

# ***Interactive comment on “Carbon isotopes support Atlantic meridional overturning circulation decline as a trigger for early deglacial CO<sub>2</sub> rise” by A. Schmittner and D. C. Lund***

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Thank you for your comments. In our reply below we include some of your comments in quotes.

"There is a broad literature on freshwater hosing experiments and their impacts on CO<sub>2</sub> and d13C in the land, ocean, and atmosphere system. This body of literature is simply disregarded by the authors. While I appreciate that it is getting increasingly difficult to follow the literature, the authors apparently did not pay any attention to refer to earlier work."

It is not true that we did not pay any attention to or simply discarded earlier modeling

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work addressing CO<sub>2</sub>/d<sup>13</sup>C effects of hosing. In fact in our original manuscript we did cite five of those studies: Menviel et al. (2008, 2014), Schmittner and Galbraith (2008), Schmittner et al. (2007a) and Tschumi et al. (2011).

"This is very disturbing in particular as both researchers have a long-standing track record in the field. Clearly, the authors should do a literature survey and point the reader to earlier work to place this work in the appropriate context and to discuss their findings in comparison with earlier results."

The scope of our manuscript is not an extensive review of the literature on modeling AMOC effects on CO<sub>2</sub>. However, as outlined in detail below we now do cite more of those studies as suggested by the reviewer.

"1.1) There are issues which need attention regarding the simulated rise in atmospheric CO<sub>2</sub>. First, the simulations are started from a preindustrial steady state. This caveat and its implication must be fleshed out more clearly and already be stated in the abstract. While discussed in the conclusion, the obvious implication, an overestimation of the CO<sub>2</sub> change by the model, is not mentioned."

We have stated now in the abstract that the model starts from modern conditions. We also state clearly now in the abstract that the model overestimates the d<sup>13</sup>C amplitude in the North Atlantic. Acknowledging the uncertainties in the CO<sub>2</sub> response, the title and abstract were modified to put more emphasis on the more robust conclusion that the AMOC was reduced and to more clearly highlight the uncertainties in the CO<sub>2</sub> response. However, as discussed in the revised conclusions/discussion section and below we do not think that overestimation of the CO<sub>2</sub> changes by the model is an obvious implication.

"The AMOC was shallower in the NA during the LGM than at preindustrial. Consequently a smaller body of water was affected by a slow-down of the AMOC in the real ocean as compared to the model run."

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It is not clear that a smaller body of water was affected. E.g. Kwon et al. (2012, Paleoclimatology 27, PA2208) suggest that that NADW took over more of the global ocean interior during the LGM.

"This mismatch is reflected in the too large changes in  $\delta^{13}\text{C}$  in the North Atlantic as evident in Figure 6A-C and even more clearly in Figure 7, where all model results show substantially larger  $\delta^{13}\text{C}$  changes than reconstructed. (The high correlation given in Fig 9 and the text is a bit misleading as the slope is quite different from the 1:1 line)"

To address the reviewers concerns, we have expanded the discussion of the model data differences. We note that in addition to the correlation coefficient other metrics (Tab. 2) support the conclusion that model FW0.15 fits the data best.

"Similarly, the Brazilian margin data show a  $\delta^{13}\text{C}$  change between 1.6 and 2.1 km (line22 p2863) whereas the model shows a substantial  $\delta^{13}\text{C}$  change in the entire water column below 1500 m.

These features indicate that the overall change in  $\delta^{13}\text{C}$  in the North Atlantic is overestimated by the model and thus also the change in DIC and in turn the change in atm  $\text{CO}_2$  during H1."

Thank you for bringing up this point, which was echoed by Galbraith's short comment and the editor. We agree that it is possible that the model overestimates early deglacial AMOC effects on DIC and  $\text{CO}_2$  and discuss this now in the revised manuscript's discussion section.

Part of the data-model mismatch at the Brazil Margin may also be related to the depth of NADW; during the modern, it appears to be centered between 2-2.5 km whereas at the LGM it was centered between 1.5-2.0 km (Curry and Oppo, 2005).

"I suggest that the author perform a spin-up under LGM condition and repeat their freshwater hosing experiment that show a collapse of the AMOC."

