Climate of the Past Discussions, 2, S46–S50, 2006
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Climate of the Past Discussions

CPD

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Interactive Comment

Interactive comment on "Proposing a mechanistic understanding of changes in atmospheric CO_2 during the last 740 000 years" *by* P. Köhler and H. Fischer

Anonymous Referee #2

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General comments

Peter Köhler and Hubertus Fischer present a forward-modelled prediction of the variability in the concentration of CO2 in the atmosphere associated with the glacial-interglacial cycles of the past ˜750 thousand years (kyr), and find a close match to the recent Dome C plus Vostok CO2 data covering the past 650 kyr. Their model is basically identical (in terms of how the processes controlling global carbon cycling are parameterized) to that which the authors constructed for explaining the observed CO2 rise spanning the last deglacial transition (Köhler et al. [2005]; GBC volume 19). The main advance in this present work is the construction and application of a series of environmental boundary conditions forcing the model, and which reflect global biogeochemical and climatic changes important to determining atmospheric CO2 which

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cannot a priori be generated internally by the model.

While the results of this analysis do not reveal that much that is entirely new about the glacial-interglacial controls on atmospheric CO2, I did enjoy the paper. It provides a wide-ranging analysis of candidate mechanisms for explaining glacial-interglacial CO2 variability, presents a relatively new methodology for comparing model and data (i.e., the dynamical forcing of a model over a long time-scale), and gives us all some encouragement that the 'glacial CO2 question' (crudely put; the explanation for low glacial CO2) is entirely solvable to a degree of general satisfaction. I think that the paper works particularly well as a discussion point and (unintentional) review of CO2 mechanisms, and is worthy of publication for this alone.

Specific comments

I. The main concern I have surrounds how the interaction between ocean and sediments is treated in the model (specifically, how the preservation and burial of calcium carbon (CaCO3) modifies ocean chemistry and affects atmospheric CO2). I appreciate the need in the model to prescribe changes in ocean carbonate chemistry according to an assumed history of carbonate lysocline change and wouldn't go as far as to suggest that they should adopt a fully mechanistic approach (i.e., calculating the fractional preservation of CaCO3 in surface ocean sediments) here. (But I strongly recommend that they consider this for future studies using their model.) However, the implications of their sediment assumptions require some scrutiny because of the very profound contribution that ocean-sediment interactions (i.e., exchange fluxes of DIC and ALK) appear to make in this model - in Figure 4, panel 'S-CA' (bottom), ocean-sediment interactions contribute over half of the entire glacial-interglacial CO2 variability! It also appears that changes in Southern Ocean mixing (panel 'S-SOX' in Figure 4) amplified by ocean-sediment interactions could explain virtually all of the ice core CO2 record, with all other processes achieving little more than cancelling each other out. This would be a surprising result because no-one has hitherto suspected such a powerful atmospheric CO2 effect due to deep ocean ventilation plus associated 'carbonate compensation'.

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(This is not to say that the authors must therefore be incorrect, however.) I would be interested to see just how much of the CO2 record just these two processes can explain.

I am concerned that some of the remarkable amplification of what is otherwise a glacial-interglacial CO2 amplitude of not more than ˜40 ppm (Figure 4, panel 'S-CA') might be an artefact of the way in which the depth of the carbonate lysocline (actually, the depth of the calcite saturation horizon) is held constant (or forced according to a prescribed time history as in Figure 9) by adding or subtracting DIC and ALK. This implicitly assumes that the buffering of ocean chemistry by deep-sea sediments is instantaneous. However, mechanistic models suggest that this buffering is characterized by time-scales of ˜6 kyr (for changes in the CaCO3 surface sedimentary inventory (or composition)) and ˜8 kyr (or longer) for imbalances between terrestrial weathering and deep-sea burial (see Archer et al. [1997] - GRL 24, Archer et al. [1998] - GBC 12). Is the importance of ocean-sediment interactions in greatly helping to match the atmospheric CO2 record thus partly a result of the assumed instantaneous readjustment of the ocean-sediment system?

This might be tested relatively easily by adding a time constant of adjustment of the lysocline - i.e., DIC and ALK is applied at a rate scaled to drive the lysocline towards its modern position with an e-folding time of perhaps ca. 7 kyr, rather than returning the lysocline position instantaneously. I would be interested to see whether this either reduces the amplitude of the CO2 contribution (Figure 4, panel 'S-CA') and/or offsets the timing of predicted vs. observed CO2 rise at deglaciation.

Still on the subject of assumptions of lysocline change - what happens if the Atlantic lysocline is assumed to vary strongly in anti-phase with the Pacific, as observed?

II. The most novel aspect of the authors work compared to their previous paper focussing in some detail on the last deglacial transition, is the construction and application of a series of model 'forcings' - the time-dependent changes in prescribed model boundary conditions (Figure 2). However, for some of these, more information needs to be provided. For the reconstructed changes in carbonate lysocline depth (Figure 2g), although I am familiar with the reconstruction of Farrell and Prell [1989] on which is it based, it is not immediately obvious to me what criterion they have used to define the lysocline depth (i.e., which wt% CaCO3 isopleth?). It would be helpful (and educational!) to reproduce the Farrell and Prell [1989] reconstruction and overlay the derived forcing signal. The authors should also note that questions have been raised concerning a potential latitudinal-depth bias (correlation) amongst the cores used, and thus the possibility that some of the reconstructed wt% CaCO3 variability reflects changes in surface productivity rather than carbonate chemistry of the deep ocean (see Archer [1991] - Paleoceanography 6). For the dust forcing - the authors should used dust flux rather than dust concentration. How does this affect their CO2 predictions?

Technical comments

o Abstract; "This is the first forward modelling approach which covers all major processes acting on the global carbon cycle on glacial/interglacial time scales.". A pedantic point; I am not convinced that the authors can be entirely confident that all major processes have been included, although they achieve a reasonable fit with the processes they chose to include. The authors noted in their previous work (Köhler et al. [2005]) that changes in the CaCO3:POC 'rain ratio' at the sediment surface may have been important (perhaps driven by a reorganization of global silica cycling), as could an enhanced biological pump in the North Pacific during glacial times.

o Page 8/lines 20-22; "We keep a similar threshold (dustFe$>$310 ppbv) as deduced for Termination I (Köhler et al., 2005) above which iron fertilisation might occur." Very confusingly worded. Better to state something like; "... threshold ... below which iron limitation can occur." Above your threshold you assume no limitation by iron, so 'iron fertilisation' is not really a relevant concept.

o Page 10/lines 9-10; "The agreement in terms of timing and amplitude of changes is especially remarkable during terminations, but fails to reproduce some features of the data records:" The time scale in the plots, covering ˜750 kyr, makes it difficult to tell just how close or not the predicted vs. observed timing at deglaciation is. You should perhaps add additional plots spanning just the terminations and on an expanded time-scale (e.g., see Petit et al. [1999] - Nature 399, or Watson et al. [2000] - Nature 407) to illustrate your discussion here.

o Page 14/line 4; typo; "... highly model dependent".

o Page 14/lines 6-8; add reference for modern regional CO2 sources and sinks.

o Page 14/lines 6-8; note that you cannot a priori assume that the modern North Atlantic CO2 sink will be reduced by an expansion of sea ice coverage, because ocean convection will presumably shift further south (and change in character to brine rejection?).

Interactive comment on Climate of the Past Discussions, 2, 1, 2006.