# Final author comments for manuscript cp-2016-35
# Title: Ocean carbon cycling during the past 130,000 years – a pilot study on inverse paleoclimate record modelling
# by author(s): C. Heinze et al.

**RESPONSE TO REVIEWER#1 (David Archer):**

We would like to thank the reviewer for the thorough review and the constructive suggestions for improving the manuscript. Below we cite the reviewer's remarks in italics and our direct responses in normal text. The references for the responses to both reviewers are collected together at the end of this document.

*Reviewer#1:*
*This paper is a clear advance in the question of understanding the glacial / interglacial atmospheric $CO_2$ cycles, as driven (presumably) by the ocean. It's difficult to model this process because the data are impacted by the 3-D circulation of the ocean, but also span a huge dimension of time. So a box model or some of the intermediate complexity models with 2-d ocean are too simple, but a coupled primitive equation climate model would be too slow. The HAMOCC model is an ideal vehicle for exploring this question. Innovations to this work include interpolating the circulation field between glacial and interglacial values, assembly of a suite of paleo data for comparison against, and lots of creative statistical processing for optimizing the model input parameters.*

(No response required.)

*Reviewer#1:*
*Scaling the flow fields between LGM and today is a clever idea, and worth considering as an interim step as this is. In reality there were certainly fits and starts to the circulation, such as the Heinrich drop-dead mode of the overturning circulation, which will ultimately need to be addressed (by somebody, not necessarily in this paper).*

Our response:
We plan to add the following text after page 15 line 32 in order to underline that our pragmatic approach can be improved once more realistic ocean velocity fields will be available over the past climatic cycle:
"Ideally, one would need to simulate the glacial ocean circulation in a coupled Earth system model including an ice sheet model over the entire last glacial-interglacial climatic cycle. However, even given such a detailed and realistic velocity would be available, the computing times for carrying out the various sensitivity experiments would be prohibitively large due to the required short time step in such simulations. Nevertheless, in future studies it would be desirable to include also quick alterations of the ocean velocity field, especially changes in ocean overturning. Such short-term climatic changes (time scale of few hundred to thousand years) have been inferred from ice core as well as sediment core analysis known as Dansgaard Oeschger events (Dansgaard et al. (1993), Anklin et al. (1993)) where the coldest events are also marked by large amounts of ice rafted debris in sediment cores (Heinrich events (Bond et al. (1993)). Non-linear ocean-atmosphere dynamics (Barker et al. (2015); Olsen et al. (2005)) would need to be included in respective simulation attempts."
(The new references will be added to the reference list.)

*Reviewer#1:*
*Does the temperature of the deep sea decrease during LGM in the Winguth LGM flow field, the way that Mg/Ca and deep pore water oxygen isotope temperature proxy data suggests that it did? That change in $CO_2$ solubility, and the change in atmosphere / ocean $CO_2$ partitioning, may not be represented in that flow field.*

Our response:
After the new text as cited above, we plan to insert the following passage for clarification and critical appraisal:
"Also the representation of sea water temperature changes can be improved. The LGM sea surface temperatures have been accounted for through the respective glacial forcing field underlying the simulation (CLIMAP Project Members (1976); CLIMAP Project Members (1981)). The simulated deep water temperature drop below 1500 m was around 1.2◦C on the average (Winguth et al. (1999)) as compared to the pre-industrial/interglacial simulation, with some areas where the temperature difference was up to -2◦C, especially in the North Atlantic deep water. Reconstructions of bottom water temperatures through oxygen isotope pore water analysis revealed a temperature decrease of around 2◦C at the Carnegie Ridge (Pacific) and the Ceara Rise (Atlantic) (Cutler et al. (2003)) and close to deep water productions sites cooling of deep waters in North Atlantic, South Pacific, and Southern Ocean by about 4-5◦C, 2.5◦C, and 1.5◦C (Adkins et al. (2002)). Consistent with this, bottom water interglacial-glacial temperature changes have been inferred from Mg/Ca paleo-thermometry (Dwyer et al. (1995), Skinner et al. (2003), Roberts et al. (2016)). The modelled sea water temperatures may thus be somewhat higher than the observed ones, especially for the Southern Ocean. It should be note that the circulation change experiment with the biogeochemical model was carried out with preindustrial temperatures (for the biogeochemistry only) in order to separate the temperature and circulation effects properly (and to avoid linear parameter dependencies in the inverse approach)."
(The new references will be added to the reference list.)

*Reviewer#1:*
*The authors seemed to recoil from the idea that $CaCO_3$ production could have been lower during LGM, because of the expectation from lower $CO_2$ that $CO_3^{2-}$ would be higher, and thus calcification rates higher. One proposed mechanism to produce a systematic decrease in $CaCO_3$ production was "Silicate leak" from the Southern Ocean, flushing the thermocline with Si which crowded out $CaCO_3$ producers. I'm not advocating that idea, because there's no clear link in sediment traps today between Si / N ratios and the balance between $CaCO_3$ and organic carbon. Another potential $CaCO_3$-decreasing driver is colder temperatures. At any rate, the expected increase in $CaCO_3$ with decreasing $CO_2$ is not really iron-clad either. I understand about the decrease in $CaCO_3$ called for by the inversion not being robust; that is a valid argument. But I don't see that a decrease can be disregarded on first principles.*

Our response:
Yes, we agree that the rain ratio hypothesis cannot be discarded. To make this clearer, we plan to modify line 27 on page 14 and insert the following text (after: "The rain ratio change shows a dramatic decrease in pelagic $CaCO_3$ production"):

"Such a change may not be fully out of scope (see, e.g., the discussion in Broecker and Peng (1986) and Berger and Keir (1984)). Archer and Maier-Reimer (1994) argued that enhanced $CaCO_3$ dissolution on the sea floor through organic carbon degradation in combination with a rain ratio reduction would provide an efficient way for reducing atmospheric $pCO_2$. The rain ratio change itself could be provided by an increased surface ocean concentration of silicic acid by which diatoms would dominate over $CaCO_3$ shell material production. Such a change in silicic acid concentration could be induced by enhanced iron fluxes to the Southern Ocean by dust, thinner opal frustules after the iron stress has been diminished and subsequent export of "unused" silicic acid from the Southern Ocean to the rest of the world ocean ("silicic acid leakage hypothesis", Matsumoto et al. (2002); Griffiths et al. (2013)). Further it has been argued that low seawater temperatures lead to lower water column remineralisation rates for organic carbon and changes in the ecosystem community structure that would imply a rain ratio reduction (Matsumoto et al. (2007)). On the other hand, in case of a strong coupling between deep POC fluxes to $CaCO_3$ fluxes (where $CaCO_3$ works as ballast for downward organic carbon transport; see Klaas and Archer (2002) and Armstrong et al. (2002)), rain ratio shifts at the ocean surface would only have a minor impact on atmospheric $pCO_2$ (Ridgwell

(2003)). Further, one would expect rather an increase in $CaCO_3$ production at low ambient $pCO_2$ and high $CaCO_3$ saturation (Zondervan et al. (2001); Riebesell et al. (2000)). Therefore,… "
(The new references will be added to the reference list.)

*Reviewer#1:*
*So the paper could be improved by responding perhaps to these issues and by editing the text for some wordiness and Germanic idiom, but in general the paper represents real progress on a difficult topic, and is clearly worthy of publication.*

Responses to the scientific issues are given above. The language issues will be removed.

(The references for the citations above are listed in the response to reviewer#2 together with the references for that reviewer.)