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## ***Interactive comment on “Watermasses as a unifying framework for understanding the Southern Ocean carbon cycle” by D. J. Lidecone et al.***

**Anonymous Referee #2**

Received and published: 8 July 2010

general comments —————

This manuscript presents an analysis of the carbon cycle of the Southern Ocean using watermass principles. When I read the abstract, I was intrigued by this approach and looked forward to seeing it proceed. It seemed like a useful approach to gain a deeper understanding of the topic. Unfortunately, I was rather disappointed with the presentation and felt that I didn't learn much. The text was difficult to follow, and omitted crucial details about how the analysis was done. Because of these omissions, it was hard to understand the interpretations of the results of the analysis. Additionally, some of the figures are too small, have text that is too small to be legible, and some have features that are not explained in the captions, making them more challenging than is necessary to understand. Furthermore, it is not clear how much confidence

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one should have in the results when the the analysis is applied to model simulations that appear to be unrealistic. For instance, the DIC zonal mean of the model shows significant gradients that are not present in the GLODAP analysis. This makes it hard to trust results about DIC transport. I think that this manuscript needs to be significantly reworked for it to be published in Biogeosciences.

I must admit that I gave up reading the last few pages carefully. This is because the poor explanation of the derivation of the analysis made it too difficult to follow the discussion. I couldn't understand what was being discussed, so the details of the discussion didn't seem to matter. I found this very frustrating. I think that this approach to analyzing the carbon cycle in the southern ocean is promising, but I think that the poor presentation in this manuscript does it a mis-service.

specific comments —————

p. 3396, line 20: I've read this sentence many times and am not certain what it is saying. "... mass exchanges among them because of transformation processes will be used in this context ..." Something is missing here. Additionally, What is transport capacity? What is the word "latter" referring to?

p. 3402, eq (4): What are the  $G^{\{Z\}Z}$  and  $G^{\{M\}M}$  terms? Presumably they are grazing terms, but this should be stated.

p. 3404, line 10: Please give a brief summary of Ludicone et al. 2008c and Dutay et al. 2009, highlighting aspects that are relevant to ventilation.

p. 3405, line 10: Drop the words "very well". It doesn't make sense to make such a claim when it cannot be quantified.

p. 3405, lines 15-25: Please show difference plots. Also, GLODAP units are umol/kg. Please state how you converted to stated units of umol/L. Are you comparing to GLODAP's TCO<sub>2</sub>, or have you removed GLODAP's estimate of anthropogenic DIC?

p. 3405, lines 26-31: Please show difference plots.

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p. 3407, lines 25-28: It is confusing to direct the reader to Figure 7 at this point, but not explain the dashed and dotted line until page 3410.

p. 3408, line 1: The stated separation is not 'clearly evident' to me. Explain what you mean by this.

p. 3408, lines 6-8: Add area of the regions to Table 1, to justify this remark.

p. 3408, lines 10-12: From figure 7, how can you infer that SAMW fluxes are from lateral mixing of TW.

p. 3410, lines 3-13: Please explain, preferably with formulas, how the different terms of are computed. For instance, how do you remove the DIC effect? Without this explanation, it is difficult to follow the discussion of your results.

p. 3410, line 18: Given the poor representation of alkalinity in your model results, e.g. weak gradients, how meaningful is this result?

p. 3410, lines 26-28: Please connect Gloor et al.'s assertion to your physical simulation. Do their results hold in your model?

Section 4.3.1: With Figure 10 so small, it is hard to follow the discussion.

Section 5.1.1: Please define more clearly the region  $V_{\{\gamma\}}$ , preferably in the manuscript text. The only definition is in the caption for figure 11, which states  $V_{\{\gamma\}}$  is sandwiched between two surfaces  $S_{\{\gamma\}}$ . Firstly, I would expect the volume to need two gamma values, not one. Also, the figure only has one surface  $S_{\{\gamma\}}$ . Is the open boundary correspond to 30S of your analysis. If so, please make that clear. The lack of clarity about how  $V_{\{\gamma\}}$  is defined makes it very challenging to understand the analysis of this section.

technical corrections \_\_\_\_\_

p. 3404, line 1: change "used to as input to 5000 years PISCES" "used as input to a 5000 year PISCES"

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p. 3405, line 5: coherent -> coherent

p. 3406, line 25: "time for of CO\_2" -> "time of CO\_2"

p. 3409, line 23: "to the a net" -> "to the net"

p. 3411, lines 18-19: "in both three direction" -> "in three directions" "also both three direction" -> "also in three directions"

p. 3412, line 12: "expression" -> "value"

Figure 3: Make text within figure larger.

Figure 4: Units are missing.

Figure 5: Make contour labels larger.

Figure 6: Make contour labels larger.

Figure 7: Make Region labels at top of figures larger. Do not abbreviate the abbreviations, i.e. keep SAMW instead of reporting MW.

Figure 8: Enlarge the colorbars. The DIC units for the upper panel differ from those in Figure 5. Please make them consistent.

Figure 9: Make text within figure larger.

Figure 10: Make figure larger, and enlarge text. Do not abbreviate the abbreviations. What is the black dashed line? Which line is the zonal advection line, compared to meridional advection?

Figure 14: Make figure larger, and enlarge text.

Figure 15: Make figure larger, and enlarge text.

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Interactive comment on Biogeosciences Discuss., 7, 3393, 2010.

**BGD**

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