

Interactive comment on “A set of methods to quantitatively evaluate the below-cloud evaporation effect on precipitation isotopic composition: a case study in a city located in the semi-arid regions of Chinese Loess Plateau” by Meng Xing et al.

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Received and published: 5 July 2020

This study investigates below-cloud evaporation processes in a semi-arid, continental area from a 2-year dataset of paired water vapour and precipitation observations, using the recently introduced framework by Graf et al., 2019, and a method based on droplet mass balance. The study is useful in that it explores the viability of the newly proposed framework in a different climatic regime. Below I state several major comments and

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numerous detailed comments for the authors to consider for their manuscript revision.

1 Major comments

1. The samples are now taken on a per-event basis, whereas the results from Graf et al., 2019 were from intra-event rainfall samples. This important difference and the implications should be pointed out and discussed in the manuscript.
2. Evaporation and equilibration are part of the same process of thermodynamic exchange of water molecules between droplet and ambient air. It appears that the method used in Section 3.3 only seems to account for evaporative exchange in below-saturation cases - please clarify, and if so, discuss resulting limitations/implications of the approach.
3. Graf et al., 2019 used $\delta^2\text{H}$ on the horizontal axis. The slopes obtained in Fig. 4 can therefore not be directly compared at present. It would be an advantage to use the same variable on the horizontal axis in the present study, to facilitate comparison.
4. The manuscript is at several locations ambiguous or unclear due to the incorrect use of English grammar or choice of words. I made suggestions in many places, and an additional copy-editing process is needed.

2 Detailed comments:

Please see attached PDF document for highlighted sections corresponding to the detailed comments.

L. 88: unclear

C2

- L. 34: can lead to misinterpreting, if not accounted for
- L. 36: grammar
- L. 41: predict from what?
- L. 44: main
- L. 51: same as Xi'an?
- L. 81: be more specific here
- L. 87: wrong word - unclear
- L. 54: unclear
- L. 92: introduce/define term with references
- L. 93: it can be guessed what the intention of the sentences is, but needs to be written more clearly
- L. 100: add one of the original references, e.g. Dansgaard 1964, here
- L. 102: new sentence, rephrase
- L. 104: what is described here seems to be the equilibrium effect on the d-excess, but there is also the diffusional, kinetic fractionation process which should be mentioned here
- L. 112: the connection between this sentence and the references needs to be clarified
- L. 118: topic not clear, is this section only about continuous precipitation sampling, or vapour and precipitation, as the previous sentence indicates?
- L. 132: instrument ... deepens our knowledge: rephrase e.g. to "interpretation/use ... has potential to deepen knowledge"
- L. 139: add location

C3

- L. 149: disentangle
- L. 159: motivate: why is the value of this quantity important?
- L. 175: state name, coordinates
- L. 176: daily/event samples?
- L. 177: add: "for our dataset/region"
- L. 180: understand the role of the main...
- L. 183: paired
- L. 194: separate sentence, add more detail/example
- L. 245: nominal precision, or from lab analyses?
- L. 250: add precision of lab standards
- L. 262: give details on flow rate, heating/temperature, outside and inside length, precip shielding/inlet
- L. 267: give some overview of availability, is the average for UTC times? What is the typical range and variability? refer to next section for details
- L. 236: Are the per-event samples the best way to capture below-cloud effect? Some note here and discussion would be useful to differentiate between intra-event variability and per-event sampling
- L. 299: seems reasonable, but indications of isotope standard dependency. Regarding results of Weng, what is the proportion of data in the low, medium and high humidity range? Give serial number of analyzer.
- L. 303: is falling
- L. 304: make two sentences

C4

- L. 303: continuous exchange, but may lead to net loss -> evaporation
- L. 323: change eq5 such that fractionation factor is on lhs
- L. 329: proposed
- L. 330: used
- L. 332: express eq 6 such that eq. vapour is on lhs
- L. 346: also Graf et al., 2019?
- L. 350: revise this paragraph for readability/clarity. Why/how is this information needed in your study?
- L. 368: lacking some information about how representative the vapour and precipitation data are for the time period. Were both at the same time, were there data gaps, were all precipitation event sampled, what about the duration of precipitation events, the vapour data were not necessarily coincident. Since the slope/intercept are an important part of the arguments here, it would be useful to give uncertainty estimates/error bounds for these quantities.
- L. 394: use smaller dots such that data points overlap less
- L. 396: what do you mean by trend, a temporal evolution? please clarify
- L. 404: not sure this is the only conclusion, a more careful conclusion would be that both are in or close to equilibrium, for a number of reasons
- L. 411: please explain more about the assumptions regarding cloud base height, temperature, RH, and how this is calculated, I assume this refers to Sec. 2.3.3?
- L. 414: by xxx
- L. 428: What is then the reason for the difference between the Wen et al. (2010) study and your study?

C5

- L. 436: likely to experience evaporation
- L. 437: this sentence/statement does not seem to make sense
- L. 438: unclear: is it not rather a cause of the kinetic fractionation in low relative humidity?
- L. 448: this is wrong, in equilibrium conditions the theoretical value is 0 permil, i.e. no kinetic effects. The global mean value of 10 permil for the d-excess in vapour indicates that evaporation is in general a non-equilibrium process.
- L. 451: is this actually stated in Craig, 1961?
- L. 450: to you mean equilibration?
- L. 457: introduced?
- L. 478: could be explained by the diffusion speed of the ice phase to vapour as compared to liquid to vapour?
- L. 499: Graf et al based their diagram on the $\Delta\delta^2H$ on the x-axis, where as you use $\delta^{18}O$. The reason Graf et al. state is that kinetic effects affect $\delta^{18}O$ stronger than δ^2H , such that separation between the effects on both axes is more clearcut. For comparison with their charts and slopes, I would recommend to change the horizontal axis to d^2H in Fig. 4 and all panels of Fig. 5.
- L. 516: In addition to relative humidity, you may consider absolute humidity. The larger number of water molecules present in the atmosphere during warm temperatures may still enable substantial, rapid equilibration during fall.
- L. 536: maybe this sentence could be made more specific
- L. 539: this is not possible to compare, since you use $\delta^{18}O$ and Graf et al use d^2H .
- L. 545: maybe a discussion of what factors change the slope in what direction will allow to advance the understanding at this point. Furthermore, you compare a seasonal

C6

dataset to an individual weather event, which are two entirely different entities.

L. 668: what is the role of the temperature structure, in particular melting layer height

L. 715: the conclusions need to be copy-edited for English grammar

L. 748: "city of CLP" does not seem to be a common name for Xi'an. The connection of this statement to the overall scope of the paper and the results needs to be made more obvious.

Please also note the supplement to this comment:

<https://www.atmos-chem-phys-discuss.net/acp-2020-312/acp-2020-312-RC1-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-312>, 2020.