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Interactive comment on "A niche comparison of *Emiliania huxleyi* and *Gephyrocapsa oceanica* and potential effects of climate change" *by* Natasha A. Gafar and Kai G. Schulz

Anonymous Referee #2

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General comments

In this paper, the authors present laboratory experiments on E. huxleyi that assess the simultaneous impacts of changing CO2, light and temperature. These are compared to previously published data on G. oceanica. This comparison is used to compare niches between E. huxleyi and G. oceanica under present day and future conditions. Further the authors use a CaCO3 production potential metric to compare to satellite derived PIC. There is some interesting/ valuable data and ideas presented in this work, but the paper needs a better cohesion and purpose behind the way the results were presented.

While most of the work focuses on E. huxleyi, the title of the paper suggests that a comparison with G. oceanica is the main topic of the paper. A summarizing comparison figure and a greater clarity as to which figures/tables are only on E. huxleyi would be helpful. I found myself being confused when looking at the figures, tables, and even parts of the text as to which species (or both) is the focus. Since the CCPP comparison to PIC presented in the paper is based only on E. huxleyi, it's difficult to see where the CCPP-PIC data comparison fits into the story presented here. However, it is an interesting result that the addition of G. oceanica does not invoke a better fit.

I would suggest publication with some moderate revisions to the figures, abstract, and some parts of the text.

Detailed comments

Abstract:

As I understand it, the inhibitory effect of increasing CO2 on G. oceanica is the main reason for this species' projected contraction under a future scenario. This should be emphasized in the abstract. As it is now, the projection of a contracted G. oceanica niche is surprising because it is generally the warmer water adapted species.

Also, since E. huxleyi CCPP shows a better correlation with satellite-derived PIC than when combine with G. oceanica, this should be mentioned in the abstract. Otherwise, given the title of the paper, one assumes that the CCPP estimates are derived from partitioning niches between E. huxleyi and G. oceanica.

Also, maybe a sentence at the beginning of Abstract describing why these two particular species are being compared would be helpful.

Intro:

Page 2, lines 3-9: This paragraph on future changes to the surface ocean environment needs expanding. What happens to nutrient availability with increasing stratification? How could this affect CaCO3 production and growth rate in coccolithophores? How

could increasing CO2 affect growth rate/ calcification of coccolithophores? The impact of increasing light is described but not the other effects of climate change. Increasing temperature would also increase metabolic rates, unless nutrient limitation becomes too strong. Overall this paragraph just needs more development with respect to the effects of anthropogenic climate change on coccolithophore habitat and how each effect could impact growth/calcification.

Page 2 $\sim$ line 18: There needs to be a paragraph with some background about the two species discussed in this paper. Why are you comparing these two particular coccolithophore species? These are the two major bloom forming coccolithophores. It is well known that E. huxleyi is very widespread, but how abundant is G. oceanica? Where does G. oceanic tend to thrive? Also mention that there are several different mophotypes of E. huxleyi and how they might differ. A bit of biogeography background would be helpful. This would then lead into the fundamental vs. realized niche paragraph.

Page 2 lines 28-35: the CCPP- PIC comparison is left out of this paragraph. It would be good to mention this here to indicate how it ties in with the E. hux – G. oceanic niche comparison.

Methods:

Page 3, line 4: Why test such high CO2 values? Are these even realistic? For instance, if end of the century CO2 concentration of 985 μatm (about 50 μmol kg-1 aqueous CO2), corresponds to a 4.8 deg C temperature increase, then why go up to 250 μmol kg-1 CO2? The range of CO2 is therefore bigger than the temperature range in terms of real world conditions. An explanation for this experimental setup would be helpful.

Page 3, section 2.1: The authors need to mention the particular E. hux morphotype being tested (PML B92/11 is morphotype A).

Page 4, line 22: Why would there be a lag phase? It seems the growth rate is calculated correctly (after the lag phase is over), but a quick explanation of why there is a lag phase

at extreme CO2 and whether this is a normal phenomenon in phytoplankton culturing and physiological testing would be helpful.

Page 5, section 2.7: I find this section about the data transformation confusing, particularly about the temperature. Is this just for growth rate? How do the resulting temperature-dependent growth rates compare to other studies on coccolithophores (Fielding 2013, Buitenhuis et al., 2008)?

Page 7, line 4: Unneeded commas before and after “relatively simple” . . . or just rewrite for clarity “As such we wanted to examine how projections of productivity using our relatively simple equation compared to coccolithophorid productivity patterns observed in natural systems”

Page 7, line 13: A citation of the CCPP metric is needed.

Page 8, line 14: Need citation for the PIC:POC ratios used for E. huxelyi and G. oceanica

Page 8, last paragraph: I took me awhile to figure out the CCPP estimates were made in three ways: 1) just E. huxleyi 2) just G. oceania 3) both species combined

Is this correct? Only results for E. huxleyi CCPP was presented so maybe clarify here that only the results with the highest correlation to satellite PIC are shown. It’s confusing because there are details described in the previous paragraphs about deriving CCPP for each species but then the results only show E. huxleyi CCPP.

Page 8, lines 26 and 27: Need parentheses around year for citations Gregg and Casey (2007) and Longhurst (2007).

Results:

Page 9, Results section in general: Please specify in the headings that these are only the results for E. huxleyi (not G. oceanica).

