Review of Raisbeck et al. by Christo Buizert

Raisbeck et al. present new high-resolution ^{10}Be records for several ice cores, which allows for the most precise interpolar ice core synchronization to date during MIS 3. The authors use this synchronization to (1) estimate the spectral properties of ^{10}Be variations, (2) investigate the Δage/Δdepth evolution in Antarctic ice cores, and (3) investigate the phasing of the bipolar seesaw. All three problems are very relevant to the ice core and paleoclimate communities, making this paper a valuable contribution. Overall the paper is clear and well written.

Even though I disagree with the authors' interpretation of the bipolar seesaw phasing (see below), I fully recommend this work for publication in Climate of the Past. Disagreement on the interpretation of data is a normal and healthy part of science, and it does not detract from the main contribution of this work (which is the high-precision ^{10}Be synchronization).

Comments:

(1) My main concern is the author's interpretation of the bipolar seesaw phasing (section 6). In my experience, it is not meaningful to investigate the phasing of a single AIM event in a single core, because the climatic seesaw signal is overwhelmed by high-frequency δ^{18}O variability due to weather, deposition and other local events. The authors clearly demonstrate this point for AIM 10. However, once the signal from several AIM events is averaged, the shared climatic seesaw signal is clearly revealed. This averaging strategy was used in WAIS Divide project members (2015) – I will refer that paper as WDPM15.

To demonstrate this principle, I averaged the ^{10}Be-synchronized AIM 10 event in the WD, EDML and EDC δ^{18}O time series (see figure R1 below). I synched the EDML record myself using Table 1, and Grant Raisbeck kindly provided the synched high-res EDC record. I did not have access to the VK record. The resulting average is plotted at the bottom of the figure (orange), on top of the WAIS Divide AIM3-18 stack from WDPM15 (purple). It is clear that the multi-core AIM 10 average agrees well with the WDPM15 stack, and shows a clear cooling trend some ~200 years after the abrupt DO 10 warming event. The AIM 10 stack is more noisy than the WAIS stack simply because it averages over fewer events.

Landais et al. (2015) and other papers have shown clearly that AIM events are expressed differently at various sites, and I do not dispute that. However, it is also clear that whenever several AIM events are averaged to improve the signal-to-noise ratio, the ~200 year time scale shows up (it is also visible in many individual AIM events). This timescale must tell us something about the climatic coupling between the hemispheres. The peak δ^{18}O value for a given AIM event at an individual core site can of course be different from this 200 year lag, due to local high-frequency weather and deposition effects. Interpreting only the position of the δ^{18}O -maximum is therefore too simplistic, in my view. At EDML the AIM events seem to have a flat top, as opposed to the more triangular shape at e.g. Byrd, WAIS, EDC and TALDICE. However, adding EDML to the multi-core averaging does not seem to alter the fact that there is substantial (Antarctic-wide?!) cooling 200 years after abrupt NH warming (my Fig. R1 below).

I request that in a revised MS the authors include a multi-core AIM 10 average (i.e. average of WDC, EDC, EDML and VK) in Fig. 7, and discuss some of the points I made above. I do not think our interpretations are mutually exclusive. On average, there is substantial Antarctic cooling 200 years after NH warming (my preferred interpretation), yet the $\delta^{18}O$ isotopic maximum during a single AIM event in a single core can differ substantially from this 200 year delay due to local effects (the authors' interpretation). I trust that the authors are willing to present both interpretations side by side. I think this will make the bipolar seesaw phasing less confusing to readers (who may be familiar with WDPM15), and make the paper overall more robust.

(2) Section 2 describes the new NGRIP, EDML and VK records in detail, but not EDC. Please add a few lines describing the EDC ^{10}Be record also.

