

Interactive comment on “Individual and interactive effects of warming and CO₂ on *Pseudo-nitzschia subcurvata* and *Phaeocystis antarctica*, two dominant phytoplankton from the Ross Sea, Antarctica” by Zhi Zhu et al.

Anonymous Referee #1

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This paper reports the results of experiments designed to elucidate the impacts of temperature and CO₂ increases on the growth of a diatom and *Phaeocystis*, and their competitive interactions. While the paper is of some interest, I am puzzled by many aspects of the study. Furthermore, we recognize the extreme limitations of these types of experiments, even if done within the bounds of reality (which this one was not). When attempting to look at only two factors alone, multiple other factors are changing simultaneously in situ, and any one of those factors might have an impact that is far greater than those tested. The simplest example is iron. Increasing temperature might alter the rate of regeneration of iron to a great degree, as well as supplies from below

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(and potentially other pathways, such as scavenging). Given that the authors have long published papers on the role of iron, it seems odd that this paper focuses solely on temperature and CO₂.

Regardless of my many criticisms, I am ambivalent about the publication of the paper. I feel that the methods used are robust, but in truth the paper will have hardly any impact on our understanding of climate change and the oceanic response to change. While there are no “fatal flaws” that I see, it is not a citation classic either. Hence I vacillate between suggesting “rejection” and “publish”, and will simply leave that decision to the editor.

Specific comments 1. I realize this is a judgement matter, but I feel that a number of recent papers and reviews were not cited. Furthermore, some that were cited were inappropriately used – e.g., line 37 – Arrigo et al. deals with the Ross Sea, not the Southern Ocean, as does Smith et al.; Line 39: Sarmiento argued that temperatures will not change significantly in the next 100 years, but stratification will (although other papers argue that a small increase will occur; line 52: Caron said nothing about zooplankton and only looked at microzooplankton; Line 32: Gille looked at deep water changes in temperature, so this statement is misleading when talking about phytoplankton 2. Choice of temperatures. Nearly all models of the Ross Sea suggest that a temperature increase in the next century will be on the order of 2C. If the study is designed to mimic future changes, why were temperatures from 0 to 10C used? This feature alone makes the results far less interesting to the oceanographic community as a whole. In particular a competition at 6C doesn't really tell us much. Why not run it at 2C? 3. Was the *Phaeocystis* culture composed of colonies or solitary cells? 4. Growth rates. Growth rates increased with temperature; certainly no surprise here. 5. Fig. 3. C:chl ratios of 150 are NEVER seen in *Phaeocystis* dominated assemblages in the Ross Sea. Why are these so anomalous? 6. Fig. 5. Were experiments really run with zero CO₂? Or was the origin simply assumed? If the latter (which of course makes sense and is implied in the figure caption), that single value alters the entire curve.

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It would have been good to run the experiments with ca. 25 or 50 ppm, as it is not impossible that at least colonial *Phaeocystis* might utilize mucilage oxidation as a CO₂ source. 7. Line 318. Light intensity is an incorrect (and dated) term. Use irradiance or photon flux density. Intensity is measured in lumens! Additionally, the discussion of temperature and irradiance as “co-variables is a bit misleading. At temperatures encountered in the Ross Sea, stratification is overwhelmingly dominated by salinity, not temperature. Usually temperate variations with depth can only arise after stratification due to salinity is initiated (such as near the ice edge). Furthermore, during spring and parts of summer, often warmer water occurs at depth, so (line 324) mixing does not necessarily lower temperature, and can even increase it. 8. Line 338. Again, if the Si:C ratio increases in a warmer ocean, flux does not necessarily increase, because Si remineralization rates are temperature dependent. And because the temperature changes in the future ocean will be initially limited to the upper surface layer (at least relevant to this study), the scale of remineralization likely will be only modestly impacted, if at all, especially in the regions where Si accumulations in sediments is greatest. 9. Line 343. This discussion is too restricted. First: will changes in the next century increase or decrease *Phaeocystis*? A recent JGR paper suggests that it will increase *Phaeocystis* in the Ross Sea on an annual basis due to the earlier “ice off”, which coupled with the absolute irradiance levels and solar angles, generate a longer “low irradiance” environment. While I agree that N and C export will change with an assemblage change, I think it is not clear how future Ross Sea phytoplankton will change. Certainly in previous work (Tortell et al.) different diatoms became dominant under different conditions, so extrapolating ONE diatom to all diatoms is very speculative.

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