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Biogeosciences Discussions

Interactive comment on "Enhanced biological carbon consumption in a high CO_2 ocean: a revised estimate of the atmospheric uptake efficiency" *by* R. Matear and B. McNeil

Anonymous Referee #3

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Summary:

The paper uses a biogeochemical GCM to re-assess the oceanic CO2 uptake by elevated biological carbon consumption as proposed by Riebesell et al. (2007)(R07). The authors show that the R07 estimate of 74-154 PgC by year 2100 was too high by at least a factor 2. The paper is concise and nicely illustrates how difficult it is to get extra carbon into the ocean via the biological pump. Apart from a number of technical flaws (see below) it is well written and easy to follow. I have, however, a major problem with recommending this paper for publication: Most of the results are not new, and this is not properly acknowledged in the manuscript.

First, as already pointed out by reviewer 1, the paper is largely an echo of the paper by Oschlies et al. (2008), who also applied the R07 results to a GCM and obtained an even lower additional oceanic CO2 uptake of 34 PgC by 2100. Second, the more general conclusion that the oceanic CO2 uptake efficiency of any fertilization procedure is low, particularly in the Southern Ocean where iron fertilization has been suggested by some as efficient method to sequester atmospheric CO2, is not new neither. Similarly low and even lower oceanic CO2 uptake efficiencies in response to ocean fertilization have been reported earlier (Gnanadesikan et al., GBC 2003; Bopp et al., PA 2003).

I do not think that the paper in its present form contains enough new findings to warrant publication. However, I believe that there are interesting aspects (see below) not covered by the present manuscript, which would provide enough novel material for an exciting scientific paper of interest to the readers of Biogeosciences.

General comments:

I see two potentially new aspects of this study that could be explored in more detail and add sufficient new material:

1. The model used here contains a DOM compartment (Oschlies et al. do not have this), which supposedly plays a prominent role in explaining the R07 experimental results, as suggested Arrigo in his News&Views article commenting on R07. The current version of the manuscript does not explicitly say how the C:P ratio of DOM is treated (presumably it remains constant?). Does it make a big difference whether the excess C is channeled into the DOM or the POM pool? Is there any observational evidence that could support either path? There are substantial lateral and seasonal differences in surface water pCO2 in today's ocean. Is there any correlation between pCO2 and elemental ratios of the DOM or POM pools?

2. The oceanic CO2 uptake efficiency depends on the treatment of the atmospheric boundary condition. The current study prescribes atmospheric CO2, which is known to lead to overly high uptake efficiencies (Moore et al., Tellus 2006; Jin et al., BG 2008,

re-iterated by Oschlies, BG 2009). The Oschlies (2009) analysis suggests that such a treatment overestimates the uptake efficiency by about 50% on centennial time scales. Interestingly, both Oschlies et al. (2008) with interactive atmospheric carbon pool and the current study with prescribed atmospheric CO2 yield similar uptake efficiencies of about 0.3 in both cases. This might indicate that the results of Oschlies (2009) are less general than that paper suggests. The model used here could be used to investigate this in more detail.

specific comments:

It is difficult to remember which experiment belongs to which experiment name. Either provide a look-up table or use more associative names.

Interactive comment on Biogeosciences Discuss., 6, 8101, 2009.