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Climate of the Past
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Interactive Comment

Interactive comment on "Phase relationships between orbital forcing and the composition of air trapped in Antarctic ice cores" *by* L. Bazin et al.

Anonymous Referee #2

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Bazin et al. present new d18Oatm and dO2/N2 data from the EPICA Dome C ice core, which were measured on ice samples stored and transported at -50oC to prevent gas loss. This procedural step is particularly important for the integrity of the O2/N2 signal, and to a lesser extent the d18Oatm signal. These new data have the potential to improve orbitally-tuned ice core chronologies by providing additional age constraints. In this manuscript the authors aim to better understand the phasing relationship of d18Oatm and dO2/N2 relative to orbital variations. The authors focus on two specific cases:

The first case is a comparison of the dO2/N2 minimum around 137 ka between the Vostok, Dome F and Dome C ice cores. The authors argue for a ~2 ka delay of the O2/N2 minimum at EDC relative to Vostok/Dome F. This conclusion seems untenable

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in the face of the scatter inherent to O2/N2 data, a data resolution of around 2-3 ka, and the obvious chronological errors exposed by the misalignment of the water isotopes. The discussion of different confounding influences on the O2/N2-orbital relationship remains inconclusive.

The second case is an analysis of the timing of d18Oatm relative to O2/N2 in the Vostok and EDC cores. No details are provided on the analytical methods of establishing the lag, nor on the uncertainty in the result. Based on wiggle-matching the authors argue that the lag of d18Oatm behind O2/N2 (or behind insolation, this is unclear) increases as a result of Heinrich events, but this is not obvious to me. Also, no dynamical pathway is provided.

Unfortunately, the overall result is that after reading a relatively long and dense paper, the reader is not much wiser as to what controls dO2/N2 and d18Oatm on these timescales, or how robust the timing relations are that the authors derive. The new data presented by Bazin et al. are obviously of great value. However, for this paper to be acceptable, I believe the analyses will need to be done in a more robust way that incorporates realistic uncertainty estimates.

Comments:

Please label subpanels (A,B,C etc) in the graphs. When referring to "Fig. 4" the reader is not sure which of the $\sim$20 curves to look at.

Page 1445/Fig. 2: Isn't the AICC2012 EDC chronology in this time interval largely based on the assumption that d18Oatm follows insolation? In that case discussing the power spectrum is not meaningful, given that the orbital frequencies are included by design. Has the d18Oatm data been corrected for mean ocean d18O? Ocean d18O has a lot of power in the 100ka band.

Page 1446/Fig 2: The 100ka signal in the O2/N2 spectrum is a very nice observation. Wouldn't this argue for an influence of climate on O2/N2, for example through

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accumulation, dust or temperature?

Section 3.1 / Fig3: As I mentioned earlier, the conclusion that the EDC O_2/N_2 minimum lags by 2ka is not tenable. This analysis is done by assigning a single datapoint as the minumum, which is probably the least reliable way to do so for noisy, low-resolution records such as these. A more reliable way to assess the timing may be to perform a cross-correlation between the records, or apply filtering to the records. Any analysis regarding the timing of the O_2/N_2 minimum should at the very least consider the following:

- There is quite a lot of scatter inherent to O_2/N_2 data, both in your record and the VK/DF data. Note that this is no reflection on the quality of your data, but just a general problem with O_2/N_2 data. The scatter is clearly much larger than the pooled SD of replicate analyses. Due to the low resolution it is not clear whether this variability represents noise or a real ice core signal.

- data resolution; I don't think you can identify a 2ka lag in a record with 2.4ka average resolution.

- uncertainty in ice age and Delta-age; this is clearly larger than 2ka, considering the alignment of the water isotopes.

- to avoid circular reasoning the chronologies must be completely free of O_2/N_2 age constraints.

I am afraid that much longer and/or higher resolution dO_2/N_2 records are needed to address this question satisfactorily. At the very least the authors should provide a realistic uncertainty estimate on the phasing – my sense is that this uncertainty will be much larger than 2ka.

Page 1449- 1451: The discussion of confounding influences on the link between O_2/N_2 and insolation is important. Personally I think the observation of power in the 100ka band is a stronger motivation than the putative 2ka lag. For all four lines of argument the authors don't provide a clear mechanistic link to O2/N2 fractionation in the deep firn. Bender (2002), Severinghaus and Battle (2006) and Fujita (2009) provide such frameworks for understanding O2/N2 fractionation in relation to firn processes, and these mechanisms could be briefly addressed.

