

Interactive comment on “Sources and distribution of trace species in Alpine precipitation inferred from two 60-year ice core paleorecords” by A. Eichler et al.

Anonymous Referee #2

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General comments

This manuscript deals with possible sources and origin of various chemical species in two ice cores drilled in the Northern and Southern parts of the Alps, with the precise aim to study the north-south gradients in precipitation chemistry. This is a highly debated and controversial topic and I think that this manuscript helps in better understanding these transport phenomena. The manuscript reports and compares well the two chemical data sets. The authors have a large experience in the sampling, sample pretreatment and analysis of trace substances in ice core samples. The manuscript represents a further step in their research by the combination of the interpretation of the chemical data with the best knowledge in the weather regimes, but the way in which

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it is presented is not totally convincing. There should be stronger arguments with the comparison of other published data, those of Colle Gnifetti and Col du Dôme in order to justify the argumentation. I would recommend also more precautions when doing statistical analysis; it should be better explained how it has been performed; the correlation coefficient alone does not give sufficient information from a geochemical point of view, it is only a statistical relationship, that could lead to bad interpretation of the dataset. The English can also largely be improved. I also found some references difficult to find in the scientific literature, because they are in German and therefore I would recommend to take it into account while revising this manuscript.

Specific comments

Introduction

Are there any references for the north-south gradient of pollutants distribution? I don't agree with the fact that snowpack and glaciers from the Alps are well suited for. In the Alps few sites are suitable for such studies, the coldest one, and it is not the case in all the Alps. Third paragraph, the comparison made by Preunkert et al 2000 between Col du Dôme (CDD) and Colle Gnifetti does not show similar atmospheric conditions but similar atmospheric pollution. In the third paragraph I don't understand why the author makes a quite large description of the SNOSP program rather than presenting the different data obtained on longer records. Authors mention only Preunkert et al (2000); the purpose of this paper was to see if CDD was suitable for temporal studies of chemical records. Comprehensive interpretation and also detailed information on the seasonality and the deposition pattern associated to different meteorological conditions can be found in Preunkert et al (2001a and 2001b). In these papers the difference between winter and summer is well discussed and gives interesting information on the potential anthropogenic sources (geographical areas) in function of the season.

The fourth paragraph deals about other types of archives, but the authors speak only about studies made in Switzerland. I think over studies of major importance can be

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added, especially those of Van de Velde et al in CDD, Shotytk et al in various peat bogs archives, etc..

Site description What is the borehole T that has been observed at GG? It should be mentioned to argue that they are in the cold infiltration zone. I donŠt know if it is necessary but it has been showed in the past that borehole temperature in Alpine glaciers can have a high variability at a short spatial scale. For example at CDD and also at Colle Gnifetti, 2°C variability has been observed depending on the slope considered. I should recommend the author to develop this part, precision about the measurements (same borehole or other studies) and may be comparison with other data from the same area should be added, if they exist. I would prefer to have the drilling description in this part rather than in the following section.

Chemical analysis and datingŠ The sentence: ŠThe ice core sectionsŠ in the process taking off..Š is not clear enough; the English should be improved. I would recommend a more precise description of the proxy used for dating. How Sahara dust and maximum, bomb testing events have been identified (what kind of analysis, dust, beta radioactivity, tritium, etcŠ) ? Why donŠt they used the same reference horizons for the two cores? The core from FG (from 1944-1988) should have preserved also the first bomb test maximum (1958), the two Sahara dust events and also Chernobyl accident signature. The authors should add some precision about the variability of accumulation from one season to another at both sites. Is there any post deposition phenomenon like wind erosion that lead to a seasonal variability of the accumulation rate. This is of first importance to proof if the ice core record can give a continuous record and therefore valuable atmospheric information. What are the associated uncertainties on the dating for both cores?

Precipitation characteristicsŠ

I donŠt agree with the difference the author makes between Northern Alps and Southern Alps for the two sampling sites. The sites are different in term of moisture and

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precipitation sources, but is it possible to say that one belongs to northern and the other one to southern Alps with only 80 km distances between the two ? Dealing with the precipitation characteristic of the two sites, authors refer to an analysis of weather type. This is based in a 20 years old classification, and although based on a large data set, it is sometime criticized by the scientific community. In addition the cited reference is in German and it is quite difficult to find. I would suggest to the authors to use a better description regarding the weather conditions of the sites. I don't think that Fig 2 is essential; one sentence in the text is sufficient. Moreover where are Interlaken and Mosogno. There is no description at all. The main important thing should be to know what are the consequences at the drilling sites. Does the accumulation exhibit the same pattern as the precipitation regime? The last paragraph is a bit speculative; this kind of argument should be in the results and discussion section Sources of trace species It is not clear why the correlation analysis was performed on the concentration data (taken as the logarithm). In this section, it assumed that species have almost a single source i.e. seasalt, dust, etc.. . This is not true, especially for Na. Na contribution from windblown dust can be really high as it has been seen in CDD. I think that the correlation presented is made without any precaution and may be is not representative of the different sources as claimed by the author.

Annual cycles

About the Figure 3, it should be mentioned in the legend to what correspond T (St Bernard) Figure 3, the observed variations are quite strange, T gradient is only 6 mil between winter and summer. This is quite strange for Alpine site and also compared to T variation in Saint Bernard. Does the observed variations correspond to the real T variations or is due to another parameter? For instance, the diffusion process could explain such a smoothing of T variations. This may be an evidence that the ice core record is not so well preserved ! A comparison of seasonal differences observed at different sites in other studies must be done for the different species, such data exist for CDD and Colle Gnifetti.

Long term trends

This subject has been treated by many authors for SO₄, NO₃ and NH₄ and for different sites, a comparison between the different studies is required as it can give valuable information and confirm the good quality of the record. Figure 9 does not show evidence of climatic changes; especially Na, as mentioned before, can have a strong crustal component, I recommend to look at this additional source of Na before saying anything using the Na trends. Moreover does the increase observed after the 60s has described at other sites like CDD for instance? This could be a good argument. In Table 2 there is a question mark (?) close to the Ca data for FG. Has it any meaning?

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