

# ***Interactive comment on “The devil’s in the disequilibrium: sensitivity of ocean carbon storage to climate state and iron fertilization in a general circulation model” by Sarah Eggleston and Eric D. Galbraith***

## **Anonymous Referee #2**

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This study deals with an interesting aspect of the global carbon cycle, relevant to understanding past natural changes of atmospheric CO<sub>2</sub> mixing ratio reconstructed from geological archives. As such it is relevant to the readership of Biogeosciences.

The decomposition of Dissolved Inorganic Carbon (DIC) into component parts based on a process-driven approach is an established procedure in the literature. This main advance here is a detailed look at a particular component of DIC (the ‘disequilibrium’ DIC) that has been overlooked in some previous studies.

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Generally, the literature is well covered by the references. Although, I believe DeVries et al. “The sequestration efficiency of the biological pump”, GRL (2012) is directly relevant to Figure 3 in this study, and should be considered when the findings linked to figure 3 are discussed.

The main findings are the de-coupling of the DIC<sub>dis</sub> from the (previously) expected behaviour based on DIC<sub>soft</sub> in some circumstances. This is shown in a number of idealised numerical model experiments. This finding is important and relevant to the literature.

Specifically, equation (15), figure (10) and the insight gained from these for the idealised simulations are important and deserve to be published provided they are found to be robust.

With the manuscript in its present form, I have some reservations as to whether the findings are robustly supported by the work undertaken. I am unable to say for certain if this can be addressed by re-writing of the manuscript, or would require altered or additional numerical model runs. Below I detail my concerns on this issue.

Major concerns:

(1) Understanding the methods described in the text and their relation to the figures.

As noted by Anonymous Reviewer 1, this study uses a fixed and constant atmospheric CO<sub>2</sub> mixing ratio of 270 ppm in the numerical model experiments. While I see nothing intrinsically wrong with this approach, the manuscript as it is does not adequately describe the impacts of this choice on their results.

For example, consider Figure 1 and accompanying text: On p5 lines 15-17, the manuscript states how a constant CO<sub>2</sub> for gas exchange of 270 ppm is used. Then, the manuscript states how the largest changes in DIC<sub>sat</sub> is driven by the changes in ln(CO<sub>2</sub>). Figure 1 does indeed plot DIC<sub>sat</sub> against CO<sub>2</sub> with CO<sub>2</sub> shown to vary.

I do not understand this figure or the text: precisely how is DIC<sub>sat</sub> a function of

$\ln(\text{CO}_2)$  if a constant  $\text{CO}_2$  of 270 ppm is used for gas exchange. If a constant  $\text{CO}_2$  of 270 ppm is used for gas exchange in the numerical simulations, then surely the  $\text{CO}_2$  cannot be changing on the axes in figure 1. Also,  $\text{DIC}_{\text{sat}}$  should always be calculated relative to a  $\text{CO}_2$  of 270 ppm, and so  $\text{DIC}_{\text{sat}}$  will not change with  $\ln(\text{CO}_2)$ .

The issue recurs in Figure 2, the caption to which indicates that some experiments are run at 180 ppm  $\text{CO}_2$  concentration.

This confusion is critical for assessment of the manuscript in its current form (e.g. also see major point 2, which derives from this).

Note that there are other studies in this topic that have dealt with similar issues well. For example, Marinov et al (2008) (cited by this manuscript) uses GCM simulations with a fixed temperature for air-sea gas exchange, but a varying dynamical temperature for ocean circulation. In that manuscript, the issue is well described and the findings are clear.

Is the issue here that one  $\text{CO}_2$  is used for the radiative forcing of climate, but another  $\text{CO}_2$  is used for air-sea gas exchange?

I cannot tell precisely with the manuscript in its present form. If this is the case, then all mentions of  $\text{CO}_2$  in ppm {and  $\ln(\text{CO}_2)$ } that are different from 270ppm could be converted to radiative forcing (in  $\text{W/m}^2$ ) with respect to  $\text{CO}_2 = 270$  ppm. For example,  $\text{CO}_2 = 180$ ppm would be re-defined as 'Glacial radiative forcing' (or a numerical radiative forcing of  $\sim -2.2 \text{ W/m}^2$ ).

For example: P11 Line 20 to line 24 reads: "The model simulations show a clear minimum  $\text{DIC}_{\text{dis}}$  at intermediate  $\text{CO}_2$  (270-405 ppm). . . . the  $\text{CO}_2$  driving gas exchange . . . is held fixed at 270 ppm."

This could be changed to something like: "The model simulations show a clear minimum  $\text{DIC}_{\text{dis}}$  at intermediate Radiative Forcing ( $0 - 2.2 \text{ Wm}^{-2}$ ). . . . the  $\text{CO}_2$  driving gas exchange . . . is held fixed at 270 ppm."

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I am currently confused by the way this issue is written about in the study. Please clarify.

(2) DIC<sub>sat</sub> and DIC<sub>dis</sub> definitions:

There are two ways of defining DIC<sub>sat</sub> and DIC<sub>dis</sub> in the present-day system of rising CO<sub>2</sub>, or over different periods when CO<sub>2</sub> has changed in the past.

Firstly, you can define both DIC<sub>sat</sub> and DIC<sub>dis</sub> relative to some fixed atmospheric CO<sub>2</sub> concentration (such as the preindustrial). Or secondly, you can define them relative to the current atmospheric CO<sub>2</sub> concentration at a particular point in time (for example it would be relative to 180 ppm at the LGM, or ~400ppm in the present day).

This choice makes a big difference. Consider the present day: if DIC<sub>sat</sub> is defined relative to present day atmospheric CO<sub>2</sub> then DIC<sub>dis</sub> is small in the surface ocean and negative in the deep ocean. However, if DIC<sub>sat</sub> is defined relative to preindustrial CO<sub>2</sub> then DIC<sub>dis</sub> is positive in the surface ocean and ~ zero at depth.

In the original discussions of DIC<sub>sat</sub> (p2, lines 14-17) and DIC<sub>dis</sub> (p3, lines 14-34), it is unclear whether a fixed or rising CO<sub>2</sub> concentration will be used to define DIC<sub>dis</sub> and DIC<sub>sat</sub>.

It eventually becomes clear (I think) that in this study, the DIC<sub>sat</sub> is calculated relative to a fixed CO<sub>2</sub> of 270 ppm (page 4, line 15). However, the point is only made when discussing the numerical model set up.

A clearer indication of how DIC<sub>sat</sub> and DIC<sub>dis</sub> are treated in this study from the outset is required. Especially given the confusing 'fixed but changing' CO<sub>2</sub> issue from my other major concern. If the experiments are run with CO<sub>2</sub>=180ppm, and DIC<sub>sat</sub> is defined relative to 270ppm then this will have a large impact on the results.

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