal mean? Or just the seasonal mean of that particular year?

We have made the definition clearer in the manuscript (lines 319-322):

"We also use the multi-year monthly-mean outputs from iso-GSM (March monthly for the pre-monsoon period and August monthly for the monsoon period) for each observation location from 2011 to 2020 to quantify the relative contributions of seasonal-mean and synoptic-scale variations."

43) Section 4.7: If possible, this should come much earlier in the manuscript to guide the reader in the interpretation of the observations. Also as mentioned in the major

comments, I like the analysis, but I doubt that the number of observations in each region is large enough (sample size) to robustly characterize the synoptic variability encountered in that region. I also suspect that Iso-GSM underestimates the residual 18Osynoptic= 18Odaily- 18Oseasonal. A critical discussion of the results would be very beneficial here.

We have placed the analysis of seasonal versus synoptic drivers of isotopic variation earlier in the manuscript (in section 3.3).

We have now improved our decomposition method to account for errors in Iso-GSM for both seasonal and synoptic-scale variations. Taking into account these errors, we slightly modify one of our conclusions by noting the greater influence of synoptic-scale variability during the monsoon period than seasonally averaged spatial variability.

44) Urban emissions: this is an interesting side-discussion. I would however rather see this as a methodological paragraph.

We accepted your suggestion and moved this section to data and method section (section 2.2.6 Data processing).

Also, I strongly recommend discussing the potentially important baseline effects emerging from rapid changes in concentrations of different trace gases, which can lead to biases in the isotope observations from CRDS systems (see Johnson and Rella, 2017 AMT, Gralher et al., 2016).

We added a short discussion about the potentially important baseline effects emerging from rapid changes in concentrations of different trace gases (lines 228-230):

"Some of these d-excess anomalies are not excluded from being affected by the baseline effects emerging from rapid changes in concentrations of different trace gases. (Johnson and Rella, 2017;Gralher et al., 2016)"

Technical comments: - The isotope observational dataset should also be made available online. It is only available upon request according to the data availability statement. A thorough documentation of the data (with adequate metadata description) is key, for making the data accessible to other scientists.

Thank you for your reminder and detailed suggestions. We had submitted the isotope observational dataset to the PANGEA repository. We also described the data in detail and indicate the time of observation. However, publishing data in this database requires a long review period. Therefore the data was not available to readers online at the time we submitted this article. The datasets are now available.

We modified the data availability section as (lines 874-880):

"The data acquired during the field campaigns used can be accessed via the following link or DOI: (1) Wang, Di; Tian, Lide (2022): Vehicle-based in-situ observations of the water vapor isotopic composition across China during the monsoon season 2018. PANGAEA, https://doi.org/10.1594/PANGAEA.947606; (2)Wang, Di; Tian, Lide (2022): Vehicle-based in-situ observations of the water vapor isotopic composition across China during the pre-monsoon season 2019. PANGAEA, https://doi.org/10.1594/PANGAEA.947627."

- All figures showing observational data: it would be nice to add the corresponding year in the captions (2019 & 2018).

We accepted your suggestion and add the corresponding year in the captions (2019 & 2018).