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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "The isotopic composition of precipitation from a winter storm – a case study with the limited-area model COSMO$_{\text{iso}}$" *by* S. Pfahl et al.

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Response to comments of Referee 2

We like to thank the referee for the review and her/his constructive comments, which helped us to improve the manuscript. Below, detailed responses to all comments are given.

1. p. 23, line 25: "comparable to _those used in_ GCMs."

This has been changed.

2. p. 25, line 14: “German and Swiss weather _services_.”

Corrected.

3. p. 25, line 16: If the word “one” is spelled out, I would be inclined spell out “kilometer” as well.

“one” has been changed to “1”.

4. p. 26, line 12: “Only during phase transitions _do_ they behave ...”

Changed.

5. p. 30, line 4: The author’s last name is “Stewart”, rather than “Steward”.

Corrected.

6. p. 34, line 6: “... switched _off_ ...”

Corrected.

7. p. 38, line 15: Fix the second half of the sentence that ends “... but also high δ^{18}O at the western shore of the lake.”

This has been reformulated: “... but does simulate high δ^{18}O at the western shore of the lake.”

8. p. 40, line 9: “Isotope ratios were _on_ the order of ...”

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Corrected.

9. p. 41 bottom, p. 42, top: I thought that the following reference might be relevant to the argument here, if the authors have not seen it: Liu, Z., Bowen, G.J., and Welker, J.M., 2010: Atmospheric circulation is reflected in precipitation isotope gradients over the conterminous United States. J. Geophys. Res., 115, D22120, doi:10.1029/2010JD014175.

The reference has been added.

10. p. 42, lines 23-25: In figure 9d, I thought that I could see the imprint of cloud depth (or at least IWC) on the delta O18 of precipitation in variations on the warm side of the front. I agree that this may not be part of the systematic cross-frontal variation of δ O18, but it seemed like there was some signal though it may be complicated by the presence/absence of cloud below the melting level along with other factors.

We are not sure where exactly such an impact can be seen. Physically, it is clear that rain formation at higher altitudes leads to lower isotope ratios, and this may of course be visible at certain locations. However, we tried to confine our discussion here to the systematic cross-frontal variation, where the effect is not obvious (in agreement with the referee's statement).

11. p. 43, lines 10-15: The fractionation/equilibration of rain has a different character depending on whether the relative humidity is close to 100% (equilibration) or much lower (fractionation). A plot of relative humidity might be useful for interpreting the effect of isotopic exchange between rain and vapor. If that plot leaves the figure out of balance (with five panels), another plot showing the δO18 of rain in equilibrium with vapor (where rain is present) might be interesting to add if it adds some insight and the authors think that could be helpful to the reader.

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Following the referee's suggestion, we have added plots of relative humidity and δ^{18}O of rain in equilibrium with water vapour. The following paragraph has been added to section 3.2: 'In regions where the relative humidity is close to 100% (see Fig. 10e), equilibration drives the isotopic composition of the rain towards the equilibrium composition with respect to the surrounding vapour, which is displayed in Fig. 10f. In particular on the eastern flanks of the precipitation regions, lower relative humidity causes evaporation of rain drops, and the associated isotopic fractionation leads to an enrichment of the rain compared to the equilibrium composition (cf. Figs. 10d and f).'

12. p. 43, line 25: I believe that figures 9d and 10b are those referred to in the parenthesis.

Yes, this has been corrected.

13. p. 45, lines 25-28: I think of the amount effect as referring to greater depletion rainfall in locations with a greater amount of rainfall. The progressive depletion of air as heavy isotopes are removed by precipitation seems to me to describe the temperature effect, with greater depletion at lower temperatures because of the lower temperatures required for condensation of drier air. Is there something else going on here that I'm missing?

Here, the greater depletion later in time does not go along with decreased temperature (as it would be the case for the classical temperature effect). Instead, it goes along with increased temperature, meaning that there are other processes leading to this decrease of δ^{18}O (e.g., gradual depletion of the boundary layer vapour due to post-condensational exchange). These processes are the same as commonly used as an explanation for the amount effect (see, e.g., Risi et al., 2008), this is why we mentioned the effect here. However, we do not want to go into too much detail at this place in order not to over-interpret the local correlation patterns (see also our response

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to point 8 of referee 1). Hence we have just changed the formulation 'which is usually referred to as the amount effect' to 'which relates to the so-called amount effect'.

14. p. 46: Does the improved correlation for melting level height relative to surface temperature suggest that rain evaporation works in the same direction or the opposite one to the temperature effect? Is it possible to sketch out what is the mechanism (equilibration or fractionation or some combination of the two) that drives the improved correlation?

Post-condensational effects clearly contribute to the temperature effect, i.e., work in the same direction, as is also evident from the results of the sensitivity experiment shown in Fig. 13c (former Fig. 12c). The physical mechanism is that if the melting level is higher, there is more time for the rain to equilibrate with the isotopically heavy boundary layer vapour (as also sketched in section 3.3). Since the fractionation during raindrop evaporation mostly depends on relative humidity (as also mentioned by the referee in point 11), we think that equilibration is the more important mechanism with respect to the temperature effect.

15. p. 47, line 25: "isotopic" is the adjective, so that "isotopic fractionation" is the correct phrase. This probably applies elsewhere in the paper as well.

This has been corrected at all relevant places.

16. p. 48, line 18: "constraints"

Corrected.

17. Comment on p. 50, eqns A2-A4 (see original document for full reference)

We are grateful to the referee for sharing her/his thoughts on the advection algorithm

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with us. We agree that the inconsistency between the transport equations for moisture and total mass of air (in COSMO, the latter is not solved explicitely, but in terms of the pressure tendency equation) is a major problem for numerically consistent tracer advection. Most probably, this issue could only be solved completely by reformulating the dynamical core of the COSMO model, introducing a numerically consistent solution of the transport equations for all density fields. Nevertheless, we see the referee's point and will consider this idea in future tests of the COSMO advection scheme. Since this implies a change also of the standard moisture advection, it will require detailed testing (note that so far the isotope scheme is only a diagnostic module, not influencing other meteorological forecast fields).

18. p. 50, eqn A6: The argument of the limiter function is a polynomial of order eight in this case. Does that make it more difficult to apply the limiter?

The limiter is not applied to the polynomials themselves, but to the numerical results of the integrations from equations (A4) and (A7), and thus its application is straightforward also in equation (A6).

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 26521, 2011.

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