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Biogeosciences Discussions

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4, S825–S829, 2007

Interactive Comment

Interactive comment on "Adjoint sensitivity of the air-sea CO_2 flux to ecosystem parameterization in a three-dimensional global ocean carbon cycle model" *by* J. F. Tjiputra and A. M. E. Winguth

Anonymous Referee #2

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General comments:

This paper uses an adjoint model of an ecosystem model to perform several sensitivity studies with regard to the air-sea carbon fluxes over the global ocean. Although the adjoint model can be a powerful tool that could lead to some further insight into the ocean ecosystem control on air-sea fluxes of CO2, I do not feel that this current study is well thought out or helpful in this regard - it seems to be a rather disjoint selection of experiments, some of which do not seem to make much practical sense. I do not recommend publication. However, I do feel that with more careful thought, the authors

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will be able to use this tool in a productive manner and a new manuscript may be publishable.

The authors look at some timeseries of components of pCO2 tendencies and components of DIC changes - these are just repeats of similar analysis by other studies and seem not to add anything substantial. The more unique results are those of the adjoint sensitivity studies: the first set is perturbations of nutrients and plankton concentrations. Although the nutrient perturbations may have some interest, I am perplexed by the difference in results between PO4 and NO3 sensitivity and worry that these are from problems in the ecosystem model configuration. 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. The second set of sensitivity studies consider some of the ecosystem parameter values: these I see as more interesting experiments which could offer some insight to the working of this particular ecosystem model. 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) - and given the strong caveat that the results will still be model specific.

I do have some serious reservations about the model run: see the specific comments below (Section 2, 3.2, 3.3). Some more details of the modelling procedure and equations need to be included. As read here, I find some things of grave concern in the modelling: timestepping and NO3/PO4 control on phytoplankton growth to name two most worrying - but this could be just poor explanation and maybe cleared up by the authors' responses.

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? 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.

pg 1381 line 2: phase lag could quite easily come from the asynchronous timestepping.

Section 3.1: seasonal variability of pCO2.

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 pCO2.

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.

pg 1384, Line 1: N=min (PO4, NO3) – is this really what you mean? (ie. PO4 is probably always less than NO3 - in terms of mol/L) , or do you mean the more normal parametrization: min(NO3/(NO3+kNo3),PO4/(PO4+kPO4))?

Also, how is Si included in this formalization. (some of these details could be given in an appendix)

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?

Section 3.3:

pg 1384, line 27: this seems at odds with many observational and modelling findings

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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(PO4, NO3) 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.

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.

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....

Section 3.4:

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

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

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

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?

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Or could your long timestep mean that processes with longer rates are emphasized?

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

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

Conclusions:

pg 1389 Line 15: this study has shown some of the ecosystem controls on the air-sea exchange of CO_2 - 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"

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

Figure 4: Would be very useful to have zero-contours.

Figure 5: top colorbar has 3.0 twice; again zero-contour would be useful.

Figure 6: zero contour lines

Figure 7: units?

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