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Climate of the Past Discussions

Interactive comment on “Glacial CO₂ cycle as a succession of key physical and biogeochemical processes” *by* V. Brovkin et al.

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The authors’ response (in *Italics*) to comments by the Reviewer 1

The manuscript entitled “Glacial CO2 cycle as a succession of key physical and biogeochemical processes” by Brovkin et al. Simulates changes in the carbon cycle over the last glacial inception and deglaciation with a model of intermediate complexity CLIMBER 2. The model is forced with changes in orbital parameters and radiative forcing due to CO2, CH4 and N2O. The model is coupled to an ice-sheet model which simulates about 110m lower sea level at the LGM. The processes thus taken into account in the study are temperature, salinity, circulation changes as well as responses of the ocean-sediment system, iron fertilization and land carbon uptake/release. It is an interesting study, worth publishing in Climate of the Past, as transient simulations of the carbon system since the last interglacial have not been really performed before and can provide additional information on the behavior of the marine carbon cycle.

However I think that some elements are missing for a good understanding of the carbon system in these transient simulations. Mainly, land carbon changes and the weathering variations imposed in these simulations really have to be described in more details if not shown for the paper to be publishable (see below).

We thank the reviewer for very useful comments. In the revised manuscript, we will describe in more details changes in land carbon storages and weathering fluxes.

1) CLIMBER-2 comprises a simple vegetation model and the carbon changes in this terrestrial model are taken into account in experiment PCBL. However it is just mentioned in the text that the land carbon stock at pre-industrial times is lower (-200 GtC) than at the last interglacial. The total glacial/interglacial change in carbon stock is never mentioned. As land carbon changes have a significant impact on atmospheric CO2 and the marine carbon system (d13Cdic), the authors should give more information about the land carbon changes and eventually show the evolution on a plot. In addition a difference of 200 GtC between 125ka B.P. And 0 ka B.P. seems quite large, the authors could comment on that.

The total glacial/interglacial changes in land carbon cycle are about 600 GtC (between a maximum at MIS5 and a minimum at LGM). The equilibrium land carbon changes in CLIMBER between LGM and pre-industrial were discussed in Brovkin et al. (2002) and Brovkin et al. (2007). As suggested by the reviewer, we will provide plots of land carbon storage changes over the cycle. Let us note that the new development in the land carbon modeling is to include dynamics of carbon stored in peatlands (see Kleinen et al., 2011) and the permafrost regions (e.g., Schneider et al., 2011). We will discuss this in more details in the revised paper.

2)The model is apparently forced by changes in weathering. These changes in weathering are scaled by the changes in runoff. Weathering changes can have a non neg-

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ligible effect on the marine carbon system. It is therefore also important to at least state in the text if not show the exact weathering changes applied: the direction, the amplitude... also did silicate and carbonate weathering changes equally? I know that G/IG weathering changes are very uncertain and that estimates of carbonate weathering changes go in all direction. I do not contest how it is implemented in the model but I think that the reader really needs to have more details on this.

Silicate and carbonate weathering is calculated separately in the model. As suggested, we will plot the weathering fluxes and discuss their dynamics in more details in the revised paper.

3)You briefly mention changes in export production in the text. Again it might be nice to have a

We will plot changes in the global export production as suggested.

4)I appreciate the separation of factors and the fact that the 4 simulations (P, PC, PCB, PCBL) are shown in figure 2. However for clarity, it would be nice if the different contributions of temperature, salinity, circulation, CaCO3 compensation, iron fertilization to the glacial CO2 drawdown were clearly stated in the text. In addition, the authors could comment on the impact of sediment processes on atmospheric CO2.

It is difficult to disentangle contributions of temperature, salinity and circulation in the P simulation because these factors are tightly liked to each other. We tried to separate SST and circulation effects based on time scale of these processes in the LGM equilibrium experiments (Brovkin et al., 2007). We have decided not to present results of PC, PCB, and PCBL simulations without CaCO3 compensation since it is difficult to interpret these results.

5) On fig. 4. the [CO3] changes obtained are quite high. The authors go quite fast at discussing their results compared to paleoproxies. In the Equatorial Pacific, Yu core GGC48 displays in agreement with proxies about 15umol/L change. However core

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GGC15 displays no change. How are the changes at high latitude? Higher or smaller than the ones showed for 30S:30N? I would expect the Southern Ocean changes to be even higher. Rickaby 2010 find a âĹij45 umol/L changes in the Weddell Sea, which is usually described as a "upper limit" change (Zeebe and Marchitto, 2010, Nat. Geosciences).

As explained in more details in the reply to Andy Ridgwell's comments, we will extend the discussion of simulated [CO3] changes and their comparison to [CO3] proxies.

In addition p1777, L4-7 do not seem correct. As seen on fig 4. [CO3] in both the Atlantic and Pacific decrease the deglaciation. In both the Atlantic and Pacific there is high [CO3] content at 10 ka B.P.

By the phrase "Contrary to the Pacific, [CO3] in Atlantic decreases during deglaciation as a shutdown of the Atlantic meridional overturning circulation brings in Antarctic bottom waters characterized by lower [CO3]" we meant a strong difference in Atlantic and Pacific response to the AMOC shutdown which was in the simulation before 10 ka B.P. By 10 kyr BP, the AMOC was restored and [CO3] concentration increased again.

6) About millennial-scale changes in pCO2 You mention L27 that the rise in pCO2 (10-20ppmv) during millennial-scale AMOC shut down is mainly due to DIC decrease between 1 and 3 km in the Indo-Pacific region. Is it over the whole region or centered into a more specific area? You suggest this is due to the weakening of the reverse cell of the Indo-Pacific overturning circulation by 2Sv. First I am a little surprised that just a 2Sv change leads to a 10ppm CO2 rise. Then could you please precise which water mass you are talking about. Are you saying that the AAIW weakens by 2Sv? L. 6, p1782, you could also cite Obata et al 2007 (j. Clim, 20)

The lowering of the DIC concentration at the 1-3 km depth in the Indo-Pacific during simulated Heinrich events occurs over a very large area. We would not interpret this circulation change as AAIW weakening but rather as a change in the global-scale upwelling of the deep water masses. We cannot say whether such sensitivity of CO2 to

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the global upwelling is reasonable or not but we will address this issue in more details in the revised paper. We will cite Obata (2007). Thank you for bringing this paper to our attention.

Minor point: fig1 a is not so useful, changes in land carbon and weathering would be much more informative.

We will add figures on changes in land carbon and weathering. We prefer to keep the Fig. 1 as it is as an illustration of applied external forcings.

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