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Interactive comment on "Ocean acidification of a coastal Antarctic marine microbial community reveals a critical threshold for CO_2 tolerance in phytoplankton productivity" *by* Stacy Deppeler et al.

Anonymous Referee #2

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This manuscript uses 6 minicosms to investigate the effect of CO2 on the Antarctica microbial plankton (phytoplankton and bacteria) community. The authors' conclude there is a critical threshold for CO2 and above this threshold of 953 -1140 uatm, phytoplankton productivity diminishes, with no observable effect on bacterial production. The great advantage of minicosms is their capability to test a community response, however, they are large undertakings, requiring significant investment of time, resources and people and thus results are often split into multiple publications, as is the case here. Unfortunately, without the data in the other manuscripts, we get only a partial

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story and it is difficult to give an accurate review.

Overall the authors' did a good job on the CO2 manipulations and the manuscript is well written. While there have already been a number of minicosm experiments with CO2 manipulations, most polar studies have focused on the Arctic and it is interesting to see an Antarctic focus on this scale. As a general comment, minicosm experiments often produce conflicting results and there should be more effort discussing possible mechanisms that underlie the variable results between experiments. For example, the authors' mention how their results differ from other studies but do not provide possible explanations of why e.g. differing setup, differing communities etc.

My main concern for this manuscript is that I am not convinced the results support their conclusion of a CO2 threshold between 953 and 1140 uatm. Only GPP14C showed that treatments over 953uatm CO2 had lower productivity. In other figures, either only 1641 uatm appeared different, no significant difference was found, or a mid range CO2 treatment was an outlier. The only statistical analysis they used was ANOVA, which identifies statistically different treatments instead of looking for trends related to CO2 concentrations. Because of the type of statistical test chosen, only a threshold rather than a CO2 trend was tested.

I understand that other results are being published in other papers but considering there are no replicates, the authors need to do a better job reassuring the readers that the differences between minicosms are directly a response to CO2 and not due to other changes e.g. community shifts. The methods section details how community composition was measured but no results were presented and instead will be presented in Hancock et al.

The authors' focuses their story around CO2 with little mention of the effects of pH. I think this should be expanded upon.

There are a few issues with the 14C and O2 measurements used for GPP. There have been a number of studies demonstrating that incubating for 1h for 14C does not cap-

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ture GPP, and that O2 respiration in the dark does not always equal respiration in the light. Both would result in errors in GPP. While this data can be used (as it is hard to measure true GPP), these caveats should be acknowledged in the manuscript. The units used for GPP based on 14C and GPP based on O2 are different, making them difficult to compare. Comparison would provide an idea on whether there is a realistic photosynthetic quotient, and this would also go a long way as to helping interpret NPQ and other non-carbon assimilatory processes.

The authors' state that CO2 had no effect on bacterial production. However, looking at figure 7, there appears to be higher bacterial abundance between days 8 – 14 in the high CO2 treatments, which are not observed in bacterial productivity, indicating that bacterial production per cell is lower at high CO2? Surely, this is a CO2 response?

The C:N data for POM is interesting but it is hard to discount carbon overconsumption without also looking at DOM. This would also be useful in interpreting the GPP 14C results. Respiration rates would also be useful. I realize these measurements can't be taken but the authors' should discuss these factors.

In the methods section the authors' should justify the length of acclimation, why it was done under low light and why a blue filter was used.

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