Climate of the Past Discussions, 2, S1–S3, 2006
www.climate-of-the-past.net/cpd/2/S1/
European Geosciences Union

Climate of the Past Discussions

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

E. Wolff

ewwo@bas.ac.uk

Received and published: 23 February 2006

This is a very interesting study, because it does provide a basis for picking apart different mechanisms that contribute to CO2 changes across glacial cycles. From this point of view, this was exactly the kind of work the EPICA challenge intended to stimulate, and offers a real chance of insight into what controls the CO2 values actually found in the Vostok and Dome C cores. I cannot comment on the model itself, and hope that more expert reviewers will be able to do this. I congratulate the authors on a highly articulate paper, that even a non-expert learnt a lot from.

I would just like to raise with the authors a couple of issues that really come from the EPICA challenge, in the hope of improving the discussion about what is needed next

to confirm or improve the authors' conclusions.

The authors correctly congratulate themselves on the fact that they have managed to reproduce not only the magnitude of glacial-interglacial changes, but also the change to smaller amplitude of change before 440 kyr BP. As they say, an equally good reproduction can also be obtained with extremely simple correlations, involving essentially only Southern Ocean temperature. The authors point out that this is because several of the processes in their model rely directly or indirectly on Southern Ocean temperature. The implication of this is that there must be a number of different parameterisations of these processes that would give an equally good fit to the data. The authors have obviously chosen a good set of parameters, but further work would need to consider if any of the other parameter choices that would fit are also reasonable (clearly for example one could arrange that all the change was due to Antarctic sea ice, and nothing else had any effect, but this would not be reasonable). With this comment, I mean only to point out that the challenge was not intended to find a right answer (with the implication that everything is understood), but to draw out the hypotheses behind the answer, and to highlight the uncertainties we need to reduce.

In this respect I think it would be useful to point not only to improvements in models that might be needed, but also to where new data are needed. Some new data would allow one to differentiate between some of these mechanisms. For example, Antarctic sea ice in the model is considered simply as a function of Southern Ocean temperature, and therefore two processes cannot be separated. However, we have hope that good independent records of Antarctic sea ice might be obtained from a combination of ice core sea salt data and marine diatom assemblage data. Such data will almost certainly show slightly different timings and non-linear relationships between sea ice and temperature.

We should also ask whether the input data going into the current model are adequate. In this respect, I think the authors need also to comment on the fact that their standard scenario simulation in this paper is noticeably a better fit to the data than that presented

CPD

2, S1–S3, 2006

in the original EPICA challenge paper [Wolff et al., 2005]. In particular, at 560 kyr, in MIS15, the present simulation gives around 255 ppmv, while the one presented earlier gave over 270 ppmv. Other early interglacials also had higher concentrations of CO2. I suspect that this is because the authors have used different input data that have become available since (for example the Bintanja sea level and temperature based on the Lisiecki/Raymo stack), rather than on any changed parameterisation. This would be a very legitimate change, but if I am right about it, then it deserves discussion, because it shows how sensitive the model is to input data, and therefore highlights the need for better data (and better synchronisation of the data from different sources) in certain areas if we want to test models adequately.

Finally on these general points, I wonder if the authors could draw more out of a separate discussion of the processes that control the glacial values, as opposed to the processes that control interglacial values. From figure 4 (and from our knowledge of the data), it is obvious that, with this model, iron fertilisation has a significant effect on glacial CO2, but none at all on interglacial values. NADW looks very important for interglacial values, Southern Ocean vertical mixing less so. And so on. Such a discussion might help to focus attention on the processes where we most need work.

Wolff, E.W., et al., Modeling past atmospheric CO2: results of a challenge, EOS Transactions, 86 (38), 341,345, 2005.

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