Clim. Past Discuss., 2, S981–S984, 2007
www.clim-past-discuss.net/2/S981/2007/

Climate of the Past Discussions

CPD
2, S981–S984, 2007

Interactive Comment

Interactive comment on "Application of sediment core modelling to understanding climates of the past: An example from glacial-interglacial changes in Southern Oceansilica cycling" *by* A. Ridgwell

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Received and published: 26 June 2007

I am grateful to the Referee their helpful and insightful comments & suggestions. I have taken on board these reviews and revised the manuscript accordingly:

I have expanded the model description to address the criticism that "The model methodology is not detailed enough". I have also added a new figure (#1) to illustrate the physical configuration of the model and ocean circulation as per the recommended inclusion of "A 3D cartoon of the model" and which also addresses the "initial condition [of] circulation, upwelling". The inclusion of a schematic of the baseline (modern) state of global biogeochemical cycling for Si (new Figure 2) goes some way to addressing the recommended inclusion of "initial condition [of] nutrient content". As for further details

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and model-data comparison regarding other aspects of nutrient and carbon cycling in the baseline model, I do not believe that the full referencing of the publications in which this information is contained is insufficient. I have included additional references when appropriate, however.

I have re-phrased the text where necessary to avoid the incorrect impression that changes in sea-ice cover as an explanation for the observed features of the sedimentary opal record is a new hypothesis per se.

The Referee also lists several specific comments, which I have addressed as follows:

I appreciate the Reviewer's comments about the sea-ice forcing, both in terms of seasonal limits (CLIMAP) and interpolation (Vostok dD). I agree that it is crude and could be improved upon. However, I believe there would be a danger of creating false confidence in the model predictions if a highly detailed sea-ice reconstruction was applied to such a low resolution model. Given the context of this paper (an illustration of model-data comparison using sediment core modeling rather than an exact simulation of the biogeochemical impact of glacial-interglacial sea-ice extent) the forcing applied is not inappropriate. However, I have re-written the discussion of the sea-ice assumptions, making the caveats clearer, and have highlighted how further progress can be made in the future by means of better resolved models and more detailed sea-ice reconstruction (Section 3.2).

I agree that making model-data comparison using 230Th normalized opal accumulation rates, for instance the records from the Indian sector of the Southern Ocean presented by Dezileau et al. [2003] would be beneficial. However, at the time of the original work (2002) this data was not available. Regardless, because the 6 cores from north of the Polar Frontal Zone presented by Mortlock et al. [1991] show a relatively coherent glacial-interglacial pattern and anti-phased with the 5 cores from south of the Polar Frontal Zone, which also exhibit a coherent pattern between themselves, it seems unlikely that the analysis I have carried out is compromised by changes in sediment focusing or lateral transport. Indeed, the differences between 230Th normalized opal fluxes between LGM and Holocene presented by Kumar et al. [1995] on the same cores as used here (i.e., RC13-259, RC13-254, V22-108) are consistent with the changes in wt% opal [Mortlock et al., 1991], and exhibit the same anti-phased relationship either side of the Antarctic Polar Front. The referee's point is valid though and these matters are now discussed in the text (Section 4). I have also made it clearer that the observations are characteristic of the Atlantic sector of the Southern Ocean (and not necessarily also of the Indian and Pacific sectors, per se).

I am grateful for the pointer to the Referee for the pointer to the Gaspari et al. [2006] reference for "new and higher resolution ice records indicate that the increase in CO2 is concomitant to the drop in dust fluxes" - I have added a little more by means of explanation in discussing the model predicted response of CO2 to dust (Section 3.1) and included this reference.

I am happy to provide explanation for why "If there is a 35% increase in opal export during glacial times south of the APF, why is there no imprint in the sediment?" and which is a very fair question. The reason is partly that there is an increase in opal dissolution in the sediments, which itself is enhanced by predicted glacial bottom-water H4SiO4 concentrations due to a 14% draw-down of the glacial H4SiO4 inventory compared to the subsequent (present) interglacial. There is also an increase in the dust dilution of opal, decoupling the change in wt% opal as recorded in the sediment core records from changes in opal preservation. I have expanded on the description of the Fe fertilization results section (new Section 3.1).

As to why there is a "continuous increase of the modelled opal record in core RC13-259 (Figure 1)?" - firstly, I assume that the Referee is looking back-to-front (upside down?), as there is a slight decline (not increase) in mean wt% opal with time over the glacial-interglacial cycles. This is caused by a small long-term imbalance between the sources and sinks in the model, with the oceanic H4SiO4 inventory declining by about 10% over the course of 4 glacial-interglacial cycles. I have now highlighted this issue and make this clear in the text (in Section 3.1).

Interactive comment on Clim. Past Discuss., 2, 1371, 2006.