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Interactive Comment

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

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In this paper, the authors draw on compiled radiocarbon data from a range of water depths in an attempt to constrain changes in the ocean's carbon sequestration capacity across the last deglaciation. Their method is to 'calibrate' deep ocean radiocarbon depletion against total dissolved inorganic carbon (DIC), potential alkalinity (POTALK) and absolute oxygen concentrations (O2), using modern data from the main basins of the global ocean. With this 'calibration', the authors propose to estimate DIC, POTALK and O2 at different stages across the last deglaciation (i.e. the last glacial maximum, LGM; Heinrich Stadial 1, HS1; and the Bølling-Allerød, B-A), based on estimates of deep ocean radiocarbon depletion at these times. I refer to a 'calibration', even though there are very clear physical mechanisms that underlie the observed modern relationships between radiocarbon, DIC, POTALK and O2 concentrations (which the authors describe), because in extrapolating from the modern relationships the authors must assume invariance in a range of unconstrained processes. Specifically, one must assume a constant relationship between export productivity and mass advection rates (mean transit times) in the ocean interior. This in turn would imply a past ocean circulation 'geometry' and nutrient cycle that was broadly similar to (or at least not drastically different from) today's. The broad similarity of the 'calibrations' that are found to apply to different ocean basins today is presented as an empirical argument that the line of reasoning adopted in this manuscript should be broadly robust to changes in ocean circulation/export productivity that are analogous to the spatial variability in the modern global ocean; i.e. the modern 'spatial calibration' is taken as a viable 'temporal calibration' too.

Thus, this paper presents a working hypothesis for large-scale ocean circulation and export productivity changes in the past (premised on a set of crucial assumptions), and argues that if past temporal variability in the large-scale ocean circulation can be taken as broadly analogous to the spatial variability we see in the modern ocean, then we might be able to calculate various aspects of past ocean chemistry if we know at least one of a set of correlated variables. In this case radiocarbon is the chosen known variable.

I think that the ideas and discussion presented in this manuscript represent a valuable contribution to the wider investigation into glacial-interglacial marine carbon cycling and the role of the large-scale ocean circulation therein. The discussion of radiocarbon in this context has become too narrowly focussed on the existence of otherwise of an extremely radiocarbon-depleted reservoir somewhere in the ocean, and this paper is refreshing in doing what many workers in this area would like to see more of: an attempt to link the spatial and temporal variability of marine radiocarbon concentrations explicitly to changes in the ocean circulation and marine carbon cycling, via clearly stated premises/assumptions and geochemical relations.

I think that the approach taken in this manuscript is not perfect and cannot be defended as being totally unequivocal, but it is nonetheless very useful and thought provoking, specifically as a working hypothesis that is premised on key assumptions (that are not proven, but might be tested). I think that a great deal of the utility of this approach is obscured by an overly complex and (apparently overly) forceful description of the methods as being completely robust. My impression is that the methods are indeed robust, but ONLY relative to a number of key assumptions. These could probably be stated more clearly and especially more simply in the manuscript. Furthermore, I think that tests of some of these key assumptions, using simple model (box- or EMIC) experiments, or using auxiliary proxy data (e.g. for oxygenation, carbonate ion or nutrient concentrations), are possible and might be included in a revised manuscript. Thus for example if it is proposed that a linear calibration of radiocarbon against phosphate, DIC and/or POTALK will remain invariant over time, then it should be possible to test this explicitly by assessing the past invariance of the scaling between radiocarbon and nutrient or carbonate ion proxies for example; in a quantitative sense, and not just a qualitative sense. I am thinking for example of a more direct comparison of radiocarbon and Cd/Ca, d13C or redox indicator proxies for example. Even a few data-point comparisons like these, if reliable, would go a long way in supporting the arguments of the manuscript. Of course, some discussion regarding the assumptions of the method and is possible tests is already included in the manuscript, but it is arguably not very transparent and is certainly not quantitative.

