

Interactive comment on “Glacial – interglacial atmospheric CO₂ change: a possible “standing volume” effect on deep-ocean carbon sequestration” by L. C. Skinner

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I didn't appreciate until I read this manuscript that the recent literature on the glacial-interglacial CO₂ problem contains two seemingly contradictory views on the role of Antarctic Bottom Water (AABW) in the problem. By one view (View 1), filling the ocean with AABW at the expense of NADW should raise atmospheric CO₂ because of the high preformed nutrient content of AABW compared to NADW. This view builds on the long-standing understanding of the relationship between atmospheric CO₂ and preformed nutrients. By the second view (View 2), filling the ocean with AABW at the expense of NADW lowers atmospheric CO₂ because AABW has a higher dissolved

C236

inorganic carbon (DIC (also called TCO₂)) content compared to NADW. View 1 is perhaps best expressed in papers by Toggweiler (e.g. Toggweiler et al., 2003), while View 2 is expressed e.g. by Brovkin et al. (2007). This paper plays a valuable role by highlighting this apparent contradiction and making some progress towards understanding how the views can be reconciled.

The paper, however, is only secondarily focused on this apparent contradiction. The main focus is developing the case that atmospheric CO₂ can be reduced simply by increasing the volume of AABW without changing flow rates relative to its formation. The focus, in other words, involves fleshing out View 2. This volumetric effect is given the moniker “standing volume” (SV) effect, and the effect is presented as a mechanism distinct from earlier mechanisms.

The manuscript is generally well written and engaging. Nevertheless, several major technical and conceptual issues detract from the paper's ability to “make the case” for the SV effect and to resolve the apparent contradiction highlighted above. These major issues and several other minor issues would need to be addressed before the manuscript is suitable for publication.

Major Issue 1:

It seems that the concept of the standing volume (SV) effect is not used consistently throughout the paper. The basic concept, as developed in the Section 1, seems simple: The effect is the change in atmospheric CO₂ that results from changes in the volume but not flow (i.e. volume flux) of different water masses. In Section 2, the concept is given further elaboration: The effect results from changes in volume, but not in either flow or in chemical properties such as DIC, radiocarbon, etc. of the water masses. In Section 3, the concept is explored in a six-box model, where it is further assumed that particle fluxes into the relevant boxes increase or decrease in proportion to the box volume.

It is easy to show that these different expressions of the SV effect are inconsistent.

C237

Consider the mass balance of a single well-mixed box of volume V with a single input and output flow Q :

$$VdC/dt = Q(C_0 - C) + P_i$$

where C is the concentration (e.g. DIC) in the box, C_0 is the concentration flowing into the box, and P_i is the input from decay of sinking particles in the box. In steady-state ($dC/dt = 0$), the volume V drops out of the expression, so one is left with an expression involving only Q , $C_0 - C$ and P_i . To achieve the behavior envisioned in Section 2, i.e. to keep $C_0 - C$ and Q constant while changing V , it is sufficient to keep P_i constant. But this contradicts the assumption that P_i increases with box volume, as assumed in the six-box model. Furthermore, for radiocarbon an additional decay term is needed. ^{14}C must increase with the turnover time V/Q , as more time elapses for radiocarbon decay, even when $C_0 - C$ remains constant.

The assumption used for the six-box model that P_i increases with box volume is certainly reasonable, because the particle flux that is intercepted by the box will sensibly increase with box volume. But if this is how the SV effect is defined, it is inconsistent to assume, as was done in Sections 2 and 5, that chemical properties should remain constant as the volumes change. Indeed, an examination of the results of the six-box model (Tables 2 and 3) shows that the chemical gradient between the northern and southern deep boxes changes as the volumes are changed, confirming the behavior expected from the 1-box model above with variable P_i . So the 6-box model is being used to explore a different effect (or a different mixture of effects) than the SV effect as described in Sections 2 and 5.

The manuscript needs to be revised to settle on one consistent definition of the SV effect and make the various sections consistent with this definition. My suggestion would be to define the SV effect as rendered in the model: flows remain constant, particle input to boxes scales in proportion to intercepted particle flux, chemical differences vary as required by mass balance. This will some require adjustments in the Sections 1, 2,

C238

and 5. Any of the other possible definitions would appear to be purely mathematical manipulations, disconnected from physical or biogeochemical concepts.

With this definition, an important aspect of the SV effect may involve changes in chemical content associated with changes in turnover time. For example, the increase in residence time of AABW associated with an increase in its volume will allow more CO_2 of respiratory origin to be stored, not just because the volume is larger, but also because the storage per unit volume is larger. It would seem reasonable to include this also as part of the SV effect.

Major Issue 2:

Although the contradiction concerning the two views of AABW is nicely laid out (page 1277) the subsequent discussion is problematic. The core of the argument is the statement (page 1277, line 24-28) that View 1 (filling the ocean with AABW at the expense of NADW raises atmospheric CO_2) is correct only "under the assumption that an increase in the volume of AABW-like deep-water would result in an increase in the surface nutrient (i.e. phosphate and TCO_2) content, which is in fact impossible as long as AABW has an elevated total nutrient content compared to the water-mass it replaces". This statement is then followed by supporting equations.

But there seems to be an oversight here. Suppose the coupling between the AABW layers and the atmosphere were so strong, due to interior mixing, surface outcropping and air-sea exchange around Antarctica, that AABW effectively maintained equilibrium with the atmosphere? In this case, increasing the AABW volume would not have the envisioned effect, because the carbon that is released into AABW along with phosphate due to organic matter degradation would be free to escape into the atmosphere. Atmospheric CO_2 would be expected to rise as the AABW volume expanded because the expansion would entail less efficient usage of nutrients for storing carbon in the ocean as a whole. The CO_2 increase would occur despite (indeed because) AABW had more phosphate than the water it replaced.

