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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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1. The manuscript of Sarmiento et al. reports and discusses the results of the simulation of small scale iron fertilization at four different sites in the ocean. Different scenarios are simulated and the impact on the uptake of atmospheric CO2 and biogeochemical processes in the ocean are investigated. A summary of the results of field studies (artificial and natural iron fertilization experiments) is also compiled and used for comparison with the results of the modelling work.

   One of the major qualities of the manuscript is to present and discuss a limited set of results among the large number provided usually by such modelling studies. Based on these results, the authors emphasize important results concerning the efficiency of the fertilisation on CO2 sequestration, and the impact on nitrogen and oxygen distributions, but also put forward important unknowns which require further investigation. In a general manner this manuscript is clearly written and well structured. This makes it easy to follow, including readers who are not familiar with modelling work. This is important because this study points out unknowns as for example iron-light co-limitation, long-term fate of added iron, spatial and temporal time scales of observation which require further experimental investigations. The review of the results of the different iron fertilization experiments and the comparison with the outputs of the model is also very interesting. For all these reason I recommend the publication of the manuscript in Biogeosciences once the comments below have been addressed and the minor revisions have been done.

   The introduction is pretty long but useful to - Clearly define the different parameters used in the different studies. This is particularly noteworthy for the different ratios characterizing the efficiency of the fertilization (page 10385) - Summarize the "sea of uncertainty" that results for the examination of the results coming both from field and modelling studies.

   RESPONSE: Thanks!

2. In this section the authors report an estimate of the biogeochemical

response for two natural iron fertilizations and point out the large discrepancy that exists between them (page 10386, line 24-30). As mentioned, the reason for the discrepancy is not clearly understood but it can be mentioned that half of it is due to differences in the estimate of the carbon export and the other half is due to differences in the excess of the iron supply. In addition, in a recent manuscript, Chever et al. (BGD Page(s) 6803-6837. SRef-ID: 1810-6285/bgd/2009-6-6803) have refined the seasonal dissolved iron budget during KEOPS which leads to a ratio of 154,000 mol C (mol Fe)-1. This number could be included and discussed in the text, in the appendix C and in the table C1. Nevertheless, it remains that this estimate has large uncertainties.

RESPONSE: Will do.

3. Presentation of the objectives of the studies. I would suggest shortening this part by removing the brief description of the results given in the different subsection a, b.

   RESPONSE: Good suggestion

4. Iron fertilization scenario and impact on biogeochemistry. The amount of iron added during the fertilization simulations was based on 0.02 mmol m-2 yr-1. The reasons for this choice are explained in the text and different sensitivity studies have been conducted. It is interesting to compare the amount of Fe added during the simulation with in situ observations. For the natural iron fertilization at Kerguelen or Crozet, the iron supply is very likely continuous and ranged between 0.020 and 0.160 mmol m-2 yr-1. For the model simulation, the scenario with continuous fertilization (x 1200) supplies 0.240 mmol m-2 yr-1. This is in the same order of magnitude as the natural iron fertilization. In addtion, in the model the fertilized surface area is around 100,000 km2 (table 2) which compares well with the size of the natural patches around Crozet or Kerguelen. Interestingly, the simulation (x 1200) for the Southern Ocean site results in chlorophyll concentrations and nitrate depletions that are similar to in situ observations. Page 10403: It is mentioned that the physical and chemical (or physico-chemical) efficiency in the 100x case compares quite well with the estimates at Kerguelen and Crozet. This is true but for the reasons above it would be better to compare it with the 1200x case. The similarity between the natural fertilization and the 1200x scenario is also important to be mentioned because the simulation x1200 is discussed further in the manuscript. This is done in the conclusion section, where the authors discuss the ability of the model to draw down nitrate during the summertime minimum in the Southern Ocean, and the authors explain the differences with other models that gave opposite results. This is a very

useful discussion that clearly points out the need for a better understanding and parameterisation in models of the iron-light co limitation.

RESPONSE: We will emphasize the closer analogy of Kerguelen and Crozet to the 1200x case as recommended.

5. However, the observations made at the end of the bloom in the naturally fertilized regions of Crozet and Kerguelen also showed that $Si(OH)_4$ was almost depleted in the surface mixed layer. Is this also the case for the simulation (x 1200)? It would be interesting to add a column in figure 8 showing the changes in mean silicic acid concentrations for the different scenarios and to discuss the following issues. Add a short comment on the comparison of $Si(OH)_4$ depletion at the different sites of fertilization. In the case of the Southern Ocean - if the simulation x 1200 shows a complete depletion of $Si(OH)_4$ how would it be possible to increase the nitrate depletion when the iron supplied is increased above x 1200? (pas clair pour moi) - if the simulation does not show a complete depletion in the $Si(OH)_4$ for the scenario x1200, what are the reasons for such a large discrepancy with in situ observations? Does it mean that at naturally iron fertilized site, the iron flux is not high enough to fully relieve the iron limitation leading to an uptake ratio of $Si(OH)_4$ / NO_3 different from 1, that is the ratio expected for non limited diatoms? Or does it mean that if enough iron is supplied another limitation (e.g. by light or silicic acid) prevents the depletion of nitrate? Can the model help to determine when such limitations take place during the bloom? Or does it mean that the parameterization of the coupling/decoupling between the N and Si cycles in the model should be improved. It is mentioned in the appendix describing the model that "silica uptake is made to be consistent with the Si:N ratio synthesis of Martin-Jezequel et al (2000) and droop quota argument of Mongin et al. (2003)". It is not clear for me looking at the equation at p7 whether the Si:N ratio is dependent or not on iron limitation. Because diatoms are the main phytoplankton species responding to iron fertilization, the response of the model is likely very sensitive to the type of parameterisation used to couple the Si and N cycle. A comment on this issue would be welcome. In the same manner as the authors discuss the nitrate depletion it would be also helpful that they compare the results of different models for $Si(OH)_4$ depletion following fertilization.

RESPONSE: Adding a discussion of the Si response to iron fertilization is an excellent suggestion. The plots and analysis have been completed and we will add this to the paper.

6. As I mention above, the discussion on nitrate depletion was part of the conclusion. I would recommend to move it to the section "model sensitivity studies" as a novel sub section which would also include the comments on Si(OH)4 depletion.

   RESPONSE: Will consider how best to deal with this after deciding how to deal with the silicic acid depletion results.

7. The section "conclusions" is too long, and looks more like a continuation of the discussion. Parts of this section, e.g. those referring to figures or presenting new numbers should be moved to the discussion (see for example my suggestion above). The conclusion would be strengthened if it contained only a very short summary of the findings, some important recommendations emerging from this work, and the list of important unknowns that will have a major impact on the predictions of the CO2 removal from the atmosphere following iron fertilization.

   RESPONSE: Good suggestion. We will see what we can do about this.

8. It is a pity that the comparison of the model simulation and the observations from iron fertilizations is only reported in the appendix C and not in the discussion section. This would certainly reinforce the interest of the paper for non modeller readers. I would also recommend adding a sentence in the abstract mentioning that the paper compares the simulations with in situ observations during both artificial and natural fertilizations.

   RESPONSE: Will do this.

9. Minor corrections:
   Page 10406 line 15 0.02 mmol m-2 yr-1

   Appendix for model
   Table 3.2 page 16 parameter KSiO-2 to be replaced by KSiO-4, (but I would recommend to replace in the manuscript SiO-4 with Si(OH)4 and silicate with silicic acid.)
   On the same line of the table replace "nitrate silicate" with "uptake of silicic acid")
   Table 3.5 page 19 in the column units, replace "a" with "yr"

   RESPONSE: Will make these corrections.