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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Monthly resolved biannual precipitation oxygen isoscape for Switzerland" *by* Z. Kern et al.

Anonymous Referee #2

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Review

This manuscript by Z. Kern and co-workers examines the distribution of oxygen-18 in precipitation over Switzerland for the period 1995-2000. The presented analyses are based on data sets from 39 stations. The analyses reveal a strong linear delta O-18 versus altitude relation for the summer season for nearly all stations, but a similar strong relation (with steeper gradients) during wintertime for low elevation stations, only. Combing the delta O-18 data with estimates of the planetary boundary layer (PBL) height at the various locations and months, the authors explain their findings by shallower vertical mixing heights during wintertime in the Swiss Alps. Next, for the biannual period 1995/96 when the station network was densest, Z. Kern et al. apply a spatial interpolation method including residual kriging for generating monthly maps

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of delta O-18 in precipitation. According to the authors' findings, this biannual data set can be extended to recent years as well as backwards for the last 2-3 decades with some reduction in reliability.

The basic scientific idea presented in this manuscript is sound and well outlaid. I also acknowledge the novelty of the compilation of Swiss delta O-18 data in precipitation, which most certainly is worth to be published. However, in the current form I rate the presented analyses and results as too narrow for a high-impact journal like ACP. So far, the authors are simply performing a monthly linear correlation analyses between delta O-18 in precipitation and altitude for various Swiss station locations and months. The following isoscape construction, which considers PBL height and a digital elevation model (DEM), only, is rather simple and straightforward, too. The found breakdown of the linear relationship for the high-altitude sites in wintertime is very interesting, but the analyses for explaining the breakdown fall short. E.g., on page 9903, lines 5-11, the authors speculate about several different atmospheric conditions and processes that might be responsible for the observations. It would be certainly worthwhile if the authors go one step further with their analyses and actually try identifying the key processes, e.g. by analyzing wind speeds, cloud and precipitation distribution for the different locations in wintertime versus summertime. Depending on the outcome of such analyses it would be interesting to see if the findings for the Swiss Alps can be generalized and applied to other regions, too. The authors already discuss some potential application of their findings for the interpretation of isotopic ice core data from Greenland (e.g., p. 9905). However, without a more detailed analysis and discussion of the meteorology of high-altitude alpine sites and polar regions like Greenland, I find the presented comparison rather speculative and non-convincing.

As a final recommendation I suggest to substantially shorten method section 2.3. In its current form it is rather bloated. Giving so many details on the used software packages is rather unusual. If the results do not rely on the specific software packages/programs the authors can easily omit such information; if not, the authors should explain in detail

ACPD
13, C3829–C3831, 2013

how much their results might depend on the specific software.

As I ask for major text revisions and enhancements of this manuscript, I omit any list of minor corrections at this stage of the review process.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 9895, 2013.