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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

A. Schmittner and D. C. Lund

aschmitt@coas.oregonstate.edu

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Thank you for the prompt handling of the manuscript and the thoughtful comments, the paper has improved considerably as a result of your input and that from the reviewers. In our response below your original comments are quoted.

"The most important point here is that referee #2 is criticising the fact that your model runs are based on interglacial boundary conditions. This is also seconded by the SC and has been mentioned in my initial editorial response after your submission. Referee #2 and the SC make a convincing point that due to this, the effects of a shut down of the AMOC in your model are most likely overestimated."

In our revised manuscript we provide arguments that the effects MAY be overestimated but we don't think that they are MOST LIKELY overestimated. Such an assessment (most likely overestimate) would require knowledge of the state (both physics and carbon cycle) of the LGM ocean, which unfortunately remains elusive.

"In view of this criticism, there are two ways forward: The first (and clearly from an editorial point of view preferred) possibility is that you redo your model runs based on glacial boundary conditions. If that is not at all possible, the minimum requirement to go forward is that you extensively discuss this limitation and the potential effects this limitation has on your results and to compare your work with other modeling studies using interglacial and especially glacial boundary conditions."

As we mentioned in our initial response the state of the circulation and carbon cycle of the LGM ocean is poorly constrained; no 3-D model simulation exists to our knowledge, that is consistent with the ocean interior observations/reconstructions including carbon isotopes. Until this situation changes we cannot perform simulations from realistic initial conditions. Whereas simulations with glacial boundary conditions (lower GHG, orbital parameters, ice sheets) are easy and have been done before (e.g. in PMIP) their value is low as long as they are not assessed by observations.

In short, realistic initial conditions are not available at this time. Hence, we have to resort to your option 2, which is an in depth discussion of this important point. This is now provided in the revised manuscript's discussion and conclusion section. We have also re-written the abstract and title in order to put more emphasis on the robust conclusion that the AMOC was reduced and to more clearly outline the model-data differences and uncertainties with respect to the CO2 response.

"The latter is also required, as referee #2 criticises the lack of an in-depth discussion of previous work or in one case its misinterpretation (work by Tschumi)."

We don't agree that we have misinterpreted the work by Tschumi. Please refer to our response to the referee, who cites a different paper (Tschumi et al. 2008) than the one

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we were referring to (Tschumi et al. 2011).

However, although the scope of our manuscript is not an extensive review of modeling of AMOC or Southern Hemisphere wind changes on CO2, in our revised manuscript we now provide almost all the additional references suggested by the referees.

"From an editor's perspective I have another request, that can be easily accommodated. To give the paper the full credit and to help other scientists in this field, you should provide your d13C data compilation in a supplement to the paper together with the used age scales (as also requested by referee #1)."

We have produced a supplement including all the sediment data plus some selected model output. The file is 15 MB and available here: http://people.oregonstate.edu/∼schmita2/data/schmittner14cp/schmittner14cp_sup.tar.gz

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