Indeed, I think that one way to improve the communication of the approach advanced in this manuscript and to defray over-zealous and non-specific criticism of it as 'too ambitious' or subject to 'too many unknowns' would be to simplify its description greatly, and to recast it as a 'thought experiment' or 'working hypothesis' based on clearly stated assumptions, as opposed to an unequivocal derivation from fundamental/necessary chemical principles. If the arguments presented in the manuscript are intended as robust derivations, they will be critiqued as such, and are likely not going to be able to stand up to such criticism. However, if presented as a working hypothesis that deserves some consideration and that is based on clear unproven assumptions that have yet to be tested, then the manuscript becomes less prone to criticism and becomes more useful for the palaeoceanographic community. Again, some simple numerical model tests might go a long way in helping to demonstrate what this method can tell us and what it cannot (or what needs to be tested before we can infer much more).

My specific comments are the following:

p.927, line 19: I offer a clarification: the concept of a 'standing volume effect' on marine carbon sequestration advanced in Skinner (2009) is not an exclusive one that stands in opposition to other possible changes in ocean circulation/chemistry or even biology. This concept was intended as a deviation from the apparent precept in the literature that the role of the 'ocean circulation' in past CO2 change must be exclusively through a vaguely defined 'mixing' or 'overturning' term (essentially a transport rate that is very hard to pin down with proxy data). The idea is that changes in water-mass geometry (volume specifically, where the residence time of a reservoir depends on the ratio of its mass or volume to its mass flux) can ALSO have an impact, and more importantly that such changes might enhance the effects of other changes in e.g. mass transport rates, air-sea exchange in polar regions etc. . . If the authors mean that Skinner (2009) presented a 'ceteris paribus' thought experiment to explore the impacts on marine carbon cycling of water-mass volume changes, while the present manuscript presents a ceteris paribus thought experiment to explore the impacts of changing transit times in the ocean interior, then this is a useful statement that might be made more clearly.

p.930, line 7: would 'newly formed' be better than 'juvenile'?

p.930, line 8: I would alter this line to: ". . .Sigman et al., 2010), and later on by the "biological pump"; that is the. . .'

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p.931, line 24: is it worth noting here that the equilibration time for radiocarbon across the various carbon species in seawater is much longer? What implications might this difference have (order year vs. order decade)?

p.932, line 3: It would be helpful for the reader if this paragraph stated something like "Below/In section 2.3 we assess the storage capacity. . ."

p.933, line 10: please state exactly what volume is considered here (is it >2,000m water depth; i.e. what % of the total ocean volume?).

p.933, line 12: This paragraph is not extremely clear. It might be better to use a word like 'input', rather than 'picking up', since (although vague) the latter implies to me a net change, rather than just a positive flux, that is augmented by other positive fluxes and balanced by other negative fluxes. Also, the last line of this paragraph is not clear at all to me; please clarify.

p.933, line 29: please add depth range after "deep ocean".. is it >2,000m?

p.934, from line 3: The whole discussion of the 'probability argument' regarding the 'variability test' is not at all clear to me, in the sense that I cannot see that it provides anything beyond an ad hoc argument that the modern spatial variability in radiocarbon-DIC calibrations is likely broader than the temporal variability since the LGM. This argument is backed up with estimates of the likely sense and magnitude of changes in some of the parameters, such as dust flux and export productivity, that might have occurred since the LGM; but no further test of the argument can be advanced, and it is noted that GCM simulations might eventually be able to provide such a test. This section reads as a survey of the modern spatial variability in the radiocarbon-DOC scaling, where the most divergent regional relationships are discounted as being outliers. This comes across as a weakly convincing argument that the modern spatial variability provides a conservative estimate of the temporal variability (driven by spatially heterogeneous changes in circulation times/routes and export productivity) since the LGM. My view is that this argument might be more convincing if it was presented not as a test, but as a hypothesis that can be defended but that also needs to be tested fully with clearly stated measurements/experiments.