We agree that repeating the simulations with more realistic LGM initial conditions would

be highly desirable. However, realistic deep ocean LGM conditions are currently not available. Even though it would be possible to use a simulation with LGM boundary conditions its value would be low if the deep ocean circulation and carbon cycle simulation is inconsistent with observations. Furthermore, it is important to note that our results serve as a sensitivity test and provide an initial estimate of the AMOC's impact on deep ocean d13C and the subsequent effects on the biological carbon pump.

"I suggest that the authors continue their simulations and force their model (e.g. by freshwater removal) to trigger an onset of the AMOC, ideally for an LGM spin up. This would allow the authors to compare the simulated d13C changes not only during H1 but over the period from H1 to the end of the B/A."

We agree that it would be interesting to simulate the full deglacial but extending the simulations is beyond the scope of our study. We emphasize this point now at the end of the introduction and include a brief discussion of the later deglacial at the end of the discussion/conclusion section.

"2. Reflection of earlier work is missing

I find the introduction weak. In particular the literature on the subject is not reflected. Why? There is a range of (modelling) studies available that address the influence of AMOC changes on atmospheric CO<sub>2</sub> and/or d13C and both from a terrestrial and oceanic perspective. Example that come immediately to my mind are (Menviel et al., 2008a; Marchal et al., 1998; Marchal et al., 1999; Menviel et al., 2012; Menviel et al., 2014; Köhler et al., 2005; Bozbiyik et al., 2011; Obata, 2007) and there is certainly much more in the literature.

I encourage the authors to do a thorough literature research, to discuss this earlier work and to compare their findings with earlier studies. The findings of earlier studies have also implications regarding the authors' main conclusion. Earlier studies find that the response in CO<sub>2</sub> and the carbon cycle is sensitive to the initial state (e.g. (Menviel et al., 2008a; Köhler et al., 2005))"

The purpose of our manuscript is not an extensive review of modeling studies regarding AMOC influence on CO<sub>2</sub>. We had already cited Menviel et al. (2008a) and Menviel et al. (2014) in our previous version. In the discussion section of the revised manuscript we now cite most of the additional references you mention. We also discuss the issue of the initial state.

### "3. Literature on SO wind changes is poorly represented

Again, the authors should search and carefully read and reflect studies addressing SO wind changes. The SO wind hypothesis has been challenged meanwhile by a broad range of studies. See (Tschumi et al., 2008) (Menviel et al., 2008b) and follow-up studies by others (e.g. (Lauderdale et al., 2013;d'Orgeville et al., 2010). Unlike stated in the introduction and reiterated in the conclusion, (Tschumi et al., 2011;Tschumi et al., 2008) do not support the suggestion that SO wind changes are responsible for the deglacial CO<sub>2</sub> rise. To the contrary, Tschumi et al., 2008 state in their abstract: "Our results are in conflict with the hypothesis that Southern Hemisphere wind changes are responsible for the low atmospheric CO<sub>2</sub> concentrations during glacial periods" "

Contrary to the reviewers implication (as interpreted by the editor) we have not misrepresented the work by Tschumi. In our original manuscript we did not cite the Tschumi et al. (2008) paper, which discusses Southern Hemisphere winds as an explanation of the full glacial-interglacial CO<sub>2</sub> change. We did cite a different paper Tschumi et al. (2011) because it focuses on the early deglacial CO<sub>2</sub> rise, which is more relevant to our manuscript. In this paper, the authors perform model simulations driven by wind changes over the Southern Ocean. Here is a quote from their abstract: "Our results provide support for the hypothesis that a break up of Southern Ocean stratification and invigorated deep ocean ventilation were the dominant drivers for the early deglacial CO<sub>2</sub> rise of 35 ppm between the Last Glacial Maximum and 14.6 ka BP." This is consistent with our original quotations in the introduction and in the conclusions. We have added "and/or stratification" to the sentence quoting Tschumi et al. (2011) in the introduction because in their above quote the word "wind" is missing and in the main

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text they note that they use wind changes only as a “knob” to change ventilation in the Southern Ocean.

"P2860, line 18ff: please provide details how freshwater forcing is applied. Is there a compensation of salinity elsewhere? How are tracers affected by the freshwater input?"

This info has been added.

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

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