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Interactive comment on "No observed effect of ocean acidification on nitrogen biogeochemistry in a summer Baltic Sea plankton community" *by* A. J. Paul et al.

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Response to Reviewer #1 (D. Campbell)

We thank D. Campbell for his useful and constructive comments on this manuscript which helped to clarify a number of points and tune the figures in the manuscript. Our responses to reviewer comments, including modifications to the manuscript, are detailed in the following:

REVIEWER COMMENT 1 by D. Campbell: The mesocosms were closed at the bottom. Would this alter their response by cutting off upwelling supplies of NH4+?

Author Response: As the reviewer correctly points out, once the mesocosm bags were pulled up above the water surface during closure on t-5, the mesocosms were then closed water masses with no exchange with the outside environment, only with the atmosphere. Therefore, there was no supply of nutrients through upwelling inside the mesocosms. Sporadic, wind-driven upwelling events are known to stimulate blooms of N2-fixing filamentous cyanobacteria in the Baltic Sea in summer (Nausch et al., 2009; Wasmund et al., 2012) by bringing up phosphate rich water (Nausch et al., 2007). We observed this phenomenon outside the mesocosms during Phase II (t17 to t30). Hence, it is likely that the response of the plankton community would have been altered by the addition of nutrients (either ammonium/NH4+ or phosphate). However, here we were primarily interested in the response of the plankton assemblage and N cycle to CO2 in a low nutrient, closed system.

REVIEWER COMMENT 2 by D. Campbell: Abstract: "(average treatment fCO2: 365–1231 μatm)" This statement needs to be clarified; I think: (average treatments fCO2: 365, 1232 μatm). Line 12 in the Materials & Methods has a different range of fCO2.

Author Response: The two reported ranges of CO2 are different as the one in the Abstract refers to the average fCO2 over the study period, whereas in the Materials and Methods, the fCO2 range refers to the initial treatments present on t4 after fCO2 was manipulated (p. 17511, line 11-12: 'Initial fCO2 ranged from $\sim$ 240 μatm in the two ambient control mesocosms to up to 1650 μatm (Fig. 1a).'). These are not the same range because fCO2 was not constant during the study period due to outgassing of CO2 from the treatment mesocosms (Fig. 1a). A statement, as included in the Fig. 1 caption, will also be added to the text in the revised manuscript to clarify this important distinction to read (p. 17517, line 10): 'These phases are also used to assist with data interpretation in this manuscript. Average fCO2 was calculated for each mesocosm between t1 and t43.'

REVIEWER COMMENT 3 by D. Campbell: line 22: nor, not or. Nor follows a negative.

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Author Response: We thank the reviewer for pointing this out and this will be changed for p. 17508, line 22 in the revised manuscript.

REVIEWER COMMENT 4 by D. Campbell: Materials & Methods: line 26, "KOSMOS,"??? Undefined acronym/abbreviation?

Author Response: KOSMOS stands for 'Kiel Off-Shore Mesocosms for future Ocean Simulations'. This definition will be added to p. 17510, line 26 in the revised manuscript.

REVIEWER COMMENT 5 by D. Campbell: Table 1: I do not understand why this table is organized into 10 columns. It looks to me like it should be 5 columns, twice as many rows.

Author Response: The format of Table 1 will be changed accordingly in the revised manuscript.

REVIEWER COMMENT 6 by D. Campbell: Figure 1. Would it be worth showing DIC? Is there any change?

Author Response: Changes in DIC are the primary driver in changes in fCO2, hence mirror changes in fCO2 over the study period. Details on the carbonate chemistry are presented in an accompanying paper (Paul et al., 2015).

REVIEWER COMMENT 7 by D. Campbell: Figure 1: insert legends are very small; I cannot read them at printed page size. I am getting weird colour changes (artefacts) in the roman numerals for the experiment stage labelling I, II, III. This is probably a .pdf generation issue, but it is distracting.

Author Response: The inserted legend will be removed and added to a separate panel in the figure to improve readability in the revised manuscript. Regarding the colour changes for the roman numerals, this artefact appears to disappear if the figures are viewed highly zoomed and in the printed version. The figures will be revised with changes to the legends and we will try to resolve this issue for computer screens.

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REVIEWER COMMENT 8 by D. Campbell: Again, the listed fCO2 levels differ from the materials and methods, and from the abstract.

Author Response: Please see Author response to Comment 2 by D. Campbell.

REVIEWER COMMENT 9 by D. Campbell: Figure 3: It would be good to have the colour/symbol legend for each figure, to stand on its own without reference back to Figure 1.

Author Response: The colour/symbol legend will be added to all figures in the revised manuscript.

REVIEWER COMMENT 10 by D. Campbell: Figure 6: Data points with uncontaminated gas are below detection, all detected rates are from the contaminated period. Should this data be presented? I am reading page 17519 but am not clear on the origin of the data in Figure 6.

Author Response: The detection limit was determined to be 0.15 nmol N L-1 day-1 and we detected rates above the detection limit in some mesocosms from t3 and in all mesocosms from t11 until t21 (apart from M1 on t15). For clarity we will remove the data points below the detection limit from Figure 5 as done in Figure 6. The data in Figure 6 is to indicate the sudden and large increase in apparent rates which is an artefact and the result of contaminated gas used for labelling. This figure will be removed from the revised manuscript as suggested by Reviewer #2.

REVIEWER COMMENT 11 by D. Campbell: Results: P.17521 The extrapolations in the absence of actual N2 fixation rates seem reasonable, but are based upon multiple assumptions on N2 rates, N:P ratios and N exudation rates.

Author Response: As correctly highlighted, there are a number of assumptions included in the calculation of N inputs through N2-fixation which are acknowledged in the methods section (p. 17520).

REVIEWER COMMENT 12 by D. Campbell: P.17521, the N contamination issue is

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serious given the patchy cyanobacterial data.

Author Response: We agree with this statement by D. Campbell and have ensured that this is transparently presented in the manuscript.

REVIEWER COMMENT13 by D. Campbell: Discussion: "In fact, nitrate concentrations continually increased throughout the experiment at an average 10 net rate of 1 nmol N L-1 day-1 (Fig. 1c) …. " Summary: "Thus N uptake rates were well balanced with supply or any net differences were too small to be detected in N pool sizes across the range of simulated ocean acidification scenarios" These statements appear contradictory. I think the summary needs to be clarified that fCO2 did not provoke changes in N pool sizes?

Author Response: We thank D. Campbell for highlighting an apparent inconsistency between these two statements. 'CO2-related ' will be added to the Summary in the revised manuscript to clarify this point and will then read: "Thus N uptake rates were well balanced with supply or any net CO2-related differences were too small to be detected in N pool sizes across the range of simulated ocean acidification scenarios."

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Wasmund, N., Nausch, G., and Voss, M.: Upwelling events may cause cyanobacteria blooms in the Baltic Sea, Journal of Marine Systems, 90 (1), 67 – 76, doi:10.1016/j.jmarsys.2011.09.001, 2012.

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