

## ***Interactive comment on “Enhanced biological carbon consumption in a high CO<sub>2</sub> ocean: a revised estimate of the atmospheric uptake efficiency” by R. Matear and B. McNeil***

**Anonymous Referee #1**

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### Summary

The manuscript builds on a 2007 study (R07) of mesocosm results by extrapolating its findings to the global scale using a general circulation model and a simple biogeochemical model. The latter is modified such that an effect observed in mesocosms, namely that aquatic ecosystems under high atmospheric pCO<sub>2</sub> exhibit a higher C:N ratio, is included and its consequences for the ocean carbon cycle examined. The main thrust of the paper is the role that 3D circulation plays in redistributing the extra carbon taken up by the biology. Its central result is that total oceanic CO<sub>2</sub> uptake is less than that estimated from studies based on simpler box models, in part because recirculation of

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waters enriched by the studied mechanism decreases the air-to-sea flux of CO<sub>2</sub>.

Unfortunately, while the manuscript is generally scientifically sound, the study it reports and the results that it finds are largely an echo of a paper published last year by Oschlies et al. (2008, *Global Biogeochem. Cycles* 22, GB4008). Matear & McNeil briefly cite this paper (in the context of its oxygen cycle results), but they do not compare their results with those from the earlier paper, nor do they expand on what their study offers by way of extension or improvement. In fact, the earlier study has a number of aspects that make it (to a first glance) superior, including: use of an ESM rather than a forced, ocean-only GCM; a more sophisticated ocean biogeochemical model; the presence of a land carbon cycle model; dynamic, rather than forced, atmospheric pCO<sub>2</sub>; and the treatment of second order effects such as expansion of the oxygen minimum zone.

As a result, the lack of novelty of this manuscript makes it extremely difficult to recommend it for publication. In principle, there are probably ways in which the manuscript's focus could be shifted to give a novel analysis, but the current version is far from this. Unless the authors can convince me that I've overlooked the novel aspects of their work, my recommendation is that the manuscript is rejected on grounds of its essentially repetitive content.

### General comments

One angle not thoroughly explored by either this manuscript or that of Oschlies et al. (2008) is whether the effects of an increasing C:N ratio in marine export production can be discerned in observations. Does it leave a signal in the fields of biogeochemical variables that could in principle be measured, and if so, is this signal large enough to be separated by measurement techniques?

Similarly, determining the significance of any such changes for anthropogenic CO<sub>2</sub> deconvolution methods (e.g. CSTAR from Gruber, 1996) would be of interest. Such methods often assume fixed Redfield stoichiometry, and any changes in this induced by elevate air / sea pCO<sub>2</sub> could be important for carbon cycle budgeting.

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On these points, the current manuscript is even missing a figure showing the distribution of extra, anthropogenic DIC between the control and pAtm experiments. The signal is typically associated with the solubility pump (e.g. high concentrations of anthro in deep water formation regions), but the introduction of a mechanism driven by the biological pump must alter the pattern of anthro.

#### Specific and technical comments

Pg. 8103: the inclusion of text on iron fertilisation in the manuscript's introduction is misplaced and slightly confusing. Drawing out a comparison would make more sense in the discussion section.

Pg. 8105, ln. 2: ambiguous wording – "... would increase oceanic uptake of CO<sub>2</sub> from the atmosphere by 74 to 154 Pg C ...". This could be read as either uptake increasing somewhere between 74 and 154 Pg C, or that uptake could increase by 74 Pg C to reach 154 Pg C.

Pg. 8105, ln. 17: surplus "the" in – "... we describe the our OGCM-ocean ...".

Pg. 8106: the model description here misses out a number of important details. For instance:

- The OCMIP-2 protocol does not include climate change (and cannot do so because of the manner in which biological export production is forced by a fixed set of surface nutrient fields). The authors should point this out

- The OCMIP-2 biological model includes a representation of DOM, which has its own C:N (actually, C:P) ratio. This is not mentioned in the description, but is complicated (at least for this reader) by a later discussion of DOM.

