

We thank the reviewers for insightful and constructive comments. Below our point-to-point responses to the reviewer comments (in bold):

Anonymous Referee #1

Received and published: 7 December 2012

This paper uses ice core and other data to address one of the most intriguing environmental events in recent earth history, the Toba supereruption. The authors synchronize Antarctic and Greenland ice cores with decadal precision in order to search for Toba and examine interhemispheric climate coupling. The authors are expert ice core geochronologists, and ice cores are certainly a vital resource for understanding both Pleistocene climate change and volcanism. The scientific quality, significance and presentation are all excellent. My main complaint is about the interpretations and conclusions of Svensson et al. In my view, their conclusions about the climatic significance of Toba are strained.

Several of the authors of this paper were also coauthors on a recently published paper highlighting some of the perils of assessing volcanic events using ice cores, Coulter et al., 2012. These include:

- 1) the erratic spatial distribution of volcanic products, both acid and tephra, on ice sheets
- 2) that multiple eruptions can contribute to a given acid signal, confounding estimation of stratospheric loading
- 3) that many volcanic eruptions, particularly the most powerful ones, may be underrepresented in ice cores and detectable only through tephra.

Based on the available evidence, then, the authors would seem to have a predilection for dismissing the long-term climatic importance of Toba, and volcanism in general. This seems far-fetched, and some of their conclusions are surely overstated:

"...the Toba eruption did not initiate a long term cold period."

In this context, 'long-term' refers to periods longer than a century as the ice core water isotopes do not allow us to discuss periods much shorter than that. Based on the bipolar temperature-proxy ice-core records (Fig. 9) there is no evidence to support a long-term global cooling caused by the Toba eruption. This is the main conclusion resulting from the bipolar linking. Without the bipolar linking, there would be evidence in Greenland to support Toba as a potential candidate for initiating the cooling associated with GS-20, but when we have Antarctica lined up on the same timescale, it is evident that there is no long-term global cooling initiated at this point. This conclusion is based solely on the bipolar linking and is independent of whether we have actually located Toba in the ice cores or not. There simply is no period of long-term global cooling occurring around the time of the Toba eruption. As suggested by reviewer #2, we now emphasize the importance of this statement by including it in the abstract.

"...the initiation of all of the other DO-events was independent of major volcanic events."

We agree that this statement would need support by data from other DO events. We, therefore, modify the statement to '... the initiation of other stadials and interstadials are thought not to be related to large volcanic eruptions.'

Really? The authors explain that even after decades of trying, we are still unable to find the largest volcanic eruption of the last 2 million years in an ice core. Yet they have basically concluded that we can now put to rest the long-term impacts of Toba, and that there were no other consequential volcanic events in the last 100,000 years. I'm not sure what the urgent need is to close the books on

prolonged volcanic cooling, but could we leave open the question of the role of explosive volcanic events in millennial climate change, again, based on the available evidence?

As stated above, we are quite confident about the long-term influence of Toba. What happens on shorter time scales is a different story.

One point on methodology. These records have been synchronized by experts, and the reader is left to stare at figures to try and judge for herself whether the matches the authors have chosen are good ones. But is it possible to assign some sort of objective figure of merit, a statistical significance, to these choices?

The bipolar matching was done by trial and error without use of statistical methods, and we do not have an objective assessment of the synchronization. What distinguishes the present synchronization from most other volcanic matching of ice cores is the layer counting of the records that allows for pattern matching of bipolar events. From both Holocene and glacial records we know that we can only make very limited assumptions about the bipolar volcanic signatures in ice cores. We cannot assume that a volcanic eruption has similar strength in both hemispheres or even at two different sites in Greenland or in Antarctica. We can also not assume that the signatures of different volcanic proxies are comparable at one site, and we can also not assume that a bipolar event shows up in all records at all sites. Therefore, it is difficult to set up criteria for an objective evaluation method. The most robust criteria for identifying bipolar events seems to be the temporal separation of events that we take advantage of in the bipolar pattern matching. Shifting the northern and southern records in time with respect to each other is fairly easily done by eye, since the ice core layer thicknesses are quite constant over the investigated period (Figure 7).

An interesting aspect of this study is the variability of the sulfate signals, which the authors have uncovered, even among relatively nearby sites. There is a persistent view that ice core sulfate levels are a reliable metric for eruption intensity. This analysis clearly calls this assumption into question, I'm pleased to see this point developed in the paper and I suggest it be further emphasized.

We have added the text below to the manuscript. Please note, however, that in this work we're not attempting to quantify the magnitude of the Toba eruption(s) based on ice core records.

'The inter-ice-core and inter-proxy variability in the magnitude of the identified Toba-related tie points is surprisingly high. In particular, the lack of coherence between GRIP and GISP2 at the T1 and T2 events is quite significant considering the sites are only located some 30 km apart. The strong variability suggests a substantial element of local variation in the magnitude of the volcanic signal during this period.

In Greenland, it is generally more challenging to use volcanic proxies, such as sulfate and conductivity, to identify volcanic events during cold climatic periods, such as glacial stadials. During cold periods, the snow accumulation is low, the flux of dust and calcium sulfate (gypsum) to the ice increases dramatically, which, in turn, increase the sulfate and conductivity background levels (see Fig. 3). The occurrence of the T1 and T2 events right in the transition zone from the mild GI-20 to the cold GS-20 makes the peak heights very sensitive to the amount of dust in the ice that can vary significantly from site to site on the annual and seasonal scale. This may explain part of the important difference between T1 and T2 at GRIP and GISP2. The issues described here do not apply to the warm and climatically stable Holocene, where the magnitude of volcanic eruptions is likely to carry a more quantitative signal. In general, however, it is recommended to compile as many ice core records as possible in quantitative studies following the approach of (Gao et al., 2008).'