C239

The corresponding oversight in the equations (page 1278) is the assumption that the deep carbon reservoir can be varied as a free parameter independent of atmospheric CO₂ and the assumption that carbon and phosphate are so tightly linked. (With strong coupling between AABW and the atmosphere, pCO₂ and C_{nd} would be coupled by a second equation, incompatible with the Equation on line 14 on page 1278). Alternatively, the oversight can be understood as resulting from neglecting changes in the ΔC term in the formalism (Eq. 12) of Ito and Follows (2005). Although Ito and Follows also neglect changes in ΔC , Toggweiler et al. (2003) showed that this is not a reasonable starting point for understanding Antarctic controls on CO₂.

The argument is thus not generally valid concerning the links between carbon and phosphate, and therefore does not adequately resolve the question of how the two seemingly contradictory views can be reconciled. More thinking is needed to put this discussion on a proper footing.

Let me suggest a way forward. I think the argument is on the right track in drawing attention to the DIC differences between AABW and NADW, but enters shaky territory when assuming total phosphate is also diagnostic because phosphate and carbon are too easily decoupled by air-sea exchange. Furthermore, an interesting distinction can be drawn between the Atlantic versus Indo-Pacific oceans. In the real ocean, the water that overlies AABW has lower DIC in the Atlantic but higher DIC in the Indo-Pacific. Thus while an expansion of the volume of AABW in the Atlantic would be expected to decrease atmospheric CO₂, an expansion in the Indo-Pacific would be expected to increase it. The sense of the effect in the Pacific is fully consistent with View 1. Indeed, the lower DIC content of AABW compared to circumpolar-deep water is almost certainly partly driven by CO₂ losses to the atmosphere associated with AABW formation, which can be viewed as a reprocessing of deepwater at the Antarctic surface, which also leads to the high preformed nutrient content of this water. The key is to articulate why, despite this CO₂ loss to the atmosphere, an expansion of AABW in the Atlantic is expected to cause atmospheric CO₂ to decrease.

C240

Major issue 3:

The last point raises an issue with the box model. On page 1271 it is stated “. . . although the flow scheme of the box model essentially represents an Atlantic Ocean (i.e. it has two deep overturning “limbs”), it is scaled to global proportions so that the volumes and concentrations of the atmosphere and the ocean balance with global budgets (all of which are fixed input parameters).” This approach, however, neglects that, in the Indo-Pacific, AABW actually has lower DIC content than waters immediately above it (and north of it). In a realistic global model, therefore, the gradients between the SD and the ND boxes would be reversed compared to the model used here, since the Indo-Pacific would sensibly dominate in the global description.

This issue with the box model is likely related to confusion about how watermasses are “sourced” in the Southern Ocean. In Section 1 (page 1263) it is suggested that Circumpolar Deep Water is sourced at high southern latitudes and then exported to the north into the ocean interior. More conventionally, CDW is viewed as part of the southward-flowing return flow of the deep overturning circulation which is upwelled, not sourced, at high southern latitudes. It appears that the approach taken, both conceptually and in the six-box model, involves lumping together CDW and AABW into a single watermass. In fact, AABW and CDW occupy completely different realms in the ocean, and it is hard to justify lumping together in the context of this study. If anything, it would be better to treat CDW as aged NADW or a blend of aged AABW and NADW. CDW should upwell, not be sourced, in the Southern Ocean.

To address this issue some reconsideration of model architecture and parameters is needed. It might be better to avoid lumping the Atlantic and Indo-Pacific together in the first place, since the contrasting behavior of these two realms is likely central to reconciling the two views on AABW (View 1 and View 2). Certainly, one should not lump together AABW and CDW.

Minor Issues:

C241

Section 1 and Abstract: The notion that prior work on the glacial CO₂ drawdown focused only “dynamical or kinetic” processes is perhaps a bit misleading. The idea that CO₂ is largely controlled by pre-formed nutrients has a very long history. This idea also transcends the notion of exclusively dynamical or kinetic control.

Page 1268, line 4: Why is it relevant to consider nitrogen in the box model? Unless the model tracks nitrogen sources and sinks (e.g. fixation and denitrification) there is no reason to mention nitrogen here. Better to clarify that this is a single-nutrient model, based on phosphate alone.

Page 1269: The equations defining the box model need to be presented in a more orderly fashion. The second Equation on page 1269 contains a factor R_{ns} which is not defined until much later. Does this equation perhaps have a spurious factor of C^*/P^* on the last term? It would be better to write this Equation in terms of the variable $P_i(sd)$ shown in Figure 1, followed by the Equations clarifying how $P_i(sd)$ and $P_i(nd)$ are vary with box volume (these critical equations are currently buried in the caption to Figure 1). Why are two Equations given for dC_n/dt ? This seems unnecessary. The quantity $(CO_2)_{SOL-n}$ is not defined. There is no need to present the equation for the temperature dependence of CO₂ solubility since there are many suitable versions in the literature that could just be cited. Housekeeping of this sort is needed to make model details clearly understandable.

Figure 1: The fluxes $P_i(sd)$ and $P_i(nd)$ are drawn as originating from intermediate depth. This looks odd. I think it would probably be better to draw multiple arrows from the surface box, each terminating in a different subsurface box.

Figures 2 and 3: The text in these figures is rendered too small to be clearly legible.

- R. Keeling, 18 May 2009

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