p.935, line 21: for the ‘projection [age] method’, it is probably most appropriate to cite its originator: Adkins, J.F., and Boyle, E.A., 1997, Changing atmospheric Delta-14C and the record of deep water paleoventilation ages: Paleoceanography, v. 12, p. 337-344.

p.936, line 23: The point is well made I think. There is now a host of studies that could be cited as demonstrating large changes in ‘near surface’ water reservoir ages. . . in case this is deemed necessary in strengthening the case for their occurrence (e.g. Dokken and Jansen, 1999; Waelbroeck et al., 2001; Siani et al., 2001; Skinner, 2008; Skinner et al., 2010; Thornalley et al., 2011. . .). But I also have a question regarding the ‘plateau tuning’ technique: if this technique demonstrates in a sediment core large changes in surface reservoir ages, should the technique not be re-applied to the sediment core with those reservoir ages subtracted from the original radiocarbon dates, at least as a test to see if the apparent position of the plateaus does not change, and therefore that zero reservoir ages can be inferred for the ‘corrected dates’ as a result? In my experience, plateaus in radiocarbon dates in sediment cores can represent a large change in surface reservoir age that need not correspond to a radiocarbon plateau in the atmospheric record. How does the plateau tuning technique hold up when there are large changes in surface reservoir age? In extremis, it seems that the method should work best if there is zero reservoir age variability and not at all if there is enormous reservoir age variability.

p.937, line 16: To ignore intermediate water records on the basis of their ‘complexity’ might be more defensible it if was shown that they all yield conflicting results. If we ignore a large volume of the ocean, it becomes difficult to infer changes in ‘average ocean chemistry’. Can the authors provide an estimate of the extent to which changes in intermediate ocean chemistry could counteract the changes that are inferred in this manuscript in the deeper ocean?

p.938, line 1: Is it hard to get good ‘average’ estimates for HS1 because the records conflict, or because they are all changing in different ways? This starts to sound like the case of intermediate waters described by the authors previously; is it therefore just as difficult to say anything robust about HS1, across the whole ocean?

p.939, line 28: ‘evaluate’ might be better than ‘value’.

p.940, paragraph from line 12: I am not convinced that changes in AABW formation rates or air-sea exchange would have no impact on the calibrations that are proposed. It is suggested that this will have no effect because the modern calibration is the same for NADW and AABW, but why does this follow? Surely the potential problem is precisely to occurrence of a non-analogue situation in the past. Again, my understanding is that the modern spatial calibration is taken here to apply as a temporal calibration, regardless of possible changes in circulation geometry/rates, and the question is now can this be defended as plausible and what tests might exist for verifying this? Indeed, the reference to the study of McCave et al. (2008) is problematic, rather than helpful I think, because this study found more positive stable carbon isotope values in the deepest (abyssal) south Pacific and inferred that is represented the input of newly formed deep-water from around Antarctica, underneath more ‘aged’ water that included an Atlantic component. How does this support the authors’ claims?

p.940, last paragraph: In my view, there is an absolutely fundamental point that needs to be made with regard to the method presented in this manuscript and with regard to Figure 3. This is that changes in average ocean chemistry can only be inferred from point estimates from around the ocean if the volumetric ‘representativity’ of each point estimate is known (or guessed). An average that is calculated, for example from data in Figure 3, must represent a volume weighted average (and must take into account assumptions regarding the ocean <2,000m for example). This problem is why many workers in this field have struggled to make strong statements regarding past deep ocean radiocarbon concentrations and their implications for the global carbon cycle. The authors really need to address this limitation in their study I think.

