Clim. Past Discuss., 5, C311–C314, 2009
www.clim-past-discuss.net/5/C311/2009/

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of the Past
Discussions

Interactive comment on "Glacial – interglacial atmospheric CO_2 change: a possible "standing volume" effect on deep-ocean carbon sequestration" *by* L. C. Skinner

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Received and published: 2 June 2009

In a box model, atmospheric CO2 is observed to decline upon transferal of a greater volume of the deep ocean from the dominantly North Atlantic-ventilated to Southern Ocean-ventilated deep ocean box. This CO2 decrease is claimed to arise from a new dynamic (a "standing volume" effect), providing a CO2 decline that will contribute significantly to achieving the full glacial CO2 reduction. I am not sure what is going on in the model, but I don't think that I agree with this. The model's CO2 reduction is due to an increase in the efficiency of the global biological pump (which the author recognizes) but also to an increase the strength of the temperature-affected solubility pump (which I don't think the author has identified). The change in deep ocean volume could be argued to be a novel mechanism for changing the biological pump (although highly

derivative from arguments that have been made with regard to "nutrient deepening"). However, the biological pump effect seems to me to depend on arbitrary aspects of the model and/or the experiments undertaken (discussed below). Moreover, thinking about the model experiment and its results causes me to guess that the biological pump effect is weak, so that the dominant CO2 effect is actually from the solubility pump. Finally, regardless of the relative roles of the biological and solubility pumps in the experiments, no new corner of parameter space has been recognized that will make it easier to explain the total decline of CO2 during ice ages. To the degree that these model experiments cause a CO2 decline, they have siphoned away CO2 decreasing capacity from other specific ocean changes that are still burdened with the larger part of the CO2 decline, in particular, nutrient drawdown in the polar surface ocean and cooling of the deep ocean.

With regard to the biological pump, the model parameterizations are such that export production out of any given surface box changes only weakly as a function of the nutrient supply to that box (or, more specifically, the nutrient concentration in that box). The result is as follows. The southern deep box is the highest nutrient-concentration box in the model. Therefore, as the southern deep box is expanded, the nutrient concentrations of all the subsurface boxes decline (including that of the southern deep box), which lowers the nutrient supply to all the surface boxes (as highlighted by the author on p 1278). Because productivity does not decrease as much as the nutrient supply into the surface boxes, the degree of nutrient consumption increases in all of the surface boxes (including the low latitude surface ocean), which lowers CO2 in a way that is expected from previous work. It seems invalid to develop a model framework that allows a significant CO2 decline to be achieved by an increase in the efficiency of the biological pump in the low latitude surface ocean, as the low latitude surface ocean is already essentially completely nutrient deplete (or rather, such a study should be about C/N/P ratios). Yet this happens in the model experiment (phosphate in the low latitude surface drops from 0.28 to 0.02 micromolar). Moreover, with regard to the high latitudes, a decrease in nutrient supply may or may not be met with a comparable decrease in productivity; here, effectively constant productivity is assumed but not justified within the manuscript in terms of relevant biological oceanographic concepts.

However, I question whether the biological pump component is important at all, based on two observations. First, as the Southern Deep (SD) box grows, a greater fraction of the ocean interior will be filled with largely preformed phosphate. What this means is that most of extra phosphate stored in a larger SD would be without respired CO2. Thus, I expect little of the CO2 drawdown to be achieved by storage of additional respired CO2. Second, in Fig. 6, the author shows a special model experiment in which the NDW/SDW ratio is varied while holding the S.O. surface box at a phosphate concentration of zero (i.e. 100% efficiency with respect to the biological pump). Since most of the efficiency of the biological pump to be gained in this model derives from the Southern Ocean and its unused surface nutrient pool, I take this experiment as a rough indicator of the changes in the solubility pump (although the biological pump does become more efficient in the low latitude ocean and the North Atlantic as the NDW/SDW ratio is decreased, as discussed above, and they must cause part of the CO2 decrease). The CO2 decline that occurs with decreasing NDW/SDW in this case is, I am guessing, due to the fact that the SDW is 4C colder than NDW, such that the mean ocean temperature is dropping by $\sim$3C as the NDW/SDW ratio is decreased over its full explored range. As a result, CO2 is taken up by the ocean, and CO2 declines. The observed CO2 change is about the right range, given previous studies of this effect. Thus, this experiment effectively preempts the global deep ocean T decline that has been documented in the glacial ocean: the CO2 decline it produces is at the expense of a global deep ocean cooling experiment that is justified by the available data, separate from considerations of northern versus southern origin for deep waters.

I have two final criticisms, that I have not fully thought through. First, the author has argued that thinking about ocean volumes keeps the hypothesis more tightly connected to the observations. But the Atlantic is only a quarter of the global ocean, so extrapolating its ND/SD volume changes to the Indo-Pacific is questionable. For example, North

Atlantic-derived water in the Pacific should be nutrient-rich because of the accumulation of the products of regeneration. It is unclear that transitioning from ND to SD in the Indo-Pacific will significantly redistribute nutrients into a sequestered deep reservoir. Second, and in a related vein, there is something very strange about attributing significance to box volumes in a box model, separate from exchange rates among boxes. In a box model, the relative importance of ventilation from two distinct source regions, on a global basis, is not set by the volumes of the respective boxes they directly ventilate but rather by their relative inputs into the subsurface.

I look forward to learning the resolution of the questions raised above, and I accept that my criticisms may be incorrect. However, this is where the information supplied in the manuscript leads me.

Interactive comment on Clim. Past Discuss., 5, 1259, 2009.