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Climate of the Past Discussions

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Interactive Comment

Interactive comment on “The carbon cycle during the Mid Pleistocene Transition: the Southern Ocean Decoupling Hypothesis” *by* P. Köhler and R. Bintanja

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The two referees, David Archer and Kai Schulz, pointed to individual parts of the MS where some clarification and some more text might be needed. We agree with most parts of both reviews and feel able to satisfactory reply to all specific comments below. We furthermore will implement the content of this discussion in the revised version of the MS accordingly.

The comments of the referees and our replies are in detail:

1. Reply to comments of David Archer

   (a) *Where comes the statement from that "the factorial analysis of the model results has shown that the Southern Ocean is responsible for the different responses of the 40k and the 100k worlds"?*

   This statement comes from the analysis of Fig. 6 of the MS, in which in sub-figures C and E the contribution of individual processes to changes in both atmospheric CO_2 and deep Pacific δ^{13}C are analysed. We extend in Section 3.3 how we came to this conclusion in greater detail and discuss a little bit longer what can be seen and learnt from Fig. 6.

   (b) *Box models versus continuum models; box model artefact; models at hand for long time scales; caveats of box models*

   David Archer points out that box models and continuum models behave in principle different in some aspects, and that box models especially invoke processes in the high latitudes to be responsible for observed changes in the carbon cycle. He furthermore suggests, that our proposed hypothesis focusing on the Southern Ocean might therefore be more a box model artefact than a possibility for the real ocean. He nevertheless agrees, that for these long time scale no other models than box models can be applied. Finally, he asks for more informations on the caveats to the known bias of box ocean carbon cycle model.

   We agree with the referee about the differences between simple box models and more complex carbon cycle models and also think, that for these long time scales no other models than box model are available. Therefore, an additional part of the discussion is focusing on the general performance of BICYCLE. Special focus here is on the caveats in box models.

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However, we like to point to the results of a recent paper published after the submission of our MS (Marinov et al., 2008), who finally state that they "contradict the findings of Broecker et al. (1999) and Archer et al. (2000) who suggested that vertical diffusion acts to break the monopoly of high latitudes in determining atmospheric pCO$_2$". Marinov et al. (2008) furthermore state, that "one needs to be extremely careful when comparing model and designing comparison indices", because different ocean circulation regimes "can result in significant differences in preformed nutrients and thus atmospheric pCO$_2$ sensitivity to high-latitude forcing". Marinov et al. (2008) furthermore claim that their study "support a strong role for the high latitudes and in particular for the Southern Ocean in the cycling of organic carbon".

These new aspects will also be included in the discussion.

To the question, if our suggested hypothesis is a box model artefact, we like to point out that every single model application, independent if box or continuum model, is at first only one realisation of a specific problem and to a certain degree model dependent. Various model-intercomparison project have shown that even models of similar complexity behave differently, e.g. the 11 carbon cycle-climate models of the C^{4}MIP intercomparison calculated atmospheric CO_2 values at the end of this century ranging between 730 and 1020 ppmv (Friedlingstein et al., 2006).

(c) *Figure 8 could use the observations, for comparison.*

In Figure 8 both atmospheric CO_2 and deep Pacific δ^{13}C data where already in the submitted version plotted for comparison, so we do not understand this final comment.

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2. Reply to comments of Kai Schulz

   (a) *"To which extent can the measured δ^{13}C of the tropical Pacific be considered a global (local) signal? In other words, how do corresponding δ^{13}C in the Atlantic and Southern Ocean box change and how does this compare to measurements?"*

   We focus only on changes in δ^{13}C in the deep Pacific Ocean, because changes in the Atlantic might depend largely on the core site due to changing deep and bottom water fluxes between glacial and interglacial times (Kroopnick, 1985; Curry and Oppo, 2005). These detailed changes in ocean circulation and the consequences for local δ^{13}C can not be represented in our model due to the coarse spatial resolution. For the Southern Ocean not enough data sets exist to compile one record, which would be a representative of the whole Southern Ocean as it is defined in our model (south of 40°S). Those long records of which we are aware are all located around 40°S from the South Atlantic / Atlantic sector of the Southern Ocean. In the deep eastern Pacific, where our Pacific sites are located, the horizontal and vertical gradients in δ^{13}C in the modern ocean are very small (Kroopnick, 1985). A similar uniform distribution of δ^{13}C exists in large parts of the glacial Pacific (Boyle, 1992). Therefore, observed changes are assumed to be representative of basin wide variations and not merely a recorder of local changes in ocean circulation. Purely local effects should be minimised by averaging two different cores. To support our assumption that especially local gradients in the Pacific are minimal, an additional figure compiling GEOSECS measurements and LGM reconstructions on δ^{13}C is included in the MS.

   (b) *"The invoked changes in Southern Ocean mixing impact δ^{13}C and at-*

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mospheric carbon dioxide in a similar way. Are there any mechanisms which would impact only one parameter? Otherwise it would seem that variability of atmospheric CO_2 was similar in the Early compared to the Late Pleistocene. This would be an interesting aspect as pCO$_2$ oscillation is generally believed to have gradually increased towards the MPR."

According to our simulation results, the variability of atmospheric CO_2 was NOT similar in the Early compared to the Late Pleistocene. This is clearly summarised in Table 4. In our best guess scenario (S_FINAL) the glacial/interglacial amplitudes in the Early Pleistocene (40k world) were only 69% (81% in the Mid Pleistocene) of those in the Late Pleistocene (100k world). According to this analysis the variability in pCO$_2$ would gradually increase over time.

