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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.

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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 Henry Hooghiemstra, Referee 2 (normal font).

- p. 1821, line 30: The conclusion that "Northern Atlantic perturbations cause a southward shift of the ITCZ as a result of cooling in the Northern hemisphere, whereas perturbations from the Southern Ocean cause a northward shift" (p. 1821/1822) is a very relevant conclusion. This paper does not make clear how these ocean-driven latitudinal migrations are related to the precession-driven latitudinal migrations of the

ITCZ.

Ocean driven latitudinal migrations of the ITCZ that are related to the millennial-scale abrupt climate change events take place in a different time-scale than the long-term precession-driven migrations of the ITCZ, which have a time-scale of 100 kyr. The climatic changes they induce are also about an order of magnitude larger than those induced by the precessional forcing (Clement et al., 2004). In our study, the time-scales of the experiments we perform are too small to consider precessional changes.

Minor comments:

- p. 1813, line 2: 16K = 16°K

Changed to $^{\circ}C$.

- p. 1813, line 13: 12.7-11.6kyrBP = 12.7-11.6 cal kyr BP (I assume this paper has calibrated ages)

Done.

- p. 1819: why is the period of the YD here roughly indicated in comparison to page 1813 ?

Changed to 12.7-11.6 cal kyr BP as in page 1813.

- p. 1825, lines 2-4: I have problems with considering soils as an important dynamic sink for carbon. Of course, soils store a significant volume of carbon. But on the long-term of a glacial-interglacial cycle the storing capacity will not be ever increasing. Looking at soil development over a glacial-interglacial cycle, soil carbon content

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is maximal near the end of an interglacial period. However, the actual carbon storage capacity of soils as claimed in many papers, to me seems the potential capacity created by deforestation during the last centuries. Thus, deforestation and land degradation have created an artificial storage capacity which is not sustainable in a long-term perspective.

The soil carbon pool in our model is not taken to be a dynamic sink for carbon. Depending on the conditions, soil can emit or store carbon, hence it is neither a sink or source in the absolute sense.

- p. 1826, line 7 vs. line 26: Fig 9 has been introduced in the text before Fig. 8. This seems not correct.

Figure 8 is introduced now at page 1826 line 1:

The net change in the total carbon stocks in the northern part of South America (the region marked in Fig. 8, top left) ...

- p. 1827, line 16; the dipole relation was also elaborated by Martin et al. Quaternary Research 47, 117-122 (1997).

Reference included at page 1827, line 17.

- p 1834, line 28: this reference is missing text (which journal?)

Corrected.

- p. 1837, line 1: The correct abbreviation of this journal is: Palaeogeogr. Palaeoclimatol. Palaeoecol.

Corrected.

References

Clement, A.C., Hall, A., and Broccoli, A. J.: The importance of precessional signals in the tropical climate, Clim. Dynam., 22, 327–341, 2004.

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