

## ***Interactive comment on “Adjoint sensitivity of the air-sea CO<sub>2</sub> flux to ecosystem parameterization in a three-dimensional global ocean carbon cycle model” by J. F. Tjiputra and A. M. E. Winguth***

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Received and published: 11 September 2007

The authors thank referee #2 for his/her valuable and critical evaluations and comments, which significantly improved the overall quality of the manuscript. Below are the responses for every comments raised by referee #2:

Referee#2: The authors look at some timeseries of components of pCO<sub>2</sub> tendencies and components of DIC changes - these are just repeats of similar analysis by other studies and seem not to add anything substantial.

The authors could not find any published manuscripts similar to these studies. Other studies such as McKinley et al. (2004), Wetzel et al. (2005), and Le Quéré et al. (2000) applied the same technique, but they mainly focuses on interannual variability

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and specific regions. Therefore, we decided to leave the section in the manuscript and included more detail discussion and comparison with other studies mentioned above in the revised manuscripts.

R2: Although the nutrient perturbations may have some interest, I am perplexed by the difference in results between PO<sub>4</sub> and NO<sub>3</sub> sensitivity and worry that these are from problems in the ecosystem model configuration.

The authors have conducted more analysis regarding the sensitivity towards nitrate and phosphate addition. The strong sensitivities toward phosphate is mainly due to the strong nitrogen fixation processes, which is now discussed in the revised manuscript.

R2: The sensitivity studies with plankton additions worry me as there is a difficulty in separating the response of the system to the extra biomass in terms of its consequences on the ecosystem and in the fact that extra carbon has been added to the system.

After careful consideration, the authors agree with the reviewer that the perturbation in zooplankton and phytoplankton mass will add extra carbon to the overall system and complicate the analysis. Therefore, we decided to remove the sensitivity experiment, with respect to phytoplankton and zooplankton mass perturbation, from the manuscript.

R2: This latter part might be worth publishing with further thought into the experiments and what they mean (I don't feel that the authors have fully explored these sensitivity maps - eg. why so little sensitivity to phytoplankton growth rate in Southern Ocean).

The revised manuscript has expanded the analysis section of sensitivity toward the ecosystem control parameters. The little sensitivity in the Southern Ocean is mainly due to the limited iron concentration in this region.

R2: As read here, I find some things of grave concern in the modelling: timestepping and NO<sub>3</sub>/PO<sub>4</sub> control on phytoplankton growth to name two most worrying - but this could be just poor explanation and maybe cleared up by the authors' responses.

With regards to the time step in the model, the original ecosystem model was developed by Six and Maier-Reimer (1996) who have explored the stability of the solution for different time steps. The uncertainties by selecting a time step of three days are lower than the uncertainties of the predicted growth rate. They also have tested that the solution for three days is approximately the same for slower time step, since a Runga Kutta implicit scheme has been used in the model

With respect to NO<sub>3</sub>/PO<sub>4</sub> growth limitation, the revised manuscript has identified that the multi-nutrient uptake is based on Redfield ratio: .

R2: Specific comments: Sections 2, pg 1380: Model description: How can the ecosystem be resolved with a 3 day timestep (and for that matter how can you use a month timestep at depth - surely there are some numerical issues)? Ecosystem dynamics take place on the timescale of hours/days not weeks. What sort of timestepping scheme are you using?

The ecosystem formulation uses an implicit time step scheme (see above). Only the ecosystem and air-sea gas exchange processes are occurring within a three-day time step. Other processes (e.g. POC vertical flux, DIC remineralization, CaCO<sub>3</sub> dissolution) below the euphotic layer are occurring within a one-month time step.

R2: Is the model spun up? You say you "start" in 1995 and run 10 years, but what do you start from? If not, then you results will be very suspect.

The revised manuscript has clarified that the model has been integrated for 10,000 years to reach a steady state using climatology physical forcing in section 2.

R2: Section 3.1: seasonal variability of pCO<sub>2</sub>. What is the purpose of this section? If it is showcase the use of the adjoint technique then this needs to be further specified - otherwise it is "just another" model decomposition of the tendency of pCO<sub>2</sub>.

This section is designed to highlight different components controlling the seasonal pCO<sub>2</sub> cycle in the model. To the author's best knowledge, there is not any study,

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which has done this for the seasonal cycle and for all the selected regions.

R2: Section 3.2: What are  $f(T)$  and  $f(L)$  - I assume temperature and light functions controlling growth? But temperature this needs to be clarified.

The revised manuscript have clarified the description of  $f(T)$  and  $f(L)$ .

R2: pg 1384, Line 1:  $N = \min(PO_4, NO_3)$  ? is this really what you mean? (ie.  $PO_4$  is probably always less than  $NO_3$  - in terms of mol/L) , or do you mean the more normal parametrization:  $\min(NO_3/(NO_3+kNo_3), PO_4/(PO_4+kPO_4))$ ?

This formulation is modified to , and the text has clarified that the comparison is based on the Redfield ratio.

