**Biogeosciences
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Interactive comment on "Efficiency of small scale carbon mitigation by patch iron fertilization" *by* J. L. Sarmiento et al.
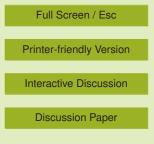
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General comments:

The paper describes and analyzes the results of simulated small-scale iron fertilization with special emphasis on its carbon sequestration potential and the different efficiencies with respect to added iron and realized export production. Comparing the results at four different fertilization sites of their global model, the authors find that the site located south of the "biogeochemical divide" close to Antarctica has the largest sequestration potential for the longest duration, whereas this potential is lowest for the Equatorial Pacific site. According to their model, fertilization in the equatorial Pacific leads to substantially enhanced denitrification and hence to a net loss of fixed nitrogen from the ocean. Fertilization at the other sites, on the other hand, leads to a slight

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decline in simulated water column denitrification.

The paper gives an excellent and objective overview over the results of previous observational and modeling work looking at possible carbon sequestration by iron fertilization. The study goes beyond previous studies by looking in more detail at the mechanisms explaining the different sequestration efficiencies at the different fertilization sites and at the impacts on oxygen-sensitive processes like denitrification. The authors also investigate the sensitivity of the results to different assumptions of the retention time of the added iron in their model. Overall, this study is very timely, concise and well written. It is very well suited for publication in Biogeosciences. I have, however, some questions and comments about some of the interpretations put forward in this manuscript. These are detailed below, and I recommend publication of the paper once these concerns have been addressed.

Main points:

(1) Jin et al. mechanism. It is interesting that the authors explain the differences in simulated physical-chemical efficiencies by the mechanism put forward by Jin et al. (2008). One of the authors previously questioned this explanation and gave good reasons for this (Gnanadesikan, BGD review of Jin et al. 2007; Gnanadesikan and Marinov, MEPS 2008). It would be interesting to know what has led to this apparent change of opinion. Is there new support for the Jin et al. mechanism, e.g. from sensitivity experiments with different mixing of passive tracers among the upper few model levels?

(2) Ross Sea site. At the end of section 3 the authors state "we believe that the enhanced uptake at the Ross Sea site is due mostly to the shallowness of the summertime mixed layer..". The results shown do not seem to fully justify this statement. If the Jin et al. mechanism explained the enhanced uptake (efficiency) at the Ross Sea site, it should do so already during the first decade of the fertilization experiments. Figures 9e and 9f show, however, that during the first decade(s) the physical-chemical uptake

efficiency at the Ross Sea site is lower than that at the Southern Ocean or PAPA sites. It appears that the long-term uptake efficiencies at the Ross Sea site are only higher because this site is the only one that does not display a decrease of annual CO_2 uptake under continuous iron fertilization (Fig.7g+h).

In order to better constrain the contribution of the Jin et al. mechanism, one could apply the Ross Sea fertilization to a sensitivity run with all passive tracers homogenized within the top 20m. I'm not sure how easy it is to perform such an additional experiment. Alternatively, one could look at differences in the physical environmental conditions other than the summer mixed layer. For example, the winter mixed layer is much deeper than at the other sites (greater dilution of any summer fertilization signal), the site is the only one located south of the biogeochemical divide (long isolation from atmospheric contact), and the circulation (up-/downwelling) may be different compared to the other upwelling-dominated sites. For all of these aspects, I could imagine mechanisms that contribute to a larger sequestration efficiency at the Ross Sea site.

In particular, it would be interesting to better understand (i) why there is no fertilization-induced CO_2 outgassing in winter (Fig.7a-d), (ii) why there is no rebound in CO_2 fluxes upon cessation of fertilization (Fig.7f) , (iii) why there is an increase in annual pertur-bation air-sea CO_2 fluxes during the first decades of experiment 1200x (fig.7h, though this is not the case in the 100x experiment)? It is puzzling that (ii) suggests that there is no accumulation of remineralized carbon within the surface mixed layer, whereas (iii) suggests that iron accumulates in the surface mixed layer. Possibly, the evolution of CO_2 uptake in the sensitivity experiment with "iron added and removed" could provide some information here.

