



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

Anonymous Referee #1

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The authors tackle one of the most difficult problems in the fields of global carbon cycle, geochemistry, and climatology. It is the natural and cyclical variation in atmospheric CO₂ content over the Pleistocene. As evident from ice cores, CO₂ content was high during peak interglacial periods than during peak glacial periods by 80–100 ppm. A convincing explanation has eluded us for the past two decades since the first discovery of the CO₂ variations in Vostok ice core. Using a simple box model of ocean carbon cycle, the present authors propose to have gained a mechanistic understanding of this mystery. However, this is not quite the case despite their good intentions and effort, and so they will need to temper their enthusiasm.

In this clearly written manuscript, the authors use a number of external forcings obtained from proxy data to drive their simple, forward model. The chosen forcings are

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meant to represent major components of the climate system as they relate to the global carbon cycle. They highlight that their “prediction” is very good, because it compares very well with data ($r_2=0.79$ for Vostok and 0.8 for EPICA). Their implication is that they now have a mechanistic understanding of the problem. I challenge the authors on this main point.

I raise four major issues:

1) This manuscript deals more with attributing the causes of CO₂ variations and not necessarily with explaining the fundamental puzzle of the CO₂ variations themselves. The authors’ answer to the question of why there are large CO₂ variations would be that it’s a combination of the Southern Ocean ventilation (their biggest reason) and other forcings of their choosing. This is attribution. Well, WHY is there Southern Ocean ventilation change that causes CO₂ to go up and down? They don’t have an answer, because it’s a prescribed external forcing. Any claim to have solved this important problem “mechanistically” needs to be able to explain its fundamental nature.

2) The chosen external forcings are all prescribed and there are no valid feedbacks within the model. While some attempt is made to understand the nonlinearity in the system (one-process-at-a-time vs. all-but-one processes), the nonlinearity represented in the model is grossly underestimated due to the lack of both physical and chemical dynamics. In terms of physics, there is no ocean dynamics, no atmospheric dynamics, no sea ice (thermo)dynamics, no ice sheet dynamics, and most importantly there is no coupling between these components. In terms of chemistry, the model has no sedimentary calcite dynamics. The point is there is a fundamental limit to how much “mechanistic” understanding one can gain in a simple model that lacks feedbacks of a highly complex system, and this is not acknowledged in the manuscript.

3) Because of the weak nonlinearity in the model, its predicted atmospheric CO₂ is actually not very dissimilar to a linear combination of the responses to the individual forcings. In this respect, there is much in common between this study and the regres-

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sion models in Wolff et al. [2005]. So it should come as no surprise that the deuterium record “correlates” well with pCO₂ in the regression models and this model. The regression models do not attribute the deuterium record to any climate component per se, but this study “attributes” it to Southern Ocean ventilation by design, which may or may not be correct. By labeling BICYCLE as “forward” model, the similarity (or lack of, if the authors may prefer) between the regression models is not readily appreciated but should be.

4) The treatment of the sediments and lysocline in the model is too simple and does not fundamentally give quantitatively reliable results in terms predicting atmospheric CO₂. It’s been shown by Sigman et al. [1998] that the closed system response (a gross simplification of adding and removing ALK and DIC in 2:1 ratio) is not realistic compared to open system response in which sedimentation and diagenesis is explicitly modeled. Such simplification has been employed previously, but those studies rightly do not make the grand assertion of achieving a “mechanistic understanding”. In addition to the amplitude of the response, the timing of the response in a closed system is prescribed but is shown in open system models to be O(10 kyr) [Archer et al., 1998]. This is glossed over in the present study.

The authors need to come to grips with these major issues. Instead they are self-congratulatory throughout, repeatedly using “remarkable” to describe their results. Phrases like “we have to acknowledge that this effect is model dependent” (p.14) appear as if other aspects are not problematic. The authors should tone down.

I conclude with a couple technical issues:

1) What’s termed “sea level” effect is just a salinity effect, no? I don’t see how a simple model can account for actual changes in sizes of the boxes. Previous models just say “salinity effect”.

2) Usually when a simple model such as BICYCLE is forced with external functions without feedbacks, the model can go into parameter space that is not physically allowed

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(like negative nutrient or oxygen concentrations). Does this happen in the forward run at all? If so, that should be indicate.

My recommendation is that the authors tone down their enthusiasm, fully acknowledge the simplicity and limitations of their model in relation to the complexity of the problem, note the similarity with the regression models, and most importantly delete explicit and implicit claims of “mechanistic understanding”. I think the study is done carefully and is interesting but find its claims to overarching.

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