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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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Paper: The isotopic composition of precipitation from a winter storm - a case study with the limited-area model COSMOiso Authors: Pfahl, Wernli and Yoshimura doi: 10.5194/acpd-11-26521-2011

Summary:

This study describes the implementation of water isotopologues in the COSMO regional model and the validation of that model through comparison with observations from a winter storm over the eastern US in January 1986. The results suggest that the model does a good job of simulating the storm in general and highlights some of the processes that fix the isotopic content of precipitation at different locations.

Recommendation:

Minor revisions. This is a nice study that highlights a good method and case for the validation of regional models that incorporate water isotopologues. I would suggest that the paper is nearly ready for publication, and would ask the authors to consider the comments below during revisions.

Comments/suggestions (all page numbers start w/265):

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

p. 25, line 14: "German and Swiss weather _services_."

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

p. 26, line 12: "Only during phase transitions _do_ they behave ..."

p. 30, line 4: The author's last name is "Stewart", rather than "Steward".

p. 34, line 6: "... switched _off_ ..."

p. 38, line 15: Fix the second half of the sentence that ends "... but also high \delta^{18}O at the western shore of the lake."

p. 40, line 9: "Isotope ratios were _on_ the order of ..."

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.

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 delta

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.

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 \delta O18 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.

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

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?

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?

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.

p. 48, line 18: "constraints"

p. 50, eqns A2-A4: This comment probably does not need to be addressed in the context of the present paper, but could be useful for the authors' work with COSMOiso going forward. To my mind, there is some appeal to reformulating the scalar flux as

F_i+1/2 = dx/dt * (rho*u*dt)_i+1/2 * L(q_i+1/2)

where (rho*u*dt)_i+1/2 is the mass flux across i+1/2 during the time step and L(q_i+1/2) is the filtered, mean value of q advected across i+1/2 during the timestep (from A4) with q in place of Psi. I might have an extra factor of dt in there, but hopefully my point is clear. This might not be crucial in your case, but I believe that there are two reasons why this might be desirable:

- Including the spatially-varying rho in A2 and A4 distracts from the reconstruction of the scalar mixing ratio, which should be the focus here.

- Consistency (i.e., that a uniform scalar mixing ratio is preserved under advection) may not be preserved if the mass flux used in the evolution equation for density is not computed using the Bott scheme. To see this, note that equation A1 should reduce to the evolution equation for density when q=1 everywhere. If COSMO computes the mass flux in some other way, then the change in scalar mixing ratio will include errors due to the difference between the mass flux implied by A4 with \Psi=\rho and that computed elsewhere in the dynamical core of COSMO.

The authors do not need to act on this comment for the present paper. However, since the authors did spend a lot of time thinking about how the advection affected the isotopic signal, I thought that this perspective might be useful for their work going forward.

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?

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