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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.

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Response to comments of Referee 1

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. (Page) 26528, (line) 4/5: "Nevertheless, for the present case study these land surface processes are not assumed to be crucial." – Please explain this statement in

more detail. Does it mean that evaporation and local water recycling is negligible for this storm event? Why?

We did not mean that land surface evaporation is negligible, but that the differences in water isotopes between experiments with our simple scheme and a more advanced parameterisation would be small compared to the isotopic variability observed in surface precipitation that is induced by the in-cloud and post-condensational fractionation processes. We however agree that this is hard to see at this point, and therefore removed this statement from the manuscript.

2. 26532, 4-11: Please explain in more detail, (1) why the lower limit for mixed liquid and ice clouds was set to -23° C, (2) why a quadratic (instead of linear) increase of the liquid water fraction with temperature is assumed in COSMOiso.

The choice of this lower limit and of the interpolation exponent is largely empirical and, to our knowledge, cannot easily be justified on theoretical grounds. Therefore, we have simply adopted these values from the standard COSMO convection parameterisation scheme (where they are applied, e.g., for calculating the effective saturation humidity). In this way, consistency between the different components of the scheme is sustained (a note on this has been added to section 2.2.4). The COSMO values of these empirical parameters in turn have been taken from the ECMWF IFS model (cycle 31r1; see http://www.ecmwf.int/research/ifsdocs/).

3. 26532/33, paragraph 2.2.5: It is not clear why a mixing of ERA-40 and IsoGCM boundary data was chosen for the model setup. How can the authors exclude that setup inconsistencies between the standard model variables and the isotope ratios exist and might erroneously influence their simulation results? Why are not simply all required boundary data taken from the IsoGCM?

The main reason why we did not take all boundary data from IsoGSM is the coarse

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resolution of this model. Due to this, it would not have been possible to drive a 7 km COSMO run directly with the IsoGSM meteorological fields. Moreover, the fact that no prognostic information on cloud water and cloud ice is available from IsoGSM might also derogate the quality of the meteorological simulation. We think that inconsistencies between the standard moisture fields and the IsoGSM isotope data are minimised by the way the latter are processed: we only take isotope ratios from the IsoGSM and multiply these with the COSMO specific humidity; furthermore, cloud water and ice isotope ratios are set by assuming isotopic equilibrium, making these consistent with the vapour data. Overall, we think that in this way the large scale $COSMO_{iso}$ isotope distribution is set consistently. Nevertheless, in future research this method should be further evaluated by comparing it to an initialisation using all boundary data from a higher resolution isotope GCM.

4. 26533, 26-28: The simulated period of this test case is 5 days, 6 hours. Only the last 3 days are analysed. The spin-up time of 2 days, 6 hours seems rather short and should be further justified.

Such a spin-up time of roughly two days is rather typical (or even long) for numerical weather prediction applications, for which the COSMO model has been designed. For the standard meteorological fields, it should thus be sufficient. With respect to the isotopes, it is more difficult to judge how much spin-up time is required. We have performed a test simulation with a increased spin-up time of 198 hours, which produced very similar results as the reference run. We hence think that these results are robust, but in future research certainly more tests focusing on this aspect should be performed.

5. 26536, 25: Why is the isotope data not weighted with precipitation intensity, as it is done in many other isotope studies?

In other studies, weighting with precipitation intensity is performed when calculating

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averages of isotope ratios at single locations, reflecting the natural weighting when precipitation accumulates over extended periods. Here, we try to assess the model's ability to reproduce the isotopic variability in a set of temporally and spatially distributed measurements, not by calculating averages, but statistical distributions (which is a different approach). We do not see a reason why, e.g., locations with more precipitation should have a stronger weight in this comparison compared to locations with light or moderate rain.

6. 26538, 13-15: Is it possible to quantify the influence of local evaporation from Lake Ontario and the other Great Lakes on the delta O-18 signal in precipitation? Which delta O-18 values are prescribed in COSMOiso for these water bodies?

For quantifying the impact of lake evaporation, a complex numerical approach would be required (either in the form of a sensitivity study or with the help of numerical tracers), and thus this is, as we think, beyond the scope of the present paper. For the lake water, the same constant δ^{18}O ratios are used as for ocean water, which most probably are too high. Nevertheless, the model does not systematically overestimate the δ^{18}O at the stations close to the lakes, suggesting that the influence of these too high values is negligible (or at least the available data for this case are not sufficient for its determination).

7. 26543, 25: "(cf. Fig 9a and 10b)." – The authors refer in the text to Fig. 9d, not 9a.

Corrected.

8. Please explain, why the sensitivity experiment does not only lead to reduced and more patchy values of the correlation coefficient r, but also to some additional regions with a clear negative correlation between temperature and delta O-18 in precipitation (see Fig. 12c).

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Such a negative correlation can, for instance, be due to the fact that precipitation at a specific location was formed in deeper clouds in the beginning of the event and in more shallow clouds towards the end (recall that this effect seems not to be important for the systematic gradient across the front, but may nevertheless be crucial on local scales). This would then be associated with a larger depletion in the beginning and, together with the typically decreasing temperature, a negative correlation. In the reference run, this effect is often compensated by equilibration below the cloud base. Nevertheless, such a correlation should not be over-interpreted, since it is only based on a few hours of data, and the corresponding effects may be less important when longer time scales are considered. Therefore, we focus only on the spatially consistent, large-scale positive correlation patterns in Fig. 13 (the former Fig. 12).

9. Figure 2 & 3: Why do the chosen dates of these two figures differ?

In Fig. 3, the same dates are shown as in Fig. 5 in order to allow a direct comparison between precipitation fields and the corresponding isotope ratios. These dates are prescribed by the availability of the isotope observations (a short note on this aspect has been added to section 3.1.1). In contrast to that, in Fig. 2 we have intended to give an overview also on the early and late phase of the storm in order to provide a complete synoptic reference, and therefore shifted the first and the last date by 12 hours. To reduce the resulting inconsistencies, we have added two panels to Fig. 9 (former Fig. 8), which now shows the isotopic composition of the vapour on 850 hPa for all five dates (see also comment 12).

10. Figure 3: The contour lines as well as the green dashed line are hardly recognizable. Please improve this plot.

The colour of the contour lines has been changed, and the thickness of the green dashed line has been increased.

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11. Figure 8: I suggest plotting delta O-18 in vapour not at 1km above the surface, but rather at 850hPa. This would enable a direct comparison to the temperature pattern shown in Fig. 2.

The plots have been adapted, following the referee's suggestion.

12. Figure 11: Why are the vapour data and the precipitation data shown for different dates? This incompatibility makes it difficult to follow statements as on page 26544, line 27 "The higher (precipitation) values there are caused by the less depleted water vapor."

This has been changed, all panels now show the same date, 12 UTC 19 January. In addition, two panels have been added to Fig. 9 (former Fig. 8) to show the vapour data at this date also for the reference simulation.

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