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Climate of the Past Discussions

Interactive comment on "Glacial – interglacial atmospheric CO_2 change: a simple "hypsometric effect" on deep-ocean carbon sequestration?[1]" *by* L. C. Skinner

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Below I lay out my responses to the comments provided by both of the anonymous referees with regard to the CPD manuscript entitled: 'Glacial - interglacial CO2 change: A hypsometric effect?' A revised manuscript (submitted online) accompanies these responses, and includes changes that are intended to address the referees' comments.

I would like to thank the two anonymous reviewers for taking the time to provide comments on the manuscript under discussion; their efforts are much appreciated.

General comments:

[1]Invited contribution by L. Skinner, one of the EGU Outstanding Young Scientist Award winners 2006

Both reviewers are, in the event, rather critical, and remain essentially unconvinced by the manuscript. Both offer the same hypothesised error in my methods, in addition to some more specific qualifications. I think it is important to stress that the core criticism shared by both referees represents a suspicion of an inability to maintain mass balances in the model; a suspicion that can certainly be demonstrated to be false. I do concur however, that for such suspicions to have arisen in both reviewers (assuming their comments to be independent, despite their cross-reference), the manuscript cannot have been as clear as is necessary. Accordingly, requisite changes have been made to the manuscript, and these are discussed below. I will address the main suspicion shared by both referees first, and afterwards will address the specific comments of each referee in turn.

The main suspicion shared by both referees (hinted at by Referee #2, and subsequently taken up and expanded by Referee #1) is that I may have performed a 'non-physical' experiment. This is not correct: I have not violated the conservation of the oceanic phosphorous, or alkalinity, or DIC etcĚ budgets. This is what was proposed implicitly in the 'thought experiment' described on page 717-718 (section 2, page 7 in the revised manuscript PDF) as pointed out by Referee #2, but it is also precisely what is NOT done in the box model - indeed this is the whole point of using a box model in the first place. Boxes are prescribed a certain geometry (this is how all box-models are created, although they are usually not changed for different experiments), and it is the circulation pathways and rates, with respect to the particulate export pathways and magnitudes (as well as the gas exchanges etc...) that determine the ultimate distribution of chemical species amongst the various boxes, and hence the ultimate partitioning of carbon between the ocean and the atmosphere.

In this box model (perhaps trivially), in order to shift nutrients/carbon into the deep sea by expanding AABW-like deep-water, they must be drawn from elsewhere in the system: i.e. the other boxes, in particular the surface boxes. This is illustrated in a new figure after Fig. 8 (and discussed on page 14-15 of the revised manuscript

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PDF), where an illustration of the output from the 'southern flavour ocean' experiment has been added for increased clarity. This figure (revised PDF Fig. 9) indicates that when the 'southern flavour ocean' experiment runs to equilibrium there is a transferral of nutrients and carbon from the surface boxes to the deep boxes, and in particular to the 'southern deep-water' box, primarily due to the invariant flow paths/rates and particle fluxes, which cause the southern deep-water box to have the highest alkalinity, TCO2 and phosphate concentration of all of the boxes. An increase in its volume thus requires a transferral of carbon away from the other boxes, precisely because the amount of carbon, phosphate etc... in the model must be conserved. A discussion of the difference between 'concentration conservation' versus 'mass conservation' has been added to the revised manuscript to make this point clear (revised PDF p.14-15). As originally stated on page 719, line 18 (revised PDF p.8), the global biogeochemical budgets are kept constant, and all model runs are initiated with concentrations in all boxes equal (set to zero for atmospheric pCO2, and set to the global average for other constituents). Furthermore, all model parameters are kept constant for the 'modern' and 'hypsometric' experiments; it is the geometry of the boxes alone that is changed (but not the total budgets or the initial values). This was originally stated on page 725, line 13 (revised PDF section 5.2, p.13-14), where it was indicated that the 'hypsometric experiment' is effected by only changing the volume ratio of northern- versus southern sourced deep-water in the box model.

The suspicions of both referees (and their negative recommendations) are therefore unjustified in my opinion, though I do concur that it is possible to state more explicitly in the manuscript why this is the case. This has been addressed in the revised manuscript by including an Appendix detailing the box-model equations, as well as an illustration of the 'southern flavour' experiment output data (both of these additions are discussed further below).

