

## ***Interactive comment on “Effects of elevated CO<sub>2</sub> and temperature on phytoplankton community biomass, species composition and photosynthesis during an autumn bloom in the Western English Channel” by Matthew Keys et al.***

### **Anonymous Referee #2**

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The manuscript 'Effects of elevated CO<sub>2</sub> and temperature on phytoplankton community biomass, species composition and photosynthesis during an autumn bloom in the Western English Channel' by Keys et al. presents much needed data on the combined effects of two stressors of ocean change on the base of the marine food-web. Furthermore, a twenty year long monitoring record of phytoplankton community composition at the experimental site is shown. However, in its present form, I do not agree with the way the experimental data was analyzed and hence with certain conclusions.

Major comments and suggestions:

C1

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1) The time-series data on the natural variability of phytoplankton community composition and biomass does not complement the experimental data set and is actually disconnected. It is not clear how both data sets would complement each other which is also reflected in the fact that the time-series data is not mentioned in the abstract. It hence appears to be an unnecessary add-on.

2) Most critically, however, I consider the choice of the authors to restrict their analysis of the experiments to the last data point at the end of incubations on day 36 as being problematic. This appears to be arbitrary as many of the main conclusions would be different for the sampling days before, and most likely for the ones to come, if the experiments would have been run for a longer period of time. A different approach is needed.

3) I found it interesting that phytoplankton biomass in the combined high CO<sub>2</sub> and temperature treatment, and the control did decrease from day 20 and 25 onwards (Fig. 3B). Why is this not reflected in Chl a development and what is the cause? In the semi-continuous culturing set-up of the experiments with a daily dilution of about 10% of the incubation volume with fresh media containing 8 and 0.5 L<sup>-1</sup> nitrate and phosphate, respectively, a decline in phytoplankton biomass suggests that net growth has slowed down and is lower than the dilution rate. The PON data, however, suggests that there should be ample amounts of dissolved inorganic nutrients left for phytoplankton growth, thus it appears that there are indirect effects at work which should be discussed.

4) The photosynthesis versus irradiation curves for the four treatments are based on the assumption that the electron requirement for carbon uptake is independent of seawater CO<sub>2</sub> concentration, temperature and species composition. Since this is most likely not the case, any conclusions drawn (if any) would need to be discussed with much more caution.

Additional comments and suggestions:

1) P2, L36: I assume you mean 'concentrations of CO<sub>2</sub>', not 'uptake of atmospheric

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CO<sub>2</sub>'.

- 2) P2 L63-66: The grammar in the last sentence of this page seems wrong.
- 3) P3, L70: Citing only Engel et al. (2008) and Moutaka-Gouni et al. (2016) here is very selective.
- 4) P4, L127: It should read 'was' not 'were'.
- 5) P6, L170: Strictly speaking, the calculation of carbonate system parameters from DIC and TA require to account for the contribution of silicate and phosphate to the latter. Have dissolved inorganic nutrients been measured in the incubations?
- 6) P6, L187: How was size determined for the particles measured by flow cytometry, e.g. forward scatter calibrated with fluorescent beads of known size?
- 7) P14, L446: Most phytoplankton also uses CO<sub>2</sub> as an inorganic carbon source, not only HCO<sub>3</sub><sup>-</sup>. Furthermore, 'more efficient' use of CO<sub>2</sub> in comparison to what? Finally, I did not understand the rationale behind the notion that *Phaeocystis* would have an advantage at higher CO<sub>2</sub> levels.
- 8) P14, L458: I agree, but what could be an explanation for this finding (see also comment above)?
- 9) P14, L467: The four references on CO<sub>2</sub> effects on phytoplankton community biomass appear to be a very selective choice. Furthermore, this paragraph lacks any conclusions.
- 10) P16, L501: The temperature for maximum photosynthetic rates should be species specific.
- 11) P16, L517: I would assume that almost any autotrophic organisms growing at pH levels above 9 would slow down in growth because of inorganic carbon limitation, not only dinoflagellates.

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12) P18, L578-609: This discussion on *Prorocentrum* is unconnected to the experimental data and I do not see any added benefit or conclusions to be drawn.

13) P19, L618: It is not clear to me what the authors mean with the 'present upper limit of the pCO<sub>2</sub> threshold increase'.

14) P20, L648: Why are there potential positive feedbacks?

15) P20, L651: Why is there a potential for negative impacts on ecosystem functioning?

16) P20, L556: Why do 'little response and no effects' suggest 'negative feedbacks'?

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