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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 #2

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Synopsis:

This paper presents results from a series of modelling experiments designed to simulate the effects of the initial mixed layer depth and plankton biomass on the outcome of an iron enrichment experiment. The model data points to the importance of the mixed layer depth (MLD) in determining the maximum (volume based) biomass found with iron enrichment. The authors also explore the initial diatom and zooplankton biomasses and found that they are mostly less important than MLD in determining the maximum response. This is an interesting modelling study as it will help to better understand the experiments that have already been performed and guide those that are yet to be

undertaken.

General Comments

Horizontal Mixing: During SEEDS there was very little horizontal mixing (Tsumune et al. 2005) compared to the other Iron enrichment experiments (Boyd et al. 2007). Indeed patch size data for SEEDS II (Tsuda et al. 2007) indicates that a major difference between SEEDS and SEEDS II was the initial horizontal mixing rates with the SEEDS II patch rapidly diluted with out-patch waters. While the authors mention physical mixing briefly it appears they have no mechanism in their model at present to include this important parameter. By choosing to use SEEDS as their template the model is automatically set to replicate the experiment where there was almost no horizontal mixing occurring over the first few days. Thus for comparison with the other field experiments it misses one of the key controls on the bloom development. I would strongly encourage that any future work include this important parameter in the 1D model.

Light Limitation & Svedrup: The ‘Elephant in the corner’ (we know it is there but it is not spoken of) throughout much of this work appears to be light limitation, which is the real control on the surface chlorophyll (volume) concentrations via the MLD. This phenomena of course was well described long ago by Sverdrup through his idea of critical depth (Sverdrup 1953) and it would be worth examining the model data using this approach. Furthermore a frequently overlooked paper by Nelson and Smith (1991) that clearly indicates the relationship between MLD, critical depth and chlorophyll concentrations for Southern Ocean conditions could be applied to the present work in order to test these concepts in the model data.

Problem with Model Design: It appears that the model does not consider Fe limitation of waters below the mixed layer and thus overestimates the productivity of the shallow mixed layer cases by including enhanced production from below the mixed layer where Fe should still be limiting. If this is not the case the authors then need to supply the horizontal diffusion terms that could be mixing these iron rich waters to depth but not

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eroding the mixed layer.

Area vs Volume: The de Baar et al. (2005) work did not examine biomass on an areal basis (column integrated) which would have probably lead to some slightly different conclusions regarding the total biomass in each experiment. While the authors do mention that this could be important in their conclusions it would be better to examine it more thoroughly in the main text itself. The reason this is important is that if you look on a total biomass or drawdown of CO2 basis, experiments with deep mixed layers such as EisenEx and EIFeX have larger mol m-2 values than shallow experiments like SEEDS, despite the higher mixed layer concentrations on a volume basis in SEEDS. In the present paper at least the 1st experiment should be plotted on a column integrated basis for comparison purposes to truly test the validity of the idea that the MLD is important for the biogeochemical response. I am not suggesting here that it is not but that it needs to be seen in more than just a volume based approach.

Other Zooplankton studies: There are other recent zooplankton studies from the Fe enrichment experiments that are not discussed and will have bearing on some of the results here (Jansen et al. 2006; Schultes et al. 2006).

Specific Comments

P4413 Line 9. A reference is missing here for the CO2 data in EisenEx (Bakker et al. 2005).

P4414 Line 2. Actually the physical mixing was apparently radically different between these experiments if one compares the patch sizes over the critical first few days (Tsuda et al. 2007; Tsumune et al. 2005), thus horizontal dilution would be an important physical constraint. See also general comment above.

P4414 Line 19. The lateral mixing of the patches has been reasonably well described in several experiments (Abraham et al. 2000; Law et al. 2006; Stanton et al. 1998; Tsumune et al. 2005) and was briefly summarised recently in the review paper by Boyd

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et al. (2007). Given the importance of this process it is disappointing that modelling studies have not focussed on it yet.

P4416 Line 1. The use of a 1D model clearly simplifies the physics and thus without dilution effects clearly biases the model unrealistically (in my view) towards diatom abundance being simply in terms of growth vs grazing. The effects of dilution have been shown recently to be a very important factor for diatom abundance during EisenEx (Assmy et al. 2007) with apparent growth rates almost 50% less than the dilution corrected gross growth rate. Some discussion on the blindness of the model to these real effects would be helpful in this context.

P4417 Line 20. Thus in the present model there is no direct dependency on Fe concentration or bioavailability for the growth rates and the model is thus also independent of the total amount of Fe added. These aspects should also be stated clearly in the manuscript.

P4418 Line 22. I am not sure why the temperature is different for each MLD? Surely the MLD was specified to be the same in each experiment for comparative purposes? It appears now that the MLD temperature was constructed from an average of some water column profile and it is for this reason alone that the water temperature in the mixed layer shows an inverse relationship with the MLD.

P4419 Line 1. Not sure what is really being calculated here as PAR would normally indicate a surface flux [W m-2] and would be expected to be the same for all experiments. If it is the mean PAR throughout the mixed layer then it is related to the light attenuation coefficient, which must change with time during the experiment as the diatoms bloom, so exactly how is this value calculated? A better way to show this data would be to calculate the euphotic depth and compare it to the MLD (See also general comment above on light limitation).

P4419 Line 26. Again the real experiments have horizontal dilution which must have a major effect on export ratios. P4420 Line 23. Why is the diatom biomass in terms

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of N in Figure 7? Also if the biomass is larger by a factor of 7.1 on a volume basis then based on an areal basis the deeper mixed layer (10 x deeper) should be higher than the shallower model run. See general comment on this above.

P4420 Line 25. What is fuelling the diatom growth below the MLD? It appears from the model construction there is no iron limitation for diatoms below the fertilised patch. Surely this is a flaw in the model construction that there is no difference between the fertilised and unfertilised vertical dimensions of the patch? If horizontal diffusion is low then little Fe would be bioavailable below the MLD and phytoplankton in this part of the water column would be iron limited. Thus overall it appears that the shallow MLD cases are overestimating the primary production (see general comment above also).

P4421 Line 23. See also paper by Assmy et al. (2007) on this subject and the importance of dilution for the field experiments.

P4425 Line 9. It is mentioned in the conclusions here but it should also be discussed more in the main text as the column-integrated values are more important on a climate and CO2 drawdown basis (see also general comment above).

Figure 6. This figure appears to be redundant as there is no valid physical reason in the field experiments for a link between MLD and temperature.

Figure 7. For what day of the model run is each profile taken from?

Figure 7. Why is the diatom biomass in terms of N here? This implies also that the diatom total growth rate is also expressed in terms of N, as there are no units quoted in the figure legend?

Figure 8. Why is all this data expressed in terms of N? Surely C would be a better measure for comparison to the field experiments and to the results in figure 5?

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