

## ***Interactive comment on “Response of <i>Nodularia spumigena</i> to <i>p</i>CO<sub>2</sub> – Part 2: Exudation and extracellular enzyme activities” by S. Endres et al.***

**Anonymous Referee #1**

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The paper of Endres et al reports data on the physiological response of the cyanobacterium *Nodularia spumigena* to different levels of CO<sub>2</sub>-induced seawater acidification. Data are mostly novel and interesting. The experiment is correctly planned, described and carried out. The manuscript is straightforward and clear. For these reasons I think that the manuscript deserves publication in *Biogeosciences*.

There are, however, a couple of points of relatively minor nature that the authors should take into account in a revised version of the manuscript: 1) The article often refers to a twin article by Wannicke et al., especially about the experimental set-up. The review process of Wannicke et al. raised some points I agree with about the early stages of

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the experiments, in particular about the lack of achievements of desired CO<sub>2</sub> concentrations. For this reason I also suggest not to refer to past, present and future CO<sub>2</sub> levels (e.g. page 5110, lines 14-15), but the authors should name their experimental conditions with the actual CO<sub>2</sub> level (or mean among replicates), or (as often written in the ms) low, medium, high CO<sub>2</sub>. Furthermore, since this paper is highly connected to Wannicke et al. which I guess is in the revision stage, I recommend the authors to follow any change (relatively to experimental description, carbonate chemistry and other data, when possible) applied to the twin manuscript.

2) Heterotrophic bacteria: in general I understand that heterotrophic bacteria did not ‘grow’. In lines 4-6 of page 5121 it is stated that cell number remained around  $5 \times 10^5$  cells l<sup>-1</sup>. The authors need to be more precise about this, adding an error value or describing more in details this lack of growth for each treatment. Although it is stated that variability among replicated was high (p5123, l28-29) these data are important. I am convinced that degradation processes are mainly carried out by *Nodularia*, but  $5 \times 10^5$  bacteria per liter in a batch culture can in theory be responsible alone for the V<sub>max</sub> values detected in this experiment. The fact that cell numbers did not increase along with time, suggest that they were not particularly active (as discussed by the authors), but I would not stress the main role of *Nodularia* too much, since it is not proven. This for example in p5123, l 11-13; p 5127 l 14-16; p 5129 l 27-29 (in this case the author cannot state that the bacterial community was not active because (i) *Nodularia* is a bacterium and (ii) they have no data about heterotrophic bacterial activity).

3) As for the previous comment I think that specific APA values related to chlorophyll can be misleading (APA attribution to *Nodularia* is a very plausible speculation, not an evidence). Since the authors do not deeply discuss sAPA data, and they are not among the major findings of the experiment I suggest to remove them.

4) p 5127 l 6: the four tested enzymes are not the ‘major’ enzymes, although they are among the major enzymes. Lipolytic and chitinolytic activities in the field, for example,

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are very often faster than glucosidases.

5) Unger et al (p 5128, l29) is not reported in the reference list.

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