References:

Ambrose, Late Pleistocene human population bottlenecks, volcanic winter, and differentiation of modern humans, *J. Hum. Evol.* 34(6) (1998).
Coulter et al., Holocene tephtras highlight complexity of volcanic signals in Greenland ice cores, *JGR* 117 (2012).

Anonymous Referee #2

Received and published: 21 December 2012

Although I am generally not a fan of papers that correlate volcanic events in ice cores only based on the acidity or sulfate records, because of the ad-hoc nature of some of the correlations, I am very impressed with the careful and thoughtful work that these authors present. I am convinced by the bipolar correlations presented, and believe that the conclusions that they draw are based on defensible analysis of the ice core data. The paper is well written and well illustrated.

Despite my overall very positive impression of the paper, I think that there are a few points that could be addressed more rigorously. Comments below keyed to “callouts” in the annotated PDF of the MS. Some other comments are also noted in the MS.

Comment #1. The annual layer counting appears to be very challenging, and the correlations shown in Fig. 1 are only moderately convincing. But, given how the whole analysis ties together, I am willing to believe that the ties are valid. However, could the authors offer explanation for the big discrepancy in the number of years between Be-1 and L1? Also, an explanation of how the analytical uncertainties for the layer counting age difference between the volcanic ties in NGRIP and EDML need to be explained (unless I missed this in the text). Errors on layer counted ranges between tie lines are presented in Figure 2, but are absent from Table 1.

We have added the following text: ‘The larger discrepancy in number of years between match points 10Be-1 and L1 between the two cores can be readily attributed to the 30-50 years uncertainty of the North-South 10Be linking (Raisbeck et al., 2007)’

The analytical uncertainties are introduced in section 3: ‘We date the Laschamp section of the EDML ice core by layer counting in the CFA and VS datasets following the same principles as applied for NGRIP. We identify ‘certain’ and ‘uncertain’ annual layers that are counted as 1.0 ± 0.0 and 0.5 ± 0.5 years, respectively. The accumulated uncertainty of the uncertain annual layers provides the maximum counting error estimate of the dating following the approach outlined in Andersen et al. (2006).’

Later in section 4 we now state: ‘For layer counting in the potential Toba region we apply the same counting technique and the same NGRIP and EDML datasets that were utilized for the Laschamp matching.’

Table 1 has been completed.

Comment #2. Are there several Toba eruptions? The authors suggest that their analysis of the cores may indicate that the enormous 74kyr Toba eruption may have consisted of multiple events. However, after suggesting this, they do not really delve into the question in much depth. I would suggest reading and citing more of the Toba field literature to try to find more support (or lack of support) for this idea, and provide a more in-depth discussion in the paper.

We have added the following discussion:

‘Recently, Sigl et al. (2012) identified some 50 bipolar volcanoes over the last two millennia. It is, therefore, not a surprise to find several bipolar events in a similar time span at around 74 ka. The surprising thing is that none of the events stand out as an exceptional event, considering the magnitude of the Toba eruption. In the literature, the 74 ka Toba eruption is generally regarded as a single event (Westgate et al., 1998; Zielinski, 2000; Chesner, 2012). If there is just one Toba eruption, however, then the ice core records suggest the existence of several other large, unknown, low-latitude eruptions occurring in the same time window.

Another well-known mega-eruption, the Huckleberry Ridge Tuff (HRF) ‘Yellowstone’ eruption occurring some 2.1 million years ago was recently determined to consist of three distinct eruptions separated by some thousands of years (Ellis et al., 2012). In this case, however, the individual eruption events could also be distinguished by other means, such isotopic fingerprinting of the lava flows. In the case of Toba, the YTT composition is much more homogeneous and the potential temporal separation of individual events is hundreds rather than thousands of years. In contrast to the Yellowstone case, it is therefore not possible to decompose several potential Toba eruptions based on precise dating alone. Several closely-spaced large Toba eruptions could, however, help to explain why none of the identified ice-core events are as strong as would be expected from current geological evidence.’

Comment #3. The information about the variability in the magnitude of the volcanic signal (acidity and sulfate spikes) between the Greenland cores and the Antarctic cores is interesting. I think that it would be worth having a more detailed analysis of this in the paper as well as some discussion about the possible origin of these differences. Although this may be hard to assess with only 3 Antarctic cores, is there any geographic control on the magnitude of deposition of same age spikes from core to core? Or is the variability completely decoupled from geography? I suspect that the reason for the variability may be a function of wind reworking of the snow that carried the chemical signal of the eruption, causing concentration of the signal (leading to a higher spike) in some places and reduction of the signal in others. I would think that this would result in some randomness in the magnitude of the signal on a very local scale).

We have added some text concerning the variability in magnitude of volcanic signals within Greenland (see comment to reviewer #1). Concerning a geographical control of the signal magnitude within Antarctica, we find that the variability among different proxies at the same site is simply too large. For example, there is an absence of sulphate at T4 in EDC although ECM shows a clear peak. Likewise, there is almost no ECM signal for T1 and T3 in DF-1 whereas they are associated with significant ECM spikes in the nearby DF-2. This clearly demonstrates that local effects are very important, and we, therefore, hesitate to speculate about the causes of long-distance variability in the present dataset. One way to push this approach forward would

be to obtain comparable sulphate and conductivity records from more Antarctic sites, such as Vostok, Talos Dome, and Dome Fuji, but such records are currently not available.

Replies to additional comments in the annotated PDF of the MS:

EDML sulphate is now shown in Figure 2, but, unfortunately, the EDC sulphate record is currently not available for Figure 3. EDC sulphate is shown in a shorter interval in Figure 7.