R2: As with previous section - what is the point of this section - it doesn't say anything new but rather "just consistent" with other model finding?

This section is included to compare the role of different components regulating the seasonal DIC variability in the model. To the author's best knowledge, there is no study, which has done this for the seasonal cycle and for all the selected regions. The authors would certainly be more than welcome to reference any additional similar studies.

R2: Section 3.3: pg 1384, line 27: this seems at odds with many observational and modelling findings that much of the ocean is nitrate not phosphate limited (see for instance Fig 7 of Moore et al, GBC 18, GB4028, 2004) ? which means that most models would be far more nitrate sensitive. If you really do model  $N = \min(PO_4, NO_3)$  then this might explain this discrepancy: phosphate would always be the minimum and then so results would be more sensitive to this nutrient. If this is the case I suggest the sensitivity was a fault of a poor parametrization. But either way this is a very troubling result.

The discussion on the sensitivity of the air-sea  $CO_2$  flux toward phosphate and nitrate perturbation has been significantly revised to address the referee comments. After careful and detail, analysis the sensitivity magnitude and pattern toward phosphate

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addition is due to the cyanobacteria (nitrogen-fixer) in the model, which significantly reduce the PO<sub>4</sub> concentration in the tropical and subtropical regions.

R2: pg 1385, line 1: you do not appear to have nitrogen fixing in your model, so why do you mention this? In fact some of your results could be changed if you did include this procedure and is an argument against believing your results.

The sentence has been revised accordingly with the new analysis results (see above).

R2: pg 1385, line 12: phytoplankton growth is also limited by light and nutrients.... so I am not sure why you say "In general". I think it is difficult to compare quantitatively between the nutrient perturbations and plankton perturbations - since it is difficult to say that a 0.16umol N/L perturbation is "equivalent" to a 1.27umol C/L plankton perturbation. I find these perturbation experiments a little difficult to fathom. By adding plankton, you are also adding carbon to the system, so it would seem very difficult to tease apart what the response is of the increased biomass consequences (e.g. increased photosynthesis) as opposed to the just increased carbon added. This might be why DIC increase in equatorial regions with phytoplankton additions: it is just more carbon....

After careful consideration, the authors strongly agree with the referee's feedback that perturbing either phytoplankton or zooplankton can increase the carbon mass in the system (i.e. contradict with the mass conservation), and complicate the analysis, therefore, the authors have remove the sensitivity experiments with respect to phytoplankton and zooplankton perturbation from the manuscript.

R2: Section 3.4: This seems the most useful section, and could maybe be extended and enhanced in a new manuscript.

The analysis from this section has been improved and expanded. The sensitivity of air-sea CO<sub>2</sub> flux toward herbivore ingestion parameter is removed due to the similar sensitivity results to grazing parameters, thus the authors think it is redundant.

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R2: pg 1387, line 2: what sort of values are  $P(\text{new})$ ? (It would also be better to use another symbol rather than "P" here as you use this for phytoplankton already).

The authors agree with the referee and revised the description into "the parameter units are normalized to be ratio between the a posteriori and the a priori control parameters (i.e. ."

R2: pg 1387 line 10: why so little sensitivity over most of the Southern Ocean?

The analysis of the regional sensitivity toward phytoplankton growth rate has been modified. Additional analysis suggests that the Southern Ocean is not sensitive due to lack of micronutrient iron concentration in this location.

R2: pg 1388 Line 1: Could the importance of the zooplankton rates be model specific? Or could the perturbations have been relatively higher than those for the phytoplankton?

The perturbation value is selected to be equal for all parameters at 10%, and since all the parameters have the same units [day<sup>-1</sup>], the authors believe that the perturbations are equal for all parameters in section 3.4. It is possible that the strong sensitivity toward zooplankton is model specific, and this point is addressed in the conclusion (section 4) of the manuscript.

R2: pg 1388 Line 10: Why is the Southern Ocean sensitive to herbivore parameters, but not phytoplankton ones?

This result is due to the fact that change in grazing rate is less dependent on the iron concentration. Changing in grazing rate would simple increase or decrease the live span of phytoplankton in the surface, thus increasing or reducing the length of biological photosynthesis.

R2: pg 1388 line 27: Is a 25% reduction of ingestion rate really "quantitatively" comparable to a 25% change in the other parameters?

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The authors agree and have removed the word "quantitatively" from the sentence.

R2: Conclusions: pg 1389 Line 15: this study has shown some of the ecosystem controls on the air-sea exchange of CO<sub>2</sub> - NOT that it is important. You'd have to look at the sensitivity to changes in T,S,Alk etc as well to show that is is "important"

The authors agree with the referee2 and have revised the conclusions substantially. We have included our analysis regarding sensitivity toward alkalinity, SST, and salinity as well.

R2: Table 1: Would also be nice to have the values used in the experiments

The parameter values in Table 1 have been added accordingly.

R2: Figure 7: units?

Units have been added in the figure caption of the revised manuscript as suggested.

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Interactive comment on Biogeosciences Discuss., 4, 1377, 2007.

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4, S1341–S1347, 2007

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