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:

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?
   - 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 pre-monsoon 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.
   - 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?
   - 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.

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)

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.
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.

3) L. 38-39: Why is the performance of the Iso-GSM model weaker over the monsoon period?
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.
5) L. 54: Are only Tibetan plateau ice cores questioned in terms of their use as past temperature records?
6) L. 56: “significant role of large-scale”
7) L. 57: is a specific teleconnection mode meant here?
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.
9) L. 97: Dry intrusions bring… cold and dry **upper tropospheric** airmasses
10) Fig. 1: the route is a bit difficult to make out from the otherwise very nice figure
11) L. 122: is computed
12) L. 164: no drift in the standards: could you indicate the standard deviation of your series of standard measurements?
13) Section 2.2.3: should be humidity-dependent isotope bias correction
14) Was the specific humidity calibrated as well using an independent sensor?
15) L. 212: why was only one air parcel started from 1000 m above the surface?
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.
17) L. 230: refer to Fig. 2a,b
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.
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.
20) L. 310-311: I don’t understand this sentence. What is the separation line of the seasonal variation of d-excess? Reformulate.
21) P. 11-12: this is a methodological part and should not be in the results section.
22) L. 439: bring -> 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).
24) L. 369: reduction of humidity and water isotope ratios.
25) Figure 6 is very interesting. Could it come earlier?
26) L. 391: this sounds a bit speculative: couldn’t it be another Rayleigh line or mixing?
27) L. 493: “all observations taken together” because WR2 and WR3 do not show a “temperature effect”.
28) L. 501: influence**d**
29) L. 509: degree of rain out
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.
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.
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).
33) L. 554: “deeper convection” instead of “more active convection” ?
34) L.. 593: non-equilibrium fractionation at the moisture source
35) L. 590-605: this is a bit repetitive, could be shortened
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)
37) L. 610: what about the role of dew deposition during night over the continent in northern China? See Lee et al. 2021 HESS.

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?
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?
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 $\delta^{18}O$ and *q*?
41) L. 649: is the strongly smoothed topography in Iso-GSM the reason behind the bad representation of the altitude effect?
42) L. 664: what is the temporal-mean output of $\delta^{18}O$ from Iso-GSM? A multi-year seasonal mean? Or just the seasonal mean of that particular year?
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 $\delta^{18}O_{synoptic}=\delta^{18}O_{daily}-\delta^{18}O_{seasonal}$. A critical discussion of the results would be very beneficial here.
44) Urban emissions: this is an interesting side-discussion. I would however rather see this as a methodological paragraph. 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).

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

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