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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.

Anonymous Referee #3

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Bazin et al. have developed a common timescale for 4 Antarctica cores that updates the statistical approach (DatIce) described by Lemieux-Dudon et al. (2010). They use previous measurements and some new data of 3 main types, d18Oatm, dN2/O2, and air content tuned to orbital parameters, as well as a few absolute age markers. The statistical framework for dating multiple ice cores is a methodological improvement which may lead to a better quantification the timescale uncertainties. The authors have compiled a wide variety of ice dating information and developed a coherent way of integrating that information.

The authors have a difficult challenge of presenting such a complicated method concisely and overall do a good job. I do, however, have a number of questions about the uncertainty values that are reported. I am not an expert at Bayesian statistics which

probably contributes to my confusion. I think this paper would greatly benefit from a discussion of how the uncertainty is determined using a few example time periods. My questions will focus on the bottom part of the EDC timescale. I have chosen this time period because only the EDC record extends this far back and therefore there are no age markers from other cores that could be influencing the timescale or uncertainties. For ages 720 to 780 ka, EDC is dated by d18Oatm with uncertainties of 6 ka. The Bruhnes-Matuyama reversal is also included but has an uncertainty of 10 ka. The dO2/N2 measurements were excluded because this is a period of low eccentricity and the ages conflict with the Bruhnes-Matuyama reversal.

My first question is how the uncertainty is less than 4 ka throughout this entire period even though all of the age markers have a 6 ka uncertainty? Are the uncertainties of each of the d18Oatm age markers being treated as independent from each other? If so, is this a valid assumption? I wonder if the uncertainties might be systematic - if the age marker at 749 ka is too young by 6 ka because the assumed orbital relationship is off, might the 758 ka point be similarly 6 ka too young? On a different note, I also wonder what effect the background scenario has on determining the uncertainty. I'm unclear on what background scenario was used – whether flow-modeling only or corrected with d18Oatm measurements (Parrenin et al., 2007a,b; Dreyfus et al., 2007). But given the large adjustments to the thinning function between those two possible background scenarios, it seems like the background scenario is quite uncertain.

My second question is why the gas-age uncertainty is larger than the ice-age uncertainty during this period. I believe d18Oatm is a gas-age marker (unlike dO2/N2 which is an ice age marker even though it is preserved in the gas). Shouldn't the ice-age uncertainty be equal to the gas-age uncertainty plus an uncertainty for the delta-age?

My third question is if the dO2/N2 markers with age uncertainties of 6ka need to be excluded, why is the total uncertainty not increased? Isn't this an indication that the uncertainties of the age markers are not properly characterized? The authors suggest that this is because of the low eccentricity, but doesn't this at least add uncertainty. I am also curious why the uncertainties between 720 and 790 ka are smaller than between 360 and 400 ka when there are other age markers in both the EDC and Vostok cores. Shouldn't the greater density of measurements lead to smaller uncertainty?

I have little intuition for how the uncertainty values are being determined when multiple cores and multiple types of age markers are combined. Because I do not understand the uncertainty values in the deep EDC case where there are not complicated interactions, I am skeptical of the uncertainty values for the remainder of the ice core chronologies. The authors provide a nice description of the uncertainty methodology at the end of the supplement, but stop short of investigating the uncertainty during a specific period and explaining how and why the uncertainty varies. I think this would be most useful and probably worthy of being in the paper itself rather than at the end of the supplement.

A different area I would like the authors to discuss more fully is how the physics of ice flow are included. Certain tie points were excluded because the thinning function became unreasonable. What was the criteria used? Also, there are many examples where deeper layers have thinned less than shallower layers. One prime example is the period 550 to 590 ka in the EDC record. The thinning function at 540 ka is 0.07, goes to .12 at 560 to 580 ka and then falls to 0.04 at 600 ka. It seems unlikely this can be ascribed to differences in rheology as the isotopic and chemical impurity concentrations of the ice between 600 and 620 is quite similar to between 560 and 580 ka. Despite the large change in thinning function, the uncertainty values are quite low (less than 2500 years). Doesn't a large change in thinning function indicate something strange is going on and the uncertainty should be quite large? I would like the authors to be more specific in their criteria for evaluating the thinning function and in particular discuss the ice flow conditions that would lead to deeper layers having thinned less than shallow layers. What influence is the background scenario having during this period?

In conclusion, I am impressed with the undertaking of Bazin et al., Veres et al., and the entire group developing this dating approach. However, I am not convinced that the

statistical approach is assessing the uncertainty properly. Some of my concern may reflect my lack of understanding of how the uncertainties are being combined and I challenge the authors to provide a fuller discussion of the uncertainties in a more intuitive fashion. This will enable readers to have confidence in the timescale and associated uncertainties while also understanding the limitations of the statistical approach.

Interactive comment on Clim. Past Discuss., 8, 5963, 2012.