Biogeosciences Discuss., 4, S2440–S2445, 2008 www.biogeosciences-discuss.net/4/S2440/2008/ © Author(s) 2008. This work is distributed under the Creative Commons Attribute 3.0 License.



**BGD** 

4, S2440-S2445, 2008

Interactive Comment

# 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.

X. Jin et al.

Received and published: 19 January 2008

Reply to Reviewer 2 (Anand Gnanadesikan) — Part 3: Comments and responses: more detailed points

**Comment:** More detailed points for the authors to consider are listed below.

p. 3865, lines 23-26. Fertilization does not only affect the downward component especially on a time scale of a century. I emphasize this because of it reinforces the idea that "all we care about is export", which is so easily misinterpreted.

**Reply:** We altered the sentence by adding "initial" to it. The reviewer is correct in that iron fertilization eventually will alter the upward component as well.



Printer-friendly Version

Interactive Discussion



**Comment:** p. 3866, while I think the split into a physical-chemical efficiency and a biological iron utilization ratio is an interesting way of thinking about the problem, I do have some worries about it. A simple thought experiment will illustrate why. Suppose I take a region where most of the nutrient is eventually utilized and iron fertilization results in some of this nutrient sinking out of the water column away from the recycling zone. Since I haven't actually changed the concentration of remineralized nutrient, I shouldn't expect a change in carbon, but I would expect a decrease in biological cycling. So I could actually get a case where the iron utilization efficiency was negative, but where there might be some small uptake (as happened in one my nutrient depletion runs). That's not to say that it is a useless measure- clearly for the runs here it is not. On the other hand if I take a region with a lot of preformed nutrients, activate them by adding iron and keep the iron and nutrients together, I'll get an uptake that goes as regardless of how rapidly these nutrients cycle back through the surface layer. In this case the efficiency that we care about (carbon to iron fertilization ratio) is the same but the atmospheric uptake efficiency and utilization ratio compensate each other. My point is that if I think about preformed nutrients I understand instantly what's happening for all these cases.

**Reply:** We would agree with the reviewer that the concept of preformed vs remineralized nutrients is a useful one (as mentioned) above. We also mentioned that this concept has some potential flaws as well. It is correct that the separation of the iron fertilization ratio into an iron utilization ratio and an atmospheric uptake efficiency needs to be viewed with some care, but we maintain that the atmospheric uptake efficiency is a very important quantity per se. It describes how a change in the amount of biologically exported carbon changes atmospheric  $CO_2$ . And as argued above, this is something that is important for a much wider community and not just for those interested in the use of iron fertilization as a mitigation option. Finally, we are a bit puzzled by this request, as this is a separation that was developed as part of the Iron Fertilization Intercomparison Project (IFMIP), which included a wider group of scientists, including this reviewer.

### BGD

4, S2440-S2445, 2008

Interactive Comment

Full Screen / Esc

**Printer-friendly Version** 

Interactive Discussion



**Comment:** *p.* 3867. Again, Figure 14 of Gnanadesikan et al. clearly demonstrates that the suppression of biological activity after fertilization also represents a mechanism for reducing the carbon flux from the atmosphere. This appears to be active in these runs as well. Also, my nutrient addition runs appear to contradict the interpretation that it is simply a question of vertical location of production.

**Reply:** The reviewer is correct in noting that the suppression of biological productivity after fertilization is reducing the uptake of  $CO_2$  from the atmosphere. Our figure 5d from the 3MON-ONETIME shows exactly this. Nevertheless, our atmospheric uptake efficiency is much larger than that of Gnanadesikan et al. (2003), even after accounting for the different definitions (see Table 2). What is different, however, is that our export is coming primarily from the near surface, while the export in the runs of Gnanadesikan et al. (2003) comes from greater depth, as shown in our new nutrient-restoring simulations with the model that was used by Gnanadesikan et al. (2003) 8211; exactly as predicted by our argument.

We think that one needs to be extra careful with the nutrient addition experiments. These simulations are of entirely different nature, as in all our normal Fe experiments, we are primarily redistributing the macro-nutrients, whereas in the nutrient addition simulations, the total inventory of nutrients in the ocean is changing. We therefore think that it is not valid to use this run for comparison. Even if we did, we don't see a contradiction. The nutrient addition simulation yielded higher efficiencies in Gnanadesikan et al. (2003) in comparison to his standard simulations. This is again as expected, as in this case, a higher fraction of export in such a nutrient restoring simulation comes from the upper levels of the euphotic zone.

