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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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This paper focus on determining the dependence of the climate and carbon cycle to freshwater perturbations. It tries to identify fingerprints of responses by using an atmosphere-ocean global circulationmodel coupled with a land surface model. Model results are compared with paleo-records in South America and this aspect must be the reason to ask me as a reviewer. I cannot really judge the model aspects, which cover the largest part of this paper. I expect the assessment of this part of the paper will be covered a another reviewer.

p. 1821, line 30: The conclusion that "Northern Atlantic perturbations cause a south-

ward shift of the ITCZ as a result of coolling 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 de precession-driven latitudinal migrations of the ITCZ.

Minor comments:

p. 1813, line 2: 16K = 16˚K ?

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

p. 1819: why is the period of the YD here roughly indicated in comparison to 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 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 arteficial storage capacity which is not sustainable in a long-term perspective.

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

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

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

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

Palaeoecol.

The conclusions of this model simulation are important for a better understanding of climate variability in the (sub)tropics. It is helpful to arrive at better sinterpretation of paleo-records at (sub)tropical latitudes. Apart from the precession-driven ITCZ migrations, also shifts in SST appear a driver of ITCZ migrations. I like to suggest to the authors to clarify better the relationship between both drivers.

The text is clear and concise. Title and Abstract are informative. Figures and tables are well done and relevant. The results have been well checked against paleo-data. The results fit well the scope of this journal and are of interest for a wide international audience. In conclusion, my impression is this paper shows a fine and well presented modeling study. This paper makes important conclusions on the mechanisms behind ITCZ migrations and, therefore, contributes also significantly to further explanations of pollen-based record of climate change at low latitudes. I advise publication after minor revision.

Henry Hooghiemstra, Amsterdam, 9-11 November 2010

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