At this stage it is worthwhile commenting on a point made by Referee #1. Referee #1 states that "since the preformed nutrient content of North Atlantic Deep Water is much lower than that of Antarctic Bottom Water, a simple mechanism for lowering CO2 during ice ages is to reduce Antarctic Bottom Water formation while maintaining North Atlantic overturning", referring to the box-model experiments of Toggweiler [1999] in particular, although the study of Toggweiler et al. [2003] is perhaps most relevant in this regard. This statement illustrates perfectly the whole point of the approach I am adopting. The only reason that it makes sense to reduce the export of a high preformed nutrient water-mass in order to reduce atmospheric CO2 is because of the eventual upwelling of that water-mass (i.e. the 'leakage' of nutrients back to the atmosphere)... not its export per se. It is the deep-water upwelling rate (equivalent to the export rate, obviously), in relation to its nutrient content, that counts in this regard. I try to draw attention to this point on page 713 (line 28) through to page 714 (line 5), as well as on page 715 (line 24) and page 716 (line 4-19 especially) with reference to the studies of Toggweiler [1999], Keeling and Stephens [2001] and Gildor & Tzipermann [2001] in particular. In order to clarify this point further I have added a short discussion of this specific issue to p.15 of the revised PDF.

Trivially, if the export of a high preformed nutrient water mass was not balanced by an equivalent upwelling (of associated nutrients/carbon) then there must be a net export of carbon to the deep sea. It is only a small step from this 'unphysical' thought experiment to a more 'physical' box-model experiment (and one that I try to test) to demonstrate that one can, with correct mass balance, effect a net export of carbon to the deep-sea if the volume of the high preformed nutrient deep-water reservoir increases in size without an increase in its overturning rate. I cannot say whether or not I agree with Referee#1 as to why the overturning rates of both deep-water masses directly correlate with CO2 (Referee #1 is not sure that I understand why), since I do not know his/her opinion, but I can say that I understand that this is because of how efficiently nutrients/carbon are supplied to the surface by the overturning circulation, as balanced by a given biological export productivity. In this, I think I am not saying anything different from Toggweiler et al. [2003] for example, or indeed Marinov et al. [2006].

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It is also worth raising at this stage a specific statement made by Referee #2, who affirms that it is not true that the bulk of the deep-sea is ventilated by southern-sourced water-masses. Firstly, this goes against the findings and interpretations of (for example) Worthington [1981], Orsi et al. [1999] (see page 61 in particular), Mantyla and Reid [1983]. Referee #2 cites the export rates of AABW and NADW, ˜ 10 and ˜ 13 Sv (10ˆ6 m3s-1) respectively, as support for their statement. Again, this illustrates perfectly a point that I am trying to address in the manuscript: that deep-water overturning rates (dimensions [L]ˆ3/[T], like Sverdrups) need to be distinguished from deep-water mass volumes (dimensions [L]ˆ3). Obviously it is possible (indeed it is currently the case; and this is crucial to the 'southern flavour ocean' hypothesis I attempt to advance, or at least to flesh out a little) that a deep-water mass can be exported at a high rate and yet represent a small volume of the ocean (c.f. modern NADW). Conversely, a very large deep-water mass can be renewed relatively slowly. Furthermore, the larger a water-mass (carbon reservoir) is, for a given overturning rate, the longer its residence time and carbon 'storage' capacity, given constant or increased biological export. The volumetric analyses carried out by Worthington [1981], among others, demonstrate very clearly the point about volumes versus export rates in the modern ocean. The model studies of Cox [1989] are also revealing in this regard, indicating that a highly idealised yet reasonable simulation of the global ocean circulation has ˜ 72 % of the deep ocean ventilated by southern sourced deep-water, and with North Atlantic sourced deep-water contributing ˜ 10 % to the Indo-Pacific ocean basins (crucially, this is not completely insignificant, despite being a minority contribution). Simulations performed using a zonally averaged ocean circulation model [Stocker et al. 1992], can also be used to demonstrate a dominance of southern-sourced deep-water in the world's ocean basins for 'modern' simulations when water-mass provenance tracers are included (ongoing work). Furthermore, this southern water mass dominance can be shown to increase for 'last glacial maximum' equilibrium runs using fully coupled AOGCMs [Kim et al., 2003; Shin et al., 2003], not to mention the host of palaeoceanogrpahic evidence listed in the manuscript (and underlined by Referee #2 themselves).

