Atmos. Chem. Phys. Discuss., 11, C11961–C11963, 2011
www.atmos-chem-phys-discuss.net/11/C11961/2011/

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 #1

Received and published: 16 November 2011

S. Pfahl and colleagues present in their manuscript a first case study with a newly developed version of the limited-area model COSMO, which has been enhanced by the explicit simulation of stable water isotopes H218O and HDO. The incorporation of stable water isotopes into COSMO follows in many aspects the previous implementation of these hydrological tracers into different atmospheric general circulation models (AGCM) and regional models. The COSMOiso model is evaluated using a winter storm event over the Eastern US in January 1986 as a first test case. For this event, isotope measurements with a sufficient spatial distribution and high temporal resolution exist. The authors convincingly show that the COSMOiso model is capable simulating both major meteorological and isotopical aspects of this event. With help of the COSMOiso

results the major processes influencing the isotopic signature of frontal precipitation of this storm event are identified and analysed in detail.

In my opinion the authors present a very good study on this subject, and I highly recommend a publication of the manuscript in ACP. The incorporation of water isotopes into a limited-area weather forecast and climate model is a substantial contribution to the research field of water isotope modelling, closing the gap between coarser global or regional climate models and cloud resolving models. The authors do not only focus on their own model development and results, but also give an excellent overview about the general topic, citing and reviewing most of the relevant studies published so far. The presented results appear robust and go in several aspects clearly beyond the status quo of isotope modelling with coarser climate models. They are surely of great interest to the wider scientific community.

For further improvement of the manuscript, I suggest the following minor changes and additions:

(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?

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.

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?

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.

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

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?

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

26546, 20: 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).

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

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

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.

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."

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