Clim. Past Discuss., 6, C1537–C1541, 2011
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Climate of the Past Discussions

Interactive comment on "Deep ocean ventilation, carbon isotopes, marine sedimentation and the deglacial CO₂ rise" *by* T. Tschumi et al.

Anonymous Referee #2

Received and published: 7 March 2011

The paper by Tschumi et al. investigates the effect of three scenarios that have been put forward to explain (part of) the glacial to interglacial change in amospheric $p\mathrm{CO}_2$. These scenarios are

- a global reduction (glacial to interglacial) in the rain ratio, i.e. the ratio between the vertical sinking flux of CaCO3 shells and organic carbon, through a reduction in pelagic calcification
- a global increase in the vertical sinking flux of organic matter as would e.g. be expected from a release of iron limitation in the world's oceans through increased dust deposition

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Discussion Paper

- a reduced ventilation of the deep ocean through changes in the strength of vertical water mass exchange in the Southern Ocean that leads to a pool of dissolved inorganic carbon that is more efficiently isolated from the atmosphere than it is now.

It does so by using a carbon cycle model that contains a terrestrial, an oceanic and a sedimentary reservoir, coupled to the simple Bern3D ocean circulation model. The paper is remarkable in two ways:

Firstly model results are discussed extensively in the light of the paleooceanographic evidence from marine sedimentary cores, especially the isotopic composition of marine carbonates, the accumulation of opal and the location of the calcium carbonate lysocline. I learned a lot especially from reding the very good discussion in section 2.3.3.

Secondly, the model results show the transient behaviour of the coupled system after a change in one of the considered 'forcing' parameters, and follow the results over several ten thousand years until the sedimentary composition and the burial rates of silicate and carbonate come into equilibrium with the weathering input again. This is in contrast to the often-used 'time slice' approach, where a stationary forcing representing glacial or interglacial conditions is applied and the model then integrated into steady state. The paper shows that one gains much more information that is useful for comparison with sedimentary data this way, e.g. the behaviour of the opal deposition in Figure 8. This has been seen before using a simple box model of the ocean circulation, but it s a big leap forward to see this now done in a more realistically described ocean circulation set-up.

The paper is clearly very innovative and brings many new aspects to the interpretation of paleocoeanographic proxies. It should therefore be published in climate of the past. But I still find that the explanation and presentation of the modelling strategy needs some improvement. Otherwise the paper is well written, contains enough illustrations

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for the reader to be able to follow the argumentation, and the discussion seems to be balanced, mentioning also the weaknesses of the model results. The introduction seems a bit long, but that is maybe justified given that three hypotheses for explaining the glacial-interglacial pCO2 change have to be presented here.

My main comment concerns the modelling strategy. The sensitivity studies that Tschumi and coauthors present in this study work the following way: First a stationary state of the model is reached in several steps, by bringing first the ocean and then the ocean carbon cycle into an equilibrium, them finally bringing the sediment into equilibrium by prescribing weathering fluxes of silicate and carbonate that compensate the burial losses and further integration over an extended period. It is important to note that the stationary state corresponds to a preindustrial climate and carbon cycle, not the state during the last glacial maximum. This does make a lot of sense to me, because we do not know the glacial climate state so well to be able to judge model performace in the reached stationary state. However, the reasons for doing this could perhaps be discussed. After that, stepwise changes in one of the three control parameters (rain ratio, nutrient inventory and southern ocean ventilation) are applied and the behaviour of the model, especially its carbon cycle is followed over several ten thousands of years. The starting point for the sensitivity studies was the preindustrial state. At least this is what I gathered, I did not find a place in the manuscript where this is stated explicitly.

In the presentation of the sensitivity studies (section 2), the discussion focusses on the cases where the applied stepwise change goes in the direction of the change from interglacial to glacial (backwards in time), i.e. the rain ratio is reduced, the nutrient inventory in increased. In the third, main sensitivity study (section 2.3) the discussion also focusses on the case of reduced Southern Ocean ventilation, the case suggested for glacial climate. Because model experiments go from the preindustrial state towards the LGM, the model time-series should not be interpreted to reflect the temporal evolution of paleo-proxies directly, as clearly stated on the last three lines of p. 1920.

What I found somewhat confusing then is that in a large part of section 3 (and in Figures 8 and 10) the case discussed is that of an increase of ventilation, i.e. going from LGM towards the preindustrial. The starting point of that integration, however, is the preindustrial state with already relatively high (compared to the hypothesised LGM) ventilation. This implies a strong assumption, namely that the reaction to a change in ventilation is the same, independent of the initial state and its ventilation, i.e. whether we start from a preindustrial or a LGM state. I think this should be stated more clearly.

Minor comments:

p1903 "In contrast to ^{13}C cycling of ^{14}C is not affected by fractionation effects in the model": Maybe a citation would help the reader here if he/she would like to understand the reason for this in depth.

p1904, lines 22-23: The net burial flux of tracers was diagnosed from the previous 10000 model years?

p1912, line 27: "biologcial" -> biological

p1914, lines 4-5: is the wind stress in the SO latitude band scaled with a uniform factor? This would lead to strong discuntinuities of Ekman transport at 51S. Does this have an effect on model results? Are only the zonal winds scaled, or both horizontal components?

p1915-1916: Figure 7 is referenced before Figure 6

p1922-1923 "in response to a more vigorous deep convection": is it really the convection, and not an increase in the divergence of the Ekman transport?

References:

- Capitalization in titles of referenced papers is treated inconsistently, see e.g. Chicamoto et al. (2008) vs. Chicamoto et al. (2009)

- The journal Paleoceanography is misspelt in several places, e.g. Archer et al. (1993), Heinze et al. (1991), Menviel et al., (2008), Ridgwell et al. (2003).
- Global Biogeochemical Cycles is abbreviated inconsistently. I think the official abbreviation is Global Biogeochem. Cycles
- Fischer et al. (2010): "asynthesis" -> a synthesis

Caption to figure 2: "Pacfic" → Pacific

Interactive comment on Clim. Past Discuss., 6, 1895, 2010.