If one is nevertheless interested in the question if there are any mechanisms which impact only one parameter, one might generate the answer from Fig. 6B,D of the submitted MS. There is no mechanism which impacts on deep Pacific δ^{13}C but not on atmospheric pCO$_2$, however some mechanisms changing pCO$_2$ do only very little for deep Pacific δ^{13}C. They are sea level change and changes in the gas exchange rates due to sea ice.

(c) *"Page 814, last paragraph.: "... sediment box with initially 50000 PgC and a δ^{13}C of 2.75‰ is introduced". How sensitive is the model to the absolute δ^{13}C used for the sediments and the assumption that this would be the average signature of $CaCO_3$ produced in surface waters? In this respect, what are the components of the particulate inorganic carbon cycle in the model?"*

The value of δ^{13}C = 2.75‰ for carbon in the sediments was derived from long-term sensitivity experiments with the model. It corresponds to the av-

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erage signature of $CaCO_3$ produced in surface waters in the model. Thus, this is not an assumption, it is the result of implemented equations and parameters used within the model. If the δ^{13}C value of the sediments does not match those during $CaCO_3$ production, then a long-term drift in δ^{13}C occurs. For example, if we choose initially δ^{13}C = 1.75‰ for C in the sediments, then δ^{13}C in the sediments increases by more than 0.25‰ over the 2 Myr simulation time (in comparison to changes of less than 0.04‰ now), because δ^{13}C in newly accumulated $CaCO_3$ is always higher than the δ^{13}C in the sediment boxes. Accordingly, δ^{13}C in the ocean/atmophere/biosphere decreases, mean ocean δ^{13}C by about 0.02‰ over the 2 Myr in our example.

Furthermore, the components of the particulate inorganic carbon cycle are the following: Hard shells ($CaCO_3$) are exported out of the surface waters with a fixed ratio to organic carbon (OC) of $C_{OC} : C_{CaCO_3}$ of 10 : 1, while C_{OC} export itself is prescribed for present day to 10 PgC, but depends on available macro-nutrients. This is realised with maximum productivity in equatorial waters. Increased export might occur in the Southern Ocean, if macro-nutrients are available and the proxy for iron input into the Southern Ocean suggest the stimulation of additional productivity due to iron fertilisation. $CaCO_3$ is partially remineralised in the intermediate layers. In the deep ocean the relaxation approach calculates either sedimentation of $CaCO_3$ or dissolution of sediments as function of offset from initial (present day) CO_3^2- concentrations.

We extend the model description with more details on the origin of δ^{13}C = 2.75‰ for carbon in the sediments, the sensitivity of the system to those values on and the components of the particulate inorganic carbon cycle.

(d) *"Page 823, last paragr.: "we have to reject our Null Hypothesis to explain the observed variations in the carbon cycle over the MPR" What are the observed variations the authors refer to?*

Wouldn$-1ttheonlyrelativelysmallincreasein\delta^{13}$*C amplitude after the MPR argue for no variations in the carbon cycle?"*

The observed variations we refer to here is indeed the increase in δ^{13}C amplitudes across the MPT. There are two different aspects to this comment:
(1) The increase in δ^{13}C amplitude (from 0.40‰ to 0.55‰, an increase by 0.15‰ or by 39%) is in our view not small, and does not argue for no changes in the carbon cycle.
(2) The simulation results of the Null Hypothesis (which is a synonym for no additional changes in the carbon cycle or in other words: climate and climate impacts on the carbon cycle do not change over the MPT) show an even larger increase in δ^{13}C amplitude (from 0.17‰ to 0.43‰, an inrease by 0.26‰, or by 150%). One might argue if the rise in δ^{13}C amplitude by 39% is (relatively) small or not, especially in the light that the standard deviations of the δ^{13}C amplitude in the 40k world is as big as the change of the MPT. However, we think it is out of question, that changes in amplitude over time in our Null Hypothesis simulations are certainly different than in the observations. On this final argument is the rejection of the Null Hypothesis based on.

To clarify this issue we extent our reasoning in this paragraph.

(e) *"Page 831, last paragr.: "This approach can explain terminations I-VII, but none of the smaller G/IG transitions, which occurred earlier in time. However, it also indicates that the climate in the southern high latitudes was remarkably different..." The insolation canon hypothesis by Schulz & Zeebe 2006 cannot explain any of the G/IG transitions themselves, although there are hints (the combination of negative and positive insola- tion forcing). Nevertheless the hypothesis demonstrates that orbital forcing changed at the MPR which leads to the observed dominant frequencies of 41 kyr prior*

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and 100 kyr after the MPR. If this indicates that the climate in the Southern high latitudes was remarkably different in the Early compared to the Late Pleistocene, I am not sure."

We revised the paragraph according to the comment, to clarify (a) that the insolation canon hypothesis cannot explain any of the G/IG transitions themselves; and (b) that mainly orbital forcing, and not necessary climate in the Southern high latitudes was different in the Early compared to the Late Pleistocene.

Finally, the original Figure 6 is revised. In subfigures C and E the contributions from individual processes to changes in atmospheric pCO$_2$ or deep Pacific δ^{13}C are calculated by a factorial analysis. One of the processes is CaCO$_3$ compensation.

Because CaCO$_3$ compensation is active in this factorial analysis the contributions of the individual processes include the partial effect of the sediment/ocean interaction. Processes are thus called to be "equilibrated with the sediments". The contribution of CaCO$_3$ compensation itself sumes up the sedimentary effect for all processes, but needs not to be included individually, otherwise the contribution from CaCO$_3$ compensation would be counted twice. This is also included in the MS, in the paragraph, in which the factorial anaylsis is explained, which was according to our reply to referee comment 1.a extened anyway. This revision is not based on referee comments.

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