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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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General Comments:

The manuscript of DeAngelis et al. represents a detailed study of proxy signals of recent volcanic eruptions in a high-elevation tropical ice core. At least in the near-equatorial latitude band, a detailed examination of this kind into the timing and Şchemical fingerprintsŤ of multiple eruptions has not been previously seen. Yet given an understanding of spatial variability in chemical proxies across the surface of a glacier, and the temporal limitations due to the predetermined 10-cm sampling interval (for the upper 30m), some analysis and interpretation herein appears to be quite speculative, or at least, ambitious.

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Certainly, having these ambitions helps to Śpush the envelopeŠ in ice core research towards better understanding and expanded use of the powerful glaciochemical histories. But of course, paramount to that end must the mechanisms (or logic) underlying the interpretation scheme be presented in full, along with the chronologies and characterizations of unusual or extreme events. In this respect, the authors have not satisfactorily demonstrated these important developments, though they are obviously in hand. At several key points in the discussion (or application) of chemical parameters as volcanic indicators, what would appear to be critical details are omitted in favour of a citation to another paper (with the same first author) still in preparation. This phenomenon can lead one to presume that perhaps the two manuscripts in question have been prepared and submitted in a reverse order. For example, the application of the calcium concentrations in Illimani ice in order to produce an ŚexcessŠ sulphate time series is hardly conventional. Hence, it would be very advantageous to include some discussion of the in-transit reaction chemistry (producing a ŚgypsumŠ composition) alluded to in the paper, or indeed have this other work at least quickly/simultaneously put into press. Furthermore, it is not clear in the discussion how (or why) the two highly-unusual events (labelled A and B) alone are utilized as the means for ŚcorrectingŠ the sulphate record to show volcanic signatures. Indeed, the reader is unable to precisely understand how the author has determined relative fractions of calcium species from carbonate or gypsum dust and that produced by reaction with volcanic aerosols, the latter being the critical point of study here.

Tables & Figures:

p. 2455 Ű Table 3 It would help the reader to include an indication of which eruptions from this list are believed to be recorded in Illimani, according to the identification of 16 events in the sulphate record, shown in Fig. 8. Of course, given the rather inclusive list of 20th century eruptions here, in conjunction with the continuous sequence of numbered peaks after 1970, makes differentiation between a true connection and random chance a bit more problematic.

p. 2456 Ű Fig. 1 It would be helpful to also include the Fluoride record in this figure.

p. 2457 Ű Fig. 2 (cf p. 2459 Ű Fig. 4, p. 2460 Ű Fig. 5, and p. 2461 Ű Fig. 6) In Fig. 2, it is apparent that the ECM record has been averaged as in Fig. 1, in intervals of 10 values, whereas in subsequent figures, it varies from original data to what appears to be a mixture of both (Fig. 6). What was the reasoning behind these resolution changes? Also, two are presented on the original depth scale, and the other two on a ŚlinearizedŠ time-scale (with apparent mild rescaling of features with respect to depth). One must then ŚinvertŠ one profile mentally to compare to the others, which is less than ideal. Perhaps best would be to present all on the original depth scale, with the annual-counted chronology indicated along the top of all four diagrams.

p. 2458 Ű Fig. 3 Is it possible to confirm the relationship between the gypsum-derived excess-sulphate and H+ concentration via independent means (such as direct pH measurements, or the titration method of Legrand et al. to eliminate the Ścarbonate problemŠ)? Otherwise, the relatively strong correlation here might be believed to be (at least partially) due to the fact that a large portion of the overall ionic strength of the meltwater is due to sulphate and calcium (Table 1); and not only are these both utilized for determination of exc-sulphate, but of course also major contributors to the H+ determination method used, i.e., by way of simple differencing between total anions and cations.

p. 2462 Ű Fig. 7 No mention of flux calculation (i.e., accumulation reconstruction) is given in the text.

p. 2463 Ű Fig. 8 Relative peak heights (an important matter of discussion on p. 2444) and background levels are difficult to compare visually in this plot, given the large change in resolution due to the nature of layer thinning vs. sampling interval. Better to present a profile that has this effect eliminated, or at least minimized.

These comments were contributed by Margit Schwikowski, Keith Henderson, Anne Palmer and Stephanie Knuesel.

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