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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 #1

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This paper tries to reconcile — based on ^{14}C ventilation ages of deep ocean waters — how much and where old carbon, depleted in ^{14}C, might have been injected into the atmosphere to explain both reconstructed atmospheric CO_2 and Δ^{14}C data during Termination I, and especially the Δ^{14}C drop during the so-called Mystery Interval.

The most important aspect is the question if the paper can really address the question it was made for and is the paper supporting the final conclusions it is drawing. Here I have to say, that especially on the explanation of the Mystery Interval I am not convinced by the paper. As this is utterly important I try to clarify where I see deficits: On

page 945 it is claimed that by outgassing of 730 or 980 GtC from the deep ocean with a rather low Δ^{14}C values a drop in atmospheric Δ^{14}C of 210-230‰ can be reached. For that effort the authors have to eject the carbon until the end of the Heinrich 1 stadial into the atmosphere. They correctly state later-on, that this is at odds with the CO_2 data, because at the end of HS-1 or at the beginning of the Bolling/Allerod around 14.6 kyr BP atmospheric CO_2 was only 240 ppmv, and therefore 85% of the carbon that was injected from the deep ocean into the atmosphere needs to taken up again by the intermediate and surface waters. I believe if such a huge transition of C had indeed have happened (from deep ocean via atmosphere to surface and intermediate waters) — and this huge transition is necessary to explain the drop in atmospheric Δ^{14}C by 190‰ — then we would see signals of it in other records, at least in atmospheric CO_2 and $\delta^{13}CO_2$ records from the ice cores (*Schmitt et al.*, 2012). At least the overshooting in CO_2 should be visible in the record.

Furthermore, what is missing in the calculations how atmospheric CO_2 might vary is the fact that it is not only about moving carbon from the deep ocean to the atmosphere, because C can exchange between both pools only as CO_2. But in the ocean the dissolved inorganic carbon pool DIC consists by 90% of HCO_3^-, by 9% of CO_3^{2-} and only by 1% of CO_2. This is also the reason, why — if such a large peak would have happened — atmospheric C anomalies are not very fast brought down to background levels again, because the gas exchange via CO_2 is the bottleneck of the whole system.

For the whole story it is also necessary to consider changes in alkalinity. Because the marine carbonate system has two degrees of freedom, it is not enough to calculate only changes in DIC, one has to make assumptions on alkalinity, even if stated that it is constant. Furthermore, the assumptions / changes in DIC and alkaninity at the surface ocean are relevant for atmospheric CO_2.

Nevertheless, the question is, what can be learnt from the study:

1. I believe for their main interpretation on the Mystery Interval, the authors need to use available data to restrict WHEN and HOW large such a flux from deep ocean C to the atmosphere at maximum might have been, e.g. using δ^{13}C and along the line of *Schmitt et al.* (2012) where it is discussed that a drop in δ^{13}C by 0.3‰ and a rise in CO_2 by 30 ppmv at the begin of Termination I might be connected with a drop in atmospheric Δ^{14}C of 20‰. Maybe these kind of calculations can be streched to its limits and thus claim how much of the Mystery Interval can be explained with it.

2. I also believe that part of the Mystery Interval can be explained by changes in ^{14}C production rates, but I agree that uncertainties are high here and the drop in atmospheric Δ^{14}C is too fast be be completely explained by them, but see *Köhler et al.* (2006) for some model-based scenarios.

3. Although not yet published and therefore difficult to finally judge, my understanding is, that the gradient in atmospheric Δ^{14}C will be a lot smaller if data are based on the upcoming INTCAL13 Δ^{14}C compilation. Maybe it is worth to wait for its release in RADIOCARBON later this year to redefine the target again. The amplitude might be the same but the drop has some more centuries / millennia time to take place.

Some more details:

1. This paper is difficult to read. The authors switch between various ways how they describe the rather lengthly units of some of their calculations, e.g. "μmol DIC kg^{-1} per ‰ ^{14}C", sometimes found as "μmol DIC kg^{-1} seawater ‰$^{-1}$ ^{14}C". This example is given just for illustration. Please unify.

