

Interactive comment on “Proposing a mechanistic understanding of changes in atmospheric CO₂ during the last 740 000 years” by P. Köhler and H. Fischer

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Our responses to the points raised by referee #2 are the following:

1. A main concern of the referee is the simplicity of the interaction between sediment and ocean. In detail, the referee asks for a few additional experiments to give answers to some specific problems/results which might arise from our approach:
 - The temporal delay of the response of the carbonate compensation is now implemented in our model. We expand the result section widely on the chemistry. However, we find different evidences for the size of an e-folding time τ which might describe the temporal delay of the sediments. While

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models [Archer et al.(1997), Archer et al.(1998)] found a τ of 6 kyr, our interpretation of the data set of [Marchitto et al.(2005)] find a τ of 1.5 kyr. To show the implications of these uncertainties we will compare simulations with these different response times and a simulation with instantaneous reaction sediments (which is in the results very similar to the previous approach with prescribed changes in the lysocline).

- It was asked to add scenarios with different changes in the lysocline in different ocean basins. As this approach is not used anymore, this question becomes obsolete. From our new analysis we have to say that for a detailed investigation of changes in the lysocline (or the calcite saturation horizon) models with better vertical resolution in the water column and with an interactive sediment model are needed.
- A description of our approach to derive changes in the lysocline depth in the Pacific based on the work of [Farrell and Prell(1989)] is asked for. Because of the revision of our model and the implementation of the time delayed response of the sediments to changes in deep ocean [CO_3^{2-}] this previous approach on data-based evidences for changes in the lysocline is not used anymore. This revision was also based on the critics raised by the reviewers that the data [Farrell and Prell(1989)] might be interpreted differently [Archer(1991)].
- In the discussion paper it seemed that a combination of Southern Ocean mixing together with its amplification through carbonate compensation has the potential to entirely explain the observed variability in CO_2 . We expand our sensitivity study on this combined effect, but have to repeat our warning statement already given in the discussion section of the original manuscript: Neglecting additional processes operating on the global carbon cycle for which there is compelling evidential support from paleo records might give a false impression on the importance of those selected processes. Surely, there are processes with weak support from the data sets such as the iron

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fertilisation hypothesis. Others (e.g. temperature, sea level) might vary in the interpretation of their glacial/interglacial amplitudes, but they can not be discussed down to zero. It turns out, that the combined effect of Southern Ocean mixing and its amplification through carbonate compensation (with a time-delayed response of the sediments with $\tau = 1.5$ kyr) gives only about 40 ppmv to the rise in CO₂, and is thus no candidate to fully explain the observations. The combined effect of all Southern Ocean processes (vertical mixing, sea ice, SST, all amplified through carbonate compensation) can account for a rise in CO₂ of 60 ppmv during Termination I, which is about 2/3 to of the amplitude seen in the data.

2. Furthermore, it was pointed out correctly that the dust flux might be a better proxy of aeolian iron input than the dust concentration. We like to point out that the origin of this work lay in the call for the EPICA challenge [Wolff et al.(2004)] following the publication of the EPICA Dome C deuterium and dust records [EPICA-community-members(2004)]. Our approach therefore was based on what was available during that time, but we acknowledge, that indeed for Antarctic ice cores the fluxes are better variables for the input of atmospheric particle into the Antarctic/Southern Ocean region than their concentrations. Due to the newest measurements on the EPICA Dome C ice core [Wolff et al.(2006)] we can even go a step further and use iron fluxes as our driving record for the iron fertilisation hypothesis. The differences between simulations using different iron proxies, however, are small due to the threshold effect in our approach: Iron and dust starts to fall before the rise in CO₂ during Termination I. The start of the CO₂ rise determines our threshold. All changes in the iron proxy before the the rise in CO₂ (above the iron proxy threshold) are assumed to have no iron limiting effect on the Southern Ocean marine biota and thus on the export production.
3. All technical comments including the suggestion to plot the dynamics during terminations on a higher resolved time scale are taken up in the revision of the

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manuscript.

4. In the final technical comment it is noted, that we “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”. We have to point out that our argument of a reduced North Atlantic CO₂ sink due to an expansion of sea ice was only covering the effect the sea ice would have on the gas exchange rates, but not any additional changes in the ocean circulation. We clarified this in the MS.
5. Additionally, we update our model forcing by the usage of the model-calculated deep-water temperature contribution to the benthic $\delta^{18}\text{O}$ signal [Bintanja et al.(2005), Lisiecki and Raymo(2005)] as a proxy for ocean temperature change. Through these changes in our model, including the temporal delay of the sediments, a new standard scenario was defined and all simulations and results were revised and updated in the new MS.

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