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Climate of the Past Discussions

Interactive comment on "Fingerprints of changes in the terrestrial carbon cycle in response to large reorganizations in ocean circulation" *by* A. Bozbiyik et al.

Anonymous Referee #1

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Review of:

Bozbiyik et al. [2010]

"Fingerprints of changes in the terrestrial carbon cycle in response to large reorganizations in ocean circulation"

The manuscripts describes global climate and carbon cycle responses to freshwater (FW) pulses applied in the North Atlantic and the Southern Ocean. The authors provide an analysis of how different carbon cycle compartments interact with a focus on the low latitude vegetation feedbacks to changes in precipitation and surface air temperature (SAT). The authors suggest a 20 ppmv increase in atmospheric CO2 mainly driven by

vegetation loss in northern South America in response to 1Sv FW-perturbation applied over 100 years, while changes in atmospheric CO2 caused by Southern Ocean FW-perturbations are small.

Overall evaluation:

The experiments conducted in the manuscript are relevant for an in-depth understanding of observed past climate changes. The results that are presented are interesting and meaningful for a broad scope of readers. Thus, I believe that the manuscript will be publishable after some major revisions. My major point of criticism arises from a lack of discussion of the results with respect to the studies of Laurie Menviel and her co-authors. Furthermore, some crucial features of the results and of the mechanisms behind them are not explained and/or discussed sufficiently in the manuscript. In the current version, the paper does not nearly tap its full potential.

Major points:

1. Two recent publications of L. Menviel and co-authors deal with the carbon cycle response to FW-forcing applied in the North Atlantic and Southern Ocean respectively. (Menviel et al. [2008, Paleoceanography] and Menviel et al. [2010, Paleoceanography, in press]) While the second paper was accepted only 2 weeks ago, I do not understand why there is not a single reference to her first study. (This is in particular surprising, because -according to the University of Bern webpage- both of the corresponding authors seem to work in the same building.) The experiments are very similar and the results are as well to some extent. Furthermore the Menviel et al. [2008] study also covers LGM background climate conditions which was not feasible for the present manuscript.

In their revised version the authors need to present a detailed comparison of their results to the findings of Menviel et al. [2008, Paleoceanography], also with respect to how a different background climate and a different FW-pulse can affect the simulated carbon cycle response.

2. Given the fact that ~50% of the CO2 response is driven by northern South America, the authors do not provide a sufficient explanation of the physical mechanisms that lead to the observed patterns and magnitudes in SAT and precipitation anomalies. I appreciate the presentation of paleo-climate reconstructions in section 3.4. However, they cannot verify the magnitude of the simulated response.

Is it the trade wind response that leads to the warming of the southern tropical Atlantic? A figure of the wind anomalies would be very helpful. What causes the temperature dipole between northern South America and the adjacent Atlantic? Over most of the lower latitude Atlantic region we see a clear correlation between changes in SAT and precipitation (warmer SAT <> more precipitation and vice versa). What causes the decoupling over northern South America.?

In the revised version the authors need to provide a detailed analysis that allows for a better understanding of to what extent this feature might be model specific.

3. The effects of the Southern Ocean FW-perturbations also need to be analysed and explained in more detail. The stability of the West-Antarctic Ice Sheet with respect to global warming and sea level rise is currently a hot debate. Thus, understanding the mechanisms behind the changes triggered by a weakening of bottom water production in the Southern Ocean is very important.

A few questions that definitely need to be addressed in this context:

What drives the global SAT response of 1.0Ros and 1.0Wed? Is it the strength of the AABW formation? At least a timeseries of AABW would be desirable.

What drives the differences in the SAT response of 1.0Ros and 1.0Wed? The authors speculate very briefly that the differences are driven by the fact that the Weddell Sea is located downstream of the Ross Sea. The AMOC response shows a similar dilution effect for both experiments which puts a question mark on this explanation. The differences should be investigated with respect to the Southern Ocean sinking regions in

the model.

I highly recommend to discuss the results of the Southern Ocean FW-perturbations with respect to the findings of Menviel et al. [2010, Paleoceanography, in press]

4. Figure 1 shows a decoupling of SAT and AMOC strength for experiment 1.0NA. While the AMOC remains low after the end of the FW input, SAT suddenly jumps up and decreases again. This is a very puzzling feature that needs to be analysed. Also it looks like that for 0.5NA SAT starts to recover before the AMOC does.

5. A couple of recent studies (e.g. Schmittner et al. [2007, Paleoceanography], Schmittner & Galbraith [2008, Nature], Okazaki et al. [2010, Science]) show significant changes in ocean ventilation in response to FW-forcing in the North Atlantic. Do the authors find similar features in their experiments? For example, Okazaki et al. [2010, Science] found North Pacific deep water formation as a result of a Heinrich event. However, the flux correction used by the atmospheric model in this study (EC-Bilt) might contribute to an artificial increase of salinity in the North Pacific. The present manuscript has a much more sophisticated atmospheric component (also compared to the one used by A. Schmittner). Thus it could do a great job in deriving more reliable results in the context of atmospheric teleconnections triggered by an AMOC shutdown.

Minor points:

page 1815 / line 8 Could the authors provide references for "have been shown to be plausible ..."

page 1815 / line 22 "support their conclusions" should be "support the conclusions"

page 1815 / line 25 The kudos of presenting a "novel feature" is shared with Menviel et al. [2010, Paleoceanography, in press]

page 1816 / line 17 I am just curios what vertical diffusivity parameterization is used in the modified version?

page 1820 / line 24 "(Fig. 2, top row)" should be "(Fig.2 left column)" same for page 1821 / line 23

section 3.3.1 It would actually be interesting to see the evolution of the carbon stocks for the experiments 0.3NA and 0.5NA. At least for 0.3NA one could see how they evolve after the AMOC recovery.

page 1823 / line 26 ... Is Figure 4 really needed? More words are used to describe what it "cannot capture".

page 1824 / line 13 ... Could the authors mention the individual contributions of gas-exchange, primary production, expansion of DIC-rich AABW, ... to the increased ocean carbon stock in the North Atlantic?

page 1825 / line 7 How was the linear regression calculated? Using the control run or the FW-perturbation run?

Figure 1b. SAT is given in C not in K

Figure 5. The manuscript only refers to changes in carbon stocks in the North Atlantic. What causes the changes in carbon stocks in the other oceans?

Figure 9. Why do we see a decoupling of SAT and precipitation right after the FW-forcing was stopped for 1.0NA? This has probably to do with my major point #4.

Interactive comment on Clim. Past Discuss., 6, 1811, 2010.