2. Throughout: If talking about "‰" changes in radiocarbon, it should be "Δ^{14}C", not "^{14}C".

3. Throughout the MS: when writing about ventilation ages, does this mean "calendar years" or "^{14}C year". This should be very clear every time an age is given.

4. Abstract line 2: 530 GtC: should be transfered from WHERE to WHERE?

5. Abstract line 12: One strong assumption is that the gradient DIC versus Δ^{14}C did not change over time. Is this reasonably when we know that ^{14}C production rates varied and were for a long time in the glacial period more than 30% higher than today? See reconstructions based on ^{10}Beand the geomagnetic field *Köhler et al.* (e.g. 2006). My understanding is that a lot of the paper is based on that assumption so some more support is necessary here, e.g. estimate what a rise in ^{14}C production rate by 30% for 30 kyr would imply for deep ocean Δ^{14}C values.

6. Page 927, line 25, Fig 1: You nicely use *Matsumoto* (2007) to argue about ventilation ages of water masses. However, the dominant work of *Matsumoto* (2007) is to redefine ventilation ages based on the fact that southern-sourced waters change all this calculations. I think this is later-on taken up once, but I think it need to be addressed right here and maybe also with some arguments. Does your data / approach implies, that the revised ventilation ages of *Matsumoto* (2007) (which are about a factor of two smaller than the conventional ages plotted in Fig 1) are wrong?

7. page 928, line 9: when referring to other section, please use section number.

8. page 929, line 25: POTALK not explained.

9. page 932, line 26: Ratios "0.48-1.43" have no units, maybe okay, but not clear, because I do not know what quantities are divided in detail.

10. page 933, line 11: “the modern abyssal ocean is picking up almost 1.1 GtC per year”. This is not clear to me. Today the C content should be about in steady state, which should be deep ocean accumulated C? I can also not follow, where the numbers come from, please clarify.

11. Fig 3 and Fig 4. If remembering correctly, Fig 4 is cited before Fig 3, thus please change both figures.

12. page 936, line 10: Please change “cm ky^{-1}” to “cm kyr^{-1}”

13. page 937 lines 9-10: Please state all the time windows chosen here: What is the selected LGM and HS-1 time windows? Why is the Bolling/Allerod so short? For my understanding it should be 14700 to about 12700 yr BP (until the beginning of the Younger Dryas), you chose to stop at 14.0 kyr BP. Furthermore, here it is called “Bolling”, in most other places it is called “Bolling/Allerod”. Please clarify and unify throughout the MS.

14. Fig 3: is not labeled as Fig 3a, 3b, 3c, but as such referred to in the text.

15. Section 4.3 and Fig 6: The whole discussion on oxygen is difficult to follow. From Fig 6 is can not follow how water masses might get suboxic or anoxic (but maybe I missed it). Fig 6: labels of both axis need revision.

16. Table 1: I do not understand the table: In columns 2, 3, 6 (DIC, alkalinity, PO4) what is the meaning of the “x-”? Maybe I have missed it.

17. Fig 1a: Do you have copyrights for reproduction from *Matsumoto* (2007).

18. Fig 4: Slopes of the regressions are not mentioned, please insert either in the figure or in the caption.

19. Fig 5: Maybe you shift this figure to become as subfigure “c” part of Fig 2.

20. Supplementary Material: This should either be part of the main text (e.g part 2 might be a footnote) or omitted. It is either important discussion, then include or not necessary, then delete.

References

Köhler, P., R. Muscheler, and H. Fischer (2006), A model-based interpretation of low frequency changes in the carbon cycle during the last 120 000 years and its implications for the reconstruction of atmospheric Δ^{14}C, *Geochemistry, Geophysics, Geosystems*, *7*, Q11N06, doi: 10.1029/2005GC001228.

Matsumoto, K. (2007), Radiocarbon-based circulation age of the world ocean, *Journal of Geophysical Research*, *112*, C09,004, doi: 10.1029/2007JC004095.

Schmitt, J., R. Schneider, J. Elsig, D. Leuenberger, A. Lourantou, J. Chappellaz, P. Köhler, F. Joos, T. F. Stocker, M. Leuenberger, and H. Fischer (2012), Carbon isotope constraints on the deglacial CO_2 rise from ice cores, *Science*, *336*, 711–714, doi:10.1126/science.1217161.

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