Biogeosciences Discuss., 4, S2446–S2452, 2008
www.biogeosciences-discuss.net/4/S2446/2008/

Biogeosciences Discussions

BGD

4, S2446–S2452, 2008

Interactive Comment

Interactive comment on “The impact on atmospheric CO_2 of iron fertilization inducedchanges in the ocean’s biological pump” *by* X. Jin et al.

X. Jin et al.

Received and published: 19 January 2008

Reply to Reviewer 3 (Olivier Aumont)

Comment: *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.*

Reply: Thanks. No comments.

Comment: *However, I have one major concern, which is the same as Anand Gnanade-*

sikan. 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 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).

Reply: It was actually not our primary goal to explain why Gnanadesikan et al. (2003) obtained such low efficiencies, but to explain why we obtain such different atmospheric efficiencies in response to variations in size and other factors. The comparison with Gnanadesikan et al. (2003) comes only after we had established an explanation for our results. It is correct that it is difficult to make direct comparisons with the results of the restoring model simulations of Gnanadesikan et al. (2003) as such a model indeed shows an unrealistic behavior after the end of the fertilization. Nevertheless, all our prior analyses and those that we have done in response to this review cycle point toward the rebound period not being that critical (note that we are refering to the atmospheric uptake efficiency as defined by us and not by Gnanadesikan et al. (2003) - see our responses to reviewer 2 for how the two are related to each other). In fact, our new simulations with the nutrient restoring model of Gnanadesikan et al. (2003) actually do support our depth argument. In summary, we therefore maintain our key hypothesis and chose to disagree with this reviewer.

In detail: First, if the rebound effect was really critical, one would expect a large difference between continuous and one-time fertilization experiments. No such effect was found, as they all have high atmospheric uptake efficiency. This is because both exported organic carbon and the air-sea CO_2 flux decrease at same time, resulting in only a small impact on their ratio.

Second, we have done a few new simulations using the same nutrient restoring model as used by Gnanadesikan et al. (2003). These new simulations support our interpretations that the depth distribution of export production is a key factor that determinates the magnitude of the atmospheric uptake efficiency.

Third, the efficiency estimated from Figure 14 of Gnanadesikan et al (2003) also shows that the rebound effect could not explain the low efficiency identified by them.

Fourth, the ADD experiment is different in nature from iron fertilization because macronutrients are added to the ocean, resulting in increase in the global inventory of them. We therefore are very hesitant to make comparisons with this particular simulation. Nevertheless, in terms of the atmospheric uptake efficiency, the results of this experiment still support our interpretation.

Comment: *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?*

Reply: As shown in Figure 8b in our submitted manuscript the net carbon exchange, including both DIC and organic carbon, between the upper and lower layers of the euphotic zone is very small,

53.8 TgC yr^{-1} (DIC vertical transport) - (30.9+21.5) TgC yr^{-1} (organic transport) =1.4

BGD

4, S2446–S2452, 2008

TgC yr^{-1}.

Thus only 2-3% of the exported organic carbon comes from the upper layer of the euphotic zone. It is the large lateral supply in DIC that supports the export of organic carbon. This large increase occurs because the stimulation of net community production and eventually export near the bottom of the euphotic zone causes a 8220;hole8221; in DIC, resulting in a large increase in the lateral DIC gradients. Given ample isopycnal mixing, this leads to a large increase in the lateral transport. On a larger scale, this large lateral supply of DIC is finally compensated by vertical mixing from the deep ocean and lower NPP in other places, but only to a very small degree from the upper euphotic zone as demonstrated by the small air-sea CO_2 flux in this case.

Comment: *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.*

Reply: The size of the patch indeed results in a substantial change of the atmospheric uptake efficiency. We interpret this, however, to be mainly a consequence of a change in the depth of the stimulated net community production. The reason for why a larger patch leads to a deepening of the stimulated NCP is associated with the nutrient distribution in the eastern tropical Pacific. Once a patch extends over a substantial enough region, the near-surface supply of macro-nutrients will tend to decrease relative to the size of the patch (essentially a surface are to circumference argument), so that a larger fraction of the nutrients deeper down in the water column, where they are supplied from depth, are being used. As a consequence, a larger fraction of net community production occurs at depth, resulting in a lower atmospheric uptake efficiency.

BGD

4, S2446–S2452, 2008

The magnitude of the downstream reduction in export production is of little consequence for the atmospheric uptake efficiency, since this will tend to reduce both the export and the uptake of CO_2 from the atmosphere (as argued elsewhere). Note, however, that such downstream effects have a large impact on the iron to carbon fertilization ratio (which is NOT the focus of this article).

Comment: *Specific Comments:*

Introduction: perhaps you could mention the study by Aumont and Bopp (2006) who studied some patchy and large-scale iron fertilization experiments with a model similar to the one used here.

Reply: We apologize for the oversight. We have added this reference to the introduction.

Comment: *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, ...).*

Reply: We didn't state that it is eddy-resolving, but actually wrote that it is eddy-permitting, a view that we maintain. We added more on the experimental design to the method section.

Comment: *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?*

Reply: Yes, the model includes all these phytoplankton groups and changes in the species composition induced by the iron supply indeed occur. Per se, such changes have no consequence on the atmospheric uptake efficiency (it matters for such things as the export ratio), with the important exception of a shift in the relative contribution of coccolithophorids to the total export. It turns out that such changes indeed occur, but they are relatively small and of no consequence for our analyses. In response, we have added a comment on this to the manuscript.

(note on the side: There is, of course, also the possibility that shifts in the ecosystem results in a change in the depth distribution where net community production is really stimulated. We couldn't identify such a shift.)

Comment: *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.*

Reply: As the reviewer has noticed we have analyzed the simulated chlorophyll and nitrate distributions and evaluated them in some degree by comparison the observations available. Lacking of observations of iron in the ocean makes it very difficulty to evaluate the simulated iron distribution. The Figure 11 is to give a general sense about the iron distribution in the model. More detailed features about the iron cycle in this model please refer to the papers of Moore et al. (2004, 2006). On the other hand, our conclusion is not sensible to the iron simulations in the model. Because the atmospheric uptake efficiency is more related to organic carbon export.

Comment: *Page 3877: the one-time fertilization experiment suggests a decoupling between export and air-sea CO_2 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.*

Reply: The decoupling between export and air-sea CO_2 fluxes can be explained by the rebound effect found so prominently in the nutrient restoring experiments. With the accumulation of CO_2 in the ocean, some of it will be released back to the atmosphere.

BGD

4, S2446–S2452, 2008

This will compensate part of the effects of the organic carbon export, resulting in air-sea CO_2 flux arrives its minimum earlier then that of the organic carbon export.

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

Reply: The amount of iron we decided to add is indeed rather high in comparison to the experiments that have been undertaken so far. The value to use was chosen as part of the Iron Fertilization Model Intercomparison Projet (IFMIP). A large value was selected in order to ensure that we will obtain a decent response. For the purpose of this paper, the actual magnitude of the iron addition is actually of secondary relevance, as we are interested in a ratio, i.e. the ratio of air-sea flux over export, which is relatively insensitive to the actual magnitudes of the fluxes (as illustrated for example by the very similar uptake efficiencies found in the X-LARGE and the 2X experiments, where the entire Pacific was fertilized but with rather different levels of iron).

Interactive comment on Biogeosciences Discuss., 4, 3863, 2007.