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Interactive comment on "The Arctic picoeukaryote *Micromonas* pusilla benefits synergistically from warming and ocean acidification" *by* Clara J. M. Hoppe et al.

Anonymous Referee #1

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The authors assessed the combined impact of ocean acidification (OA) and warming on the growth, photochemical characteristics, and cellular composition of the prasinophyte Micromonas pusilla. M. Pusilla is a common species in Arctic waters, especially in the vertically stratified and nutrient-poor waters found in summer and early fall. Previous observations indicate that the current increase in stratification in the Arctic favors M. Pusilla. Results from this study suggest that, in addition to this trend, OA and warming could also contribute to increase the importance of this key Arctic species in the future.

This is a nice, well-crafted physiological laboratory study. The experimental design is straightforward but appropriate. The cells were well acclimated to their new pH

and temperature conditions (7 generations), which already makes this data set different from the ones generated during mesocosm studies were the assemblages are exposed to an abrupt change in conditions. Measurement of photochemical and overall growth parameters nicely complement each other, showing how the light and dark metabolisms to OA and warming shape the final response. The results are convincing and well generally discussed.

I am however concerned by the way the authors extrapolate their results to the natural conditions and suggest they be more cautious in this section of the discussion. In the Arctic surface mixed layer, cells do not necessarily achieve their maximum growth rate, which is in sharp contrast to the experimental setup presented in this paper where the cells are nutrient replete and growing at maximum speed. In the surface mixed layer, the biomass and gross growth rate of cells are mostly controlled by the availability of regenerated nutrients such as ammonium. Accordingly, in their introduction, the authors characterise this period of the year as 'nutrient-limited' (Page 2, line 46). Hence, the full and combined positive impacts of OA and warming, as observed during their experiments, could only be achieved in the field if nitrogen supply (regeneration in this case) was not limiting, i.e. if there was a proportional increase between the supply and the demand in nitrogen by M. pusilla. The question is : how would OA and warming affect nitrogen and phosphate regeneration in the upper mixed layer of the water column in the Arctic in summer/fall? For the same reason, I think that the authors should be prudent when comparing M. pusilla and the diatom T.hyalana. To assess which of M. pusilla or T. hyalana will dominate in a warmer and more acidic Arctic, it is important to consider the different life strategies of these two species and ways they utilise the limiting resource. While T. hyalana is a r-strategist rapidly using the resource when available (nitrate), M. pusilla is more of a K-strategist acclimated to a less abundant but regular resource supply (ammonium). The modes of supply of nitrate and ammonium are different as well: mostly upwelling/mixing for nitrate, in-situ regeneration for ammonium. I am sure the authors are well aware of these basic principles. They should consider them in the extrapolation of their results to the field. This will nuance their prediction and open the door to future interesting research.

Otherwise, the paper is well written, the data are clearly presented and statistical tests seem appropriate. The non-linearity of the response is interesting and adds a level of complexity and difficulty in our attempt to predict how global change will affect marine systems.

P2, 46: . . .nutrient limitation. . .As mentioned above, the authors should take into account the fact that nutrient supply is low in these stratified waters and that their experiment was conducted with nutrient-replete cells. Actually, it could be more appropriate to write . . . results in low nutrient concentration. . .instead to infer 'nutrient limitation'.

P3, 62: . . .in relative abundance. . .This means that M. pusilla could actually be less abundant numerically. This is probably not what the authors mean.

P3, 63: see Hussherr et al. 2017 for an example of the combined impact of OA and light on Arctic pico-phytoplankton.

P8, 229: . . .Under high temperature, growth was higher at 1000 than at 380. . .Since there is only two points; we don't know if growth 'increases' between these two points. The rest of the observations show that the response is not linear. It could well be the same between these two observations.

P9, 252: The authors should add a panel with the changes in N quotas (and N:Chl a ratios) in figure 2. These data are interesting by their own account, and it is difficult to interpret the changes in C:N ratios not knowing how N quotas vary.

P10, 279: Two 'investigate' in the same sentence.

P11, 300: . . .in the summer and autumn when temperature up to 6ïĆřC or more can be reached (REF).

P12, 333: . . .Overall, OA had. . . (delete also).

P12, 333: . . .a significantly positive effect on growth. . .This statement is an oversimpli-

fication of the actual results. The positive effect of OA is only clearly observed at 1000 PCO2 and at 6ïĆřC. This is not a negative comment. I believe that the most important contribution of this study is to highlight the non-linearity in the response and that this should not be overlooked.

P14, 399: . . .may experience growth stimulation under OA. . .Yes, but only if nutrient supply is sufficient to fulfill the nutrient requirements of exponentially growing M. pusilla. What is the main source of nitrogen in the upper part of the water column in the Arctic in summer and fall? Mostly regenerated ammonium. So, the question is if ammonium regeneration will also increase with OA. This is an interesting question. This is somewhat addressed later in the paragraph, but without mentioning the types (nitrate versus ammonium/urea) and sources (mixing or in-situ regeneration) of nitrogen.

P14, 407: The comparison with the diatom T. hyaline is interesting but should also take into account the types and sources of the limiting nutrient. Are the authors suggesting that M. pusilla would replace T. hyaline as the main blooming species following upwelling/mixing events?

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