(3) In section 3 the authors test the accuracy of their synchronization. The ^{10}Be also links the EDML and EDC cores, which can be directly compared to the volcanic synchronization of Severi et al 2007. This would provide a true test of the uncertainty in the ^{10}Be synchronization, given that volcanic matching is the gold standard of synchronization. I tried to do this (see Fig. R2), and found a small, but constant offset between the synchronizations which is ~ 70 cm on EDC / ~110 cm on EDML. (I took the Be ties from Table 1, and the volcanic links (on EDC99) from the AICC2012 documentation).

Do you have any idea where this offset could come from? I would urge the authors to double check for trivial mistakes such as converting bag numbers to depths, or similar. Or is this the offset between the EDC96 and EDC99 cores? I could not find any information on which EDC core was used. In either case, the direct comparison to Severi et al. (2007) provides a great opportunity to test the precision of the ^{10}Be ties. It may be worth including this comparison as a third panel to Fig. 3.

(4) In section 5 I am a confused by the different trends in the Δage and Δdepth. In my mind the two are exchangeable, as you can calculate one from the other using the ice chronology. For example, how is it possible that the EDC Δage for AICC2012 and Scen4 (red and black) are identical, while their Δdepth is so different? Doesn't that imply that these two chronologies have completely different annual layer thickness (while both are synched via ^{10}Be)?

The authors could provide a few more details on how the Δage and Δdepth are constructed, which may help in understanding what's going on. For example, which chronology is used for the 2 Loulergue scenarios? I would assume the authors ran the densification models using the AICC2012 T, Acc and chronology for consistency?

(5) In section 6 (P7 L25-27) the authors use the BREAKFIT routine and a MATLAB routine to estimate the breakpoint in the data. I don't think these routines are particularly fit for the problem at hand, given that the time series are short and very noisy. The data range must be picked to isolate AIM 10, and then the routines require the user to specify a range where they believe the breakpoint is located. Because it is short and noisy, these subjective choices seem to matter a lot for AIM 10. For example, I tried the fitting routine for just AIM 10 at WAIS (where I did this before), and got a timing of -10 or +205 years depending on whether I used linear or 2nd order fitting. I don't mean to suggest that the authors applied

the code incorrectly, I simply want to highlight that for this particular problem the outcome is very sensitive to the subjective choices of the operator. The authors may have had the same experience.

For longer time series with less noise the routines perform well, and become independent of the subjective choices of the user.

The isotopic maxima that the authors identify in Fig. 7 can also be picked out by eye, so I suggest the authors just remove the fitting routines from the paper (my preference) or provide more details on how the fitting routines were applied (data range, etc) and how the uncertainties were estimated.

Minor/language:

Throughout the text: I would suggest replacing “delta age” and “delta depth” with Δage and Δdepth (i.e. using Greek Delta symbol) to confirm with common usage in the ice core literature

Throughout: WAIS Divide is spelled without a hyphen between “WAIS” and “Divide”

Title: “41 k” should probably be changed to “41 kyr/ka”. Also, please include a hyphen in “beryllium-10”.

P1 L9: are these 2sigma uncertainty values?

P1 L 20: Remove “our”. The author lists of Raisbeck et al. 2007 and Raisbeck et al. 2016 are not identical.

P1 L 21: ... estimates of the DEPTH difference between ...

P1 L25: In a previous study, Raisbeck et al. (2007) have .... (same reason as above)

Section 3: Please specify confidence intervals for the uncertainty estimates. Are these 2 sigma?