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The crucial importance of the hypsometry of the ocean basins in this regard is another issue that I try to focus attention on in the manuscript (section 5.1, page 724-725). Some depth intervals are more 'volumetrically significant' than others, and if such a depth interval comes to be occupied by a water mass of high preformed nutrients (for example) this will be important for the carbon cycle (and of course for the distribution of nutrients in the ocean given their necessary conservation/cycling). I do believe that I had made this point reasonably clear in the manuscript (originally page 716 line 15 to page 717 line 4; and page 724 line 19 to page 725 line 5), and am tempted to believe that the referees' doubt of my own ability may have clouded their reading of my written word. Scientific 'doubt' is absolutely essential, to be sure, but I think it should ultimately seek explicit confirmation somewhere: scepticism must end somewhere, to paraphrase a well-known Viennese philosopher!

Above I have tried to allay the fears of both referees that I may have violated basic mass conservation rules (note in particular page 719, lines 14-16 and lines 18-20, in the original manuscript). I have further tried to indicate that their own comments help to demonstrate precisely why the experiments I carried out are neither wrong nor inappropriate, but rather revealing instead, and I think somewhat novel in their tack. Furthermore, I believe that many of the comments made above reiterate points that were expressed in the manuscript already. Nevertheless, I have tried to revise the manuscript in order to better convey these messages. Below I seek to address the more specific comments that are made by each referee in turn.

Referee #1:

Apart from the main suspicion referred to above, Referee #1 makes two further comments. The first, as indicated by Referee #1, is somewhat tangential to the manuscripts goals, but nonetheless deserves clarification. It is suggested that the d13C of DIC, to a first approximation, represents the preformed nutrient composition of the deepwater, i.e. not the sum of preformed and re-mineralised nutrients. On this basis it is argued that benthic foraminiferal d13C does not represent "a reliable indicator of glacial/interglacial changes in whole ocean nutrient reservoir". Firstly, if this is true, then my suggestion (along with the interpretations of Duplessy et al. [1989] and Curry & Oppo [2005], and othersĚ) that the glacial - interglacial changes in benthic d13C in the Atlantic tell us something about deep-water provenance changes at various depths and latitudes (i.e. in various volume classes) is justified, and merits explicit analysis with regard to carbon sequestration (which is what I have tried to do). However, the conditional validity of Referee #1's statement also requires that no organic carbon from biological export be re-mineralised in the water column, which is clearly somewhat inaccurate [Kroopnick, 1985]. Obviously the truth is (conditionally) somewhere in between these two paradigms; hence the utility of exploring the implications of one of them for example. In the manuscript I explore the implications of precisely the paradigm that is proposed as a working hypothesis by Referee #1.

A second comment made by Referee #1 was that there was no mitigating value in 'turning the knobs' of overturning rates or polar productivity in the model, primarily because this has been done better by others. I think that a number of comments provided above already indicate why this is not a fair charge. In particular, I hope that the confusion between overturning rates and volumes, made by Referee #2 (see above), underlines the potential value of looking at the interaction of these very different 'knobs' and of contrasting their effects. Note that such an exercise has not been done explicitly (even if it might have been done better) elsewhere, presumably because it is assumed that either: 1) changes in deep-water mass volumes don't matter; or 2) changes in deep-water mass volumes simply haven't occurred. If neither of these assumptions is valid, then surely it is valid to explore their antitheses explicitly. To reiterate the point: the focus is on distinguishing the effects of volumetrics, from those of overturning rates (i.e. nutrient/carbon upwelling rates) and biological export rates. The latter two have indeed been explored previously, but not in the context of water-mass volume changes. I think this lends their mention some extra value in fact.

Referee #2:

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Above I have addressed points 3 and 4 made by Referee #2. The main concern of Referee #2 (point 1) was that insufficient information was provided to recreate the model. Although it is true that the equations for each box and chemical species were not originally provided (this can easily be done in a dedicated Appendix, which has been included in the revised manuscript, and referenced in Section 3, PDF p.8), it is not true that a complete list of the input parameters for each experiment was not provided. As originally stated on page 724 (and section 4.2 of the revised manuscript), the input parameters for the 'modern' experiment were listed in Table 1. For the 'southern flavour' version no changes were made to the input parameters; only the volume ratio of northern- to southern sourced deep-water was changed, as was originally indicated on page719, line 16 (revised PDF section 3, p.8) and on page 725, line 13 (revised PDF section 5.2, p.13-14). In addition, the output parameters for the 'modern' run were originally given in Figure 6, plotted against modern values. The only data that were omitted in the original manuscript were the TCO2, alkalinity and phosphate outputs for the 'southern flavour ocean' run. As mentioned above, an updated version of the manuscript includes a new figure (after Figure 8) showing the TCO2, alkalinity and phosphate concentrations for each box in the 'southern flavour' run (discussed in section 5.2 of the revised manuscript). This inclusion should also allow readers to calculate for themselves that global budgets have indeed been conserved during the experiment (as discussed above).