**Comment:** *p.* 3869. Stoichiometric ratios are fixed for each functional group. Are they the same. If so shifts in functional group type would result in shifts in carbon uptake. Is this happening?

Reply: The stoichiometric ratios are fixed for each functional group under optimized

## BGD

4, S2440-S2445, 2008

Interactive Comment



Printer-friendly Version

Interactive Discussion



growth condition and will change based on GD98 under other conditions. The shifts might be result in shifts in carbon uptake through the changes of vertical net community production distribution induced by iron fertilization.

**Comment:** *p.* 3871. I assume that the light limitation experiments are conducted over the same patch as the surface fertilization experiments, but this should be stated.

**Reply:** We added this into the text.

**Comment:***p.* 3877. The statement is made that the drop in production in the onetime fertilization case is simply due to a decrease in surface macronutrients. But if this were true (lower preformed nutrients), would we expect the carbon flux to drop as well? I wouldn't Apparently there must be some reconversion from the remineralized to the preformed pool. What is the mechanism behind this reconversion (denitrification? excess scavenging of iron?). This is a really important result from this simulation. Dumping iron in the ocean actually results in a decrease in POC export in the "out years". So while the efficiency appears to be 1, the carbon-to-iron fertilization ratio is in fact dropping. It's vital to understand why, as it is this carbon-to-iron fertilization ratio that will be used to put a value on the procedure. In a of lot ways I see this as putting realistic limits on one of the results of Gnanadesikan et al. (2003), namely the rebound effect from the initial fertilization. The difference is that in Gnanadesikan et al. (2003) the rebound accounts for more than 80production, but in this paper the rebound is more like 30I never believed the 80bound than an actual prediction.

**Reply:** Yes, the rebound exists in our simulations. And the POC export decreases during this period and so does the air-sea carbon flux. As we have stated in text, this is a combination of the decrease both in macronutrient and iron concentrations. However, this has little impacts on the atmospheric uptake efficiency as in Figure 5 of the paper. The carbon-to-iron fertilization ratio might be decrease due to scavenging of iron and this will be discussed by Sarmiento et al. (in preparation) in greater detail. There is no denitrification process included in our model, which, of course, can drastically alter the

# BGD

4, S2440-S2445, 2008

Interactive Comment



Printer-friendly Version

Interactive Discussion



results, because of the change in the macronutrient inventory..

**Comment:** *p.* 3881. For the record, I've long held that when iron is added the response should start off like my nutrient addition runs and then over time as iron is lost the efficiency should decrease. There is nothing in these runs that contradicts this position.

**Reply:** No reply needed.

**Comment:** *p.* 3884. Regarding the fact that there is a correlation with depth dependence. This is striking, but it doesn't get at my point regarding the mechanisms. Depth by itself cannot be the key- it is likely correlated to either enhanced cancellation between the surface and deep perturbations or to more of the enhanced production being borrowed from subsequent periods. Put another way, given the relatively high rates of mixing between the upper and lower parts of the euphotic zone that one expects over most of the ocean, one wouldn't expect there to be any difference per se.

**Reply:** By now, we believe that we have provided ample evidence that neither borrowing from subsequent periods nor the reviewers arguments about the differential volumes can explain why the atmospheric uptake ratio varies rather strongly in our simulations. In addition, we have demonstrated that variations in the atmospheric uptake efficiency found in the nutrient-restoring simulations of Gnanadesikan et al.(1993) are also related to the depth distribution of export production. We therefore conclude that the high correlation that we find with the depth indices is the result of a process-based relationship and not simply a by-product of depth being correlated with something else.

We do agree, however, that depth should not be viewed as an absolute metric. What is really the controlling factor is the degree, to which changes in DIC at some depth within the euphotic zone can be communicated to the surface by mixing and transport. If the surface mixed layer remains always deeper than the euphotic zone, then any DIC anomaly would be quickly redistributed within the entire upper ocean, and our argument wouldn't work well. It turns out, that such conditions are rarely found in the global ocean, so that our depth-based indices can be applied nearly everywhere. Some

## BGD

4, S2440-S2445, 2008

Interactive Comment



Printer-friendly Version

Interactive Discussion



of the high-latitude regions may be the exception. This caveat is already discussed in the paper. We therefore didn't have to make any changes.

**Comment:** *Minor points Efficiency is misspelt in Figure 6* **Reply:** Changed.

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

### BGD

4, S2440–S2445, 2008

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