Page 1449: I don't see why a 2 week lag of maximum temperature behind maximum insolation would influence the orbital phasing. Isn't this delay mostly due to thermal inertia? I think one could reasonably argue that summer temperature scales with summer insolation, regardless of such a small time delay.

Page 1450-1451: regarding the accumulation, do you investigate the relationship by comparing O2/N2 and Acc on an ice age chronology? Recently Takuro Kobashi argued that accumulation can influence O2/N2 via overburden pressure (doi: 10.5194/acpd-15-15711-2015), in which case you'd have to look at the accumulation during a period after deposition – the duration of this period would be roughly equal to the time of burial (i.e. Delta-age), which is different at each site.

Section 3.2: Also here the authors should provide much more detail on their methods, and assess the robustness of their result in a meaningful way. How were the records filtered? How did you determine the lag? - this is not explained at all. The elephant in the room is of course the Delta-age (which is not meaningfully investigated), but data scatter and resolution probably influence this result also.

Please define clearly what you mean by the d18Oatm – O2/N2 phasing. Are you (1) determining the relative phasing of maxima/minima in both records directly, or (2) are you evaluating the phasing of d18Oatm relative to orbital forcing? If (1): why would you expect these to be in-phase in the first place, given that one is a local, and the other a global signal? If (2): What orbital forcing do you expect d18Oatm to follow? Throughout the paper it seems that the authors expect a direct link with precession, but why not use 30oN insolation, for example.

P1453, L18-20: why is there no O2/N2 signal in bubbly ice from the last 100ka? In the

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melt-refreeze you should get all the gas, right?

P1454, L18-22: You claim that O2/N2 is synchronous with local insolation, but in Fig 3 you just argued it is not. How does the O2/n2 lag influence your result?

P1455: I am puzzled by the choice of IRD record. Why not simply use an IRD record, rather than the Ca/Sr records. The authors argue that Heinrich events must show up in BOTH records to be truly a Heinrich event. By this definition they miss many commonly recognized Heinrich events, such as e.g. H11 around termination 2, which is a very prominent event in most records, but not visible in core U1308.

The link between H-events in core U1308 and the d18Oatm-O2/N2 delay seems completely arbitrary to me. The authors pick two maxima in the delay (marked by arrows), and argue that these coincide with increased Heinrich activity. Consider the following:

- Similar increases in the d18Oatm delay are observed around 150ka, 350 ka, 530 ka, without much increased Heinrich activity.

- The prominent U1308 events around 240ka and 625ka occur during times of a small d18Oatm delay

- In both cases the d18Oatm delay starts to increase several ka BEFORE the H-events take place, making it dubious that the latter are the cause of the phasing delay.

The authors also do not provide any mechanistic understanding to underpin their proposed Heinrich mechanism. Severinghaus (2009) show unambiguously that H-events strengthen the Dole effect, but the current study does not provide any additional insight.

P1456, L10: "The phase identified over T1 and T2 may not apply for earlier transitions without Heinrich events". However, according to your own preferred IRD record (Ca/Sr from U1308) there was no H-event during T2....

The authors could elaborate on the potential of their data for refining ice core chronologies. Also, what are the possibilities for linking d18Oatm data to absolutely dated

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speleothem records?

Typos, etc:

P1438, L16: we evidence –> we find

P1439 L11 this trapping process occurs –> air is effectively sealed in (trapping occurs over a range of depths)

P1440, L4: themselves –> which are primarily

P1440, L18-19: the millennial –> millennial-scale

P1440 L29: ratio O_2/N_2 –> O_2/N_2 ratio

P1441, L3: is "effusion" the right word here?

P1442, L1: it is therefore expected –> it has been suggested

P1442, around L20: cite the work of Ikeda-Fukazawa on the effect of sample storage

P1442, L26: remove: "correction"

P1444, first paragraph: is there no refreeze procedure here?

P1444, L8: remove "in-"

P1445, L7: results –> data

P1448, L25: "glacial inception" –> do you mean glacial inception in water isotopes?

P1449, L2 "the" orbital target

P1449, L12: "the" snow metamorphism

P1452, L11: excelent –> excellent

P1453, L6: what do you mean by "re-interpolated"??

P1453, L9-10: between 15ka and 100ka: do you mean to say you used a bandpass filter with 15-100kyr setting, or the data between 15-100ka before present? Technically you defined ka to mean thousands of years before present (introduction).

P1456L7: 1-6ka: it seems from your graph it even goes to positive values, so more like -1 to 6 ka.

P1456, L23: should motivates –> motivates

P1457 L3: have evidenced –> have shown

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