

We thank the anonymous referee#1 for his/her constructive comments which help to improve our paper. Below, we reply to the comments made by the referee#1 point by point. The responses are constructed as follows (1) original comments from the referee in **bold**, (2) our response in *italics*, and (3) description of changes applied in the revised manuscript in [blue](#).

**Page 4, lines 1-8: Very little information is given about how the preformed O<sub>2</sub>, TALK, and PO<sub>4</sub> tracers are calculated by the model. Are they independent from one another? I think a short (one-two sentences) description should be given.**

*Response: We have added the following statement.*

[Revision in Sect. 2.1: "At surface, these preformed tracers are set to their non-preformed value and are advected passively by the ocean circulation in the interior without any other sources and sinks."](#)

**Page 8, lines 20-25: The experiment on the warmer condition produces lower global DIC. But since atmospheric CO<sub>2</sub> is kept constant in each experiment, where is the "missing" carbon of the warmer experiment? Does it get buried by the sediment model? Or does the 125 ka experiment have lower total carbon in all its components? This should be described in the results section.**

*Response: The difference in the sediment carbon is about  $6 \cdot 10^{-4}$  PgC between 125ka and 115ka, which is negligible compared to the difference in DIC<sup>tot</sup> (314 Pg C). The equilibrium simulations with fixed GHG and orbital forcings performed in our study are based on standard protocols (e.g. PMIP-paleoclimate Model Intercomparison Project) to study the climate or ocean circulation responses at certain periods in the past (Weber et al., 2007; Van Meerbeeck et al., 2009). When the ocean carbon cycle is included in such simulations, as in our study, it is implicitly assumed that the residual of atmosphere and ocean carbon budgets is offset by changes in the terrestrial reservoir.*

[Revision in Sect. 3.4: "Since the atmospheric CO<sub>2</sub> is kept constant in each experiment, this carbon loss in 125ka compared to 115ka is implicitly assumed to be balanced out by the changes in the land carbon reservoir. "](#)

**Page 8, lines starting at 20: I like your detailed analysis of each of the ocean carbon components. I am surprised that you do not link changes in disequilibrium DIC to Southern Ocean sea ice changes. Is this not the case? Please explain briefly in the article.**

*Response: Changes in DIC<sub>dis</sub> is complex as it can be affected by many different factors such as changes in the physical pump, overturning circulation or the biological pump. No general process could be attributed to its variation which seems to be regionally affected. In the Atlantic bottom water it is mainly driven by the sea-ice-induced NSW/SSW water masses re-organization. However, near surface changes remain relatively unexplained.*

[Revision in Sect. 5: However, the processes affecting the disequilibrium component can arise from](#)

different factors such as changes in the physical pump, overturning circulation or biological pump. No general process could be attributed to its variation which seems to be regionally affected. While the SSW seem to become more undersaturated in 125ka, the NSW seem to be more saturated. Further experiments with for instance fixed biological productivity or overturning circulation could help to identify the sensitivity of this component to such factors, but remain too expensive to perform with our model.

**Page 11, lines starting at 7: The authors interpret their results in terms of  $\delta^{13}\text{C}$  in order to compare to sediment core data from the period of study. However, since their model does not directly calculate this tracer, the analysis is subject to misinterpretations. The interpretation of changes in water mass geometry to explain the changes in mid-depth and bottom  $\delta^{13}\text{C}$  from sediment cores is plausible. However, is that the only explanation possible? Wouldn't a change in export production affect  $\delta^{13}\text{C}$  in a similar way (see Schmittner and Somes, 2016, *Paleoceanography*)?**

*Response: Indeed, bottom water  $\delta^{13}\text{C}$  can be influenced by several factors including changes to export production and local nutrient cycling. However, it is unlikely that export productivity changes alone caused the mid-depth and bottom  $\delta^{13}\text{C}$  changes. Export productivity reconstructions from the Site 1089 region show no discernible change over the time interval when Site 1089 bottom water  $\delta^{13}\text{C}$  decreased (e.g., Martínez-García et al., 2014, *Science*), while the magnitude of the Site 1089 bottom water  $\delta^{13}\text{C}$  decrease ( $\sim 0.4\%$ ) indicates a considerable nutrient concentration increase (following scaling in e.g. Eide et al., 2017, *Global Biogeochemical Cycles*). The character and spatial pattern of bottom water  $\delta^{13}\text{C}$  changes in the wider South Atlantic region also indicate that, on millennial timescales, bottom water  $\delta^{13}\text{C}$  variability in this region is most easily explained by water mass geometry changes (Oppo and Fairbanks, 1987, *Earth and Planetary Science Letters*; Charles and Fairbanks, 1992, *Nature*; Ninnemann et al., 1999, *Global and Planetary Change*), including for the LIG time interval we discuss in the manuscript (Govin et al., 2009, *Paleoceanography*). For example, they occur independently of the (core-specific) surface ocean nutrient regime (Ninnemann & Charles, 2002, *Earth and Planetary Science Letters*).*

*After careful consideration of all referees suggestions, we shortened the section describing precisely the changes in  $\delta^{13}\text{C}$  and refer exclusively to the literature instead. Therefore, we also removed the previous Fig. 9 in order to keep our study clear and avoid confusion.*

Revision in Sect. 4: "Similar to our results, expanded SSW in the late compared to early LIG is inferred from such reconstructions to explain bottom water  $\delta^{13}\text{C}$  decreases in different regions proximal to the Southern Ocean (Govin et al., 2009; Ninnemann et al., 2002). Bottom water  $\delta^{13}\text{C}$  reconstructions indicate less influence of NSW in the deep South Atlantic at 115ka than at 125ka (Ninnemann et al., 1999; Govin et al., 2009)."

**Also, you talk about the "divergence" between cores JPC8 and Site 983 and Site 1089, it seems**

**that although you are able to explain Site 1089, the increase in  $\delta^{13}C$  in the North Atlantic cores remains unexplained. Do your simulations agree with the two mid-depth North Atlantic cores that show an increasing trend in  $\delta^{13}C$ ? If yes, mention in the paper. If not, explain what the missing process could be.**

*Response: The increasing  $\delta^{13}C$  in the mid-depth North Atlantic most likely reflects an increase in the preformed  $\delta^{13}C$  (Fronval et al., 1998). This increase cannot be clearly assessed with our model as it does not include  $\delta^{13}C$ , for example. However, persistent vigorous AMOC ventilating the mid-depth Atlantic Ocean is simulated in both experiments, consistent with proxy reconstructions. Following suggestions by referees #1 and #3, we have added more references to both proxy and model studies showing this.*

Revision in Sect. 4: "In addition, persistent (millennial-scale) mid-depth NSW ventilation extending from the LIG and into the subsequent glacial inception is also suggested based on proxy reconstructions (McManus et al., 2002; Mokeddem et al., 2014) and is consistent with model simulations (Born et al., 2011; Wang et al., 2002). In our study, even though the AMOC is simulated slightly stronger and deeper in 125ka, vigorous AMOC persists during 115ka ventilating the North Atlantic mid-depth. We also note that our model may not properly represent the North Atlantic overflows due to its sparse resolution. This can further add uncertainties to North Atlantic water ventilation. "

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