

## ***Interactive comment on “Ocean acidification changes the structure of an Antarctic coastal protistan community” by Alyce M. Hancock et al.***

**Anonymous Referee #2**

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This manuscript analyses the effects of elevated CO<sub>2</sub> on the protistan community in East Antarctica. Firstly it is great to read another biological ocean acidification study being conducted in the Antarctic as well as being a community response study. Both these areas of research are not common with many questions left unanswered. It is, therefore, particularly interesting that this study by Hancock et al. addresses community level responses in the Antarctic where biota are considered to be the most vulnerable to OA due to the rising solubility of CO<sub>2</sub> in cold-waters. Overall this manuscript is well written, contains plentiful relevant data and attempts to close the gaps in our knowledge of important questions outstanding in the OA field. Before consideration for publication, there are a few points that need addressing. In particular, more explanation is needed about the carbonate chemistry analysis. DIC and pHT are the

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CO<sub>2</sub> parameters directly measured so why is fugacity of CO<sub>2</sub> used as the CO<sub>2</sub> parameter altered? pH (on any scale) or DIC are the usual parameters directly altered in OA studies and therefore using fCO<sub>2</sub> limits the continuity between this study and others. I suggest using either the measured pH or DIC measurements instead. A table of differences between the carbonate chemistry of each treatment is also necessary rather than quoting Depper et al. (submitted). Many more details are generally needed. For example, how often was DIC measured? What was the variability between measurements in DIC and pH? How often was the probe calibrated? A common problem with OA research is replication. I query why this experiment was not replicated given the short duration? In addition OA research is also moving towards long-term studies spanning many months to years. I also query why such a short duration was chosen for this experiment? Throughout the manuscript there are several references to look at Depper et al. (submitted) for information not detailed in this manuscript. I query whether this manuscript is a “stand-alone” story. Technical corrections: Page 1 line 1: remove ‘of’ Page 1 line 8: should be a semicolon instead of a colon. Page 1 line 8/9: is it a case of large cells decrease in abundance in high fCO<sub>2</sub>? That would be a better way to report these results as high fCO<sub>2</sub> is the environmental stress concerned. Page 1 line 12/13: This statement needs clarification as it implies this research is not original. Page 2 line 4-6: generally OA studies on organisms higher up the foodweb in the Antarctic are few which adds importance to your study and should be mentioned with some key Antarctic papers referenced. Page 2 line 23: insert a comma. . . ‘With increased CO<sub>2</sub>, Tortell et al. . .’ Page 4 line 5: remove ‘the’ before adding Page 6 line 22: remove ‘a’ should be ‘likely due to’ Page 7 line 3: change to ‘had increased to’ Results section: removed ‘show’ and other variations using this word as it is unnecessary. It reads better to just say ‘increased’ instead of ‘showed an increase’. Page 10 line 11: typo ‘a’ should be ‘at’ Page 10 line 17-19: why might there be differences between the results in this study and that of Feng et al. (2009)? Page 10 line 22: it is difficult to compare the results in this study to others quoted in this statement when different CO<sub>2</sub> parameters were altered. Page 10 line 23: ‘response’ instead of ‘responses’

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