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Climate of the Past Discussions

Interactive comment on "An optimized multi-proxy, multi-site Antarctic ice and gas orbital chronology (AICC2012): 120–800 ka" *by* L. Bazin et al.

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This manuscript presents an improved method and result of simultaneous dating of multiple ice cores, using age markers in each core and stratigraphic markers between cores. Establishing accurate and consistent chronologies for different cores is highly important for paleoclimatic studies, and the authors succeeded at least in the latter aspect (consistent chronology). The theme of the manuscript is well suited for the scope of CP and this special issue, and the method is explained in detail, which is welcome. With this method, the key factors to determine the accuracy of chronology seem to be the accuracies of error estimates in initial guesses (background scenarios) and age markers. In this regard, I find issues in their choice of orbital age markers and uncertainties, as well as the lack of investigation of the resulting chronology with respect to other published chronologies. I list a few below.

They arbitrarily gave 4 ka as the uncertainty associated with O2/N2 age markers for both Vostok and EDC cores. As the reasoning for this, they only refer to Landais et al. (2012) who only showed the limitation of EDC O2/N2 record (300-800 ka) due to poor data quality and/or possibly different target curve than the local summer solstice insolation at EDC. There is no evidence to justify the (approximately) doubling of Vostok O2/N2 age marker errors (down to ∼400 ka). Regarding the Vostok record, its O2/N2 chronology is very close to the O2/N2 chronology of Dome Fuji (Kawamura et al., 2007; Suwa and Bender, 2008; difference is within 1 ka), strongly suggesting small uncertainty associated with those age markers. The Vostok and Dome Fuji chronology can also be compared with Chinese speleothem records for terminations (Cheng et al., 2009; Barker et al., 2011), and the differences are within 2 ka. The subjective increase of O2/N2 marker error might be one reason for the rather consistent chronologies between those based purely on O2/N2, air content or d18Oatm (Fig. 4).

The authors increased the d18Oatm data resolution for selected periods and derive age markers around MIS 11. However, the d18Oatm record in this time interval has no similarity to precession curve. More generally, d18Oatm has variable lags relative to precession as evidenced by recent papers (e.g. Kawamura et al., 2007; Cheng et al., 2009), and it also has 100 ka periodicity. Precession influences the d18Oatm through climatic and environmental changes. This manuscript states that d18Oatm and O2/N2 to be within a same category as the tools of orbital tuning (P.5966) and different from climatic records like methane, but it is simply not true. As discussed later in the manuscript, d18Oatm is heavily influenced by climate and should be categorized in the same group as methane and other climatic records. Air content is intermediate between d18Oatm and O2/N2, because it is influenced by local insolation but also by climate (pressure, temperature). The current manuscript might give readers a wrong impression that all three records are equal as dating tools.

The resulting chronology AICC2012 is not compared with other chronologies than EDC3. For 400-800 ka, there is no other choice so it is fine. However, for the younger

part, detailed discussion on accuracy of AICC2012 is limited to MIS5.5 despite the existence of other published chronologies. They estimate the uncertainty of AICC2012 to be small: less than 2 ka for the last glacial and around 2.5 ka for the previous two glacial cycles (MIS 6-9), which are excellent if true. But the error for AICC2012 might be underestimated. For example the AICC2012 uncertainty around MIS 5.3 is estimated to be $\sim$1.5 ka (read by eyes from Fig. 6) but Veres et al. give the possibility that AICC2012 may be off by 2 ka (by comparison with U-Th speleothem age). It is stated in the text that interglacial duration is not very much altered in AICC2012 from EDC3. But if the age around MIS 5.3 (D/O 23-25) is off by 2 ka and MIS 5.5 is accurate, the duration from MIS 5.5 to 5.3 is in error by 2 ka which is about 10 % of the duration (not small at all). What can be said from this is the agreement between AICC2012 and EDC3 does not help evaluating the estimated uncertainty of AICC2012. Other published chronologies should be compared with AICC2012 and discussed in terms of uncertainty of AICC2012, with appropriate graphs (as it was done for comparing EDC3 with other chronologies, Fig 2-5 of Parrenin et al., 2007): Vostok (and Dome Fuji) O2/N2 chronology, and EDC correlated with U-Th speleothem chronology assuming bipolar seesaw (Barker et al., 2011, a few authors of Bazin et al. also authored Barker et al. paper).

With these major issues I cannot evaluate the current manuscript adequately. I think that the manuscript should eventually be published (this is the aim of this special issue), but before that, important information and discussion are missing in this current manuscript. Then I'm afraid, that the chronology may also not become really useful for wide paleoclimatic community.

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