

Interactive comment on “Volcanic eruptions recorded in the Illimani ice core (Bolivia): 1918-1998 and Tambora periods” by M. De Angelis et al.

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Received and published: 28 August 2003

Several comments of A. Palmer are justified and the new version of the manuscript has been prepared taking many of them into account.

- We agree that this paper (called paper #2) dealing with volcanic deposits would benefit of the publication of a more general paper (called paper #1) describing in detail the chemistry of Illimani ice and the various major impurity sources. This is what was planned. Unfortunately, for various reasons, paper #1 has not been finished on time. In order to improve the quality of paper #2, we have modified our text and added some explanations normally part of paper #1.

In the new version of the manuscript, we have been less ambitious, in particular about the role of minor volcanic eruptions on the chemical records. The chapter dealing with

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this subject has been modified significantly.

- In Table 3, we provide to the reader a selection of volcanic eruptions which might have an impact on snow chemistry at Illimani for the study time period. Eruptions were selected according to their location in the tropical latitude belt for eruptions with $VEI > 3$, and to a $VEI > 2$ for Andean eruptions. In the case of minor regional eruptions ($VEI < 3$), without atmospheric modelling, it is hopeless to assigne with certainty an origin to the sulfate spikes observed along the ice core. Therefore, the reader is left free to compare the list of Table 3 and the sulfate record shown Fig. 8 and to speculate about the identification of the eruptions.

- We have no mean to assess the amount of sulfate contributed by volcanoes compared to the amount originating from primary aerosol (gypsum). Moreover, a part of sulfate originates most probably from anthropogenic SO_2 emissions, in particular for the upper half of the record. The potential sources of sulfate in this region are more explicitly described in the new version of the manuscript. Typically, this is something which has to be discussed in detail in the general paper #1 still "in preparation".

- As fluoride was not considered to be essential for the discussions, we have decided to not include this element in Figure 1.

- Regarding now ECM data: in Fig. 1, for practical reasons, an averaging on ten measurements of the raw data set has been adopted. This averaging is also valid for the other figures. Note that ECM measurements are sometimes discontinuous due to core quality. In Fig 4, 5 & 6, these parts are shown by straight lines, but this is not essential for the discussion.

- The calculation of H^+ by ion balance (or ionic strength) is classical when H^+ is not directly titrated. Provided all major ions have been identified and measured, the method is reliable. pH measurements are not suitable. The titration method of Legrand et al., 1982 would be convenient, but was not available for this study. More explanations can be found in Maupetit and Delmas, 1994 for alpine snow where carbonate layers

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are frequent. The correlation shown in Fig. 3 is may be fortuitous, but it supports satisfactorily our arguments.

- Fig. 7: flux was calculated, as usual, by using concentration multiplied by annual accumulation in g of water. Accumulation changes is a parameter which will be discussed elsewhere.

- Fig. 8: the comments dealing with the content of this figure have been seriously revised.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 2427, 2003.

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