Atmospheric Chemistry and Physics Discussions

Interactive
Comment

Interactive comment on "Source and origin of atmospheric trace elements entrapped in winter snow of the Italian Eastern Alps" *by* P. Gabrielli et al.

Anonymous Referee #2

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Source and origin of atmospheric trace elements entrapped in winter snow of the Italian Eastern Alps

By Gabrielli P et al.

Scientific Significance: 2 Scientific Quality: 3 Presentation Quality: 2

Ţ Does the paper address relevant scientific questions within the scope of ACP? Yes Ţ Does the paper present novel concepts, ideas, tools, or data? Yes, novel data. Ţ Are substantial conclusions reached? Some interesting conclusions are reached Ţ Are the scientific methods and assumptions valid and clearly outlined? It would requires

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more explanations in some parts (see below) Ţ Are the results sufficient to support the interpretations and conclusions? Not totally (see below) Ţ Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes, but the references could be extended in the introduction (see below) Ţ Does the title clearly reflect the contents of the paper? yes Ţ Does the abstract provide a concise and complete summary? yes Ţ Is the overall presentation well structured and clear? yes Ţ Is the language fluent and precise? Need some improvement

General comments

Overall, this paper is based on a huge data set that was probably extremely labor intensive to gather. However, it does not seem that the conclusions are at the level of this investment. Particularly, there are no investigations on the spatial character of the data set, on the variability of the results in term of specific location of the sampling area and associated meteorological situation. The most interesting results obtained concern the potential distinction of the sources of the trace elements into several groups, including crustal and anthropogenic origins, which could have been obtained with much smaller efforts. A large fraction of the conclusion is dealing with atmospheric transport processes that are not really demonstrated in the course of the discussion. Finally, the level of English does not preclude proper understanding of the ideas, but could be well improved in many places.

Specific comments Introduction, second paragraph: all of the references are mainly dealing with metals, while it states "contaminants".

Introduction, last paragraph: you do not mention the motivation for such a large spatial coverage.

Section 2.1, 3rd paragraph: you have to describe precisely the meteorological data set that was gathered during the program. This will help understanding the section 3.5 (pages 8799-8800), where it is currently not possible to figure out the data set used for the second PCA including both chemical and meteorological data (see below).

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Section 2.2, first paragraph: you do not mention the depth of the layers that were collected, if it represented an overall snowfall event or if it was always the same. You should discuss about this sampling strategy versus the heterogeneity of concentrations during a snowfall event, the evolution of surface concentration during dry periods, and the remobilization of snow surface by the wind.

Section 3.1, second paragraph, and Table 2: this table should include the DL for all species. Also, the values of skewness and kurtosis should be discussed.

Section 3.1, 4th and 5th paragraphs: this idea of similar source regions for contaminants at winter-period-low-altitude sites and summer-period-higher-altitude sites is not well substantiated. First, you should show that the comparison is valid for more than 2 chemical species. Second, you should prove that the data series are statistically identical (taken into account the very large variability in your series). Further, the fact that the origins of the species are the same would not necessary lead to similar concentrations. Finally, several previous studies indicate that the source regions of summer precipitations at high altitude sites in the Alps are much more continental than regional (refe)

Section 3.2, last paragraph: how do you evaluate "the range of experimental error"? Is it valid to perform a comparison of fluxes at relatively high altitude sites and critical loads for forests?

Table 4: the table is too small. Also, taking into account the variability of the data set, it is not really informative to present an arithmetic mean only.

Section 3.3: It is very surprising that the data set was not explored more in depth to get more information on the spatial variability. The comments presented in this section are rather minor and ballpark examination, considering the very huge amount of work that was needed to gather the samples and obtain the results. Any attempt for statistical analysis in this direction? Also, there is not attempt to reconciliate the observation of this study with that from the moss study.

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Section 3.4.1., second paragraph: if the data series are lognormaly distributed, why don't you use geometric means and standard deviations in table 2? What are these "outliers"? You have to better explain that notion of "rank position" and how you apply it. What kind of information do you loose in doing so? What are the drawbacks of replacing missing data with the median?

Section 3.4.2., 1st paragraph: any idea on why K+ is not included in any significant result of your statistical analysis? What are the reasons of the robust character of the results independently of the data subset? How do you explain that the trace elements are all gathered on the first component, irrespective of their individual origin / source (as determined in the later sections)?

Page 8796, line 9-10: an excess of Cl- of 39% cannot be seen as "a slight excess" and the associated contribution as a "minor contribution". It is therefore surprising that Cl- is used as a marine tracer.

Page 8797, lines 14-21: the ratio of NO3- / SO42- in aged snow is governed by far more processes than just the initial concentrations of NOx and SO2 in air. Presenting a very general fact (concentrations of precursors in one of the potential sources regions) as the sole reason of concentration ratios observed in snow is really dubious.

Section 3.5 (pages 8799-8800): All this section should be more carefully explained and presented. What is the rationale for the calculation of a time period between "the snowfall day until the sampling day"? What are the meteorological data that were included in this second PCA? Does this mix both local meteorological data and large scale ones? Were these data also ranked? If not, cannot it be a reason for "the clear separation between chemical and meteorological parameters"? Why do you state that wet and dry depositions are only dependent of large scale processes?

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