Clim. Past Discuss., 9, C324–C327, 2013
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Climate of the Past Discussions

Interactive comment on "Peak glacial ^{14}C ventilation ages suggest major draw-down of carbon into the abyssal ocean" *by* M. Sarnthein et al.

Anonymous Referee #3

Received and published: 3 April 2013

Sarnthein and coworkers have submitted to CPD a modestly revised version of a manuscript of the same title that I reviewed one year ago (April 2012) for another journal (note the mention of anonymous reviews in the Acknowledgements). In that review I recommended publication following substantial revision because I felt that the principal conclusion, that greater storage of respiratory CO2 in deep waters during the Last Glacial Maximum (LGM) could be inferred from 14C ventilation ages, would be of widespread interest.

Some problems noted in my 2012 review have been corrected; e.g., a misconception concerning the solubility pump has been clarified. Others have not; e.g., an error in

comparing LGM dust fluxes and paleoproductivity of the Pacific and Atlantic sectors of the Southern Ocean remains unchanged (p. 934, last paragraph), so statements in the manuscript are inconsistent with observations (Z. Chase et al., DSR-II, 2003).

In my 2012 review I also requested a clearer description of age control for the 14C records as well as a more precise definition of terminology. For example, how is "ventilation age" defined, and does its meaning differ from that of "apparent ventilation age"? From the description on p. 936 of the paper now under review, it appears that "ventilation age" refers to the difference between DELTA14C of deepwater DIC and DELTA14C of atmospheric CO2. Is this correct? That is, are projection ages (i.e., with reference to DELTA14C of atmospheric CO2 at the time of deepwater formation) never used in this analysis?

It appears that the SOM appended to the CPD submission was written in response to some of my comments in the 2012 review rather than incorporating new information into the main text. Specifically, I noted that many paleoceanographers are skeptical of the very large surface-water 14C reservoir ages (sometimes in excess of 2000 years) reported in some studies of the LGM. The assertions in the SOM (Auxiliary Text #1) remain unconvincing. The vague remarks in Auxiliary Text #2 refer to my comment that one specific challenge to the inferred large surface 14C reservoir ages is the inconsistency between the large surface reservoir age reported by Skinner et al. (2010) for South Atlantic core MD07-3076 and the much smaller surface reservoir age reported for South Atlantic core TN057-21 by Steve Barker. Sarnthein dismisses the results from TN057-21 (without identifying the core by name) by invoking problematic age control due to CaCO3 dissolution. To the contrary, the extensive work on TN057-21 by Barker and colleagues has produced an extremely robust age model. If an inconsistency between surface reservoir ages of TN057-21 and MD07-3076 remains, then I contend that this raises questions about the reliability of the age model for MD07-3076.

Nevertheless, accepting for the moment that the large (>2000 years) surface 14C reservoir ages may be correct for some subpolar regions, one must then ask how this im-

pacts the estimation of deep-sea DIC concentrations in the past using the modern 14C-DIC relationship shown in Figure 2a. Sarnthein and coworkers argue that the slope of the 14C-DIC relationship would not have changed significantly between the LGM and today. However, they do not address the possibility that the "intercept" of the 14C-DIC relationship could also have changed. That is, could the older apparent ventilation ages of deepwaters during the LGM simply reflect an inherited signal, that is, the incorporation of subpolar surface waters bearing a large 14C reservoir age at the time of deepwater formation, rather than an increase in the residence time of the water in deep basins? Stated another way, is there any control on the preformed 14C age of newly ventilated deep water during the LGM, and how might variability of the preformed 14C age affect the interpretation of the 14C-DIC relationship?

The problem identified in the paragraph above is the single greatest conceptual problem with the analysis reported by Sarnthein et al. If the preformed 14C age of deep water masses changed significantly over time, then this would have shifted the intercept for all of the relationships discussed in this paper (14C-DIC, 14C-ALK, 14C-PO4, 14C-O2).

Other aspects of this paper require clarification before publication as well. For example, what is meant by "paleo modern carbon - 1.0"? (see Section 4.2 and Table 1). Readers should not be obliged to make assumptions about the intended meaning of critical terms such as this. Similarly, the presentation of information in Table 1 is extremely confusing. If this paper is revised for resubmission, then I recommend that information about variables other than DIC (i.e., ALK, PO4, O2) be removed and that the information pertaining to DIC be spread out so that each column provides information about only one variable. Indeed, since this paper ignores whole-ocean changes in ALK due to CaCO3 compensation, the inferences about changes in ALK (p. 943) are likely incorrect and should be eliminated in any case.

Lastly, referring again to Section 4.2, the statements about complete ventilation of the deep ocean during HS1 are unsubstantiated. For example, Sarnthein et al. claim

that the North Pacific was well ventilated to depths >3600m during HS1, dismissing evidence to the contrary presented by Jaccard and Galbraith (2012). Now, newer findings from Jaccard and Galbraith (GEOPHYSICAL RESEARCH LETTERS, VOL. 40, 1–5, doi:10.1029/2012GL054118, 2013) cannot be so easily pushed aside. It is quite clear from the geochemical evidence that deepwater oxygen levels in the North Pacific, the most reliable indicator of ventilation, did not increase significantly until the Bolling (Jaccard and Galbraith, 2013).

In fact, it is to the advantage of Sarnthein et al. to accept the conclusions of Jaccard and Galbraith. Otherwise, if the entire deep ocean were ventilated by the end of HS1, as claimed on p. 944, then Sarnthein et al. would have a mass balance problem involving the transfer of too much carbon from the deep sea to the atmosphere. By contrast, if the deep North Pacific remained unventilated during HS1, then the mass balance problem is reduced.

In summary, Sarnthein et al. present interesting ideas that are worthy of discussion in the paleoceanographic community, but several problems (including clarifications) need to be addressed before I can recommend this paper for publication.

After compiling the above review, I read the comments from the other referees and I generally concur with their assessment.

Interactive comment on Clim. Past Discuss., 9, 925, 2013.