Review acp-2022-576

Review of revised submission

A set of methods to evaluate the below-cloud evaporation effect on local precipitation isotopic composition: a case study in Xi'an, China

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Remark: In the following, I'm referring to the line numbers in the revised manuscript. The newly added text in the track change document differs partly from the text in the revised manuscript.

General:

The revised manuscript by Xing et al. describes the qualitative and quantitative evaluation of below-cloud processes in a two year time series of isotopic composition in water vapour and precipitation at Xi'an. The authors considered many points addressed in the review (post-processing of water vapour measurements, sensitivity and uncertainty analysis, re-structuring of paper) which improve the scientific analysis and reader guidance of the paper.

Two main issues remain:

1. Even though the language quality has improved, I strongly recommend another language check by a native speaker before publication. There are many grammatical mistakes and sentences that are difficult to read which makes the scientific message difficult to understand.

2. On the discussion/comparison of the two methods:
    - Both methods use an important assumption which is that the surface water vapour is (moist) adiabatically connected to the cloud-base water vapour. In method 1, this assumption is used to calculate the cloud base height, temperature and pressure. In method 2, the isotopic composition of the cloud-base water vapour is calculated assuming a moist adiabatic ascent of the measured ground-level water vapour.
      This assumption only holds if the vertical column at the observation site is undisturbed by horizontal movement (as mentioned as assumption on lines 353-354). A discussion of the validity of these assumptions (under which circumstance can we assume a vertically undisturbed and connected column and where does the assumption not apply) is currently missing in the manuscript.

    - Further, it's important to mention more prominently that method 1 only includes below-cloud evaporation by construction while in method 2 other processes can still be included. I'm missing a discussion of these differences between the methods and the learnings from the comparison. This should be added as a discussion/conclusion point on which method should be applied under which conditions (i.e. under which meteorological conditions might an assumption of local (moist-) adiabatic (not be) valid) and how to possibly improve them in future studies (e.g. could the methods be improved to better represent below-cloud processes during snowfall?).

   Currently, some of these points are addressed at different locations throughout the manuscript (349-358, 536-538 (with respect to snowfall and low temperature), 667-674, Supplement C & D). I recommend a summary of these points in the end.

Specific:

- Abstract:
  - The abstract needs a language check
  - Lines 33-37, the first sentence is difficult to read. I suggest to divide it into two sentences: "When hydrometeors fall from an in-cloud saturated environment towards the ground, especially in the arid and semi-arid regions, below-cloud processes may heavily alter the precipitation isotopic composition through equilibrium and non-equilibrium fractionation. If these below-cloud processes are not correctly identified, they can lead to misinterpretation of the precipitation isotopic signal. "
  - Line 60, "therefore" in last sentence: It's not clear to me how this sentence connects to the previous sentence(s).

- Lines 99-101: "The equilibrium fractionation would not change the d-excess while the non-equilibrium diffusional process would result in a decrease of d-excess in rain (Fisher, 1991; Merlivat and Jouzel, 1979)"

  This is not correct as a general statement. D-excess can change during equilibrium fractionation, depending on ambient temperature. Possibly rephrase to "Equilibrium fractionation does not substantially change d-excess while …"

- Line 128: what do you mean with "initial signal"? The cloud-base signal?

- Line 205: "after filtration": do you mean: "second the samples were filtered, and then immediately..."

- Lines 459-477: this paragraph is difficult to follow, many sentences are difficult to understand due to poor languange; e.g. "Hence, rain/snow formed under such circumstances, their isotopic signals will be less impacted by the environmental factors during its falling."
  Please, check the language in this paragraph.

- Fig.4: It's nice that you added two regression lines and interesting that the linear regression for rain is similar to the results by Graf et al. (2019).
  The colors of the regression lines don't match the colors of the markers for the respective precipitation type in the new figure. This makes the figure difficult to read.

- Lines 529-532:
  "During the supersaturation process, vapor deposition occurs over ice (Jouzel and Merlivat, 1984), which may cause the snow isotopic composition at the ground to be more depleted than its formation height."

  It is not entirely clear to me where this vapor deposition takes place: is it vapor deposition during falling or in the cloud before the snowfall? Why does vapour deposition during snow fall decrease $\delta^{18}O$ and δD of the snow? Whether the isotopic composition of snow becomes less depleted in heavy isotopes during this process depends on the isotopic composition of the water vapour below the cloud relative to in-cloud vapour.

- Lines 534-536: "The diameter of the raindrop used to determine the terminal velocity and evaporation intensity (Supplemental material, eq. 10-13) does not take into account the snowfall factor which results in great uncertainty in method 1.

  What's "the snowfall factor"? Do you mean the different relationship of fall velocity to hydrometeor size for snow flakes and rain drops?

- Lines 551-552: "The significant difference in winter might be related to the supersaturation process."

So far, you've mentioned supersaturation and vapour deposition as a possible mechanism leading to negative ΔδD during winter. Instead of referring to this process, I'd refer to the presence of solid precipitation during winter time. E.g.: "The significant difference in winter might be related to the predominance of solid precipitation which is not accounted for in method 1."

- Lines 562-564: "Wang et al. (2016b) explicitly pointed out that among the parameters of temperature, precipitation amount, RH, and raindrop diameter, RH generally plays a decisive role on Δd-excess in the below-cloud evaporation process."

  You're only showing ΔδD, how about the role of these parameters for Δdexcess in your data? This might be too much to add in this manuscript but mentioning the results from Wang et al. (2016b) on Δdexcess rises this question.

- Section 3.4: This part seems out of place and I didn't learn anything new while reading it (especially after seeing Figure S4 in Section 3.3.1). Further, it seems partly a repetition of the lines 549-552. Is this section needed?

- Fig.7: out of curiosity: there seems to be a seasonality on the effect of a temperature decrease on ΔδD, which is opposite for method 1 and 2. How do you explain the (opposite) seasonality?

- Lines 686-687: "the precipitation and water vapor isotopic compositions have a good relationship"

  What do you mean with "good relationship"?