Atmos. Chem. Phys. Discuss., 4, S88–S91, 2004
www.atmos-chem-phys.org/acpd/4/S88/

Atmospheric Chemistry and Physics Discussions

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 #3

Received and published: 15 February 2004

Response to:

General Comment: It is right that no study has been published yet 1) comparing Alpine ice core data with statistical weather type analyses, and 2) concerning the chemical records from the GG ice core. However the question is in what extend valuable atmospheric information can be extracted from Alpine ice cores. That requires examining in detail the degree of reliability and uncertainty of data used, if not the usefulness of new ice core data set in the Alps remains of limited interest.

1. Comment: As stated in my first comment it is crucial to check whether the ice core data and the applied methods lead to an appropriate tool which is reliable to conclude in atmospheric terms. This requires a comparison and discussion with already existing

Full Screen / Esc
Print Version
Interactive Discussion
Discussion Paper

ice core data from the same area. The authors stated that Şa comparison between GG and CG data will be not done since at CG mainly summer snow is accumulated and no seasonal information could be obtained from this siteŤ. That is not correct: Preunkert et al. 2000 compared separately winter and summer snow deposits over ten years between Col du Dome and Colle Gnifetti (CG), and obtained a very good agreement for both seasons (which among others demonstrated also that the applied winter summer dissection tool seems to be appropriate (see below)). This offers the possibility to the authors to check their GG data against other deposition data at least over 1981-1990. Furthermore Doescher et al., showed a regular NH4 stratigraphy with NH4 winter levels below 10 ppb at least from 1963 to 1980, which offer (even if the winter snow record at CG is not so reliable) the possibility to compare the summer deposition records. Such a comparison is important since it would permit to evaluate the variability of the deposition at the scale of 1 km around the CG site.

The authors stated that: Şthe apparent discrepancy between trends from CG and GG for NH4 is due to different averaging periodsŤ. I looked again at these trends I am really not convinced. I strongly recommend to see a plot comparing the trends and CG and GG.

2.Comment: Sorry but the referee has not misinterpreted the data presented in Figure 3 (but converted the per mille into °C with a roughly factor of 1) As confirmed by the author we can assume an annual temperature amplitude of ca 20 °C at this high altitude site. The seasonal shape of the temperature signal is normally close to a sinusoidal one. If we assume the maximum in summer and the minimum of the amplitude in winter and build summer and winter averages this would lead to a temperature amplitude of ca 13°C between summer and winter half year, and to an amplitude of ca 18°C between the coldest three months (taken as winter by the authors) and the warmest three months taken as summer by the authors. Converted en per mille this would lead to a delta 18O amplitude of ca 11-16 per mille and not 6 per mille as found in the GG ice core. Therefore the argumentation of the author does not convince me, and I still

ACPD

4, S88–S91, 2004

doubt on the way that authors did the seasonal dissection. In the same paragraph the author stated that their dissection criteria which gives a mean winter value (December to February) explains the 4 time higher values found in this study compared to literature. This argument does not explain the differenece since the 4 times lower concentrations found by Preunkert et al. (2000) or Maupetit et al., 1995 are also averages over the winter half year (ca October to March) . Again, this discrepancy should be discussed and the seasonal dissection criteria should be tested against other sides where the chemical characteristic of the winter snow deposition is available in literature (e.g. Col du Dome, but also Colle Gnifetti (CG)).

Further the isotopic signal used for the seasonal dissection is not conservative downwards the GG core but underlies a decrease in the seasonal amplitude (seasonal amplitude in the 1950s is about 60% of the one found in the 1980s (the time over which it is averaged in Figure 3 of the draft) Fig.7 in Doescher et al., 2000). This is of course not due to an atmospheric change but rather to diffusion artefacts as already seen and investigated in other ice cores. Thus the shown long-term trends in Figure 7 of the draft are not very convicting and I doubt that they can be used for atmospheric interpretation.

Response to paragraph 4 of the author: The paragraph added by the author is not convicting. No trends are shown, but the reader is left alone with aŤ correlation levelŤ, what is not illustrative for an outstanding reader. E.g. for NO3 a certain correlation has been found, but later it is stated that the NO3 records show a maximum in the 1970s which is not found for the Şcountry NOx inventoriesŤ in Europe, and not found in other Alpine ice core records from CG and Col du Dome (Preunkert et al., 2003). The latter deposition data follow the NOx inventories of Ardenne et al, 2001, or those made byUNECE/EMEP (Vestreng and Klein, 2002, and Simpson et al., 1997) with a monotone increase at least until the 1990s. Again my question is: in what extent are useful the deposition records for a relevant atmospheric discussion??? Further, the argumentation of the author with which the disagreement of NHx inventories and NH4 depositions are explained is not convincing (no NH3 reaches the high altitudes) since at

ACPD

4, S88–S91, 2004

Mount Sonnblick air measurements have shown that ca 30% of NHx arrives as NH3 at 3100 m asl (Kasper and Puxbaum, 1998). Finally the simultanouse maximum of SO4, NH4, NO3 found in the GG ice core (in difference to what is found in other studies) in he 1970s makes me wondering if there is not an other reason which drives the deposition data, as e.g. high vertical transport efficiency in a few adjacent years. Also a change in the deposition characteristic due to extreme low or high summer temperatures, or an enhance input of Ca (see figure 7) cannot be excluded.

In summary I have to conclude again that the ice core data used in this draft are not at all proofed concerning their quality with respect to atmospheric relevance. Thus I suggest that the authors should focus at first on the evaluation of their data and processing methods, and proof their reliability before they are applied on any atmospheric chemistry. This will imply fundamental changes within the exisiting draft.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 71, 2004.