

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

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**We thank all the reviewers for their extensive and constructive comments. Below given is a detailed response (bold/italic font) to the issues raised by anonymous referee 3 (normal font).**

- A more significant problem, and my primary concern with the paper, is the relevance of the model boundary conditions to the paleoclimate events under consideration. The simulations were carried out under pre-industrial modern conditions, but the results are taken to be representative of the effects that would be expected during the heart

of the last glacial period and last deglaciation. In the conclusions section it is acknowledged that due to this discrepancy the standing state of the vegetation was different than in the model simulation, and that this may modulate the magnitude of any carbon cycle changes. But more than this the biome distributions and climate tolerances were different in many regions, global pCO<sub>2</sub> levels were 40 – 80 ppmv lower than in the simulation, and global temperature and precipitation distributions were different. To what extent, then, is it appropriate to assert that the coupled system behavior under these very different boundary conditions can be approximated from the simulations?

**It is indeed true that a shortcoming of our study is the fact that we do not have experiments done under glacial boundary conditions in order to provide an exact comparison with paleo-reconstructions. Unfortunately, we do not have the resources necessary to repeat the experiments with different boundary conditions.**

**In the revised paper however, we have addressed this issue by comparing our results to a previous study done by Menviel et al. (2008). In that study, they perform similar freshwater hosing experiments with an earth system model of intermediate complexity under both pre-industrial and glacial boundary conditions. The results are very similar in terms of the distribution of changes in precipitation and the land biosphere. On page 1830 after line 5, we contemplate the issue as follows:**

*In a previous study (Menviel et al. , 2008), in which the experiments were done under both pre-industrial and glacial boundary conditions, it has been shown that the differences in the amplitudes of the individual contributions from the land and ocean carbon pools may lead to an opposite net effect on the atmospheric CO<sub>2</sub>, even though the nature of each contribution is qualitatively the same. In their study, irrespective of the initial state, the roles of the ocean as a carbon sink and of the land as a carbon source remain unchanged. Also, the changes on land are very similar in both cases, that is, a*

*reduction in the carbon stocks in the high and mid latitudes of the Northern Hemisphere and in the tropics north of the equator and an increase to the south. Yet, the emissions from land under glacial conditions are weaker than under the pre-industrial conditions. That is probably due to the lower moisture content of the glacial atmosphere, which leads to the dampening of the effects of the ITCZ shift (Menviel et al. , 2008), and relatively large gains in primary production in some regions such as eastern Asia and southern North America. Compared to our experiments, the main differences are the larger increase in carbon stocks in the Southern Hemisphere and the above mentioned regions in the north, which might be due to some model specific differences as well as the initial conditions.*

*Nevertheless, as the patterns of the anomalies are very similar in both cases, it is safe to assume that our results are relevant for paleo-reconstructions ...*

Specific and technical points:

-Page 1815, Lines 8-14: Please clarify these statements. Also “stimulating” not simulating”

**Text is modified as follows:**

*There are two different ideas regarding the source of this increase, that is either the ocean or the change of vegetation cover on land. While some modeling experiments have suggested that this atmospheric CO<sub>2</sub> increase was due to an oceanic release of carbon (Marchal et al., 1999; Schmittner and Galbraith, 2008), others suggest that it was due to a land carbon release (Koehler et al., 2005; Obata, 2007; Menviel et al. , 2008). Ocean outgassing, for instance, can explain the increasing atmospheric CO<sub>2</sub> levels if the cooling of the sea surface is constrained to the high northern latitudes, the warming in the Southern Ocean is more pronounced and the contribution from land is not taken into account (Marchal et al., 1999). (Schmittner and Galbraith, 2008) also identified the ocean as the source of the atmospheric CO<sub>2</sub> increase during abrupt*

*climate change events. Due to the absence of a complex atmospheric component, however, their model is probably limited in representing tropical precipitation changes that have a potentially large impact on the land biosphere.*

- Page 1823, Lines 23 – 28: This is quite vague. . .what is the expected atmosphere/ocean partitioning? You've drastically slowed the export of excess carbon to the deep ocean via downwelling? This shuts off a large part of your oceanic carbon sink, leaving you with approximately equal uptake capacity in the atmosphere and surface ocean. Indeed this is what you see at the global scale – subequal increases in the marine and atmospheric carbon stocks w/ a slight lag reflecting transfer from the atmosphere to ocean. Is the marine increase localized in the surface ocean?

