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Climate of the Past Discussions

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 #1

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Overview This paper presents new δO2/N2 and δ18Oatm measurements from the Dome C ice core. The ice was kept at very cold temperatures to avoid gas loss. Phase relationships with orbital parameters were investigated, confirming considerable uncertainty of these gases as dating tools. The phase relationship between δO2/N2 and δ18Oatm was also investigated with speculation that Heinrich events affect the magnitude of the lag of d18O relative to dO2/N2.

This paper has the potential to be a good discussion of the uncertainty associated with the use of ice-core gas measurements for orbital tuning. The new δO2/N2 and δ18Oatm measurements are a valuable contribution and provide sufficient resolution to assess multi-millennia phase relationships. Unfortunately, the writing and organization of the paper need considerable improvement to justify publication. The confusion

surrounding the timescales of Dome Fuji and Dome C during MIS5 is highlighted by Dr. Wolff's comment; the authors' brief appendix does not sufficiently improve this section.

Overall, this paper has useful new data and the potential to contribute to ice-core dating. However, the current form of the manuscript needs substantial improvement prior to publication.

Specific scientific issues:

Uncertainty, filtering and lags – Overall, statistical quantification is lacking in the paper. As discussed more below, the timing of the MIS5 minima, on which the site-specific differences in dN2/O2 rest, is not defined objectively. Later, there is virtually no description of the filtering, other than the statement "by wavelet transform". There is no reference given. Looking at Figure 4, it seems like the wavelet filtering may be shifting the timing of minima and maxima. In addition, there is no description of how the lag values have been determined. Is it by cross-correlation of the filtered data? How is the measurement noise included? How is the timescale (gas vs. ice) uncertainty incorporated? What time windows are the lags being determined for? If there are multiple estimates of lags during a period (say between 550-650 ka) how do the estimates differ within a period? Can the measurements resolve a $\sim$1 ka lag when the average sampling resolution is 1.1 ka for δ18Oatm and 2.35 ka for δO2/N2?

MIS 5 The different timescales for Dome Fuji and Dome C present a major difficulty in the presented comparison, as pointed out by Dr. Wolff. As discussed above, the authors' appendix is not sufficient to address the confusion in this section. The appendix shows two figures with either Dome Fuji aligned to Dome C or vice versa. However, this appears to align the warming of TII and not the glacial inception. While I understand the authors wanted to choose unambiguous markers in the isotopes, I think correlating the full d18Oice curves for the ice timescale and d18Oatm for the gas timescales would be much more useful.

Regardless of the timescale issues, the minima seem very difficult to determine accu-

rately. Dome Fuji has what looks to be its lowest value nearly 4 ka after the chosen value (Figure 3 arrows). Also, the sample spacing appears to be a couple thousand of years, so can anything really be said about the relative timing of the minima? I would like to see a statistical analysis used to define the minima and its uncertainty. My guess is that the uncertainty in the timing of the minima would be greater than the difference between sites. From what's presented, I feel like any discrepancies in the dN2/O2 relationship are most likely due solely to noisy data series.

The discussion of three possible explanations for site-dependent differences seems tangential. Or at least it came as no surprise that none of the three things investigated yielded better results. Since there are no physical models relating grain metamorphism at the surface to dO2/N2 values trapped thousands of years later at bubble close off, is investigating the timing of maximum temperature with 3-years of data set really even worth attempting? I much rather read a discussion of how the gas trapping at depth many thousands of years after the snow was deposited on the surface affects the expected relationship between dO2/N2 and insolation.

Orbital Tuning Uncertainties The final sentence of section 3.1 recommends an uncertainty of 3-4 ka for O2/N2. This recommendation seems to come from nowhere and is not quantified earlier in the section. Is this number (or range) from just the MIS 5 comparison at $\sim$135 ka? The value of the uncertainty needs to be supported.

Heinrich Events – invoking the presence or absence of Heinrich events in explaining the δ18Oatm lag seems unnecessarily speculative. What is the mechanism for Heinrich events affecting δ18Oatm? I think the argument the authors are making is that Heinrich events are markers of large fresh water input into the North Atlantic which suppress warming in the Northern Hemisphere. This keeps the ITCZ and southern hemisphere wind belt farther south, leading to small monsoons and less tropical vegetation. These combined effects delay the change in d18Oatm, leading to larger lags behind insolation. Regardless of what the mechanism is, it needs to be fully and succinctly stated. It is also worth noting that the Heinrich events aren't a causal part of

this system – it is the fresh water input into the North Atlantic that is critical (unless the authors are further arguing for the placement of freshwater into the North Atlantic being critical, in which case they are getting even farther afield from the focus of this paper).

General Language Reading this paper was quite frustrating due to the imprecise writing. One of the most common problems is ambiguous subjects. Many sentences begin with "This" or "Such processes" and follow complicated sentences such that the reader does not know what part of the previous sentence is being referred to. One example from the conclusion: "This should motivates(sic) further study to unveil the processes at play both for long term trends and at glacial-interglacial/eccentricity timescales." I don't know what "This" refers to. Is it "spectral analysis", the subject of the previous sentence. Is it the "peak in the periodicity band"? Is it the "the processes other than local insolation"? I also don't know what further study "This" would motivate. Do you mean firnification? Measurements of better conserved ice samples? Hydrology changes during glacial-interglacial cycles?

I have copied an annotated copy of my comments since trying to put them in digital form quickly became both confusing and time-consuming. I have asked the editor to pass this along privately. I hope the comments will help the authors identify points of confusion and frustration for a reader.

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