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Climate of the Past Discussions

Interactive comment on "Rapid changes in ice core gas records – Part 2: Understanding the rapid rise in atmospheric CO_2 at the onset of the Bølling/Allerød" *by* P. Köhler et al.

Anonymous Referee #2

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This manuscript discusses the genesis of the apparent rapid rise in atmospheric CO2 mixing ratio that was observed in the exquisite data set of Monnin et al. (2001), on the EPICA Dome C ice core. An hypothesis is presented that explains the rise as a response to rapid sea level rise that occurred at about this time, the so-called "Melt Water Pulse 1A". The hypothesized connection is that organic carbon-rich nearshore environments were flooded rapidly, releasing terrestrial carbon stocks to the atmosphere due to enhanced respiration and decay relative to photosynthetic sequestration. The authors extract the magnitude of the inferred atmospheric CO2 increase, from the measured CO2 mixing ratios in the bubbles, by taking into account the broadening of the age distribution of the gases due to the gradual bubble close-off process as the firn

becomes ice. This smoothing of the atmospheric record is a well-known but poorly-understood feature of all ice core records, but especially those records from low snow accumulation sites such as Dome C (about 0.03 m per year). The magnitude of the CO2 increase that is inferred is quite a bit larger than the observed increase in the air bubbles, presumably due to this smoothing effect.

The paper is topical, and of general interest due to the obvious relevance to possible biogeochemical feedbacks in the coming centuries under a regime that is almost certainly characterized by rising sea level and coastal inundation.

1) There are several serious scientific issues that this paper presents that warrant some further examination. One is that the atmospheric methane record from the same ice core, the Dome C ice core, contains a rather well-preserved signal of the Younger Dryas and Bølling atmospheric methane variations, which are well known from high-accumulation rate ice cores such as the Byrd ice core, Siple Dome ice core, Law Dome core, and the Greenland cores. These atmospheric signals therefore contain an invaluable testbed with which to check the author's postulated age distribution probability density function. I suggest that the authors use this methane record, smoothed with their pdf as a filter, to compare with the actual methane data from the Dome C core. Without actually doing the exercise, I can guess that the results may well require a substantial revision of the pdf.

The upshot of this revision of the pdf may very well substantially change the conclusions of the manuscript, as the authors infer such a large magnitude transient peak in atmospheric mxing ratios. This inference is entirely a product of the age distribution pdf that the authors adopt, which is based on a glaciological bubble formation model. The breadth of this pdf requires that a much higher atmospheric peak have occurred, to explain the sharpness of the rather sudden change (but lower magnitude change) observed in the bubble record.

Of particular importance in this context is the fact that the observed age distributions of gases in trapped air often deviate substantially from modeled age distributions. The reasons are not completely understood. It is possible that the process whereby air is separated from mixing with the atmosphere, effectively sealing it off, is not directly and simply related to the bubble close-off process. Instead, it may be that horizontal ice layers seal off the gas mixing prior to bubble formation. A variety of firn air pumping studies hint at this mechanism, with these studies showing an absence or near-absence of anthropogenic gases such as chlorofluorocarbons in the air in open pores from near the bubble close-off region. [If instead the closure of bubbles were the relevant event that removed gases from contact with the atmosphere, then one would expect the full burden of anthropogenic gases to be observed in all open pores.]

Also of possible relevance is the well-known fact that as climate warms, the densification process of polar firn accelerates with an Arrhenius-type exponential dependence on temperature. The enhanced densification means that the horizon at which gas is separated from the atmosphere travels through more than one annual layer per year. This has the effect of reducing the gas age-ice age difference during times of warming. For example, the gas age-ice age difference may be 2000 years during a cold period. After 1000 years of warming, the gas age-ice age difference may have been reduced to 1000 years, for the sake of argument. This implies that the closure surface migrated upward through two annual snow layers per year during this time of warming. Because the event studied by the authors occurs during a time of strong Antarctic warming, it is quite possible that this process has narrowed the age distribution of the gases.

Of potential further relevance is the fact that strong layering in polar snow is often observed in warm climates with moderate accumulation rate but not in cold climates with very low accumulation rate. Therefore the climate change itself may induce a qualitative change in the gas occlusion process, that leads to narrowing of the age distribution of the trapped gases as ice layers begin to seal off gases prior to bubble formation.

All of these unknowns caution us not to rely too heavily on a model calculation. Instead,

empirical and observational constraints are of much greater value. That is why it is key to utilize the Dome C methane record, with the known atmospheric forcing function (i.e. methane record from high-accumulation cores), to validate the age distribution pdf.

Other specific scientific issues:

2) With their term "width" the authors seem to be describing the second moment of the pdf. Why not use this metric? The description of the expected value E as a metric for the width of the age distribution is odd and confusing. Several well-known methods for calculating the second moment, or "spectral width", are available in the literature: C. Trudinger, D. Etheridge, P. Rayner, I. Enting, G. Sturrock, R. Langenfelds, Reconstructing atmospheric histories from measurements of air composition in ïňĄrn, Journal of Geophysical Research 107 (D24). doi:10.1029/2002JD002545. T. Hall, R. Plumb, Age as a diagnostic of stratospheric transport, Journal of Geophysical Research 99 (D1) (1994) 1059–1070. T. Hall, D. Waugh, Timescales for the stratospheric circulation derived from tracers, Journal of Geophysical Research 102 (D7) (1997) 8991– 9001. A. Andrews, K. Boering, B. Daube, S. Wofsy, E. Hintsa, E. Weinstock, T. Bui, Empirical age spectra for the lower tropical stratosphere from in situ observations for co2: Implications for stratospheric transport, Journal of Geophysical Research 104 (D21) (1999) 26581–26595.

3) Furthermore, the idea that the dating of the core using methane synchronization was biased by the broad age distribution is a bit odd and not well explained. This seems to assume that the beginning of the methane rise was the tie point used during methane synchronization. Yet my understanding is that the process of methane synchronization uses midpoints, not the beginning of the methane rise. These assumptions need to be explicitly stated, whatever they are. Midpoints may indeed lead to a small error due to the non-Gaussian nature of the pdf, but this needs much clearer explanation.

4) Generally it is common practice to normalize the age of a filter so that it has a centroid of zero. In other words, such a filter does not bias the age. One good test

of a filter, to see if it is unbiased, is to pass a linear trend through it. An unbiased filter will not change a linear trend at all. The authors instead have a value of E, their expected value, that is measured in the hundreds of years. This is odd and confusing and possibly represents a major error, although I cannot tell from the text whether the authors account for their nonzero value of E in a separate step.

5) The authors have not discussed, from my reading, the assumptions they must have made in order to come up with the shape of the atmospheric history. While it is true that a perfectly known filter can enable the deconvolution of a record uniquely, the error in the filter in this case is quite large. This error produces substantial non-uniqueness in a situation like this. The resulting shape of the atmospheric history that is inferred from a deconvolution is therefore quite nonunique, in addition the amplitude.

All of these issues are substantial enough, in my opinion, that this manuscript requires a thorough reworking and a resubmission. Therefore I cannot recommend publication of the manuscript in its present form.

Interactive comment on Clim. Past Discuss., 6, 1473, 2010.