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Biogeosciences Discussions

Interactive comment on "Impact of atmospheric and terrestrial CO_2 feedbacks on fertilization-induced marine carbon uptake" *by* A. Oschlies

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The manuscript 'Impact of atmospheric and terrestrial CO_2 feedbacks on fertilization-induced marine carbon uptake' by author Andreas Oschlies uses an idealised model setup to investigate flow pathways of carbon between atmosphere, ocean and land carbon reservoirs, arising from a hypothetical intensification the ocean biological pump either by CO_2-fertilization of marine primary production (carbon overconsumption) or by increasing the potential maximum phytoplankton growth rates as an analogue for iron fertilization. The main finding is that by stimulating oceanic carbon uptake via the biological pump a large part of the additional carbon that seems to be taken up from

the atmosphere in fact ultimately comes from the terrestrial carbon pool.

The study is very interesting, highly original and timely as it is touching on the role of the ocean as a sink for CO_2 from the atmosphere, which is an important issue of current carbon cycle and climate research. It is also of interest for considerations of ocean fertilization by iron as a potential option for mitigating anthropogenic climate change. As such it is certainly of great interest to the readership of Biogeosciences.

However, as far as I understand the main result should only hold for the 'natural' case, while the system would operate differently when including the anthropogenic CO_2 perturbation. In fact, this reverses the conclusion in that purposeful ocean iron fertilisation is actually more likely to draw down anthropogenic CO_2 from the atmosphere than reducing the 'natural' background CO_2 concentration. The overall low uptake efficiencies, however, are a robust result in all cases, ruling out iron fertilization as a suitable option for mitigating climate change.

General comments:

The study is motivated by the often made oversimplification in ocean carbon cycle studies that are assuming a constant atmospheric pCO_2 boundary condition, which means an infinite atmospheric carbon reservoir, causing far too high cumulative carbon uptake by the ocean. As a result, the uptake efficiency of carbon by the biological pump may be overestimated by a factor of three on millennial time scales, and the error may even grow when considering the amount cumulative carbon uptake. To quantify the effects of neglecting interactive and finite carbon reservoirs several assumptions for the flow of carbon between the atmosphere, ocean and land are made that are conserving the total mass of carbon. Two lines of experiments are introduced to simulate an increase in the efficiency (fertilisation) of the oceanic biological pump, one is based on increased C:N drawdown, one on an increase of the potential maximum phytoplankton growth rates. The experiments do not include anthropogenic CO_2 emissions which would actually increase the total amount of carbon in the system. This latter assumption is a critical point which results in the fact that the simulations can only be used to describe the natural (unperturbed) system. However, as the crucial mechanism that is responsible for the transfer of carbon from the land into the ocean can be identified, it can be deduced how the system would behave under anthropogenic perturbation, as I will explain in the following. Consequently, I think that the study contains a lot more information than is actually shown and that it would benefit from a clearer distinction between implications for the natural and the anthropogenically disturbed system.

The main finding of the study, that enhanced carbon drawdown by the biological pump in the ocean is largely fed by carbon from the terrestrial biosphere can simply be illustrated as a system of communicating vessels, where two carbon reservoirs (ocean, land) are connected via the atmosphere. When carbon is taken 'out' on the ocean side of the system this creates a low pressure in the atmosphere which is (partly) replaced by carbon from the land. This picture easily explains the natural situation as it stands in the present manuscript, where carbon release from land is caused by a reduced CO_2-fertilisation effect on the terrestrial vegetation while soil respiration remains unchanged. However, for the case of an anthropogenic CO_2 perturbation which we are presently facing, the system would most probably operate differently. Anthropogenic CO_2 emissions into the atmosphere are increasing the CO_2 pressure on both reservoirs (ocean, land), such that a low pressure that would cause a reduction in terrestrial CO_2 fixation is very unlikely to occur. This unlikeliness is clearly shown by positive airborne fractions in climate carbon cycle model simulations (Friedlingstein et al., 2006) as well as in observations (Canadell et al., 2007), which constantly keep the CO_2 pressure on the ocean and land reservoirs high. With regard to the very similar sensitivities of the ocean and land carbon reservoirs to atmospheric CO_2 in the Uvic-model (γ-land = 1.2, γ-ocean = 1.1; Friedlingstein et al., 2006) I would expect the flow of carbon from the atmosphere to be roughly equally distributed between land and ocean (53 and 47 that the calculation of Friedlingstein et al. (2006) is based on a CO_2-insensitive biological pump in the ocean, such that γ-ocean should be higher here, when diagnosed from a CO_2-sensitive biological pump, probably at the expense of γ-land and/or the airborne

fraction. Nevertheless, as long as airborne CO_2 fractions remain positive and temperature effects on soil organic carbon respiration are lower than the fertilisation effect of CO_2 on the land vegetation (see Cox et al., 2000), I would not expect a net flux of carbon from the terrestrial biosphere into the ocean.

My second point is addressing the 'natural' system and the fact that the relocation of carbon from the terrestrial biosphere into the ocean seems to depend strongly on the longer-term maintenance of an imbalance in the terrestrial carbon cycle with more material being remineralised than produced. Being not very familiar with terrestrial carbon cycle models, I'd like to ask the question how realistic it is to assume constant soil carbon remineralisation (exceeding carbon fixation) when less material is produced and thus available for decomposition?

Another interesting point would be if this mechanism would also work in the opposite direction when fertilising only the land biosphere, which is presently assumed to take place via a CO_2-fertilisation effect of the terrestrial biosphere (Luo et al. 2006)?

In summary, due to the assumption of total carbon conservation, the results shown here may not apply for a global change study, but should be more viewed against the background of natural (glacial/interglacial) carbon cycle variations. By addressing the limitation of former studies that assumed infinite carbon reservoirs, it introduces a limitation at the other end (mass conservation), so that the 'truth' in terms of oceanic uptake efficiency for anthropogenic carbon probably lies somewhere in-between. The results also seem to rely strongly on a longer-term imbalance of the terrestrial carbon pool, which may need further explanation.

Minor points:

p. 4503, l. 5 – p. 4504, l. 3: this is a description of the experimental setup and should be moved to section 2. From the description it was also not entirely clear to me how C:N ratios were treated in this line of experiments. I assume that they were kept constant and only phytoplankton growth rates were changed. This should be stated more clearly in the text.

Section 3.3 is not clear to me. Are the 2 Gt C that are additionally taken up caused by the biological pump or the solubility effect due to higher atmospheric pCO_2? What are the 9(

Table 1: when total carbon is conserved in experiments VEGconstant, COUPLED, and CLIMconst the changes in the reservoirs should balance each other. However, in the latter two experiments there is an imbalance of about 2 GtC, is there some effect missing in the calculation?

Table 3: typo: 'explicitly'

Table 4: typo: 'maxgr. 10 yr'

Table 4: are the headings of the column correct? As far as I understood the table shows sensitivities of different scenarios with respect to COUPLED, which should then be in the denominator.

Figure 2 b: as all simulations show very similar results it could be left out

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