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Climate of the Past Discussions

Interactive comment on "Bipolar volcanic synchronization of abrupt climate change in Greenland and Antarctic ice cores during the last glacial period" *by* Anders Svensson et al.

Anonymous Referee #2

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This paper is addressing a really great concept: that of synchronising Greenland and Antarctic climate records precisely through volcanic signals. The concept is bold: until recently most of us would have considered this too hard to attempt. And it is used to do an important task of refining the relationship between hemispheres across bipolar seesaw events. While the alignment done here may be improved in the future, this is likely the best that can be done for now, and it opens up a number of very interesting possibilities around global synchronisation, understanding firn dynamics, and addressing variability between D-O events. The paper is written very clearly, and although I have a rather large concern I need to raise, it was a pleasure to read. Because my concern is quite significant (but I believe addressable) I will tick the "major revisions"

box, but this does not mean I think the paper is generally flawed in any way – on the contrary it's great, but I think it lacks one major caveat that readers need to be more aware of.

It is very challenging to safely match up volcanic records from distant locations, as those of us who have been involved in comparisons across Antarctica know. This is even more the case between hemispheres because there will be numerous additional volcanic peaks in Greenland (less so in Antarctica) that do not have a bipolar signal. As the authors explain, the secret is to get a pattern of several peaks with an identical spacing. An advantage the authors have is that the methane matching already done allows them to home in on the right section with a century or so. The authors aim to achieve the pattern match by using layer counting between volcanic peaks in two cores: NGRIP and EDML. We all know this can be done at NGRIP, as it was the basis for GICC05; while it has its issues, over short intervals the uncertainties should be quite small. However, it is a huge leap to accept that it can be done at EDML, and I find it very strange that this is glossed over, and even more so that we are not shown any examples.

The only example I am aware of where layer counting has been attempted at EDML was in Sommer et al (2002, not referenced in this paper), where layers were counted for the top 2000 years. With only about 7 cm we/yr, the example given in Sommer et al makes it clear this is tricky (and required matches to known dated volcanic peaks for verification, something that would be circular in this case), but the authors nonetheless claimed an accuracy of around 3%. But now in this paper we enter the much harder realm of doing the same thing in the last glacial: where some of the key records used by Sommer are not available and where the snow accumulation rate is as low as 3 cm we/year (range 3-5 in the sections used).

The authors justify their ability to count at EDML by saying (line 142) "for the investigated time interval the annual layer thicknesses are comparable to those of NGRIP (Veres et al., 2013) and layer counting can be done in a similar way". However this

misses the point. Whether annual layers can be distinguished and counted relies on two different factors. One is whether the analysis method is well enough resolved to give several samples per year in layers that may be (in this case) only 1.5 cm thick – this is actually quite dubious (Sommer gives the true EDML resolution as 0.7 cm, implying 2 samples/year for chemistry) and it would be nice to see examples to understand this. However more important is whether annual layers were ever present, and our experience at Dome C and Dome Fuji would suggest that, at somewhere with 3 cm we accumulation, they are not (or at least not reliably), with a certainty of missing some years due to redistribution (sastrugi) that occurs at scales greater than the approximately 8 cm scale of the snow depth deposited each year.

I would have expected to see a number of strategies to overcome this:

a) Knowing the estimated accumulation rate at EDML (which is embedded in the AICC2012 age model), one could estimate the distance between volcanic peaks without counting;

b) WAIS Divide is actually counted to 31 ka. Why was this not used at least to GI5? Intrinsically the chances of counting layers below that are still better at WD than at EDML because the accumulation rate at WD was higher so signals were at least formed and may be decipherable with higher resolution analysis (which could in theory be done).

c) If the authors really think they can count layers in EDML glacial ice then they should show us some extended examples, and explain how counting is possible at a site with such low accumulation rate. Personally I suspect this cannot be done at any level better than just using the average accumulation rate, and that probably the counters are kidding themselves that small oscillations represent decipherable years. However I am willing to be convinced if the authors provide examples at different sections that they have used.

I do not see this as fatal to the paper. Strategy (a) (checked by strategy (b) until GI5)

would likely yield a reasonable result, but the authors need to be clear about what is possible. As things stand the reader who is not familiar with EDML would imagine some rather routine piece of layer counting, and it is therefore essential to explain that it is far from routine and indeed would, if successful, represent a breakthrough most of us would consider could not be achieved with any useful accuracy.

Apart from this, I have only very minor comments:

Line 21: “The last glacial period is characterized by a number of abrupt climate events that have been identified in both Greenland and Antarctic ice cores”. This is a bit imprecise as they are abrupt in Greenland and in d_ln in Antarctica but not in Antarctic climate. How about “The last glacial period is characterized by a number of millennial climate events that have been identified in both Greenland and Antarctic ice cores, and that are abrupt in Greenland climate”.

Line 23 and elsewhere “Hemispheres” should be lower case.

Line 146: for clarity it would be helpful to spell out that published AICC2012 ages are in bp (1950), and so b2k ages will be 50 years greater than those in AICC2012.

It’s not really my concern as a reviewer but it seems a little strange that the acknowledgment calls out all the participants in NEEM (which is not the prime Greenland core used here) but not NGRIP or EPICA.

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