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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This manuscript presents N2 fixation and related data from a Baltic Sea mesocosm CO2 enrichment study which has been previously covered in the companion paper Paul et al. 2015, also in BioGeoSciences. The study is well designed but suffered from contamination of a key stock of labelled N2 late in the study, which overlapped with the period when diazotrophic cyanobacteria abundances were highest (albeit at a low total level). Thus measured rates of N2 fixation were mostly below detection, until the period when contaminated N2 was used. The authors present reasonable constraints up N2 fixation, based upon P uptake rates, assumed N:P ratios and average N exudation and
fixation rates, during the problematic period, but the problem remains that a key metric was not directly determined.

Paul et al. 2015 found higher phytoplankton biomass and lower [P] under elevated fCO2 in these mesocosms. But the present study, subject to the limitations noted, does not support increased N2 fixation under elevated fCO2, contrary to some lab studies (Levitan et al.; the authors cite a list of other studies on this issue) on effects of elevated fCO2 upon N2 fixation in cyanobacteria.

The diazotrophic cyanobacterial community was low in the mesocosms compared to some other Baltic sea studies, so any putative fCO2 influence would be acting upon a small nitrogen fixation capacity.

The mesocosms were closed at the bottom. Would this alter their response by cutting off upwelling supplies of NH4+?

This paper presents additional evidence that in natural communities elevated fCO2 does not drive an increase in N2 fixation, even though culture experiments suggest it should.

Given the importance of the topic, and the difficulty of mesocosm studies, and the reasonable discussion, I support publication of the manuscript despite the noted limitations.

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

line 22: nor, not or. Nor follows a negative.

Materials & Methods: line 26, "KOSMOS,"??? Undefined acronym/abbreviation?

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.
Figure 1. Would it be worth showing DIC? Is there any change?

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.

Again, the listed fCO2 levels differ from the materials and methods, and from the abstract.

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.

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.

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.

P.17521, the N contamination issue is serious given the patchy cyanobacterial data.

Discussion: "In fact, nitrate concentrations continually increased throughout the experiment at an average 10 net rate of 1 nmolNLÁÁÁÁ1 dayÁÁÁÁ1 (Fig. 1c)"

Summary: "Thus N uptake rates were well balanced with supply or any net dierences were too small to be detected in N 10 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?

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