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Climate of the Past Discussions

Interactive comment on "Volcanic synchronisation between the EPICA Dome C and Vostok ice cores (Antarctica) 0–145 kyr BP" *by* F. Parrenin et al.

F. Parrenin et al.

parrenin.ujf@gmail.com

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Original comments in normal font and answer in bold font

This paper presents the results of correlating acidity and sulphate spikes, interpreted to be related to volcanic activity, in Vostok and EDC ice cores in order to try to put the two sets of cores on a common time scale. I think that there is significant merit in this approach, and I think that some good results have been obtained, but I have some overall concerns about inherent problems in the technique that are not addressed in the paper. The acidity/sulfate spikes that are being correlated between multiple cores at single sites, and also between the two sites, do not have any inherent qualities that allow them to be definitively correlated. From what I can tell, they are being correlated just based on where they occur in the cores, their relative heights, and relationships to other acidity spikes. I wouldn't say that this invalidates the technique, but the inherent limitations and possible errors should be clearly addressed in the paper, as well as the logic behind making correlations where they were made.

Please see our updated sections 2.2.3 and 4. in the new manuscript. But this complex problem of creating an automatic and objective synchronisation method is clearly beyond the scope of the current manuscript. Note that such a mathematical method has (to our knowledge) never been applied for synchronising ice cores from different sites, while several such volcanic synchronisations have been published.

I also think that the part of the paper discussing the location of the Toba super eruption should be removed. I think that the argument behind where the authors think that the Toba peak is located are weak, and may only serve to introduce confusion into the literature.

We disagree with N. Dunbar here. The approximative location of the Toba in Antarctic ice cores is based on the seesaw hypothesis, an hypothesis which is largely admitted in the community. We find it useful for scientists looking for the Toba chemical signature to decrease the number of candidates in Antarctic ice to only 3. Moreover, we can tell you that our study already stimulated several discussions and projects on the Toba so we see it more as stimulating than as confusing.

The paper could use a bit more attention to detail. The tables aren't well formatted, and the figures, while clear and legible, have some axes and line labels that haven't been translated from French to English (figures 6, 7, and 8).

Figures have been corrected.

Detailed comments on a few parts of the paper are below, keyed to comment numbers in the PDF document. Minor editorial comments are noted in the PDF as well.

Comment 1 (line 226) The authors state here that 104 prominent peaks, corresponding to volcanic events, were correlated. Given that none of these peaks would have had any “fingerprint” that would allow them to be definitively correlated, I think that the authors need to address what criteria they used to make the correlations. Distance between adjacent peaks, peak heights (which appear to be quite variable between the two records), or what? What kind of errors might be inherent in this type of correlation? If 3 different individuals went through the record picking peaks independently, how close would their picks be? The authors mention later in the paper that correlations between the Vostok and EDC volcanic records were done by two individuals. How close were their picks? If this technique is going to accepted by the community as having some quantitative value (which the authors seems to be suggesting that it does), the analytical error associated with the method needs to be address with more rigor that is done here.

See answer above

Comment 2 (line 255, Figure 6). The authors state that Figure 6 shows an offset of 3 meters between the 3G and 5G isotopic data. I am completely unconvinced of this looking at the figure. Neither the regular 3G date or the offset 3G data appear to fit the 5G data particularly well. This 5G data either needs to be smoothed and replotted in such a way that the correlation or lack thereof is more obvious to the reader.

We eventually decided to remove the new 5G isotopic data from the manuscript. They will be presented, together with ongoing measurements, in a future study.

Comment 3 (line 315, Figure 8) In figure 8, the 500 year phase lag between the lowres Vostok and EDC deuterium data is not apparent to me. Furthermore, why would there be an offset for the low-res Vostok data and not the high resolution? This really needs to be explained if the data is going to be presented.

See previous answer.

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Comment 4 (line 347). Age difference between VK-FGT1 model age and EDC3 age. I really had a hard time following this discussion. Either remove, or clarify.

We tried to clarify this section.

Comment 5 (line 353, Toba) The authors are unable to find an acidity spike that corresponds to the Toba super-eruption in the interval of ice where they would expect it to be. Making various assumptions, they speculate that Toba may be represented by one of three acidity spikes that occur between Antarctic Isotope Maximums 19 and 20. They note, however, that these spikes do not really stand out in the record. I would consider this correlation to be much too weak to be defensible, and recommend removing all discussion of Toba from the paper.

See answer above.

Please also note the supplement to this comment: http://www.clim-past-discuss.net/7/C2640/2012/cpd-7-C2640-2012-supplement.pdf

We thank N. Dunbar for her careful review of this manuscript.

Interactive comment on Clim. Past Discuss., 7, 4105, 2011.