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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.

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Thanks for your comments on our manuscript. We segmented the referee's report according to the topical units and our corresponding replies are given below.

Referee #2: 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.

Authors: Thanks for this supportive comment.

Referee #2: 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.

Authors: It was definitely our aim to keep the method as simple as possible. There is an alternative approach in isoscape derivation when many environmental parameters are combined. These studies usually employ weirdly large set of environmental parameters, sometimes allowing the squared or cubed relationships or combination of a dozen of variables. In these studies the station based water isotope records usually serve as "target" dataset and modelled data are compared to station records to test the model's performance. This is also a progressive field (although we are rather sceptic about the physical rationale to sum up the product of elevation and temperature with the product of latitude and fresh snow amount, for instance). In our region of interest direct observations provide an exceptionally high spatial density owing to the numerous stations obtained from the merged networks. This favourable condition offered a unique opportunity to develop an isoscape relying more on station data than global isoscapes or a mapping over a region poorly covered by direct measured data. We think that incorporating the PBL information into isoscape development is physically sound; provide much more methodological novelty than multiparametric polynomial regression. Probably it will stimulate the alternative isoscape derivation approach, because parameters of those polynomial equations using to describe the water isotopes in precipitation should be tuned separately below and above the PBL.

Referee #2: 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.

Authors: In the revised manuscript we analyse the seasonal changes in static stability (based on the estimated non-dimensional Brunt-Väisälä frequency) and explain the dichotomous seasonal isotope profile by stratified atmospheric flows. We can link our empirical data to the results of a recent modelling study (Galewsky, 2009).

Referee #2: 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.

Authors: Yes, we argue that it can be generalized and applied to high reliefs. The findings of Holdsworth et al. 1991 (this supplementary info has been posted as a Short Comment) support our results since the same situation prevails over the St. Elias and Wrangell Mts.

Referee #2: 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.

Authors: In the revised manuscript, we expand the discussion clarifying the relevance of the present results to the problem of δ18Oice-temperature scaling observed in Greenland. Monthly Brunt-Väisälä frequencies are estimated also for Greenland to show that stratified flows are likely prevailing also above this terrain which expectedly force a similarly modified isotope profile as seen for the Alpine winter above the most elevated sectors of the ice sheet.

Referee #2: 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 how much their results might depend on the specific software.

Authors: Our intention was by giving all these details to allow the reader to follow our approach more easily, however we will shorten section 2.3 as suggested.

Refs: Holdsworth, G., S. Fogarasi, and H. R. Krouse Variation of the stable isotopes of water with altitude in the Saint Elias Mountains of Canada, J. Geophys. Res., 96(D4), 7483–7494, doi:10.1029/91JD00048. 1991. Galewsky, J.: Orographic precipitation isotopic ratios in stratified atmospheric flows: Implications for paleoelevation studies, Geology, 37, 791–794, doi: 10.1130/G30008A.1

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