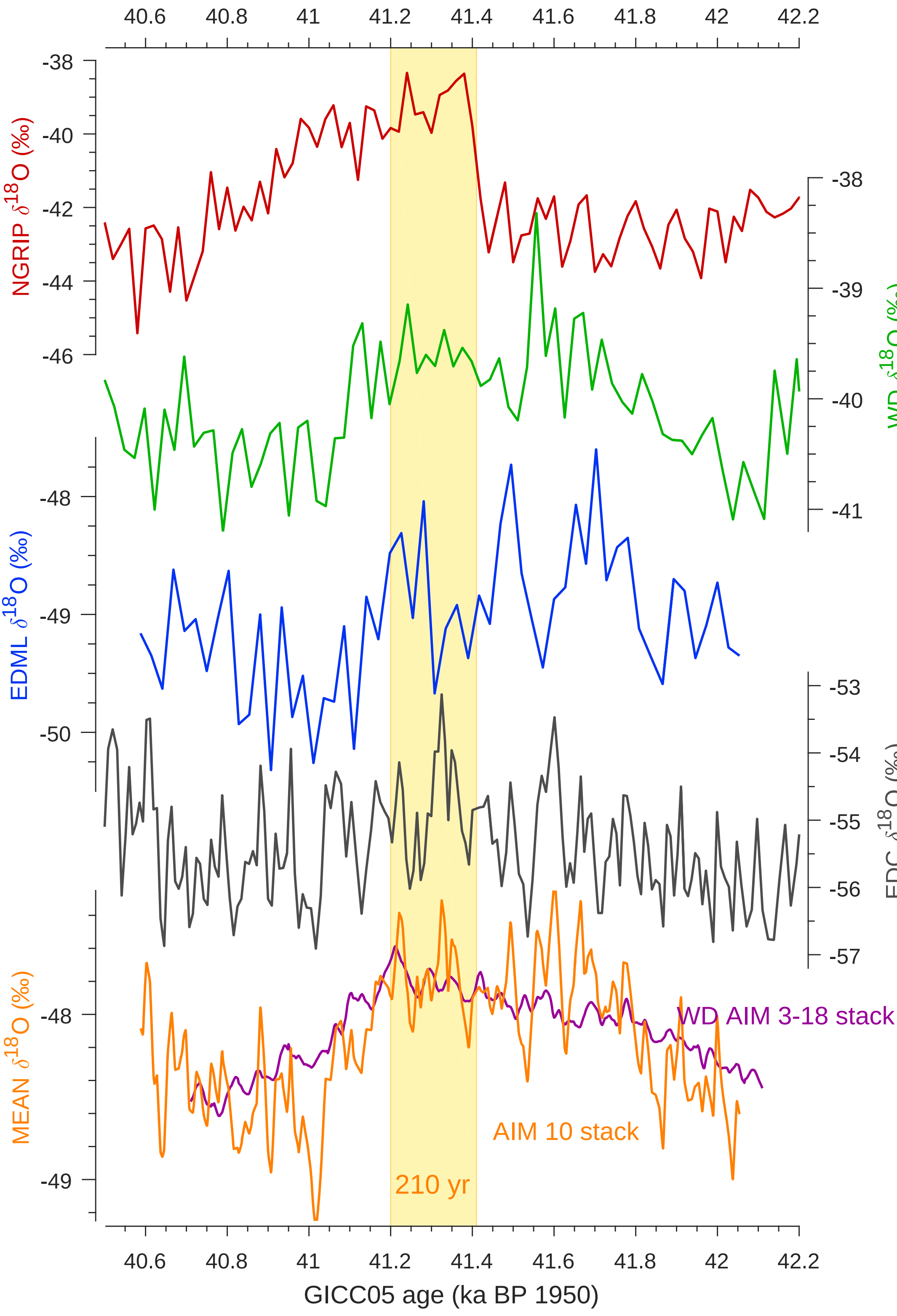
Section 3: Maybe note that the precision on the WAIS Divide CH_4 interpolar synchronization at 40ka is estimated to be +/- 73 years (2 sigma uncertainty in Δage; see Buizert et al. 2015 Fig. 3e), and therefore the new ^{10}Be synchro is more precise.

P5 L12-14: Do you mean meteorology interferes with the actual atmospheric ^{10}Be production rate, or do you mean it leads to differences in deposition, transport and dilution? Because of the annual layer count, Greenland Acc can be reconstructed much more accurately that Antarctic Acc – could this be one of the reasons the 200yr peak is better resolved at NGRIP?

