

Interactive
Comment

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

J. M. Rose et al.

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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 draw-down 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.

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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.

We agree with the reviewer that there is unlikely to be a true physiological interaction for Fv/Fm and that this value is definitely determined by Fe availability. In the manuscript, we tried to make the point that we believe that the observed response (early peak in Fv/Fm, decline after mid-experiment) occurred because the increased temperature in the high iron, high temperature treatment increased the growth rates of the phytoplankton assemblage. This resulted in more rapid depletion of available nitrate and/or iron in these bottles than in the high iron treatment at lower temperatures, and thus an earlier decline in Fv/Fm. We added some text to the discussion section making this point more clear (p. 16).

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.

We unfortunately had to mix replicates to obtain enough volume to generate PE curves so were not able to test the significance of observed differences between treatments. Since we do not have significance tests to support our observations for these parameters, we have removed the PE curve data from the manuscript.

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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 tell us something about limiting factors in the extant community, but their 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.

We agree completely with the reviewer regarding the limitations of bioassays for making predictions about climate change, and have added an acknowledgment of this limitation in the last paragraph of the discussion.

Table 2. Some of the values for the light-saturation parameter, $E_K = P_{\max}/\alpha$ are very low. It is essential that the authors show the entire P vs E curve and not just the values of P_{\max} and α . 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.

As we described above, we did not have enough volume to generate a PE curve for each replicate. The replicates were mixed and PE curves generated from the mixture. We removed the PE data from the manuscript since we were unable to test the significance of the trends we observed.

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

We expanded the table to include the results of the modified two-way ANOVA.

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.

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Unfortunately, we did not measure DON or DOP, so we are unable to definitively answer this question. However, we did address this comment in the manuscript by adding the following text to the nutrients section of the Results: “There was a discrepancy between the amount of phosphate drawdown and particulate organic phosphorus production in all treatments. This result may have been due to production of dissolved organic phosphorus, which was not measured in the experiment.” (p. 13)

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.

As described above, we removed the PE data from the manuscript since we were unable to test the significance of the trends we observed.

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

This formatting was done by the publishers and we will request that it be fixed.

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.

We changed the citation.

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