

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

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Received and published: 28 May 2015

I greatly appreciate the use of these new hard-won data to assess in detail the possible phasings and uncertainties associated with the use of gas-phase orbital tuning in ice cores. I do not intend to review the whole paper in this comment but wanted to comment on section 3.1 and Figure 3, where the authors compare the records over MIS5 at three different sites.

As the paper is written, the authors seem to assume that Dome F (on DFO-2006) and the other two sites (on AICC2012) have coherent age scales. On that basis, they assert that the O₂/N₂ maximum is later at EDC than at DF. However, even in their plots it is already obvious that this is not the case, as $\delta^{18}\text{O}$ -air should be synchronous at the three sites, but in fact is displaced by more than 3 ka across MIS5E between DF and

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the other sites. This shows that the gas age scales are not correctly synchronised, but of course this could just be due to issues in calculating delta-age. The fact that $\delta^{18}\text{O}$ -ice also shows a very different timing at DF compared to $\delta^{18}\text{O}$ -air at the other sites obviously rings alarm bells about the ice age scales as well.

In fact this question is answered in a paper also in discussion in CPD (Fujita et al, *Clim. Past Discuss.*, 11, 407-445, 2015), in which the ice age scales between EDC and DF are synchronised using volcanic markers. There it is shown (Fig 4) that the DF ice age model is systematically older than AICC2012 by 1-4 ka over MIS 5E. Fujita et al do discuss the implications for the placement of the age markers, but in any case it makes no sense to discuss the O₂/N₂ phasing between DF and EDC without first having made this correction. Looking at the arrows in Figure 3 of the present paper, I suppose such a realignment will bring the O₂/N₂ minima at DF and EDC to almost the same age (within the resolution of the data). How that alignment relates to precession can still of course not be concluded without knowing which age model is in error.

This is not to say that there are no problems with using O₂/N₂ as a precise alignment marker - indeed it is obvious from Figure 3 that aligning such records, which have considerable noise on top of the orbital signal, precisely is very difficult, and the processes the authors discuss should certainly influence the age of the minimum in ways that are more complex than a reliance on a single orbital calculation would allow. However, it is too early to assign such causes as long as the age models are misaligned, and the authors will need to realign their data to a single age model (either using the Fujita et al data, or if they prefer to use only pre-existing data, using the assumption that $\delta^{18}\text{O}$ -air must be simultaneous along with an assumption about delta-age) before redrafting section 3.1.

Interactive comment on *Clim. Past Discuss.*, 11, 1437, 2015.

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