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Climate of the Past Discussions

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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General comments

In this paper, Andy Ridgwell shows how synthetic sediment records can provide constraints on two prominent hypotheses that have been proposed to explain the glacial-interglacial changes of atmospheric CO_2 concentrations. The paper does not present novel concepts *per se* — the roots of the two hypotheses considered can be traced more than ten years back, sedimentary processes have been included in other global carbon cycle models before, and their potential usefulness discussed as well. However,

the present study is, to my best knowledge, the first one where model generated sedimentary records are used to draw quantitative conclusions directly in combination with actual data. It therefore represents an important step forward on the way to consistent application of data-assimilation techniques in paleoceanography, opening tremendous new possibilities.

The scientific methods and assumptions are clearly outlined. There are a few minor details missing which readers might want to know about (see *Specific Comments* below). The description of the methodology is adequate for this short paper. The main properties of the ocean carbon cycle model are well summarised. A comprehensive description can be found in the author's PhD thesis, the text of which has been available online for several years now; the corresponding URL is given in the reference section. There are certainly more recent reconstructions than CLIMAP for the evolution of the seasonal extent of sea-ice cover. However, the point made by Ridgwell that the CLIMAP sea-ice extent leads to such a good synthetic opal record, whereas considering little glacial-interglacial change in the summer-time limit does not, is an interesting one and cannot simply be dismissed.

The overall presentation is good to excellent. I cannot see any parts that could possibly be reduced, combined, or eliminated. A slight reorganisation of Section 3 would be beneficial for the overall readability of the paper. Each one of the tested hypotheses deserves its own subsection; part of the discussion would better be moved into an additional new section. The provided figures are informative and of good quality. I would welcome two or three extra ones to illustrate additional results that are only briefly mentioned, but would definitely add extra value to the paper (for details, please see the *Specific Comments* section below).

In the title and toward the end of the text, the spirit of the paper hesitates between a usefulness demonstration and a research paper. This is most apparent in the *Conclusions*. The results presented in this paper are important enough and the discussion sufficiently substantial to reduce that usefulness aspect to a minimum. I am not con-

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vinced that there is any need to stress the extra value of having explicit representation of ocean-sediment interaction and sediment diagenesis and preservation processes in global biogeochemical models. It is a matter of fact that sediments are part of the system and that they play a role in it. Clearly, some of the hypotheses put forward to explain the glacial-interglacial CO_2 changes will have a hard time once their imprint in the sedimentary record is taken into consideration in a truly quantitative way. The two particular ones that Ridgwell focuses on here somehow get away with it; others could be less fortunate.[1]

In conclusion, I would like to recommend publication of this manuscript after minor revision.

Specific comments

The description of the modelling methodology is all in all adequate for the paper. It would be interesting to get a few additional details about the sediment model included, which is obviously central to the paper. It would in particular be interesting to know more details about the "[...] diffusive-like transfer [...]" between the deeper sub-layers (page 1374, lines 2–11), e.g., the biodiffusion coefficient adopted, the depth to which bioturbation is allowed to extend, etc.

It would be desirable to have a few extra graphs that illustrate additional results that the author only shortly mentions in the text:

- page 1378, lines 15ff: "[...] the prominent ∼10–20 wt% opal highs; [...] in cores lying 5–10° further to the north (Mortlock et al., 1991)";
- page 1378, lines 17ff: the intersector differences;

[1]Munhoven, G., Glacial-interglacial rain ratio changes: Implications for atmospheric CO_2 and ocean-sediment interaction, Deep-Sea Research II, submitted, 2006.

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- page 1378, lines 24ff: the δ^{13}C result is important as well (although, the paper deals with silica in the first place, I admit);
- page 1379, lines 22-26: if the combined effect gives a "[. . .] better simile of the opal data particularly south of the APF [. . .] than sea-ice alone," and if "This beneficial interaction is not obvious from the effects of the two mechanisms in isolation" — and I agree that this *is* not obvious – then there is no reason for not showing that result.

I suggest to pick two to three out of these. I would furthermore be glad to see the generated atmospheric CO_2 histories, which could easily be plotted on a fourth panel on each graph.

I was somewhat disappointed about the *Conclusions*. In their present form, they remain too general. They almost only focus on the usefulness of including sedimentary processes in global biogeochemical models. There are important results in this paper that must be restated here. Part of the last paragraph on page 1379 could also be moved to the *Conclusions* section.

Technical corrections

I only spotted two typos, both of them on page 1372, at lines 20 and 21 respectively, where "Toggweiller" should read "Toggweiler."

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