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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 #3

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This manuscript combines previous hypotheses for glacial/interglacial CO2 change to attempt to reconstruct the full CO2 record from EPICA. This is done through parameterizing each of these hypotheses to some proxy for environmental change. As the authors indicate, this is the first time such an exercise has been attempted.

As to the specific calculations in the study, I have the following concerns.

It occurs to me that the authors' implementation of deep CaCO3 dynamics will overestimate the rapidity of CO2 change, for instance, upon deglaciations. By holding deep ocean carbonate ion constant or by following the necessarily gradual CaCO3 changes evident in the Farrell and Prell study will mean that CaCO3 compensation is either immediate or very rapid, despite the fact that data and models indicate that the preservation event upon deglaciation occurs over a significant time interval and thus that $CaCO_3$ compensation occurs over ~5 kyrs (Marchitto et al., 2005).

Of greater concern to me is how the parameterizations described in the manuscript address the possibility of changes in the amount of $CaCO_3$ sinking to the seafloor. If such a change in $CaCO_3$ flux to the seafloor occurs in response to the many parameter changes in the model (which I think must be occurring), this change should drive a migration toward a new lysocline depth as the model seeks a steady state, albeit in vain. It would seem to me that the model is violating this mechanistic constraint, as did the study of Broecker and Peng (1987).

Altogether, it would help comprehension of the model results to explain what the assumptions are with respect to the balance between nutrient supply and productivity in this model. For instance, if Southern Ocean overturning is reduced, will productivity decrease in step with it, so as to maintain a constant nutrient concentration, or will productivity change in some other way, so that Southern Ocean surface nutrient concentrations change? These different assumptions lead to very different sensitivities of CO_2 to Southern Ocean ventilation changes. They also lead to more careful consideration of which model changes are considered "physical" versus "biological."

Finally, it is not clear why a reduction in North Atlantic Deep Water leads to a CO_2 decrease in this model. Indeed, it lowers CO_2 in some other models as well, but not all, so an explanation of this model's response is needed. Altogether, I chafed at the authors' contentment with approaching the observational target (EPICA CO_2) without being clear as to why each of the changes affected CO_2 in they way that they did.

In general, I agree with RC S24 in their complaint that the authors overstate the significance of this manuscript. The hypothesis of reduced deep ocean ventilation through the Southern Ocean, which is the dominant driver of the model's CO_2 changes, is a good example. This hypothesis has its origin with the 1984 "Harvardton Bear" papers and was subsequently brought to the forefront by measurements (Francois et al., 1997). Since then, it has received much more thorough consideration that is given in the manuscript in question. The sensitivity of CO_2 to this change has already been explored in box model experiments, including its interaction with seafloor $CaCO_3$ dynamics (Toggweiler, 1999), with the results comparable to those shown here. One fundamental question, which is not addressed here, is whether this sensitivity is correct, as ocean GCM's yield a much lower sensitivity (Archer et al., 2000). The second fundamental question about this reduction of overturning, if it did occur, is what physically caused it under glacial conditions. This is also not addressed in the current manuscript. Rather, the authors assume that this process occurred and tied it to the Antarctic record of temperature for the purposes of CO_2 reconstruction. Given the similarities between CO_2 and Antarctic temperature proxies, it becomes clear why the fit of the proxy-driven model to the CO_2 data is so good; it does not reflect a mechanistic understanding as claimed in the manuscript's title. All of the same arguments apply to the model's treatment of iron fertilization.

Kohler et al. 2005 in GBC provides a much more thoughtful analysis than is presented here, both in its comparison to both data and previous model work. Moreover, that manuscript was much more oriented toward making real progress, and it highlighted which questions are most important targets for future work. Perhaps the authors felt that, given the 2005 paper, they could be given more latitude to proceed along a different track in this manuscript. But where any paper should point out its own limits of scope and its own weaknesses, this paper claims to be a major breakthrough, even though I felt that, having read it, I had learned nothing new. I found the 2005 manuscript to be of much more interest than the present one.

The comments made at the beginning of my review regarding $CaCO_3$ need to be addressed, particularly whether the model has an appropriate way of allowing the ocean alkalinity balance to respond to changes in the $CaCO_3$ flux the seafloor. This may require new model experiments. Outside of these issues, I feel that this manuscript is eventually publishable if, for no other reasons, that the authors clearly invested a lot

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of time in the sensitivity tests and that the paper may have some educational value to a portion of your readership - I have already indicated that I don't think I had much to learn from it.

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