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# *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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In his interactive comment E. Wolff raised some points which might need further discussion. We like to respond to these points in the following:

1. E. Wolff stated: *"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."* Our intention was to combine the available knowledge on changes in the climate system and the carbon cycle in a condensed way as basis for our modelling concept. Of course, certain assumptions can be discussed, but we feel that we found arguments for all aspects of our standard scenario. The way we understand this



comment is that it might be useful to perform a complete and very complex sensitivity study (or parameter variation) to fully explore the whole parameter space available for the model. This will give certainly good insights into the model behaviour and was done in a systematic way for all single parameters for the terrestrial module of the model [Köhler and Fischer (2004)]. An understanding of the overall model performance emerged from the studies of the different processes acting at different strength on the global carbon cycle, which was in the focus of the single process analysis and of the sensitivity studies (subsection 3.2). There might be different combinations of processes which might lead to similar results, but the additional constraints given by the carbon isotopes during Termination I [Köhler et al. (2005)] will restrict the number of scenarios which are supported by the observation. We think that more can be learned if either certain aspects of the climate system of the past are interpreted differently then today due to new or more precise data sets (e.g. ocean circulation) which would than reason us to change our assumptions, or if computational capacity will expand in a way which allows similar simulations to be performed with more complex Earth system models. While the first is a question of qualitative and quantitative improvement of paleo records in the future, the second is more a less a question of time until these experiments are possible.

2. E. Wolff asked for suggestions which new data sets might improve our understanding of the carbon cycle in the past.

   Certainly, the most improvements can be achieved through a common age scale of all relevant climate archives. This can be seen, for example, by the current mismatch (and its consequences for our simulations results) at MIS 15 of the age scale EDC2 of the EPICA Dome C ice core and that of the Lisiecki/Raymo benthic $\delta^{18}$O stack [Lisiecki and Raymo (2005)] which is the current master record used in Paleoceanography.

   Second most important from our point of view is a widely accepted and model-and data-based interpretation of changes in global ocean circulation pattern over time. Various articles concentrate on the strength of the NADW formation during certain times and the consequences for the Atlantic Ocean, but little seems to be known for the other large ocean basins. Furthermore, the interpretation of data still comes to different conclusions [Wunsch (2003)]. The interpretation of $\delta^{13}$C as an indicator for changes deep water formation seems to be difficult [Raymo et al. (2004)].

From these two improvements most paleo climate researchers would benefit.

Thirdly, an improvement of our current knowledge of changes in both the marine and the terrestrial biosphere including export processes to the deep ocean and especially the sediments would especially be of interest for global carbon cycle research.

3. Original contribution / input data: E. Wolff pointed out correctly, that our original contribution to the EPICA challenge as presented in [Wolff et al. (2005)] was slightly different. Also correctly stated in the comment was the fact, that we improved the input data used in the model. We substituted the SPECMAP $\delta^{18}$O we used originally as proxy for sea level change by the new results of [Bintanja et al. (2005)]. We furthermore used in the refined results the northern hemispheric temperature changes calculated also by [Bintanja et al. (2005)]. Additionally, our results in [Wolff et al. (2005)] did also consider the effect of a shutdown in NADW formation during Heinrich events. Because of the uncertainty of the interpretation of Heinrich events [Siddall et al. (2003), Sachs and Anderson (2005)] we only investigated this process in a sensitivity study here, but did not include it in our reference scenario. Beside these improvements in the input data used to force the model we did also refine the threshold which defines the strength of the Atlantic overturning. As can be seen in Figs. 2c and 8a and discussed in subsection 3.2 a slight variation in the threshold defining the switch from glacial to interglacial strength of the NADW formation



is possible within the uncertainty of the understanding gained from Termination I [Köhler et al. (2005)], but leads during the earlier interglacial periods of the pre-Vostok times to changes in the ocean circulation and thus to change in $CO_2$ by only up to 10 ppmv.

It should also be noted that the accuracy of our results depends on the age scale used for the Vostok $CO_2$ data. The GT4 age scale most widely used for the Vostok data set (and also used in [Wolff et al. (2005)]) leads to a smaller correlation coefficients between the $CO_2$ data and our simulation results than the orbitally tuned age scale derived by [Shackleton (2000)]. This is in our point of view an indication that again the synchronisation between ice cores and marine sediment cores is crucial for a correct interpretation. We argue, that more efforts should be undertaken in the establishment of a synchronised marine and ice core age scale. The parallel use of different age scales might lead to misunderstanding and confusions in the interpretation of these ice core data sets.

In the forward modelling mode we applied here the simulation results can only be as good as the data sets which drive the model and the assumptions which were used for the selection and interpretation of these driving forces. Also with respect to the previous point ("Which new data sets are of interest?") not only the available data sets themselves are of interest, but also their accurate interpretation. We believe that only a combination of new data sets and their interpretation by the use of various types of models will advance paleo climate research. The latter might be especially useful to disentangle local from regional or global effects. This aspect was just recently highlighted by the new editors of *Paleoceanography* [Rohling and Dickens (2006)].

4. The final point asks whether we can state which processes are important for glacial or interglacial values of $CO_2$. As example it was argued that NADW formation seemed to be more important for interglacial values, and iron fertilisation more for glacial values.

The two examples drawn here are those two where our simple modelling concept relies on threshold values. Once these thresholds are crossed in the forcing data sets, certain processes operate in a different mode, e.g. the NADW formation in a different strength. This is certainly a crude simplification of reality, but was motivated by a lack of alternatives. For the case of NADW formation again, not a better data set itself would be needed, but advances in the overall interpretation of ocean circulation in the past.

All other processes lead to a more or less continuous variation in atmospheric $CO_2$. We do not think it can be argued, that certain processes are more important for glacial or interglacial $CO_2$. The amount of carbon in the system is more a less stable, depending if one considers the sediments as part of the system or not. It is more a question where the carbon is stored at certain times, which in the consequence determines atmospheric $CO_2$. Interglacial and full glacial conditions are the two extremes the climate system could get during the past glacial cycles, but most of the time, some kind of intermediate state is found. With respect to the original point raised by E. Wolff one can say that the interglacial state is the exception and not the rule, and therefore NADW formation of interglacial strength and its consequence for the carbon cycle was seldomly found in the past. On the other hand, iron fertilisation in its fullest extend depended on maximum iron inputs in the Southern Ocean, which in the other extreme was only seen during glacial maxima. Those processes which change atmospheric $CO_2$ and thus the global carbon cycle mostly are the exchange fluxes between the deep ocean and the sediments and the changes in ocean circulation, especially in the Southern Ocean. A more elaborated understanding of these processes would therefore also enhance our understanding of the global carbon cycle mostly.



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