Replies to Reviewer #2

Raisbeck et al. present new high-resolution 10Be data (NGRIP, EDML & Vostok ice cores) that they use to synchronise ice core records from Greenland and Antarctica over the period of the Laschamp geomagnetic field minimum. They discuss the precision of this synchronisation and the phasing of water-isotope variations around D/O 10. In addition, they investigate cyclicities in the 10Be records and make an assessment of the ice age - gas age difference around this period.

I think this is a very interesting paper that shows new important 10Be records. I certainly recommend the publication of the manuscript in Climate of the Past after the authors addressed several important remarks that I have listed below. I also think that the publication of the paper should be linked to the requirement of publishing the 10Bedata as a supplementary table so that the analyses can be repeated with different methods.

Yes, the 10Be data will be submitted as supplementary table.

Comments:

Regarding the flux calculation there are three important comments that need to be addressed: 1: On Page 3, line 10 the authors say that the NGRIP accumulation rate is based on the ss09sea time scale. However, figure caption 1 says that the accumulation rate is based on the respective time scales (GICC05 for NGRIP). So there is a contradiction that needs to be sorted out. I think GICC05 should be used as time scale and as the basis for the accumulation rate.

The reviewer is right. In fact, the flux shown as a histogram for NGRIP, and later smoothed, is indeed based on the GICC05 layer counting, corrected to surface accumulation using the ss06 thinning model. The reference to ss09sea is a "leftover" from a much earlier version of the paper. This will be clarified in the revised version.

2: I do not understand that the flux calculations are based on a smoothed version of the accumulation rates (see page 3, line 8). This can introduce jumps in the 10Be flux at transitions (e.g. 10Be concentrations react immediately while a smoothed accumulation record follows slowly leading to artificial jumps in the fluxes). The flux calculation needs to be done on the raw data. Smoothing can then be done afterwards on the flux record.

Whether flux should be calculated with raw or smoothed accumulation rates depends on how accurate the estimated accumulation rates are, and how rapidly they vary compared to the 10Be production. If they are exactly accurate, the reviewer is right that the raw accumulation values should be used. If they have large uncertainties compared to the production variations, use of raw values can in fact lead to artificial variations in flux. In practice, since we later smooth the flux, the choice is not critical for the present application, as discussed below. For NGRIP, where it is observed that temperatures and accumulation can vary in a decade or less, we have, as indicated above, indeed used the raw 11 cm layer counted values, even though we believe they introduce unrealistic variations. This is because the 11 cm samples in our record can represent as little as 4 years. Thus a missed or added layer will give a 25% error in accumulation, which thus accounts for a significant fraction of the high frequency variation in the histogram of Fig 2. As a test, we have thus tried smoothing the accumulation, but found that this does not lead to any significant difference in the smoothed flux shown in Fig 2. For EDC and

EDML, accumulation rates are based on models using correlation between accumulation and water isotopes. As can be seen in Fig 1, there is considerable high frequency variation in these water isotope values, which again we believe are not representative of actual accumulation variations. In addition to analytical error, they are probably due to such things as uneven seasonal variations, mixing of surface snow etc. We have thus decided to use the bag accumulation rates given in the references cited, which represents an effective smoothing of ~50 years. Once again we have tested different smoothing procedures, and found no significant difference in the smoothed fluxes. For Vostok, where we do not have water bag values for water isotopes, but do have delta D values for the same 10 cm samples used for 10Be, we use these and a correlation between delta D and accumulation obtained in other Vostok cores.

3: It is not a priori clear that the 10Be flux indeed reflects the 10Be production rate only. It could have imprints of climate change (e.g. during D/O transitions). Therefore, the authors should discuss if the peaks they used for the synchronisation are robust, i.e. independent of flux calculation (also visible in the 10Be concentrations).

While we believe there is considerable evidence that flux is a better measure of 10Be (and other trace species) deposition in polar cores, at the suggestion of the reviewer we have repeated the Match synchronizations of EDC and EDML with NGRIP using concentrations. We found that the peaks and valley corresponding to the tie points shown in Fig 2 did not vary by more than 20 years compared to those found using fluxes.

In summary we believe our 10Be synchronization is quite robust to the choice of raw or smoothed accumulations, or even the use of concentration.

I do not want to question the synchronisation results. However, they seem to critically depend on the choice of the fix points. As visible in figure 2 there might be alternative choices for fix points due to the cyclic behaviour of the 10Be data. I would like that the authors discuss in more detail how they have chosen the fix points, how robust these choices are and if it could be possible that other choices could be done. If there are alternative choices it could influence the subsequent discussions.

The reviewer is correct that synchronization using AnalySeries requires choosing tie points, which are necessarily subjective. This is why, as described in the text, we chose also to use where possible the MATCH protocol, which does not require choosing tie points.

As an example, I would like that the authors to discuss the 3 youngest peaks in the Vostok data. The youngest peak is clearly not there. Could it be possible to shift the whole record 200 years younger which might lead to a better agreement of the peaks in the younger part of the record. This could be discussed in connection with the MATCH routine i.e. what are the reasons to exclude such scenarios. The authors mention the missing data in the Vostok record. However, for the periods of disagreement missing data does not seem to be the major problem.