P7 L21-22: I think it would be good to cite e.g. Blunier and Brook 2001 here, who were among the first to describe the asynchronous coupling clearly.

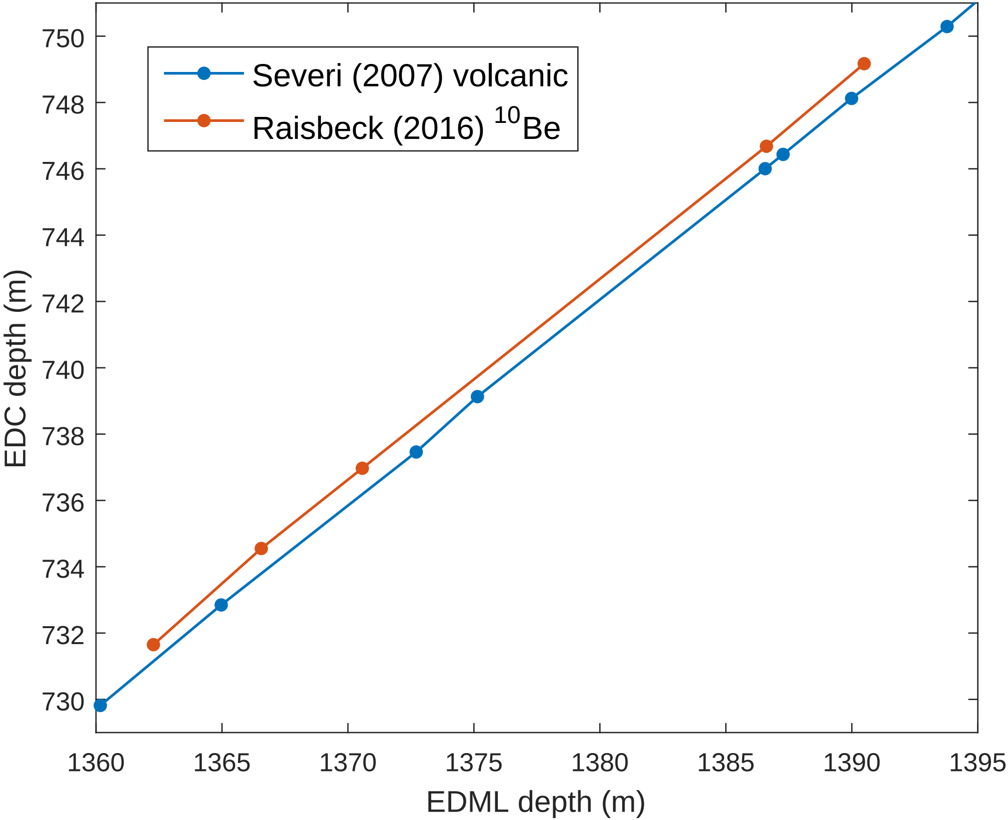
Figs 7 and 8, caption: please specify how the WD2014 chronology was transferred to GICC05. I assume you divided by 1.0063 and then added 50 years to get from BP1950 to B2k?

Figure R1



40.6
40.8
41
41.2
41.4
41.6
41.8
42
42.2
-38
-40
-42
-44
-46
NGRIP δ18O (‰)
-38
-39
-40
-41
WD δ18O (‰)
-48
-49
-50
EDML δ18O (‰)
-53
-54
-55
-56
-57
EDC δ18O (‰)
-48
-49
MEAN δ18O (‰)
WD AIM 3-18 stack
AIM 10 stack
210 yr
GICC05 age (ka BP 1950)

Figure R2


Severi (2007) volcanic
Raisbeck (2016) ^{10}Be
EDC depth (m)
EDML depth (m)
750
748
746
744
742
740
738
736
734
732
730
1360
1365
1370
1375
1380
1385
1390
1395