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Biogeosciences Discussions

Interactive comment on "Synergistic effects of iron and temperature on Antarctic plankton assemblages" *by* J. M. Rose et al.

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This manuscript presents results on a set of experiments from the Ross Sea examining the response of the natural phytoplankton community to changes in iron and temperature. The results are interpreted in terms of previous experiments in the Antarctic and the possible effects on Antarctic phytoplankton under future climate change are discussed. The overall findings were that synergies between Fe and temperature exist and the response is not a simple linear combination of the two effects. I find the experimental work well designed and executed and the descriptions of this generally well described in the text. The paper is also well written and concise. This work has clear and obvious implications for future field work and modelling studies of biogeochemistry in the Antarctic and will be an important contribution to this field. I would

recommend acceptance of this manuscript after a few minor revisions were made as specified below.

General Comments

Temperature/Iron influences on growth rate/nutrient uptake: I think the authors do a good job of explaining and summarising their data but I would like to see a little more on the possible influence of temperature and/or iron on nutrient stoichiometry (see also below). I think given the data we have from lab studies and from field work in the Southern Ocean it should be possible to have one paragraph on how temperature and iron might specifically alter nutrient stoichiometries particularly with regard to Si to nutrient/metal ratios. This could help explain some of the synergies between iron and temperature limitation of growth rates.

Season and Community response: I would also like to some discussion of how the outcome may be influenced by the timing of the experiment with respect to the seasonal cycle of the phytoplankton in the Ross Sea. By this I mean if the experiment was performed in November or December how might the response have been different due to a pre spring bloom phytoplankton community, rather than a possibly post spring bloom community.

Main Effects: Throughout the manuscript the term 'main effects' is used but without explanation, as many readers may not be aware that this is a statistical term and not simply an adverbial description (e.g. the main event) it would be useful to clarify this in the text. Something simple such as, at the start of section 2.5 stating that: Main effects (the effect of an independent variable on a dependent variable averaging across the levels of any other independent variables) for iron and temperature. . .

Specific Comments

P5853, line 14. Freshwater inputs to the Ross Sea do appear to be changing (Jacobs et al., 2002) and this information should be provided here.

P5853, line 24. The work of Raiswell and others is of more relevance here with regard to bioavailable iron in icebergs and should be cited (Raiswell et al., 2008; Raiswell et al., 2006). Also can the authors comment in the manuscript specifically about the potential sources of iceberg melt (Gladstone et al., 2001), for instance the mega iceberg B-15 originated in the Ross Sea, or Aeolian dust (Revel-Rolland et al., 2006) to the Ross Sea.

P5857, line 19. What was the precision on the nutrient analysis?

P5858, line 2. What borate buffer? This is the first time it is mentioned, so the exact molar strength should be reported. Also with so much ammonia in the sample it would be a mixed borate-ammonia buffer.

P5858, line 9. What is the precision of this technique for replicate samples? Can this method replicate the consensus values for the SAFe intercalibration samples?

P5859, line 1. Biogenic sulfur seems an all encompassing term when here it only refers to DMS and DMSP and not to other species such as glutathione or cysteine which could be more significant biogenic S sources (Dupont et al., 2006). I would suggest the Biogenic sulfur term be exchanged to simply "DMS/DMSP".

P5861, line 12. Replace of with for.

P5867, line 6. Higher temperatures also lead to lower inorganic iron solubility (Liu and Millero, 1999, 2002) and there may also be other temperature dependent processes affecting iron bioavailability and this should be discussed within the present manuscript.

P5868, line 14. Change in community stoichiometry are often critically related to growth rate which in turn is related to Fe and temperature. Given the results here is it interesting to see if the changes in Si uptake observed in the high temperature treatment relative to the high iron treatment represents the fast accumulation of Si at elevated temperature but a still iron-limited growth rate leading to increased BSi:C ratios. Obviously this would require a temperature dependent Si uptake transporter in diatoms and

the authors are referred to 3 review papers that have summarised the knowledge on this (Martin-Jezequel et al., 2000; Thamatrakoln and Hildebrand, 2008; Thamatrakoln and Kustka, 2009). This topic could be further explored here in line with other relevant field work (Twining et al., 2004a; Twining et al., 2004b) and recent laboratory experiments (Ho et al., 2003; Quigg et al., 2003). Comparison with the Twining et al. work would seem appropriate as the current treatments are roughly related to the conditions in the SOFeX north and south patches.

P5868, line 20. Thus it could be suggested here that warming of the Ross Sea in the absence of iron supply could lead to increased silica drawdown.

P5870, line 9. Some results with rates of microzooplankton grazing are available for both EisenEx (Henjes et al., 2007a; Henjes et al., 2007b; Schultes et al., 2006) and EIFeX (Jansen et al., 2006; Kragefsky et al., 2009).

Figure 4. The Fv/Fm was relatively high at the start. Does this indicate that you were looking at a post spring bloom community as the initial conditions? Does this have any bearing on the phytoplankton community that was there and would the final communities have been different if the experiment had started in November? Some discussion on this in the text would be useful about the timing of the experiment.

Figure 4. The Fv/Fm of the high iron, high temperature treatment rises slightly faster than the high iron treatment but starts to drop by day 5. Is this an expected response at 4°C? During SOIREE for instance Fv/Fm did not rise as high but nor did it drop rapidly after attaining a maximal value (Boyd and Abraham, 2001). Do the authors have any explanation for this drop that could be explained in the manuscript?

Figure 6.Are there error bars for the iron results? If not thus should be reported in the figure legend. If yes then the above comment on the precision for the Fe work is also relevant here.

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