As discussed in the paper, the Vostok core could be synchronized only with AnalySeries (the MATCH protocol is more sensitive to gaps in the records being synchronized). Thus, other choices than the one shown could be imagined. Interestingly enough, at one point we actually tried shifting with AnalySeries the younger 3 peaks mentioned by the reviewer. However, we found that this implied a large and rapid change in accumulation

rate, which was not coincident with any stable isotope variation, and did not seem to us to be reasonable. Thus, while we cannot definitely exclude such a choice, we believe that it is less probable.

I think the errors for the synchronisation are not well defined.

If I understand correctly they are based on the agreement of the 2 synchronisation methods. However, outliers can shift peaks and this could lead to biases in all synchronisation methods i.e. leading to a false sense of confirmation. I recommend that the author treat synchronisation errors with caution. An indication for systematic problems might be the systematicoffset of 10Be-synchronised time scales and the identification of common volcanicinduced spikes.

It is never discussed how the synchronisation errors should be understood. Does an error of <20 year correspond to a 1 sigma error or a 2 sigma error?

No the errors are not based on the agreement between the two synchronization methods. This estimate is a compromise between (1) the standard deviation (4+/-3 years) between EDC and EDML based on the independent 10Be synchronization with NGRIP, compared to the direct synchronization of Severi et al. (2007) as shown in the revised Fig of the reply to Christo Buizert, and (2) that (27+/-7 years) observed between the observed position of the presumed bipolar volcanic peaks L2 and L3 of Svensson et al. (2013) in EDC and EDML compared to their predicted position using the 10Be synchronization. As such it really does not have any 1 or 2 sigma meaning.

I think the spectral analysis is interesting but it shows that the results of Fourier analysis (especially significance levels) depend very sensitively on noise estimates (it is interesting that the noise levels are very different in the different records leading to the different results on the significance of cycles).

We agree that the form of the noise levels, on which the significance levels depend, are very different for NGRIP compared to the Antarctic cores, possibly because of higher sample resolution.

The wavelet analysis and the agreementof the different records indicate that there are common cycles irrespective of the significance analyses (otherwise the synchronisation would not work. . .).

As discussed in the paper, we too were surprised at this apparent paradox, but can only note that the Antarctic cores seem to have longer period variations which distort the spectra.

I think the authors should not attribute possible uncertainties to meteoric influences on 10Be records from Antarctica only (page 5 line 11), or do the authors want to imply that the Greenland data does not contain any meteorological influences?

No, but they may be smaller because of a higher accumulation rate, or different deposition mechanism (higher fraction of wet deposition?). We will specify this.

I have question regarding the approach for the methane synchronisation and delta age calculation. The authors synchronise the whole section (figure 5) but not single peaks. However, looking at methane from NGRIP one gets the impression that the data in the youngest period is placed rather too young. Can this explain the delta age difference between the 10Be and other methods shown in figure 6.

As the reviewer correctly notes, the methane peaks were matched over their whole profile using the Match protocol, which does not involve any subjective choices. Whether this accounts for the difference with the other methods is not obvious, but it is clear from Fig 5 that the relatively poor resolution of the methane data, particularly for EDML is the limiting factor in calculating delta age. Thus, although the present 10Be results have improved the ice synchronization by almost an order of magnitude, this has made only a modest improvement on the delta age uncertainty.

I appreciate figure 7 since I think it shows the data in a honest way. It also shows that in my opinion, the results from the applied statistical tools needs to be treated with caution. For example, EDC shows a similar early isotope spike as WDC and therefore it appears to me that the lead/lag discussion is very much influenced by the noise in the data and the applied statistical tools. These uncertainties could be emphasised even more.

We agree. We in fact wanted to emphasize that, looking at AIM-10 as synchronized with 10Be, and using the same statistical procedure as WAIS-Divide Project Members (2015), individual records give conflicting results.

Regarding figure 8 and the discussion on longer time scales. I understand that this discussion fits into the discussion following figure 7. However, to me it feels like a step backwards. The authors have this great 10Be data and the synchronisation and then they go back to the results from an older synchronisation. Maybe figure 8 would fit better as figure 1 in connection to the introduction of the general topic.

We accept the reviewer's suggestion to move Fig 8 earlier in the paper, which will of course require some modifications in the text.

It is not 100% clear to me on what the errors in table 2 are based on. Uncertainties in the time scale synchronisation and/or in the method to find the transition points?

The uncertainties are in the difference in the inflection points with respect to warming in Greenland, found using different parameters for BREAKFIT.

Details:

I think the introduction is quite short but OK. I was also wondering why the authors list the results in the introduction.
We wanted to give the reader enough information to decide whether it justifies his or her continuing reading (the "newspaper" rule).

Page 2 line 19: Did the Vostok samples really weigh >500g? It seems like quite a lot even considering that the measurements were done a while ago.
Yes. In fact the sampling was done more than 20 years ago.

page 7 line 12: I would say "10Be-synchronised climate records"
Will do

In general, I think the paper is very well written!
The comment is appreciated.