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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.

**Anonymous Referee #2**

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General comments.

This is a well written paper on a topical issue. A comprehensive set of biomass, community composition, nutrient and physiological variables were measured during a nine day bioassay experiment on a Ross Sea plankton assemblage. A factorial design examining the effects of Fe and temperature, alone and in combination, demonstrated slight individual and pronounced interactive effects of increasing Fe and temperature on phytoplankton biomass, the composition of the phytoplankton community and drawdown of inorganic N and P. The importance of the research lies in the demonstration of significant variability in the responses of the extant phytoplankton community to interactions amongst climatically sensitive variables.

Specific comments.

Pages 5862-5863. The authors need to be more precise in their discussion of the interaction between Fe and temperature on Fv/Fm and PE curves. For Fv/Fm and PE curves, Fe is clearly the dominant limiting factor, and given the limitations of bottle experiments I would be very hesitant to assert that there is any interaction that is of physiological significance. If a difference is not statistically significant, then the authors should not imply that the parameter may be greater in one treatment than another. Even if the differences are statistically significant, the authors need to assess their biological significance in the light of the divergence of community structure and nutrient draw-down amongst the treatments during the experiment.

Page 5969, lines 22-23. I am not convinced by this statement. To support this statement, the authors need to present the statistical test that was used to show that there was a significant difference between the Fe and high-temperature/Fe treatments.

Page 5871. In the last paragraph of the discussion, the authors should acknowledge that the difference in the time scales for bioassays (days) and climate change (decades to centuries) limit the extent to which bioassays can be predictive. The bioassays do is tell us something about limiting factors in the extant community, but there power is potentially limited by bottle effects, which increase with time during the experiment. Bioassays do not tell us what will happen when the extant community changes.

Table 2. Some of the values for the light-saturation parameter, EK = Pmax/alpha) are very low. It is essential that the authors show the entire P vs E curve and not just the values of Pmax and alpha. In addition, it appears that there is one PE curve at time zero, and one curve for each of the treatments at times 4 and 7 days. If so, there is no replication, a point that deserves comment: it appears that the errors on the parameter values are based on fitting a curve to a single experiment, not the range from replicated curves.

Table 3. For completeness, the authors should present the results of the ANOVA along with the mean and standard deviation in this Table.



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Figure 6 (Page 5888). Is there any evidence for production of dissolved organic N or P during the experiment? It looks as though the increase in particulate P is substantially less than the drawdown of phosphate in the high-temperature+Fe treatment.

Technical corrections.

Page 5856, lines 4-6. As there is no photoinhibition term in the Webb equation (previous page, Eq. 1), the authors should revise lines 4-6.

Page 5857, line 14. "aut-" should be "auto-"

Page 5867, line 22. Rather than Geider & Osborne 1992, I think that the authors want to cite Raven, J.A. and R.J. Geider (1988) Temperature and algal growth. New Phytologist, 110, 441-461.

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