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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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General comments

This paper looks at the chemical signals that reach a tropical ice core as a result of volcanic eruptions. In this aspect, it is highly novel, as such studies have, until now, been almost entirely confined to the polar regions. The authors have an 80 year sequence (plus a short section which they date to the Tambora eruption) from a new core they have obtained in the Andes. The main part of the paper consists of a general discussion of the signals that are recorded, followed by a detailed fingerprinting of 3 eruptions: Agung, Pinatubo and Tambora. The final section attempts to derive a volcanic stratigraphy for the entire 80 metre section.

Overall, the paper is an interesting and novel contribution to the literature. It is clearly

written, and contains some undoubtedly high-class analytical chemistry. The interpretation of the individual signals is detailed, and uses knowledge of what could be happening in the air and the snow, as well as the strong seasonality of deposition. In particular, the suggested discrimination of tropospheric and stratospheric eruptions is an important new idea. I am much less happy with the attempt (in section 3.4) to derive a stratigraphy for the whole core, and to label individual signals as particular eruptions. At least as written, this is highly speculative, and other solutions appear equally probable. I would favour a complete, and much less certain, re-writing of this section of the paper. However, the new ideas in the first part of the paper suggest that it should eventually be published in ACP, but only after substantial changes to section 3.4.

Specific comments

Page 2433, line 27. Since correct identification of the volcanoes is so important in this paper, an uncertainty estimate for the dating is essential here.

Page 2434, lines 20-25. The discussion about gypsum, and reactions is mainly correct, but then it seems that only excess sulfate is taken as a proxy for volcanic material. However, as the authors point out, some of the gypsum likely arises because sulphuric acid (from volcanoes as well as other sources) has reacted with calcium rich material in the atmosphere, and then been deposited as gypsum. Therefore surely the volcanic sulfate value during the eruptions could lie somewhere between excess sulfate and total sulfate. Add a discussion of this?

Page 2440, lines 25-35. The observation of a temperature change linked to the eruption would be very significant. However, for this paper, this is highly speculative when we are not shown any of the delta-D data on which it is based, and the delta-temperature relationship in the tropics remains under discussion. I think this section should be removed unless the authors are able to show the delta-D data in this paper.

Page 2441, Agung section. If H2SO4 emissions were 2-4x HF and HCl, then they are of order 3 Gg. For Pinatubo the authors report several Tg ? as they say several

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(probably 3) orders of magnitude less for Agung than Pinatubo. And yet the signal seen in Illimani is only a small factor (2-4) less than Pinatubo. This needs a comment. Does it suggest that the estimates of emissions from Agung are much too low?

Page 2443. The authors seem to treat the anthropogenic cause of the recent sulfate increase only as a last resort ? surely it is the obvious explanation for it. However, they might also like to comment on the unexplained increase in Ca.

Page 2445, lines 1-15, Table 3 and Fig 8. This is the section I found most difficult. Firstly, the authors state that their ?increase ?from the mid 1970s to the mid 1990s? is consistent with Stothers comment that the largest sulfur producing emissions fall in ?1893-1902 and 1982-1991?. But these two statements are not at all similar ? these authors can say nothing about the first period, and they have at least as much activity in the 1970s and 1990s as in the 1980s.

Secondly, it is almost impossible for the reader to make the comparison between the data in Table 3 and that in Figure 8, and they both need to be presented graphically (also the South American volcanoes should be marked in the table to help the reader). But as far as I can tell, the allocation of volcanoes in the table to ones marked on the figure is based on a pre-conceived idea, but is not supported by the data. The authors seem to have listed in lines 11 and 12, only the ones they feel they should see, and ignored even some eruptions of the same volcanoes (for example they list Reventador 1936 (VEI 3), 1973 (VEI 3), and 1976 (not in the table), but ignore the same volcano in 1926, 1929, 1944 and 1960 (all VEI 3)). I think one can only say that there are many eruptions that could contribute to the peaks, that the ones that do will be determined by location and weather patterns, and that at this point it is impossible to assign eruptions to peaks. I would be very uncomfortable with any attempted assignment along the lines in the text. This weakens the whole paper.

The last sentence of the abstract should probably be removed to reflect this.

Technical corrections

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Page 2432, line 24 and many other places: Does the journal have a style for units such as ?Eq l-1 (as written by the authors)? SI gives litre as L, and I would have expected eq rather than Eq.

Page 2439, line 8, remove ?.? After ?Fig. 5?.

Page 2444, line 10: ?late the late? needs correcting

Page 2447, line 31, ?Gggeler?, needs an a-umlaut added

Fig. 5: For the first time here, the authors use a timescale going forwards, which is fine, but it would be good to point that out in the caption, since until now the depth scales have had associated timescales running backwards. In addition this plot really needs a depth scale as well, since the text on page 2439-40 refers mainly to depth. Same applies to Fig. 6.

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