**For the mentioned experiments, 1.0Ros and 1.0Wed, the land biosphere is actually a net sink of carbon. So, there is no excess carbon emitted to the atmosphere, hence no carbon to be taken up by the atmosphere/ocean. Both the land and ocean gain carbon at the expense of the atmosphere.**

- Page 1824, Lines 3-6: I'm not sure that this statement is quite right. . .see (Beer et al., 2010) for example. . .many of your 'big change' ecosystems are probably not precipitation-limited today.

**The statement is replaced by the following:**

*This is not surprising given the fact that, as a result of an MOC shutdown, in the higher latitudes temperature anomalies are larger than precipitation anomalies and the lower latitudes experience substantial changes in precipitation due to the shift of the ITCZ and the exponential dependence of saturated water vapor pressure to temperature. Hence, small temperature changes translate into large differences in precipitation in the tropics where the ambient temperature is higher (Bard, 2002). Nevertheless, globally, precipitation anomalies are responsible for most of the change in the carbon*

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*stocks on land as most of the carbon emissions stem from the large vegetation pool of the low latitudes.*

- Page 1826, Line 5: Your model doesn't include dynamic vegetation, so the origin of this statement is unclear. Please clarify or re-cast this statement.

**The statement is re-phrased. Now, it reads as:**

*The driving factors of this substantial change are increasing temperature and reduced precipitation, the combined effect of which is the transformation of one of the wettest climates on land into an arid desert-like climate unable to sustain the carbon-rich rain-forest type vegetation.*

- Page 1827, Line 11: The table only shows results from 1 model, so it doesn't really illustrate the point made here.

**It has been re-phrased as follow:**

*A comparison of several paleoclimate reconstructions of precipitation anomalies during the Younger Dryas period (Table 3) with our model results shows a good agreement between proxy records and the 1.0NA experiment responses in most locations (Fig. 11), which supports to the robustness of the ITCZ-shift hypothesis and the existence of a dipole relation between the north of the continent and the eastern and southern Brazil ...*

- Page 1828, Lines 16-18: This statement is not straight forward and invokes a mechanism that is entirely distinct from the shift in ITCZ position discussed throughout the rest of the manuscript. This should be discussed in the body of the ms or the conclusion adjusted to focus on the mechanisms examined here.

**We removed that statement from the conclusion. It is now mentioned in the**

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**section 3.3.2, on page 1824, after line 3:**

*This is not surprising given the fact that, as a result of an MOC shutdown, in the higher latitudes temperature anomalies are larger than precipitation anomalies and the lower latitudes experience substantial changes in precipitation due to the shift of the ITCZ and the exponential dependence of saturated water vapor pressure to temperature because of which small temperature changes translate into large differences in precipitation in the tropics where the ambient temperature is higher (Bard, 2002).*

- Table 3 caption: Specify that the model results are derived from the 1.0 NA case.

**Done.**

- Page 1844, Caption 2: “statistical significance is more than 1 sigma (more than 67% confidence) according to the Student’s t-Test”confuses concepts. Please improve the description of the statistical measure, e.g., “...ensemble mean was different from zero at the 67% confidence level (Student’s t-Test).”Same in figures 5, 6 and 8. **Changed accordingly. Now it reads:**

*... stippled areas indicate where the ensemble mean anomaly is significantly different from zero at the 67% confidence level (Student’s t-Test).*

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