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p.941, line 1: There is a further study using the radiocarbon data of Skinner et al. (2010), combined with new Nd isotope data measured in parallel that argues for an increase in the southern end-member ventilation state between HS1 and the pre-Boreal. If this paper emerges in Geology soon enough, it might be useful for the authors here.

p.941, line 11: I think it is important to stress that it is “modern [spatial] variability” that is being referred to.

p.941, line 15: instead of “turned out”, it might be better to write “appears to have been” or similar.

p.941, line 20: I do not understand the meaning of the last sentence of this paragraph, please clarify (especially with regard to the suggestion of McCave et al. (2008) that the deepest southern Pacific was less depleted in 13-C than shallower depths). Is the argument that the more positive d13C values in the abyssal ocean were due to reduced air-sea exchange under sea ice? This would not work, as it would drive values more negative surely?

p.941, line 25 (cont’d p.942): This seems problematic; through their method, the authors infer a significant increase in the carbon storage of the deep ocean during the LGM that is much more than is required to account for the atmospheric change CO2 (and the glacial terrestrial carbon release). So where is this extra carbon coming from? The line of discussion seems to explore the possibility that the intermediate ocean may have provided this ‘extra’ carbon to the deep ocean, but it is decided that the available data is too sparse to decide if this is correct. Nevertheless, surely we can say more than that, or at the very least we can say what observations we would expect to be able to make IF the inferences of the authors are correct. For example, would we expect reduced ventilation ages in the intermediate ocean during the LGM, increased oxygenation, increased preservation of carbonate etc. . . and by how much, if the modern spatial calibrations are inferred to apply to the past? This point seems to be absolutely crucial; if the method applied by the authors to infer the change in the deep ocean’s carbon stock leads to a further mystery of where all the carbon came from, then that needs some discussion. I suspect that carbonate compensation and changes in alkalinity may come into this discussion, but it is not clear to me that this is resolved later in the manuscript when such things are discussed.

p.943, line 6: This paragraph is not that easy to understand. My understanding is that it is saying that we can infer a decrease in ocean carbonate ion concentration from the authors' interpretations, and that this would be compensated for (on the multi-millennial timescale of glaciation/deglaciation) by carbonate dissolution. But what does this mean? Does it mean that the modern spatial calibrations applied by the authors would in fact not apply anymore, since these are based on constant carbonate chemistry of the ocean? Does it mean that the authors' inference that the deep ocean took up way more carbon than 'needed' to account for atmospheric/terrestrial carbon inventory changes is made even more problematic, since carbonate compensation would lead to even more CO2 dissolving in the ocean?

p.943, line 18: this paragraph appears to be advancing proxy observations as evidence against which to test the inferences made in the preceding paragraph, but I can't tell what the conclusion of this is. Is it that the data confirm a change in carbonate ion, or that they indicate near-complete carbonate compensation in many places, in which case, what does this mean for the questions raised above with regard to the mystery of how the deep ocean took up so much extra carbon, and from where? My failure to follow the reasoning could well be my own deficiency, but I would hope that if the discussion was clarified I might be able to understand it; an adage (that may not always be true, granted!) is that if something can be said it can be said simply. Can the authors make this whole section clearer, for example with short statements of what the purpose of the discussion is and what its conclusion is? E.g. "The above inferences might imply that. . . but proxy data suggests that. . .. This can be seen from. . .. etc. . .".

p.944, line 8: do the authors mean: "does not yet allow even a rudimentary. . ."?

p.944, line 26: The suggestion that atmospheric radiocarbon production remained nearly constant (more accurately we might say that it cannot account for the observed atmospheric radiocarbon changes on its own) across HS1 should properly be attributed to a study of Be-10 fluxes or palaeomagnetic field strength (e.g. Muscheler et al., 2005; Laj et al., 2002).

p.945, first paragraph: I think it is important to make clear that these calculations are based on assumptions regarding the volumetric weighting of the data used in this study (see my comment above) and that they are also based on assumptions regarding the invariance of intermediate water ventilation ages and carbon storage... despite the fact that the inferred magnitude of the deep ocean carbon inventory change would suggest that some other reservoir, perhaps within the ocean lost carbon at the LGM.

p.945, line 8-19: the use of ratios (e.g. 1105/1355 GtC) is confusing... what does it refer to; the mixing ratio of radiocarbon/carbon in the atmosphere? Please clarify.

p.945, line 18: replace ‘were’ with ‘was’.

