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Climate of the Past Discussions

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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

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The main point raised by Referee #1 was, that the authors should tone down their enthusiasm what can be explained with a box model as simple as it is. We should concentrate more on an adequate description of what is happening and why and should not come to the conclusions of an understanding, if this question "Why is something happening?" is not answered.

We can see the reasoning behind these arguments and will revise the MS accordingly. Especially the title, abstract and conclusion section will be "toned down" as suggested. However, we like to clarify our carbon-cycle-point-of-view on this subject. We feel, that

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showing the potentials of those processes operating in the global carbon cycle from which we believe they are important on glacial/interglacial timescales (based as far as possible on evidences from paleo data) is a step forward in understanding. This has not been attempted for all transitions before and transient modelling is not yet possible with full GCMs including biogeochemical cycles due to a lack of computing capacity. Therefore, a study like ours which points out important processes should be understood as an invitation to analyse these individual processes in greater detail with more appropriate tools. Our reference question was always “Why is changing?” which led to the answers given in the discussion paper. Of course, the next step of asking “Why are these processes happening suggested by us which might lead to the observed changes in the carbon cycle?” was tackled as far as possible from observational evidences (mainly as explained out in the original paper Köhler et al. (2005), but this question is still open to the community. As pointed out by the referee this whole aspect is one of the most difficult problems in paleoclimate research. Thus, we understand our contribution as a first step from question one to question two, a question from which the answers will lead to a deeper understanding. Coming to the point of asking this second (set of) question(s) is a step towards a mechanistic understanding.

The referee furthermore argues that missing internal feedbacks and weak nonlinearities in our model together with our design which connects Southern Ocean ventilation with the Antarctic deuterium record make the BICYCLE model very similar to simple regression models used in Wolff et al. (2005). We discussed the importance of the role of the Southern Ocean in the discussion paper (pages 19–20). As pointed out there a simplification of the connection of the climate and carbon cycle to a regression of Antarctic temperature and CO_2 leaves out important processes, from which it is known that they are important. The possibility to draw attention to the potential contributions of these different processes is the advantage our our forward model over these regression models. Our model is furthermore simulating the full

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carbonate system including the carbon isotopes. A much wider comparison of our simulation results with paleo records is therefore possible — as for example done for pH in the article. In this respect we disagree with the opinion of the referee, that BICYCLE is very similar to a regression model. However, the limitations of our approach and the missing feedbacks will be discussed in the revised version of the MS.

It was further pointed out that the treatment of the sediments is very simple in our model. This was pointed out by all referees and we will enhance our study by additional simulations covering various aspects of the $CaCO_3$ chemistry, including the delayed time response of the sediments.

The referee asked whether what we call "sea level" effect is not just a salinity effect. It is not. The geometry of the oceanic reservoirs is based upon realistic bathymetric profiles and changes as a function of the prescribed sea level evolution. We took the bathymetric data ($1^o \times 1^o$, vertical resolution: 1m) of Scripps Institute of Oceanography (http://dss.ucar.edu/datasets/ds750.1) for this purpose. The model description will be extended on this subject.

Finally, it was asked if the model was allowed to go into a parameter space which is physically not allowed, something which might happen through the external forcing approach. This is not the case. For the most obvious situation, which is oxygen depletion in the deep ocean we introduced the denitrification pathway as done in the OCMIP model intercomparison. If the O_2 concentration drops below 4 μmol kg^{-1} the remineralisation of organic matter in the deep ocean is assumed to follow the denitrification pathway and thus does not consume any molecular oxygen. This was described in Köhler et al. (2005), but will also be explained in a revised MS.

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All-together, our concept was to extent the approach described and discussed in detail in Köhler et al. (2005) to longer time scale and see similarities and differences between different terminations. It might be that information which are important for the reader are hidden in the previous article. Of course, if the same processes are important for Termination I and earlier time they might need a similar discussion in both manuscript. From our point of view this would be a kind of double publication, which we tried to avoid. In the revised MS we hopefully find a better balance between short descriptions and necessary in-depth explanations.

References

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Interactive comment on Clim. Past Discuss., 2, 1, 2006.