- Similarly, while the OCMIP-2 protocol does completely describe the model, it would be useful to readers unfamiliar with this MIP if the authors more fully described the biogeochemical model used here. For instance, some reference to the oxygen cycle, the formulation and redistribution of export production, calcium carbonate production,

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and the use of a mixture of OCMIP-2 prescribed (air pressure, piston velocity, sea-ice mask) and model-calculated (SST, SSS) variables in air-sea flux calculations.

- The description does not draw attention to a key limitation of the atmospheric CO<sub>2</sub> forcing, namely that pCO<sub>2</sub> is unaffected by air-sea uptake of CO<sub>2</sub>. Regardless of the flux of CO<sub>2</sub> into the ocean, the atmosphere presents the same pCO<sub>2</sub>. The OCMIP-2 protocols include simulations in which total atmospheric CO<sub>2</sub> (and, hence, pCO<sub>2</sub>) varies with air-sea flux, and the authors could consider adding this here.

- The description does not give any indication of the performance of the modelled nutrient, carbon and oxygen cycles. The authors should really analyse the distributions of the control simulation's biogeochemical species. Also, some comparison with other OCMIP-2 models would be helpful. It's not even clear if this particular model was one of those included in the OCMIP-2 analysis of Najjar et al. (2007).

- Although the OCMIP-2 protocol is referenced, the historical simulation begins at a different time (1800) to that used in OCMIP-2 (1765). It's unlikely to make any difference to the results, but any deviations from OCMIP-2 should be noted.

Pg. 8107: the authors give a qualitative description of the oceanic pCO<sub>2</sub> version of Equation 1, but it would be more useful for them to reproduce the full version used. The R07 results suggest that ocean pCO<sub>2</sub> was much lower than atmospheric pCO<sub>2</sub> so the coefficients are likely to be different.

Pg. 8107, ln. 24: the first date given is 1800 rather than the 1879 used elsewhere in the manuscript.

Pg. 8109, ln. 9-10: different precision used in numbers.

Pg. 8109, ln. 25-26: the text moves between Pg C and Gt C for no obvious reason, and is sloppy on use of time in the units. The first number appears to be per annum, while the latter appears to be integrated over a longer period. The latter number is also described as an increase, but the control value to which it is being compared is not

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noted here.

Pg. 8114, In. 22-25: technically, the mesocosm experiments do not involve a “single species”, but instead involve a limited natural assemblage of species. The point the authors make about extrapolating from small to large scales is still valid, but mesocosms are not cultures in labs.

Table 1: this is just a personal observation, but I found the names chosen for the experiments rather non-intuitive and occasionally confusing. The authors also label one experiment as “pA” but then refer to “pAtm” in the definition of its successor experiment. Also, I’m unclear what’s special about 1879 in these experiments – it’s not an OCMIP-2 date.

Figure 1: legends would be extremely useful on these plots. The fourth panel uses the same colour scheme as the previous panels, but is showing something quite different (ratios between runs rather than different runs).

Figure 4: this figure and caption are very confusing. The plotted lines seem to split into “NP” and “Uptake”, but no mention is made of what “NP” is. Also, one of the lines (the one ostensibly in cyan) is invisible. Presumably it’s overwritten by another line, but noting this in the caption would help. Also, the legend seems not to be using the experiment names given in Table 1, and instead favours an idiosyncratic convention.

Figure 5: might this be improved if model output was used to show the distribution of extra carbon rather than some idealised shading? Perhaps adding numbers to the fluxes shown would also assist interpretation.

Figure 7: using blue contours on a plot in which blue is a constituent colour seems like a bad idea to me. How about white lines?

Figure 8: Figure 7 uses a conventional red/blue colour scale to distinguish positive and negative change. Here the same scale is used, but the data are no longer positive/negative, so it’s just confusing. A more continuous colour scale would be prefer-

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able. Also, the plot shows the global zonal averages, while it might be more useful (given the distribution of OMZs) to split the plot into separate basins.

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