Clim. Past Discuss.,
doi:10.5194/cp-2016-35-RC1, 2016

Climate of the Past Discussions

Interactive comment on "Ocean carbon cycling during the past 130,000 years – a pilot study on inverse paleoclimate record modelling" *by* Christoph Heinze et al.

D. Archer (Referee)

d-archer@uchicago.edu

Received and published: 13 May 2016

This paper is a clear advance in the question of understanding the glacial / interglacial atmospheric CO2 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.

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).

Does the temperature of the deep sea decrease during LGM in the Winguth LGM flow field, the way that Mg/Ca and deep porewater oxygen isotope temperature proxy data suggests that it did? That change in CO2 solubility, and the change in atmosphere / ocean Co2 partitioning, may not be represented in that flow field.

The authors seemed to recoil from the idea that CaCO3 production could have been lower during LGM, because of the expectation from lower CO2 that CO3= would be higher, and thus calcification rates higher. One proposed mechanism to produce a systematic decrease in CaCO3 production was "Silicate leak" from the Southern Ocean, flushing the thermocline with Si which crowded out CaCO3 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 CaCO3 and organic carbon. Another potential CaCO3-decreasing driver is colder temperatures. At any rate, the expected increase in CaCO3 with decreasing CO2 is not really iron-clad either. I understand about the decrease in CaCO3 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.

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.

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-35, 2016.