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Climate of the Past Discussions

Interactive comment on "On the gas-ice depth difference (△depth) along the EPICA Dome C ice core" *by* F. Parrenin et al.

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original text is plain, answer is bold

This paper presents the results of several modeled and empirical estimates of the depth difference between ice and gas for the same age, along the Dome C deep ice core. This depth difference is useful for constraining firn thickness and ice thinning (and validating the models to estimate them), which are important for constructing the timescales of ice and gas. The authors found overestimation of the depth difference in purely model-based estimates in the last glacial period, likely due to errors in the firn densification model. They also confirmed irregular thinning in the deep part of the core. The topics are well suited for the scope of the journal and the paper represents important contribution toward solving the issues around timescales of ice and gas.

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Interactive Discussion

Discussion Paper

However, there are several points that need to be addressed/clarified before the paper can be accepted for publication.

Major problems: (1) The paper uses EDC3 chronology but there is a more accurate (at least for the last glacial period) chronology for this core by Lemieux-Dudon (2010). Because an accurate chronology is necessary for the analyses in this paper, the latter timescale should be used in this study.

The LD2010 age scale only extends back to 51 ka BP while we need an time scale for the complete record. Moreover, the LD2010 age scale is constrained by GICC05 through CH4 variations, using firn densification simulations. Because these simulated Delta-ages are challenged by other methods (cf. Loulergue et al., CP, 2007), it is not clear that LD2010 is more accurate than EDC3 in terms of events durations. Finally, we evaluated the confidence interval of EDC3 so we think our approach with EDC3 is correct.

(2) Section 3.2 presents wrong observations. From fig. 8, the TD-synchro yields deltadepth which is systematically larger than the results from EDML-synchro and seesaw, and in consistent with model-based estimate, for the depths above around 800 m (latter part of the last glacial period). The differences between TD-synchro and EDMLsynchro are larger than the error bars. These need to be described and possible causes should be discussed.

We did note that the EDML-synchro method underestimate Delta-depth with respect to the seesaw estimates and that it is not the case for TALDICE-synchro. We are now clearer on this point: 'As a consequence, the TALDICE-synchro method overestimates $\triangle$depth with respect to the EDML-synchro method.' We also gave some explanation for the underestimation of $\triangle$depth by the EDML-synchro method, due to the poorly known accumulation gradient upstream of EDML. 'It thus leaves us only with an underestimation of EDML thinning, which may be due to an overestimation of EDML accumulation rates during the glacial.

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We indeed did not take into account the fact that accumulation rates are lower upstream of the EDML site, from where the ice originates [Huybrecht et al., 2007].' At the moment we have no other explanation for the TALDICE-synchro / EDML-synchro difference.

Minor problems: P1091, L10. There are several definitions for COD in the past literature. More than one definition are used in this paper so the readers will be confused. Here it is stated "the Close Off Depth (COD), where it is not possible to pump air". But in p1096, L22 it is stated, "at Close-Off, 37the definition of COD employed in this study.

In this paper the COD is defined as the depth below which it is not possible to pump air anymore. p1096, l22, it is not a definition but an observation of the closed porosity proportion at the COD.

L13-17. There are more recent papers for the temperature-CO2 timing for the last termination, and they suggest almost no lag of CO2 relative to Antarctic temperature. Loulergue, L., F. Parrenin, T. Blunier, J. Barnola, R. Spahni, A. Schilt, G. Raisbeck, and J. Chappellaz (2007), New constraints on the gas age-ice age difference along the EPICA ice cores, 0-50 kyr, Clim. Past, 3, 527. Pedro, J. B., T. D. van Ommen, S. O. Rasmussen, V. I. Morgan, J. Chappellaz, A. D. Moy, V. Masson-Delmotte, and M. Delmotte (2011), The last deglaciation: timing the bipolar seesaw, Clim. Past, 7(2), 671-683.

Loulergue et al. (CP, 2007) is now cited in this paragraph. The study by Pedro et al. (CP, 2011), does not deal with the problem of the CO2-AT phasing, to our knowledge. A more recent does, however (Pedro, CP, submitted) so we cited this one.

P1097, L7. Is this a section title?

Yes, this is a mistake introduced during the typesetting phase that we unfortunately did not see during the checking phase.

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P1097, L11-12. References presenting concept and evidence of convective zone are missing. Following papers should be cited. Sowers, T., M. Bender, D. Raynaud, and Y. S. Korotkevich (1992), δ15N of N2 in air trapped in polar ice: a tracer of gas transport in the firn and a possible constraint on ice age-gas age differences, J. Geophys. Res., 97(D14), 15683-15697. Bender, M. L., T. Sowers, J. Barnola, and J. Chappellaz (1994), Changes in the O2/N2 ratio of the atmosphere during recent decades reflected in the composition of air in the firn at Vostok Station, Antarctica, GRL, 21(3), 189-192. Kawamura, K., J. P. Severinghaus, S. Ishidoya, S. Sugawara, G. Hashida, H. Motoyama, Y. Fujii, S. Aoki, and T. Nakazawa (2006), Convective mixing of air in firn at four polar sites, EPSL, 244, 672.

References now cited.

P1098, L7. Eq (15) has error (as pointed out by other reviewers).

This is a mistake introduced during the typesetting phase and that we have not seen during the checking phase. It will be corrected in the revised version.

We wish to warmly thank the reviewer for his careful review.

Interactive comment on Clim. Past Discuss., 8, 1089, 2012.