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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.

Anonymous Referee #1

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This manuscript revisits the question of how efficient iron fertilization can be in sequestering CO2 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 conclu-

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sion about the importance of vertical distribution of production in the CO2 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 from the atmosphere can supply the needed CO2. 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.

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. 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.

Finally, I think it would be worth clarifying to the general audience that high fertilization efficiency (how much CO2 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. 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).

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