Review of 'Individual and interactive effects of warming and CO_2 on *Pseudo-nitzschia subcurvata* and *Phaeocystis antarctica*, two dominant phytoplankton from the Ross Sea, Antarctica by Zhu et al.

This paper makes an important contribution to our understanding of Southern Ocean phytoplankton response to predicted ocean change. The manuscript is well written and quite readable. I recommend publication with a some modifications and changes.

The methods used in this study are mostly robust and appropriate and the experimental design was well thought out. I am pleased to see that the cultures were established from the same location only 12 months before and that local sea water was used for culturing.

It was also good to see that the authors selected temperature and CO2 gradients rather than merely a 'low' and a 'high' treatment. Too many OA studies just look at responses at 400 and ~1000 µatm. While this might provide a glimpse of a possible future ocean, it provides little reliable physiological information. The six CO2 and six temperature settings provide a good range and not only sensibly bracket likely future senarios, they also enable physiological responses to be determined. The development and use of thermal and CO2 functional response curves to interpret impacts makes a particularly useful contribution. These authors are leaders in the application of this method.

The reasons for including an elemental analysis are not explained. There is no mention of them in the abstract or the introduction. A better initial justification is required.

The authors discuss relevant literature on the impacts of temperature and CO2 but don't adequately include comments on other co-stressors, such as iron. While I don't believe it was necessary to include additional co-stressors in the experimental design, they should at least be acknowledged.

I do have some real concerns about the competition study. Competition only occurs when the two species are resource limited. The species with the fastest growth rate is not always the most competitive; it is the species that requires the least amount of resource to reach its maximum growth rate (Titman 1976). The competition experiments described here were too short for competition to have occurred and we are merely seeing the results of the highest growth rate at the given temperature. The outcomes could be predicted solely from the half saturation coefficients given in Table 4.
It is unclear why the authors chose 6° for one of their competition experiments. Use of this temperature rather than a more realistic future temperature should be justified.
I think the competition experiment should be removed but the discussion of completion included nut limited to half saturation constants.

I found the remainder of the manuscript to be well written, well justified and well argued.