

***Interactive comment on “The impact on  
atmospheric CO<sub>2</sub> of iron fertilization  
induced changes in the ocean’s biological pump”  
by X. Jin et al.***

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This manuscript presents a study on the efficiency of patchy iron fertilization to sequester atmospheric carbon. The results are very important, especially considering the undergoing efforts of private companies which propose to sell this method as a means to buffer the carbon increase in the atmosphere. Thus, this study deserves publication in BG.

However, I have one major concern, which is the same as Anand Gnanadesikan. I think that the interpretation of the very high efficiencies obtained here relative to what was obtained with much simpler (and unrealistic) production models is wrong. The

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authors claim that the depth at which primary production is stimulated explains most of the differences. In other words, the deeper the anomaly is generated, the smaller the efficiency will be. I doubt this explains the differences (at least all of the differences) between the study by Gnanadesikan et al. (2003) and this study. In fact, most of the difference is due to the design of the models. In the nutrient restoring approach, iron fertilization is simulated by restoring surface phosphate (or nitrate) to zero. Consequently, downstream of the patch, primary production is zero because phosphate concentrations drop below the observed level. Such an unrealistic behaviour also occurs after the end of the fertilization until the nutrient anomaly vanishes (the rebound period as defined by Gnanadesikan et al., 2003). As shown by Gnanadesikan et al. (2003), this rebound period is largely responsible for their predicted low efficiencies. For instance, their ADD experiment in which phosphate is continuously added to maintain its level to values prior to the iron supply, produces a much higher efficiency. This point has been already discussed in Aumont and Bopp (2006). I also agree with Anand on his explanation of the low efficiency achieved when primary production is stimulated at the bottom of the euphotic layer. Basically, the decrease in DIC at the surface is balanced by the remineralization of the organic matter produced below. However, I don't really understand the larger lateral supply in DIC. Why such a large increase?

Another major outcome of this paper is that the size of the patch is a major factor affecting the efficiency of the fertilization. Increasing the size of the patch over a certain limit (between TINY and SMALL) induces a reduction in the overall efficiency of the artificial iron supply. This brings us back to my previous point. When the patch becomes larger, the reduction in primary production downstream of the patch is larger and over larger regions (similar effect to the rebound process). Other processes may also contribute to that reduction as well like an export in well mixed waters. Unfortunately, the authors don't really insist on that point.

Specific Comments:

Introduction: perhaps you could mention the study by Aumont and Bopp (2006) who

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studied some patchy and large-scale iron fertilization experiments with a model similar to the one used here.

Page 3868, line 20-21: the model is not really eddy-resolving, except in the narrow equatorial band. Furthermore, perhaps a little more can be said on the experimental design (length of the simulations, initial conditions, ...).

Page 3870: If my memory is correct, the model includes three phytoplankton groups plus an implicit group (coccos). Do changes in the species composition induced by the iron supply matter for the efficiency?

Page 3873, line 11: the simulated chlorophyll concentrations are really high in the central equatorial Pacific. They more than three times larger, potentially four to six times according to the color scale used in figure 1. Furthermore, nitrate concentrations displayed on figure 3 are much too high. But this is acknowledged by the authors. I am just wondering what the iron concentrations are in the central equatorial basin. Unfortunately figure 11 is not really of great help since it displays the mean iron concentration over the whole equatorial Pacific, not specifically what is simulated in the central basin.

Page 3877: the one-time fertilization experiment suggests a decoupling between export and air-sea CO<sub>2</sub> fluxes as the minimum in export lags by one year the minimum in air-sea fluxes. This may seem not very crucial but I cannot prevent myself for wondering why.

Page 3892: This is really a massive iron fertilization experiment considering the iron anomalies.

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