Clim. Past Discuss., 10, C1275–C1278, 2014
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Climate of the Past Discussions

Interactive comment on "Carbon isotopes support Atlantic meridional overturning circulation decline as a trigger for early deglacial CO₂ rise" *by* A. Schmittner and D. C. Lund

Anonymous Referee #1

Received and published: 8 August 2014

Contrary to the bullet I accidentally clicked and can't unclick, I do not need to see the revised paper, although I would be glad to if asked. —-

In this paper, the authors compare atmospheric CO2 and d13C, and foraminifera-based ocean d13C trends, from the very latest last glacial maximum (18.5-19.5kyr BP is used) into the last Heinrich event (16.5-15.5kyrBP is used) to the d13C trends predicted from hosing experiments using the pre-industrial control of the intermediate-complexity UVic model with MOBI imbedded. For the two weaker hosing experiments, the AMOC recovers immediately after the fresh water forcing is over. For the two strong experiments, the AMOC essentially collapses and remains in the collapsed state even after freshwater forcing is removed. Thus, only the early phase of the "deglacial minimum" is

evaluated, not the d13C recovery.

I am favorably impressed with the manuscript although I do have several minor comments that can be easily addressed by the authors. Importantly, while matching the data reasonably well, it achieves the initial deglacial CO2 decrease by a change biological cycling in response to a large AMOC decrease, without changing southern ocean winds. I think this paper is also an important step towards understanding the evolution of deglacial d13C signals. While not perfect, if shows reasonable matches to carbon isotope signals. It also shows that a source of isotopically depleted carbon is not needed to explain low d13C values in the mid-depth South Atlantic, as suggested might be the case by Tessin & Lund, 2013.

In Intro, there is heavy reliance on Pa/Th records for setting the stage (p2 lines 24-35). Also, page 9: the C13 data are matched best by complete cessation for many millennia, providing support for the McManus record. On the one hand McManus et al data suggest complete cessation, but on the other, Gherardi et al data suggest unlikely. Several studies show that Pa/Th strongly affected by particle flux and composition. The observation of highest production ratios during HS1 may be indirectly caused by a large reduction in the AMOC, through its affect on other parameters.

Methods: While I agree that a 1ka uncertainty is not a problem for this sensitivity study, I do think that age models of records not previously published should be provided. Age control points can be added on Figure 6. If radiocarbon reversals occur over the study interval, that can be noted on the core location table.

P5 line8. Here, high C13 in NADW is attributed to "well-equilibrated surface waters". Although it is somewhat implicit in the next sentence I think it should be clarified that NADW source waters are relatively depleted in nutrients - AAIW, with higher d13as is a better example of a water mass with well-equilibrated source water. Line 13: I'd add: (Fig4D-4) "as suggested previously". Boyle and Keigwin, 1982 might have been the first to suggest that the North Atlantic- Pacific gradient reflect the AMOC. Most recently

suggested by Yu et al., 2014 EPSL. Line22: I think best to be cautious about saying this might explain the d13C minimum. The focus of this paper is the early deglacial d13C decrease. It does not deal with the whole planktonic d13C minimum, which generally well outlasts the interval of dramatically reduced AMOC, and whose end may have other causes. Lnes25-28: I would be interested in more details on the mechanisms of these changes.

P6 line11 Not clear to me what is meant by “illustrating the local effect of freshwater forcing”.

The metric used to compare data and model is the correlation between the difference at model CTL-year 2500 and the difference between HS1 and latest LGM. Because the evolution of the signal is in some cases quite different in the data and models, it may also be valuable to use a metric that takes evolution into account. Is the ability to do this compromised by poor age control? Is this a likely reason for the poor temporal match in MD97-2120, NIOP905, 17JPC? Or does the model simply not simulate the evolution well? I understand that in some cases the poor temporal correlation is related to the modern control. Might this be often the case, and if so, why does the value after 2500 years show a correlation?

P7, top. more detail on these processes might be helpful.

Page 8, line 1, typo “largest” effect

Page 8 line 16. Since the time evolution of the signal is not well simulated, perhaps be more specific. The d13C difference after 2500 years is similar to the late LGM-late HS1 difference? Line 23 – not clear to me that there is robust evidence for a weaker LGM circulation although many believe that to be the case. Page 9 – would be useful to highlight where (in space) the differences in biological pumping are most pronounced. Lines 8-10. Unless this other mechanism is discussed here, I might change the ending.

Figures – In addition to adding age markers to Figure 6, this figure could be improved -

perhaps when there are two records on the same panel they can be shown in different colors with matching label?

I think streamfunction figures, comparable to Fig 4, would be a useful addition and would clarify the connection of isotope changes to circulation changes, discussed in some instances (e.g. upper South Atlantic).

Interactive comment on Clim. Past Discuss., 10, 2857, 2014.