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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "Synchronization of ice core records via atmospheric gases" *by* T. Blunier et al.

T. Blunier et al.

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1) More specific comments on the paper include the observation that the major remaining source of uncertainty appears to come from the estimate of the EPICA DML gas age-ice age difference. Perhaps this uncertainty could be reduced if the authors pursued high-precision and high-resolution d15N of N2 measurements in the core, which might reveal signals of local temperature change in the gas phase as has been done in Greenland. Clearly, this will be much more difficult in Antarctica than in Greenland due to the much smaller magnitudes of temperature change. However, it seems to me that it is not hopeless. If successful, the d15N signals could be directly compared with the methane variations, thus comparing two gases from the same core with each other. This would eliminate the gas age-ice age difference model calculation uncertainty at least at some discrete points. The authors probably would need to have a d15N mea-

surement every 50 years or less, with a precision of 0.002 per mil or better. This is difficult but not impossible given current laboratory methods.

We absolutely agree that the δ15N approach would help reducing the uncertainty in Δage. We made a rough calculation of the effect on δ15N from thermal diffusion in the firn. The small effect of about 0.02 per mil will further be masked by e.g. the gravitational effect. Unfortunately the analytical uncertainty of δ15N measurements is presently 0.02 per mil (e.g. Huber et al., 2006). Measurements with the required precision are thus not possible at this time.

2) Further to this line of reasoning, the authors make a strong statement that the models work, and emphasize that conditions in the past at DML were always within the range of modern calibration sites. However, the authors did not mention the fact that the models have some error when predicting Dage at modern sites that have been removed temporarily from the "training set" used in calibration. Also, even when modern sites are not removed from the training set, there is some error. It would be helpful if the authors mentioned the magnitude of this error, in years. The physical basis for this error may be that density variations within the firn column on a scale of tens of cm or so cause substantial variations in the effective bulk density at which gases become occluded, due to the existence of sealed impermeable horizontal high-density layers in firn that has a bulk density lower than the expected (model) bubble close-off density. Some sites have these layers, while others do not. The cause of these layers may be seasonal solar-driven sublimation, wind-packing, depth hoar, radiation crusts, or other complicated processes. This so-called nondiffusive zone or lock-in zone can easily change the Dage by several hundred years. Therefore the models have neglected an important physical variable, and the complexity of this variable makes it unlikely that it will be successfully incorporated in the models anytime soon. More to the point, there is no reason to believe that these layers are present in all climate regimes. They could be present today at DML, but absent during the last glacial, for example, leading to a Dage estimate for the last glacial that is too small by several hundred years.

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So far all firn densification models work with homogeneous snow. We agree that in the future more complex firn structures will have to be incorporated in the models, especially for low accumulation sites. However, at this time our knowledge about these processes is limited and more studies on firn structure are necessary before the models can be modified. A extended discussion on firn densification goes beyond the scope of this manuscript. A recent review can be found under: Landais et al. (2006), Firn-air δ15N in modern polar sites and glacial-interglacial ice: a model-data mismatch during glacial periods in Antarctica? Quat. Sci. Rev., 25, 49-62. We added a short discussion of the known discrepancy and added a reference to the above mentioned paper.

3) The authors have one very important check on the model Dage, which is the Be-10 spike at 41 ka, which provides an independent estimate of Dage at one point. But this occurs at a time that is substantially warmer than the last glacial maximum, and therefore a change in the degree of firn layering between 20 ka and 41 ka cannot be ruled out. If the authors could produce even one tie point during the last glacial maximum using d15N-based Dage, it would greatly strengthen the paper. There is a small methane variation at 24 ka, due to Interstadial 2, and there is a corresponding Antarctic Isotope Maximum (AIM) just before it. Perhaps a d15N signal of AIM 2 could be found in EDML.

See our response to point 1 above.

Minor comments: The authors say that the d18Oatm decreases from 28 ka to 15 ka. I think this should be “increases” - it just happens to be plotted upside down.

Corrected

The English needs some work. The paper is readable but sentence structure is often awkward and punctuation needs improvement.

We did our best to improve our use of the English language.

The mention in the introductory paragraph about O2/N2 is potentially confusing, since *the meat of the paper has nothing to do with this, and indeed, the gas age-ice age difference does matter for the paper. So the statement that the O2/N2 method avoids use of gas age-ice age differences is potentially confusing to the reader. Perhaps this sentence could be simplified, so it is more in the format of background information about previous dating methods. In particular, the statement that the O2/N2 method avoids gas age-ice age differences could be cut.*

We deleted the introductory paragraph on efforts to absolute dating of ice core.

Interactive comment on Clim. Past Discuss., 3, 365, 2007.

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