p.945, last paragraph: The inferences presented here are pretty crucial, but they seem to slip away from much discussion. Are the authors predicting that intermediate waters should show a net increase in their carbon content across deglaciation, while the deep released carbon, and therefore that the intermediate ocean should exhibit attendant changes in carbonate preservation (i.e. a reduction) etc...? Can the authors state all this in terms of clear test criteria for their inferences, which might be sought out in the geological record?

p.946, Conclusions: I think that in this section the conclusions need to be laid out more clearly in juxtaposition with the assumptions that they are based on. Thus for example, the first sentence might end with: “of ocean deep waters, based on the assumption that...”

Line 9 might instead read “conclude tentatively a potential rise...”

Line 13 should state the water depths that define these terms: "...relocation from ocean intermediate waters [to >2,000m..?]".

Line 17 might better read: "The peak glacial deep ocean [appears to have been a] "cold acidic ocean..".

Finally, this section does not present any of the emergent issues that will need to be addressed in order to assess the full (as opposed to the conditional) validity of the paper's claims. I think that this is the place for a short sentence or two on what tests might be performed in future, e.g. with regard to intermediate water ventilation or regional carbon preservation etc... Basically, as presented, the conclusions seem over-strident and without any possible contingencies, and therefore as a result seem to lack credibility. Can the authors redress the balance somewhat here while keeping the conclusions short and snappy? If this is not possible, then one might argue that the conclusions are far too contingent on things we do not know or cannot even identify.

Figures/Tables: A really important issue for me concerns the transparency of the data compilation and averaging. I think it is really important to show the data plotted on their respective timescales, with bars indicating the LGM, HS1, B-A and 'modern' windows from which data are collated. I also think that it is important to tabulate these data individually. If anything went into a supplement it would be a table of the compiled data (each core, its location, water depth, citation and the data points for each time-slice). Furthermore, I think it is not really acceptable to include data that are unpublished or obtained via personal communication, certainly without plotting and tabulating these data here. I would propose that the authors make a time-series plot for each region considered in Table 1a, showing the data that are used and the time windows from which data are excised for the regional averages. Furthermore, as discussed above, Table 1a should at least include volumetric weightings for each region, so that the global average can be estimated properly (these volumetric weightings might also be shown as different symbol sizes in Figure 2 for example).

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Figure 2 is absolutely crucial for the manuscript. I would suggest that it might be rendered much more clearly. The Figure contains a lot of information that can eventually be drawn out, but it is not very clear, and it is very hard to see what the plots intends to show at a glance. I can think of a few different ways of showing the same information that might be less confusing. But this might require more direct discussion of the figure in the text (e.g. “Figure 2 shows. . .. it can be seen from... that. . .”); this currently isn’t really the case as far as I can tell. One suggestion is to include a ‘cartoon’ figure before Figure 2 that demonstrates the impacts on radiocarbon/DIC of the biological pump, solubility pump etc. . . so that fewer labels and arrows need to be included in Figure 2. I would also note again, that the averages shown in Figure 2 should be volume weighted averages and they might be best included in the figure as symbols with estimated error bars.

Figure 3 is a sort of hybrid of a ‘cartoon’ and a data plot. I think it is important to stress in the caption that the arrows are not really data, but inferences (and highly simplified ones) regarding the mass transport in the ocean interior. It cannot be said that the arrows show waters with a certain ventilation age if there are no data where the arrows are, surely. They are extrapolations based on a hypothesis of the ocean’s circulation geometry – this is acceptable in my view but the distinction between inferences and data needs to be made clear.

Figure 4: again, averages need to be volume weighted surely; how is this done for the LGM?

Supplementary material: I am not sure that this is necessary. I do think that there are key supporting arguments/illustrations missing from the manuscript (see comments above regarding figures/tables), but they are not the ones included in the supplement and I would prefer to see such important things in the main text, especially in a journal that can accommodate this such as CPD.

I hope that these comments are deemed constructive and prove useful to the authors

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