The second point raised by Referee #2 was the desire to have 14C included in the model, and used to assess the overturning and mixing parameters for the model runs. This is certainly possible, and could help to constrain the overturning rates that might (in the box model at least) be compatible with the volume changes that are proposed. However, it is doubtful that any robust conclusions regarding suitable overturning rates could be concluded from this analysis, given the simplified nature of the exercise, as well as the paucity of actual radiocarbon data from the deep ocean basins ($>$2-3 km) across the last de-glaciation. Arguably therefore, including such an analysis in a revised manuscript would only require rather tentative conclusions regarding overturning

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rates to be drawn (which is not really the main focus of this study).

Point 5 raised by Referee #2 was that light- and iron limitation of biological productivity could not be ignored in a consideration of the carbon cycle. Clearly a complete model of the carbon cycle would have to include these parameters (and indeed others, such as a terrestrial biosphere, sea-ice, thermocline variability etcĚ not to mention more ocean basins); however even if these were included, they would not be changed in the experiments conducted here, because it is specifically the volumetric effects (via the ocean floor hypsometry) that are being studied. I would suggest that a line has to be drawn somewhere, so that clearly formulated and well-posed (if indeed idealised) questions can be addressed one at a time, at least initially with a simple (essentially conceptual) model such as the one outlined here. I think that this was already clearly indicated on page 720, lines 17-12 (revised PDF p.9 end of first paragraph) and on page 728 (revised PDF section 6, p.17) in the original manuscript, where it was stressed that the goal of this study is not to provide a complete explanation of glacial - interglacial CO2 change, but rather to explore a potentially important mechanism for deep-ocean carbon sequestration that has not really been raised previously.

Referee #2 also suggests that the word ‘skill’, in describing the ability of the model to simulate modern data is misleading. This point was already specifically raised on page 724 of the original manuscript (revised PDF p.12-13; section 4.2 was entitled ‘Model Realism’ on purpose), where an awareness of this issue was indicated and it was stressed that tuning to modern CO2 has indeed been performed. In the revised manuscript, for extra clarity, it is stated that tuning was not performed with regard to modern phosphate, alkalinity and TCO2. I feel that adopting the word ‘consistency’ (which is perhaps more precise) would actually confuse readers, while adopting another word such as ‘realism’ would actually be more misleading. If the Editor can suggest another more suitable word, I would be happy to use it as a substitute for ‘skill’.

Referee #2 raises two more issues. The first is that specific mention should be made of the particular value of Cd/Ca data in demonstrating glacial - interglacial changes in nutrient distribution in the Altantic. I completely agree; it certainly was not my intention to undermine the value of this proxy, nor any other for that matter. In my view the fact that benthic d13C requires 'support' for its correct interpretation lends added value to those other proxies/studies that can do so, rather than the opposite. I have tried to make this clearer in section 2 of the revised manuscript.

A final question posed by Referee #2 is how the concept outlined in the manuscript differs from the Toggweiler [1999; 2006] models. This is an interesting point, especially when juxtaposed with the apparent misgivings of the reviewers. It is certainly true that a close link exists between them, as with the concepts outlined by Watson and Naviero-Garabato [2006] or indeed Marinov et al. [2006] in particular, but it is definitely not one of equivalence. As stated on page 716, line 15 of the original manuscript (revised PDF end of section 1, p.5), these studies, and others, have all focussed on overturning rate changes, or simple chemical contrasts, without explicitly considering volumetric (i.e. water mass distribution) effects. Because this is an important distinction to be made (i.e. volume or mmol, versus 'Sverdrups' or mmolkg-1, for example), a more explicit discussion of precisely this issue has been added to the revised manuscript, Section 5.2, p.14-15.

I sincerely hope to have adequately addressed the referees' comments with the above responses, though I look forward to further correspondence should this transpire!

Interactive comment on Clim. Past Discuss., 2, 711, 2006.