

## ***Interactive comment on “An improved North-South synchronization of ice core records around the 41 K beryllium 10 peak” by G. M. Raisbeck et al.***

**Anonymous Referee #2**

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Raisbeck et al. present new high-resolution  $^{10}\text{Be}$  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  $^{10}\text{Be}$  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  $^{10}\text{Be}$  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  $^{10}\text{Be}$

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data as a supplementary table so that the analyses can be repeated with different methods.

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. 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  $^{10}\text{Be}$  flux at transitions (e.g.  $^{10}\text{Be}$  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. 3: It is not a priori clear that the  $^{10}\text{Be}$  flux indeed reflects the  $^{10}\text{Be}$  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  $^{10}\text{Be}$  concentrations).

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  $^{10}\text{Be}$  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. As an example, I would like that the authors 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

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the Vostok record. However, for the periods of disagreement missing data does not seem to be the major problem.

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 systematic offset of  $^{10}\text{Be}$ -synchronised time scales and the identification of common volcanic-induced 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?

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

I think the authors should not attribute possible uncertainties to meteoric influences on  $^{10}\text{Be}$  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?

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  $^{10}\text{Be}$  and other methods shown in figure 6.

I appreciate figure 7 since I think it shows the data in a honest way. It also shows that,

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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.

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  $^{10}\text{Be}$  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.

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?

Details:

I think the introduction is quite short but OK. I was also wondering why the authors list the results in the introduction.

Page 2 line 19: Did the Vostok samples really weigh  $>500\text{g}$ ? It seems like quite a lot even considering that the measurements were done a while ago.

page 7 line 12: I would say " $^{10}\text{Be}$ -synchronised climate records"

In general, I think the paper is very well written!

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