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Climate of the Past Discussions

Interactive comment on "Impact of oceanic processes on the carbon cycle during the last termination" *by* N. Bouttes et al.

Anonymous Referee #2

Received and published: 24 August 2011

The manuscript describes simulations of the last deglaciation using a coupled climate-carbon cycle model. The authors extend their earlier work on the LGM equilibrium change to transient model experiments. Because they have shown earlier that the combination of enhanced salty bottom water formation around Antarctica (the authors suggest it is related to brine release), a stratification dependent vertical diffusivity and iron fertilization can reproduce glacial atmospheric CO2 and d13C in the deep South Atlantic it may not be a big surprise that in transient simulations, in which these three processes change from LGM to early Holocene, the long-term change of observed atmospheric CO2 and deep South Atlantic d13C can be reproduced. Nevertheless, I think the work contributes to the discussion of glacial-interglacial CO2 change and is well suited for publication in Climate of the Past.

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Interactive Discussion
Discussion Paper

I have one major point and several minor ones.

The major point is related to the brine scenario schematically shown in Fig. 7. I think it is important to realize, and should be made clearer in the manuscript e.g. in the abstract, that this is a hypothetical mechanism. This is not a process that is interactively simulated in the model, but an ad hoc manipulation of deep-water formation in the Southern Ocean. From Fig. 7 I would assume that the deglaciation is a transition from the bottom panel to the top, so that at the glacial maximum (bottom panel) there is no brine water formation, during the deglaciation (middle panel) there is lots of brine water formation and in the Holocene there is little. Thus there should be a peak in brine water formation during the deglaciation, when the shelf is partially flooded. But this sequence of events is inconsistent with the scenario used (Fig. 6), in which bottom water formation is strong during the glacial maximum and decreases during the transition into the Holocene.

Minor points:

Page 1888, line 12: use “coupled climate-carbon”

P 1888, l 24: include “long-term” before “CO2”

P 1888, l 25: what data? Specify!

P 1889, l 28: The terrestrial biosphere decreases on areas of the continental shelf that become flooded. Cite Montenegro et al. 2006 GRL.

P 1891, l 26: here and elsewhere put a comma in front of “which”

P 1892, subsection 2.2.1: It should be discussed that this is also a highly idealized ad-hoc treatment of iron fertilization. No iron cycle is used and no realistic dust fluxes.

P 1893: subsection title: it should be “dependent”

I’m missing a description of the d13C model. Please include details such as treatment of biological fractionation in the ocean and on land. Is there a distinction between C3

CPD

7, C1309–C1312, 2011

and C4 plants?

I'm also missing a discussion of the possibility of a larger fixed nitrogen inventory during the LGM. On page 1894 it is mentioned that nutrient concentrations are increased by 3.3%, but NO3 may have been much more increased because denitrification has been much smaller.

The discussion of the "Evolution of the forcing" does not mention if Bering Strait is opening during the deglacial or not.

P 1900, l 27: insolation does not vary according to proxy data

P 1901, l 8: how exactly is the iron fertilization related to the dust record? Include formula.

P 1901, l 20: Increased AABW formation due to more sea ice cover at the LGM has been simulated before e.g. in Schmittner (2003, EPSL), which could be cited here.

P 1901, l 28-29: Why is mixing more important in the open ocean. Please include references.

P 1902, l 23: It would be nice if the authors quantified the effect of the 3.3% nutrient increase on glacial CO2.

P 1902, l 26: How was the 650 GtC determined? Was the shelf effect included? (see Montenegro paper mentioned above)

P 1903, l 13: I find it difficult to assess the reliability of the modeled calcium carbonate compensation effect. E.g. could it be sensitive to the details of the circulation that are missing in the ocean model component or is this a rather robust result? Are there modern observations, e.g. percent carbonate in sediments, that the model could or has already been compared to? Perhaps a discussion on this may be helpful.

P 1908, l 4: "considered"

CPD

7, C1309–C1312, 2011

P 1909, l 19: should “K_z” be “alpha”?

P 1910: There should be a discussion of possible reasons for the maximum in deep ocean d13C, which is not captured by any of the models.

References: What are the numbers at the end of the references? Figure 5: Caption: The last sentence makes no sense. There is no circulation from south to north.

Figures 8, 10 and 11 should be bigger.

Fig. 14: include atmospheric C13. Also, a run without carbonate compensation would be informative.

Adding to point 2 of referee #1: Was radiative forcing of dust included? (see Mahowald et al. 2006 GRL)

Interactive comment on Clim. Past Discuss., 7, 1887, 2011.