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## ***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***

### **Anonymous Referee #2**

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This is an interesting study compiling d13C data and model results. A series of freshwater hosing experiments for preindustrial climate conditions are carried out with the UVIC climate-carbon cycle model. Model results in terms of changes in d13C and in atmospheric CO<sub>2</sub> are compared to the ice core d13CO<sub>2</sub> and CO<sub>2</sub> records and to marine records of d13C as compiled from the literature for the period of the HS1 event (19 to 15 ka BP). The authors postulate that the early deglacial rise in CO<sub>2</sub> was caused by a collapse of the Atlantic Meridional Overturning Circulation that triggered a decline in ocean carbon storage.

This study presents interesting results and is a valuable contribution to the field complementing earlier studies investigating the response of CO<sub>2</sub> and d13C to freshwater

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hosing experiments. I like the figures, the model analysis and the compilation of the d13C data. The study, however, does not consider the sequence of AMOC collapse and recovery nor takes into account the LGM initial state as done by earlier studies (e.g. (Menviel et al., 2008a; Marchal et al., 1999)).

The manuscript requires substantial revisions regarding the interpretation of the results and of the main conclusion. This will require a revision of the abstract, the introduction and the discussion section and I also suggest that additional simulations are performed.

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. 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.

In addition, the range of studies on the potential influence of SH wind changes on ocean circulation and the carbon cycle is not very well reflected.

1. Main conclusions are to be refined:

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.

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. This mismatch is reflected in the too large

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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)

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.

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.

1.2) Various proxy data and studies indicate a distinct evolution of the North Atlantic ventilation and the AMOC. For example, (Robinson et al., 2005) document the radiocarbon history in the western North Atlantic and show that ventilation strength varied from LGM, H1, Bolling/Allerod, Younger Drias and the Holocene. The AMOC recovered at the onset of the Bolling/Allerod (McManus et al., 2004). This may have reversed earlier changes in  $\text{CO}_2$  associated with an AMOC slow-down. However, this sequence is not reflected by the model and not discussed by the authors.

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  $\delta^{13}\text{C}$  changes not only during H1 but over the period from H1 to the end of the B/A.

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

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of AMOC changes on atmospheric CO<sub>2</sub> and/or δ<sup>13</sup>C 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))

### 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" 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?

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