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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "The carbon cycle during the Mid Pleistocene Transition: the Southern Ocean Decoupling Hypothesis" *by* P. Köhler and R. Bintanja

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The object of the paper is to understand how the climate / carbon cycle system changed dynamically to generate the change in ice age cycles from the 40k world in the early part of our 2 my glaciation, to the Mid-Pleistocene Transition (MPT), to the present 100k world. To do this the authors drive a model using reconstructed forcings, to see what processes in the model might lead to the observed trends.

The forcings are mostly derived by correlation with an ocean del-18O stack record, but where del-18O is a poor predictor, as is the case for equatorial sea surface temperatures for example, the forcing is derived from other records.

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Interactive Discussion

Discussion Paper

The salient result that they are having trouble fitting with the model thus forced is that the O-18 amplitudes get smaller in the 40k world but the C-13 doesn't really get smaller. This they are calling an increase in the C-13 sensitivity. They try various scenarios such as variations in iron deposition rate to explain the difference, but are unable to reproduce the change in the C-13 sensitivity. The atmospheric CO2 cycles are also smaller than observed, a long-standing problem.

The only explanation they offer to get the model to simulate the data is a systematic variation in the stratification of the Southern Ocean with climate, which impacts the atmospheric CO2 and ocean C-13 records. This step was a bit rocky for me; I can understand and sympathize that they found nothing else that worked. But I didn't understand where exactly the statement comes from, that "The factoral analysis [of the model results] has shown" that the Southern Ocean is responsible for the different responses of the 40k and the 100k worlds.

Southern Ocean stratification is commonly invoked to explain atmospheric CO2 changes, and the scenario proposed here is similar to others. But this mechanism has been shown to be essentially an artifact of the way that box models of the ocean / atmosphere carbon cycle work. It is true that some mechanism exists in the real world to alter atmospheric CO2, which continuum models of the ocean carbon cycle have yet to reproduce. But the difference in the way that CO2 partitions itself between the atmosphere and the ocean between the two model types is determined by processes that the continuum models clearly do better than the box models, such as high latitude convection.

So I personally don't regard the conclusions of a box ocean carbon cycle model to be much more than speculation. Ocean carbon box models deceive and make promises that they cannot keep when things are looked at in detail. I fear that the ocean box model may be leading in a wrong direction, as it has led so many others to the Southern Ocean; perhaps this focus is a product of the box model artifact rather than a possibility for the real ocean.

On the other hand, there are no continuum ocean models that could be used to tackle these time scales, so perhaps this is the state of the art for the time being. I guess it would be fair for the editor to allow the model to be used, but ask for a bit more of a caveat in the text to the known bias of box ocean carbon cycle models to have a stronger atmospheric CO2 response to the intensity of the biological pump in the Southern Ocean (stratification affects atmospheric CO2 by allowing the biological pump to deplete surface nutrients).

This paper is a useful exercise to collate paleo-data within the unified framework of a model. The exercise tends to bring out the features of the data that provide the tightest constraint, as the C-13 data here do.

Figure 8 could use the observations, for comparison.

Interactive comment on Clim. Past Discuss., 4, 809, 2008.

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