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 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.

RESPONSE: We thank the reviewer for his kind comments.

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.

RESPONSE: We found the questions and comments to be very thoughtful and extremely helpful. Our responses are given in red. These are mostly written as responses to the review, but our intention in every case is to modify the paper accordingly, unless otherwise noted.

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?

RESPONSE: Interestingly, the Jin et al. slope of 0.673 for physical chemical efficiency versus the IP index fits our results very well at year 100 (though the intercepts are different for each set of model simulations), but the reviewer is right in what he says below that this relationship does not work in earlier decades. We no longer believe that our conclusion is correct. See further discussion below.

(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).

RESPONSE: which in turn is probably due to the deep sequestration of remineralized DIC at the Ross Sea site that one can infer from the deep low O2 layer in Figure 5i and which we see in a DIC plot that we shall add to Figure 5. At the other locations, the remineralized DIC remains very shallow. The difference at Ross Sea is that there is deep water formation occurring at or near there that sequesters the remineralized DIC in the abyss.

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.

RESPONSE: We agree with the reviewer that deep winter mixing and subduction are probably the main explanation.

In particular, it would be interesting to better understand (i) why there is no fertilizationinduced CO2 outgassing in winter (Fig.7a-d),

RESPONSE: It turns out that this is due to suppression of gas exchange when wintertime sea ice is present. When we do a simulation with free gas exchange through the sea ice, the Ross Sea has wintertime outgassing.

 (ii) why there is no rebound in CO2 fluxes upon cessation of fertilization (Fig.7f) ,

RESPONSE: When we do a simulation with no sea ice effect on gas exchange, there is a modest rebound for 3 to 5 years. However, after this short-term sea-ice effect, the CO_2 flux returns to being close to 0 regardless of whether or not we allow gas exchange through the sea ice. In other words, the sea-ice effect is only short-term. We believe that the lack of a return flux in the longer term at the Ross Sea site is due to efficient subduction of waters with remineralized organic carbon. This subduction can be seen in the oxygen perturbation plot in Figure 5i, where one sees that oxygen is depleted (remineralized organic carbon concentration is high) quite deep in the water column, as contrasted with the Southern Ocean fertilization where the oxygen utilization peak is relatively shallow (consistent with the remineralization profile; Figure 5k) as at other locations.

 (iii) why there is an increase in annual perturbation air-sea CO2 fluxes during the first decades of experiment 1200x (fig.7h, though this is not the case in the 100x experiment)?

RESPONSE: The contrast between the Ross Sea and Southern Ocean sites is useful here. Both have continued increases in biological export as iron is added (Figure 4h) due to the fact that surface nutrients recover in the wintertime and are never depleted in the summertime (Figure 8g and h). However, as the reviewer points out, the Ross sea atmospheric CO2 uptake continues to grow in the early years (albeit modestly) whereas that at the Southern Ocean site decreases. This behavior is reflected in the efficiencies shown in Figures 9 c, f, and i, where we see that it is primarily the relatively constant physical chemical efficiency of the Ross Sea site shown in Figure 9f that causes the Ross Sea site to overtake the Southern Ocean site in the air-sea flux per unit iron added. Overall, we conclude that both the Ross Sea site and Southern Ocean site have the potential to have

increased annual CO2 uptake as more and more iron is added, but that the Southern Ocean drops off due to backflux of the shallow excess remineralized DIC, whereas the Ross Sea continues to grow due to an inefficient backflux due to the fact that remineralized DIC has subducted out of reach of the surface layers.

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 CO2 uptake in the sensitivity experiment with "iron added and removed" could provide some information here.

RESPONSE: We are not sure what the reviewer is saying here, but hopefully our responses to (ii) and (iii) cover this concern.

(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.

RESPONSE: Actually, as Figure 11 (c) and (d) show, the physical-chemical efficiency is unaffected by whether or not iron is retained. It is only the biogeochemical response function that changes.

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 CO2 uptake efficiencies are more likely a high or low estimate.

RESPONSE: On p. 10394, line 2, we give Johnson et al. (1997) as a reference for the oceanic residence time. Table 3.3 of the supplementary material says where the iron cycling parameters come from. We'll add a reference to this table in the text.

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 CO2 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?

RESPONSE: We did an analysis of the % reduction in surface nutrients in KVHISOUTH and our model and they are virtually identical for all fertilization regions shown in Table 1 except the North Atlantic. Figure 8 shows that adding a large amount of iron to our model is enough to reduce nitrate to 0 during the summer time, and it is only in winter when the values jump back up. The nutrient depletion model has a 30-day damping time scale and this is long enough that deep wintertime mixing will mix nutrients back up in this case as well.

(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.

RESPONSE: We will add the following to the text: TOPAZ uses fixed C:N for all organic components. It has two DOM pools, one with fixed N:P and one with variable N:P, and a comprehensive representation of multinutrient limitation of phytoplankton with variable N:P:Fe:Si:Chl stoichiometry and physiology.

(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 100 m, rather than the 60 m 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 10 m. It would be helpful to see how well the simulated mixed layer depth compares with observations.

RESPONSE: We will add a figure comparing our estimates with De Boyer & Montegut's. The KPP mld is shallower in general than mlds defined by density, and we will show both. The model does reasonably well compared with observations except at the Southern Ocean site, where it is considerably shallower than the analysis of De Boyer & Montegut.

(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.9df). 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?

RESPONSE: An efficiency >1 in the first few years was also obtained by Xin et al. (2008). In our model this is associated with an increase in DON and therefore in DOC.

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.

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.

RESPONSE: Agree with all of the "Specific Comments" and will modify accordingly.