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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Trace elements in South America aerosol during 20^{th} century inferred from a Nevado Illimani ice core, Eastern Bolivian Andes (6350 m a.s.l.)" *by* A. Correia et al.

Anonymous Referee #1

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General Comments

The paper essentially presents trace element data analysed from an ice core covering the last 80 years, approximately. The data set is clearly unique through:

- the drill site position in the tropical Andes - the analyses of quasi-continuous records - the sub-annual time resolution - the strategy to gain total trace element concentration (via total dissolution) and - the coverage of the whole suite of elements, detectable by the IPC-MS technique

In discussing the results the authors attempt to address a rather extensive variety of geo-chemical issues ranging from ENSO driven impact, dry/wet season contrasts,

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multi-year cycle (connected to stable water isotope patterns) to long term changes of anthropogenically derived elements and the characterisation of (so called) "local soil dust aerosol". However, based on the presented material it is rather difficult to assess the significance of the findings. I generally suggest to reduce the number of addressed issues (concentrating on the significant ones) and to re-organise the paper to make the findings clearly supported by the data presentation.

Specific commands

There is a lack in the description of the glaciological drill site feature. Give details of, at least, firn temperature, occurrence of melt layer, sublimation effects, depth of firn (ice transition, see also below) and the drill site position relative to the surface topography (i.e. map with contour lines).

Chapter 3.1

Dating deserves closer attention: what is the typical uncertainty in different sections of the analysed core, is there a systematic change in the annual layer thickness (ice or water equivalent) not explained by vertical strain?

It remains unclear how the sample handling blanks are obtained (note that firn core de-contamination cannot be simulated by a refrozen blank water sample). Blank variability and minimum concentration are missed in Table 2 to assess the deployed method.

The reasoning that Li is local derived is not conclusive. Similar deviations from Bowens soil ratios are seen for other crustal elements like Ca, K, Ba and Ta. It remains unclear why the authors put so much emphasis on Li in presenting their data (a less commonly measured species in mineral aerosol).

The ENSO-Li link argument is blurred and should be skipped.

Chapter 3.2

The procedure to dissect dry and wet seasons is not clearly described. What kind

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of parameters are used, how did the authors cope with the systematic decrease of temporal resolution with depth? What I would expect to see here first are average seasonal cycles presented for some key trace elements. It is unclear how the 2-5 year periodicity in the dry season impurity levels are obtained. The link to meteorological parameters is suggestive and should be skipped (glaciological impacts are possible reasons as well).

I feel the delta-D insertion is out of scope (to be skipped as long as the issue is not really substantiated).

Following Fig. 4, I cannot recognise a significant link between delta-D and crustal (Ti) level minima as indicated. There are other events with a vice versa feature.

Chapter 3.3

Comparison with Antarctic ice core data are of little help due to the totally different spatial source distribution.

The overview of concentration levels in Table 3 lacks any indication of the Illimani data variability, whereas the number of samples is relatively unimportant. Best give the resp. log and/or linear SD values to make the means useful, possibly include the data ranges as well.

Within the above chapter, also long therm changes are addressed though to a minor extent and without respective data presentations. Having an ice core data set on hand covering so many elements I would expect that evaluation of net temporal changes of anomalously enriched elements would be a primary focus. I wonder why there is no parametrisation given on the significant temporal changes seen in the data set.

Chapters 3.4 and 3.5

The discussion in these chapters is somewhat redundant in view of the EF feature under high and low (i.e. dry and wet) conditions. It is obvious that any non-crustal excess (no matter what the reason is) becomes eventually diluted (up to non-detectable

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levels) if the crustal impurity is increased over several orders of magnitude. Expecting a more or less log-normal frequency distribution of the Al-(reference) values presented here it is no surprise that EF-values are driven by the crustal impurity content. The related material in chapter 3.4 and 3.5 may be strongly condensed, therefore.

Technical comments

Change anthropic into anthropogenic, throughout.

Plots of temporal records (either for trace elements of delta-D) should be strictly revised:

- give data as a histogram (as they are analyses), not individual points connected by a line - show original data, if necessary together with some smoothing (actually the delta-D lines in Figs. 3 and 4 cannot be ascribed to the data)

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 2143, 2003.