

***Interactive comment on “The impact on
atmospheric CO₂ of iron fertilization
induced changes in the ocean’s biological pump”
by X. Jin et al.***

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This paper presents studies of the impact of carbon sequestration induced by "patch" fertilization of the surface ocean. Such projects are currently being proposed by a number of companies, one of which is actually selling voluntary carbon offsets online. Given the price of carbon on the European market, such offsets could easily sell for hundreds of millions of dollars. It is thus extremely important for studies like this to be done that evaluate the impact and efficiency of carbon sequestration. More importantly, the previous study of patch fertilization discussed here by Gnanadesikan et al. (2003) used two very unrealistic models of production, while this paper uses a real-

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istic prognostic model with an explicit iron cycle. Consequently, the results are quite important and deserve to be published. However, I have some important objections to interpretations made in this paper that I feel have to be addressed if the paper is not going to end up being misused by those who have a vested interest in claiming more credit for fertilization than it deserves.

My major issue with this paper is the interpretation of the results. One can think conceptually about the impact of any perturbation to the carbon cycle in the following terms. Suppose that the perturbation results in increasing the carbon concentration in some locations and decreasing it in others. One can then write

$$\delta DIC^{ocean} = \delta DIC^{pos} + \delta DIC^{neg} = \frac{\partial DIC}{\partial pCO_2} \delta pCO_2^{pos} V^{pos} + \frac{\partial DIC}{\partial pCO_2} \delta pCO_2^{neg} V^{neg}$$

where the superscripts *pos* and *neg* refer to regions where the partial pressure of CO₂ increases or decreases and the V^{pos} and V^{neg} represent the volumes of these regions. The promise of fertilization is that by drawing down nutrients one can create a region of negative anomaly near the surface that will be damped out by the atmosphere, while the positive anomaly at depth will persist over time.

However, there are a number of ways by which this process could be short-circuited. First, the positive and negative anomalies could mix together before equilibration with the atmosphere has a chance to occur. Second, if the nutrients being taken up are "borrowed" from some other point in space and time, positive anomalies will be created at the surface. Third, if the additional production results in denitrification, some carbon will be released when low-nitrate, high carbon water comes to the surface. Fourth, if V^{neg} is large enough, δpCO_2 will be very small and the bulk of the carbon uptake will have to come from the ocean.

A key point of this paper seems to be that the fourth mechanism results in a substantial difference between the atmospheric uptake efficiency resulting from enhanced production over the top 20m and in the lower part of the mixed layer. I find it difficult to believe

this result for the simple reason that the volumes involved seem to me to be far too small. If one were to conserve carbon in the system and include an atmosphere with variable carbon dioxide the equation above would change to

$$R_{C:Air}M_{atm} * dpCO_2^{Atm} + B^{pos}\delta pCO_2^{pos}V^{pos} + B^{neg}\delta pCO_2^{neg}V^{neg} = 0$$

where B is the buffer factor, $R_{C:air}$ is the mass ratio of carbon to air and M_{atm} is the mass of the atmosphere. The point is that if the pCO_2 changes in the negative region come into equilibrium with the atmosphere, the fraction taken from the atmosphere will go as

$$F_{ocean} = B^{neg} * V^{neg} / R_{C:air}M_{atm}$$

But we know that if we add carbon to the atmosphere, 85% of it will end up in the ocean. So even if the entire ocean buffer capacity is mobilized 15% of a carbon perturbation to either system will end up coming out of the atmosphere. For the tiny volumes here, almost all of the carbon must therefore come out of the atmosphere- if the atmosphere can see the perturbation in time.

It is also stated that this effect, caused by production occurring deeper in the water column, is responsible for the low atmospheric uptake efficiency seen in Gnanadesikan et al. (2003). This is incorrect. As can be seen from Figure 14 of that paper, the big impact of simulating iron fertilization a pulse of nutrient depletion was that production was borrowed from subsequent months. The resulting drop in biological productivity over these months meant that no substantial change in preformed nutrients occurred and the uptake efficiency *as defined relative to the initial pulse of export* was low the runs. That this is the case can be easily verified (and was in the paper) using a simple box model.

The definition made in Gnanadesikan et al. (2003) was intentional. To this day there are proponents of fertilization who assert an equivalence between export from the initial bloom and carbon sequestration. A major point of Gnanadesikan et al. (2003)

was that such an equivalence could not be made because production could be borrowed from other times and places. In terms of Jin et al., a such a naive definition of fertilization would confound the atmospheric uptake efficiency with the iron utilization ratio. In Sarmiento et al. (2007) it is shown that when the iron added to the system is immediately lost, the iron utilization is quite different than when it is retained but the physical-chemical efficiency (defined as it is here) is the same.

Further evidence that it is not the depth per se that is involved in producing low efficiencies can be garnered from the fact that the nutrient addition runs reported in the same paper had very high efficiencies- even though the nutrient was added over the same top three boxes (85m).

It is important to make this distinction between the operational definition of fertilization and the actual efficiency. Clearly if there is additional export from the surface ocean there will be a lot of uptake from the atmosphere. But the additionality has to be properly defined and more importantly, measured. I think many of those involved in the field have come to understand this, but not everyone has.

So why is the efficiency so much lower in Jin et al. when production is increased lower in the water column? Which of the mechanisms outlined above is likely to be important? I would argue that the evidence as presented here is that it is borrowing again. Examining Figure 8 seems to show this quite clearly. The entire additional export from the lower mixed layer has come from the upper mixed layer. In terms of preformed nutrients this would imply that the preformed nutrients haven't changed at all.

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.

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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 $R_{Fe:C} \Delta Fe$ 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.

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.

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?

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.

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p. 3877. The statement is made that the drop in production in the one-time 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 80% production, but in this paper the rebound is more like 30%. I never believed the 80% rebound than an actual prediction.

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.

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.

Minor points

Efficiency is misspelt in Figure 6

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