

Interactive comment on “Towards orbital dating of the EPICA Dome C ice core using $\delta O_2/N_2$ ” by A. Landais et al.

Anonymous Referee #3

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This paper presents the first O_2/N_2 record from EDC core for the period 300–800 kyr. It is important to measure O_2/N_2 in this time range to investigate the feasibility of using it as a dating tool, and the result shows some promising variations in O_2/N_2 that are similar to those of local summer insolation. However, there are serious deficits in the data and its analyses, as well as the lack of discussion on controlling mechanisms (and therefore phasing of O_2/N_2 with local insolation) based on all previous studies. These shortcomings not only make the evaluation of the EDC data difficult towards future orbital dating of this ice core, but also give readers wrong impression that O_2/N_2 is uncertain dating tool in general (including other ice cores and different age ranges for which accuracy is indeed well validated), as I already see in other reviewers' comments.

In fact, I find that this manuscript is almost exactly the same as what I reviewed for

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another journal of similarly high quality. I don't see any improvements in the version that I read here at CPD. The manuscript failed to address many of the previous comments, including the requirements to perform and present sound evaluation of analytical tools and data, and to correctly cite and discuss earlier works. Ignoring them and submitting the same manuscript to another journal leave me both ethical and scientific concerns.

The data has value to be published but the manuscript should not be accepted in its current form. Only way forward seems to greatly reduce the paper to make it a more simple presentation of the preliminary, raw data, and focus the discussion on data quality and gas loss fractionation. Most of text in chapters 4 and 5 should be deleted. Mean phase between the EDC O_2/N_2 on EDC3 timescale and the local Dec. 21 insolation could be presented, but it should be done with a simple tool such as Blackman-Tukey cross spectral analysis. Even then, the authors cannot evaluate the phase with confidence because, in many of the precessional cycles in the O_2/N_2 data, there are only around 5 points to consist each cycle, posing phase uncertainty due to undersampling. The poor data quality and resolution over most period do not provide validity to the results of time-varying phase analyses using sophisticated tools. The Dec.–Mar. insolation should be deleted because it produces confusion. There is absolutely no physical arguments to support such a period over 3 months of rapidly decreasing seasonal insolation from maximum to almost zero. The fact that the EDC3 timescale already has ± 6 kyr uncertainty, together with the quality and resolution of O_2/N_2 data in this study, makes it inappropriate to discuss the origin of the mean 2-kyr phase in the context of the choice of target insolation curve. Simply speaking, the authors went way too far in overinterpreting their preliminary, low resolution data.

I attach an abbreviated reviews of mine for the previous (but not all) versions of the manuscript for another journal as a PDF attachment. Note that there are contexts in this document which requires 'authors responses' to fully understand, but I refrain from including them. Also note that I do not expect the authors to evaluate the filter anymore (they failed a few times already). Instead they should remove it from the manuscript.

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Other problems: Because EDC3 itself is constrained by orbital tuning of d18O_{atm} to precession with assumed lag of about 5000 years for the period of interest, there is no point in discussing the phase between the two on the EDC3 timescale (section 5.2). Variable phase of d18O_{atm} with respect to orbital forcing was reported by Kawamura et al. (2007) for younger period based on an independent and better timescale, which was also missing in the discussion. I suggest deleting this section because it adds nothing new.

The authors' attitude to keep omitting citation of Fujita et al. (2009) in the discussion of the phase between O₂/N₂ and insolation is unacceptable.

Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/7/C1554/2011/cpd-7-C1554-2011-supplement.pdf>

Interactive comment on Clim. Past Discuss., 7, 2217, 2011.