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Biogeosciences Discussions

Interactive comment on "Influences of initial plankton biomass and mixed layer depths on the outcome of iron-fertilization experiments" *by* M. Fujii and F. Chai

Anonymous Referee #1

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General comments Over the past 10 years, 10 in situ, mesoscale iron fertilization experiments have been carried out in the 3 major high-nutrient-low-chlorophyll (HNLC) regions of the world ocean. The original intention of these experiments was to test the Iron Hypothesis of John Martin: that the iron-fertilized (by dust), glacial Southern Ocean was a major sink of “missing” atmospheric carbon dioxide between past climate cycles. So far the first condition of the hypothesis: build-up of a phytoplankton bloom following in situ fertilization has been met by 9 of the experiments but the fate of bloom biomass is still under dispute because most of the experiments were too short to record processes occurring in the senescence phase of the bloom, when mass sinking of intact diatom cells and chains is commonly observed in natural diatom blooms. Nevertheless, the experiments have provided a large amount of data on the behaviour of iron-stimulated phytoplankton blooms which, after being subjected to systematic analysis and modelling, should improve our understanding of fundamental principles determining the rise of diatom blooms. This paper, as suggested by the title, compares some experiments with one another using a sophisticated ecological model.

Comparisons on a general level have been published by de Baar et al. (2006) and Boyd et al (2007) so I was looking forward to some new insights from application of their sophisticated model. Unfortunately, the authors chose to follow the approach of de Baar et al which compares the concentrations (moles m-3) instead of stocks (moles m-2) of declining nutrients and accumulating biomass in the mixed layer. Testing the iron hypothesis however, requires following the spatial transfer of bulk carbon across surfaces: the air-sea interface, various pycnoclines from the base of the mixed layer downward until the sediment surface. The total amount of carbon (mole m-2) that can be transferred from the atmosphere to the surface layer depends on the depth of the mixed layer and the concentration of the next limiting nutrient following iron (generally nitrogen) in it. It is obvious that, assuming similar phytoplankton growth and mortality rates, chlorophyll concentrations in a shallow mixed layer will be higher than in a deep mixed layer although the integrated stock m-2 could be much the same. But it is this concentration difference between the SEEDS experiment in a 10 m mixed layer and the Southern Ocean experiments SOIREE and EisenEx in 60 and 80 m mixed layers respectively that are called dramatic differences and presented as major results of the modelling exercise in this paper. In my opinion this is a trivial finding not worthy of the model employed.

It would be much more exciting and rewarding to examine more fundamental questions arising from the experiments with the model. For instance, a bloom failed to develop in the 10th experiment SEEDS II carried out in the same spot northeast of Japan under the same physical and macronutrient conditions as the earlier experiment SEEDS

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implying that a biological reason (most likely heavy grazing pressure) was responsible for suppressing the bloom. This finding violates a basic paradigm of bio-oceanography – Sverdrup’s critical depth model and is worthy of rigorous interpretation. Since the composition of the plankton during both experiments is known (?) it would be very interesting to determine, with the model, the minimum biomass of the grazer population necessary to exert enough grazing pressure to prevent bloom biomass from accumulating (relative to a given seed stock). In this context, it would be worthwhile to explore to what extent dilution of the phytoplankton crop by deep vertical mixing also reduces grazing pressure based on the rationale of the serial dilution method of Landry and Hassett to estimate microzooplankton grazing.

Specific comments I recommend separating diatom-feeding unicells (heterotrophic protists) from the diatom-feeding copepods because of the very different reproduction rates and also because interaction between the two lead to trophic cascades. It is now well established that copepods prefer heterotropic protists (dinoflagellates and ciliates) over diatoms which reduces grazing pressure of potentially fast-growing protists on diatoms. This modification could be done at the expense of the second category of zooplankton – predators of copepods – that at the time scales of the experiments are not likely to exert as much influence on the outcome of the experiment. Finally I do not see the merit of the third category of zooplankton created by separating Gyrodinium species that feed or do not feed on diatoms.

When comparing maximum stocks (or even concentrations in this case) attained in different experiments one should remember that blooms like SOIREE and EisenEx had not peaked at the time the last measurement was taken, unlike the SEEDS and SERIES experiments which reached macronutrient (nitrate) limitation. It would be interesting in this context to compare biomass accumulation rates in deep and shallow water columns in relation to the light regime and temperature by employing the Sverdrup model. According to de Baar, the lower chlorophyll concentrations in deeper mixed layers can be explained by light limitation. However, since the standing stocks in

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the respective mixed layers were similar, the efficiency of light utilisation will also have been similar. So either all the blooms were light limited or none of them were.

The detailed composition of the EisenEx bloom indicates that species-specific growth rates and the size of the respective initial stock determine how rapidly biomass is built up (Assmy et al and Henjes et al. Deep-Sea Res. 2007). Perhaps absence of fast-growing species such as Chaetoceros debilis (which dominated the SEEDS bloom) in the SEEDS II water column was the reason why no bloom developed?

Summing up, I find that there are much more interesting questions that need to be addressed with this valuable model than the problem of concentration relative to MLD examined here. I look forward to seeing the results of more gainful exercises in the next round.

Interactive comment on Biogeosciences Discuss., 4, 4411, 2007.