Page 9, line 2: Perhaps develop this small section a bit more. Which rate showed the

best fit?

Page 9, line 6: Instead of just saying “all rates”, please remind the reader what metabolic rates you are examining and refer to the equation presented in the methods.

Page 9, line 7: It’s hard to understand exactly what to look at in Table 2 and 3 to support this sentence (2nd sentence of the paragraph). It seems like CO2 concentrations of K1/2sat range form 0.85 to almost 5 μmol kg-1 depending on light and temperature. . .

Page 9, lines 8-10: Mention what are the optimal CO2 concentrations and put this into units of μatm to make it more relatable to the reader. Are we at the optimum CO2 already for coccolithophores or will it come in the near future? At what CO2 concentrations is K1/2inhib reached? More specifics would give the reader more useful information.

Page 9, line 14/15: What columns in table 2 are the reader supposed to be looking at? Are you referring to the Vmax column?

Page 9, line 18: I had to read this sentence several times before I actually understood it. Would this be a better way to put this?: “CO2 half saturation concentration were insensitive to temperature. However, under increasing temperatures CO2 optima for growth and inhibition occurred at lower CO2 concentrations”

Discussion:

Page 10, line 6: Since this is a major conclusion of the paper, it should be shown directly somehow. All the original G. oceanica data is published elsewhere, so a graphical summary of BOTH the E. hux and G. oceanic data would be helpful. This could be done through line plots comparing the metabolic rates of the two species under varying CO2 or in a bar plot comparing the rates. I just think it’s necessary to show a visual comparison of E hux and G oceanica data (or data-derived function) since the title of the paper indicates a comparison.

Page 10, line 30: A change in CO2 optima of 11 μmol kg-1 is not that small.

Page 11, line 5: Unneeded commas around “at least some”

Page 11, line 15: Again, here is where a comparison figure between E. hux and G oceanica would be helpful.

Page 12, line 3: The range tested in this study is so much higher than even what projected under RCP8.5 at the end of the century. The temperature range tested in this study is much more realistic. How warm would the world be under 5000 μatm CO2?

Page 12, line 12: I think a major limitation of this study is the focus on just one strain of one morphotype of E. huxleyi. Different E. huxelyi morphotypes show significant genetic and physiological variability (see Read et al., 2013; Langer et al., 2009; Krumhardt et al., 2017). Accounting for these differences could add significant uncertainty to the conclusions. I think that the last sentence of section 4.4 (before section 4.4.1) would fit better in a section on the “Limitations of this study” at the end of the Discussion section, where you describe how E huxleyi strain PML B92/11 is used to be representative of all E. huxleyi for determining niche and projections under future CO2 and warming in this study. This doesn’t make the results invalid, but is just a limitation that needs to be made clearer. This would then lead in nicely with the conclusion that more testing with colder water strain/species/morphotypes of E huxleyi is necessary.

Page 12, line 22: Capitalize “Figure”

Page 13, line 7: I think it’s well established that E. huxleyi is a generalist, given its widespread distribution from subpolar to tropics.

Page 13, line 9: Unneeded comma after “niche”

Page 13, line 25: Reference needed for this E. huxleyi warm water strain that outcompete G. oceanica at temps > 25C

Page 13, line 32/33: I'm confused by this lower CO2 extreme of 25 μatm. By Figure 5 it looks like G. oceanica outcompetes E hux at temps > 25C at 25 μatm CO2.

Page 14, lines 1-3: This is a major finding of this study and should be put in the abstract.

Page 14, line 4/5: The sentence seems like it shouldn't have the "under a broader range of CO2 conditions" part at the end. Under higher temperature alone (holding CO2 at about 400μatm) G. oceanica outcompetes E hux at temps > 22C. Or perhaps I'm misunderstanding this sentence completely?

Page 14, last paragraph of section 4.4.2: This would be better in a "Limitations of this study" section, as mentioned above.

Page 15, line 12: By "productivity", do you mean calcification?

Page 15, lines 14-22: Would this paragraph better fit in the Results section?

Page 16, lines 8-20: Could it be that E huxleyi CCPP just matches better because it's so much more abundant than G. oceanica?

Page 16, line 14/15: I do not understand this sentence. So the combined CCPP in the North Pacific and Atlantic is greater or less than the E huxleyi CCPP?

Tables:

Tables 2 and 3: Put parentheses around units for K1/2CO2inhib and K1/2CO2sat in tables.

Figures:

Figures 1 and 2: Indicate that this data is just for E. huxleyi in the caption. Also, show relevant CO2 range with a shaded area as in Sett et al., 2014 and indicate average oceanic CO2 concentration at present day.

Figure 3: Each "slice" looks the same.. maybe there's a better way to show differences between light levels or lack thereof? Also I do not understand the colors – add color

legend.

Figure 4: Make the μEH > μGO bigger or put it next to the color bar. It's a bit hard to notice and this is critical for understanding the figure.

Figure 5: same suggestion as for Figure 4.

Figure 7: It needs to be mentioned in the caption that these maps are CCPP for E huxleyi only.

Figure 8: Again, this is just CCPP for E huxleyi, right? This should be indicated in the figure caption. Also, a little map of the provinces (like in the supplemental section) would be great next to these bar plots. Having a map next to this data would make the figure much more relatable.

References cited:

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