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 #2

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This paper examines the potential impacts of changes in the elemental stoichiometry of phytoplankton, in particular the C:N ratio, under increasing carbon dioxide concentrations in the oceans. Earlier work by Riebesell et al. (2007) suggested that the C:N ratio might increase under higher atmospheric carbon dioxide, and suggested that this could lead to of order 100 GtC of excess export by the year 2100. They then assumed a 100 year "return time" for this carbon (it is unclear from their paper whether this refers to an e-folding time or not) and thus estimate a high additional uptake of carbon over the course of this century.

This work demonstrates that the uptake efficiency is much lower than that suggested

by Riebesell et al. (2007), with an atmospheric efficiency of around 0.3. This is comparable to the results found by Oschlies et al. (2007), who performed a similar experiment in a coupled model of intermediate complexity. They find an additional export of 104 Gt C and an additional uptake of 34 Gt C by 2100. This fact is noted by Reviewer 1 who suggests that it makes it impossible to publish this paper. I agree with the other reviewer that the paper does need to be substantially revised to address Oschlies et al. (2008). However, I disagree that the fact that they use a "more sophisticated model" is a reason to discard this work. Sophistication is no guarantee of correctness- as I will discuss below, and there is value in nailing down the mechanisms that are potentially important. My recommendation is that the paper be accepted after a major revision.

Major comment: Comparison with Oschlies et al and a discussion of mechanism

Oschlies et al. (2008) show that the uptake is well simulated by a convolution of the additional carbon export with an e-folding time of 25 years. This is very short. It seems to me that this fact is key to understanding the result, and that there are two possibilities for explaining why the time scale is so short. The explanation of Oschlies et al. (2008) is that the increased carbon results in an increase in oxygen consumption, resulting in an increase in suboxic waters. This produces an increase in water column denitrification, drawing down the inventory of N and thus (effectively) reducing the ability of the ocean to hold carbon.

How realistic is this mechanism? I would point out that most models with which I am familiar tend to overestimate the amount of suboxia in the global ocean. Whether this is due to the maintenance of an overly strong potential vorticity front at the edge of the shadow zone or to lack of vertical diffusion in the transition layer at the mixed layer base remains unclear to me. So it is potentially the case that when including denitrification and nitrogen fixation that rates have to be tuned to unrealistic level to keep these oxygen biases from overwhelming the system. Moreover Moore and Doney (2007) point out that the extent to which these stablizing feedbacks actually work (as assumed by Oschlies et al. 2008) is limited by iron. So perhaps Oschlies et al. (2008) underesti-

mate the extent of the feedbacks on nitrogen (no iron would mean that the additional carbon would consume much more nitrogen, which would then draw down the nitrogen inventory and not necessarily lead to increased fixation) or perhaps they overestimate the feedback because coarse resolution models don't ventilate the shadow zones realistically (and so the additional carbon leads to excessive denitrification).

There is, however, an alternative explanation. As discussed in Marinov et al. (2008a,b) the ability of the biological pump to store carbon is well described by the concentration of remineralized nutrient

$$C_{bio} = R_{C:P} * ([PO_4] - [PO_4]_{pref})$$

where $[PO_4]_{pref}$ is the preformed nutrient concentration. While iron fertilization works by changing the preformed nutrient, enhanced carbon consumption works by changing the $R_{C:P}$. So far so good. But as discussed in the Marinov et al. papers, there is no necessary relationship between carbon export and changing preformed nutrients. This is because the export is determined by how rapidly the remineralized nutrients cycle throught the system, while in the present case the carbon content is determined by how rapidly nutrients with low $R_{C:P}$ are brought to the surface, exposed to high pCO$_2$ and reinjected with high $R_{C:P}$.

The great value of the present work is that it *does not* include the nitrogen cycle feedbacks, and thus has the potential to isolate the circulation impacts alone. However in its current form it does not make the point strongly enough. My suggestion is that the two mechanisms be explicitly contrasted with each other, and that the degree of recycling be quantified in the new model. In particular, Oschlies et al. (2008) show that the uptake can be well parameterized using a simple Green's function with a 25 year time constant. My recommendation is that the authors perform the same calculation for this model. The focus should then be on why the time constant is what it is.

The fact that the other sensitivities are small is a good way to round out this paper, but this central result needs significant sharpening.

Detailed comments

p. 8104, lines 5-15: Carbon uptake actually requires a number things. 1. An additional unit of carbon must be taken up by biological activity 2. This additional unit must be associated with "new" nutrient- defined here as nutrient which hasn't had seen high CO$_2$. 3. The carbon must then be exported away from the surface and 4. the resulting pCO$_2$ lowering in the region of production must be able to equilibrate with the atmosphere before it recombines with the additional pCO$_2$ in the region of remineralization. The last of these is what Jin et al. (2008) argue occurs in one of their equatorial simulations of iron fertilization. This would be a good place to bring up the idea that insofar as nutrients are not "new", but recycling in place, the result is to increase production without increasing the remineralized nutrient pool.

p. 8106: I agree with the other reviwer that a bit more description of the production formulation is required here. (In particular if the restoring formulation of nutrients is being used, it should be mentioned).

p. 8108: 1st line, Again, the point is not that this carbon is removed from contact with the atmosphere (yes there is some outgassing when this water comes back to the surface but it should be pretty well compensated when the nutrient goes back into the interior). The point is that you are "double counting" its impact on export.

p. 8111-8113: The section on biological oxygen misses the opportunity to make the key point. Insofar as new carbon is being taken up in a region, the oxygen flux should be in stoichiometric ratio to the carbon flux. If the carbon is merely being recycled, the uptake of carbon in a region (broadly defined) should be less than the stoichiometric ratio. This calculation should be made and presented (I'd suggest using relatively broad regions). So instead of the oxygen flux in Fig. 7, I'd suggest looking at the change in oxygen flux. This would also get at the question of how one might see such changes-they could appear as changes in the carbon:oxygen flux ratios over broad regions.

p. 8119: pAtm is used in description rather than pA.

Reference

Moore, J.K. and S.C. Doney, (2007) Iron availability limits the ocean nitrogen inventory stabilizing feedbacks between marine denitrification and nitrogen fixation. Global Biogeochem. Cycles, 21, GB2001, doi:10.10.1029/2006GB002762,

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