(3) Explicit vs implicit iron treatment. The authors emphasize the difference between model studies that simulate iron fertilization by macronutrient manipulations, and other model studies (including this one) that include some explicit parameterization of the iron cycle. Macronutrient depletion experiments essentially simulate the effect of an iron fertilization where all iron is lost after the fertilization event. Explicit iron fertilization

experiments may retain some of the added iron for long times. Accordingly, the latter experiments can be expected to simulate a higher physical-chemical efficiency. A sensitivity experiment shows that the fertilization efficiency is very sensitive to the assumed iron retention time. It would be useful to have some discussion of how realistic the assumed retention times are and whether the model's CO_2 uptake efficiencies are more likely a high or low estimate.

The results of Table 1, on the other hand, suggest that nutrient depletion (all year round?) and infinitely intense iron fertilization (presumably which zero retention time as there is no effect outside the fertilized region) yield a very similar CO_2 drawdown. Is this a coincidence of the restoring time scale and maximum growth rates used in the explicit iron model? Would results look different for the sensitivity experiment with alternate growth formulation of section 4.3?

(4) Model description: Making available the complete model description as supplementary material is very much appreciated. However, since the study addresses carbon sequestration, there should be a more detailed description of the carbon pools in the main text. Specifically, the reader should be able to understand from a few short phrases whether there is a DOC pool (or several DOC pools?) and how relates to DON and/or DOP, if the model uses fixed C:N ratios, whether iron controls nitrogen fixation or Si:N ratio of diatoms. Also, some brief statement about the Chl:C ratio would be useful, as Chl is shown later as a model diagnostics.

(5) Comparison with observations: As much of the discussion is based on differences in the depth of the surface mixed layer, it would be good to see a comparison of observed (or estimated from hydrographic climatology) and simulated mixed layer depths. At station PAPA, observed winter mixed layers are typically deeper than 100m, rather than the 60m predicted by the model. Also, almost all of the in situ iron fertilization experiments described by de Baar et al (JGR, 2005, their Fig.11, Table 3), including those in the Southern Ocean, had mixed layer depths considerably deeper than 10m. It would be helpful to see how well the simulated mixed layer depth compares with

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observations.

(6) Uptake efficiency larger than 1: The physical-chemical uptake efficiency is larger than 1 during the first years at PAPA and, briefly, at the Southern Ocean site (Fig.9d-f). If the additional air-sea CO_2 flux, that is not exported, is caused by accumulation of organic carbon, this is not visible in the chlorophyll plots of Fig.8, and any accumulation that there is should, according to Fig.8's caption, have ended after ∼4 years. Is there perhaps accumulation of DOC that is uncoupled from DON and the corresponding drawdown of nitrate?

specific comments:

p.10390, l.20: The authors emphasize that their model, in contrast to many earlier model studies, uses a realistic atmospheric carbon reservoir. The fertilization-induced changes in atmospheric carbon do, in their model, not impact on climate, which thereby might neglect some secondary long-term effects on the efficiency of iron fertilization. This would be useful to mention.

p.10400, l.23: "reversal in the atmospheric CO2 gradient" probably means something like "the air-sea pCO2 gradient decreases". Under realistic rising atmospheric CO2, the average air-sea gradient is unlikely to reverse even for a hypothetical massive iron fertilization.

p.10401, l.12 skip one "deficit"

p.10405, l.21 "reversal" in the air-sea CO2 gradient is not generally correct (see above).

p.10407, eq.3: I did not understand what this equation is used for. Is it used to compute the curves of Fig.11? If so, this would be a different efficiency than shown in the earlier figures.

p.10411, top paragraph: The very shallow summertime mixed layers (realistic?) may also help to reduce the impact of light limitation.

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Table 3: (i) The \Delta NO3 figures should be the sum of the first three rows (with inverted sign of the two denitrification rows). I get 0.397 instead of 0.267 Tmol/100yr for the Southern Ocean site and 0 instead of 0.039 Tmol/100yr for the Ross Sea site. Is there some other nitrogen sink/source, e.g. change in biomass (DON) that accounts for the large difference?

Table 3: (ii) What is the reason for the large increase in sedimentary denitrification at the Ross Sea site? Is this caused by the export of organic matter being faster and deeper here compared to the other sites?

Fig.4: Even with iron retention, the perturbation export production should eventually decrease under continuous iron fertilization, i.e., when enough macronutrients have been moved to greater depth for surface production to become limited by macronutrients. For the small-scale experiments performed in this study, it may take very long to significantly deplete the upper ocean nutrient reservoir. However, in the view of suggested large scale geoengineering-type iron fertilization, it might be useful to mention that the perturbation export production and CO2 uptake may decline faster than for the small scale experiments shown here.

Interactive comment on Biogeosciences Discuss., 6, 10381, 2009.