Biogeosciences Discuss., 4, S2419–S2422, 2008
www.biogeosciences-discuss.net/4/S2419/2008/

Biogeosciences Discussions

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 the referee 1

Comment: *This manuscript revisits the question of how efficient iron fertilization can be in sequestering CO_2 from the atmosphere. Since John Martin's seminal work, this has attracted much attention from scientists and lay persons alike. This work by Jin et al. presents numerical modeling analysis of "patch" fertilization as done previously by Gnanadesikan et al. (2003) in the equatorial Pacific and Matsumoto (2006) in the north Pacific but is novel for a number of reasons: production is simulated with an ecosystem model (instead of nutrient restoring); horizontal resolution of the model is eddy permitting; and vertical resolution resolves the euphotic zone well (11 layers in* *the top 100 m). These new features allow the authors to draw a very interesting and important conclusion about the importance of vertical distribution of production in the CO_2 sequestration efficiency. Unlike previous modeling studies (non-patch fertilization studies in addition to the above two patch fertilization studies) that have indicated low efficiency, Jin et al. obtain much higher efficiency. They attribute this to the bias in nutrient-restoring production schemes that tend to give deep productions and thus low efficiencies. Jin et al's fertilization-induced production occurs near the surface where CO_2 from the atmosphere can supply the needed CO_2. Their Figure 9 nicely shows this. The careful analysis throughout the manuscript gives me a very favorable impression and I recommend publication of this work with minor revisions.*

Reply: Thanks for the comments.

Comment: *I have a couple comments that I suggest Jin et al. consider in their revision. First, their relatively high sequestration efficiency is directly caused by the high Fe retention, which is discussed in Appendix D. It is not clear to me that their high retention as indicated by DeltaFe* is at all realistic. If not, then their high efficiency would also not be realistic.*

Reply: The high Fe retention will certainly enhance the overall impact of the addition of iron on atmospheric CO_2, defined as the carbon-to-iron fertilization ratio in the text. However, it will have little direct impact on the atmospheric uptake efficiency. The only way how the iron retention will impact the latter is by changing the vertical distribution of the net community production induced by iron fertilization. We haven't been able to find an indication that this is occurring. In fact, in a related manuscript, Sarmiento et al. (in preparation) have conduced two experiments with strongly different levels of iron retention and found nearly the same atmospheric uptake efficiency, thus supporting our argument. The reason for the atmospheric uptake efficiency to be largely independent of iron retention is because the atmospheric uptake efficiency is a ratio. When NCP is kept high as a result of a relatively high iron retention, the ocean will continue to take up CO_2 from the atmosphere, causing the ratio to remain relative constant. The

BGD

4, S2419–S2422, 2008

reviewer also raises the issue of whether the model simulated iron retention is realistic. Based on our limited number of comparison, we would argue that the model is behaving reasonable, but given the small number of observations we have great difficulties in providing vigorous constraints on this statement (see also two recent papers by Moore and Broucher wherein they compare this iron model to observations in a more vigorous manner (Biogeosciences under revision, ms available from http://www.ess.uci.edu/ jk-moore/). But, we would like to emphasize again, that iron retention is not a crucial element for the atmospheric uptake efficiency.

Comment: *Second, and this relates to the first comment, how can their high efficiency be reconciled with low efficiencies obtained in field experiments as summarized by de Baar et al. (2005)? I understand why the Jin et al. model is different from other modeling studies, but again I am not sure how realistic their model results are.*

Reply: Since the atmospheric uptake efficiency cannot really be estimated from the available field observations (most of the time, no export was directly measured, and if it was, data are available for only a very brief period, and not integrated over 10 years), we feel that such comparisons cannot really be done in a defensible manner. But let's do it nevertheless. From Figure 22 of de Baar et al. (2005) paper we infer that the air-sea carbon flux is about 3% of primary production ranging from virtually nil to 10-27% of primary production. Assuming an export (e-) ratio of 0.1 to 0.2 would then suggest a (short-term) atmospheric uptake efficiency of anywhere from 15% to 100%. Thus, considering the large uncertainties associated with the observations, we don8217;t think that a strong argument can be made that a high atmospheric uptake efficiency is not realistic.

Comment: *Finally, I think it would be worth clarifying to the general audience that high fertilization efficiency (how much CO_2 comes from the atmosphere?) is fundamentally different from fertilization effectiveness (is the sequestration method being considered actually effective? relates to economic cost/benefit). Matsumoto (2006) actually considers this question with regard to patch fertilization with some economic analysis.*

Reply: We strongly share this view. In fact, high atmospheric uptake efficiencies do not imply high fertilization ratios either, as we also demonstrate in the paper. In response, we have expanded the discussion of the use of iron fertilization as a mitigation option somewhat.

Comment: *That paper also discusses the fact that patch fertilization will reduce future production at the expense of fertilization-enhanced production (presented as a new finding, 1st paragraph, page 3877).*

Reply: This is certainly correct. In fact, this finding goes back to Sarmiento et al. (1991). In response, we have added the following words at page 3877 line 10: This process has been referred to as the rebound period by Gnanadesikan et al. (2003) and is commonly observed in iron fertilization model studies (e.g., Aumont and Bopp, 2006 and Matsumoto, 2006).

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

BGD

4, S2419–S2422, 2008

Interactive Comment