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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

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-Comment 1. Is strain PML B92/11 (isolated from Bergen, Norway) the best strain of E. huxleyi to present a niche description of 'E. huxleyi'? My query here is whether the authors have considered the potential need to consider multiple strains when trying to describe the fundamental and realised niche of the species E. huxleyi. Though the authors state that cold-water (Southern Ocean) strains need to be considered more, would not a broader study of several strains of E. huxleyi, isolated from various geographical regions, result in a better description of the species as a whole? Related to this is whether the authors have considered examining different (geographically) strains

of G. oceanica and whether the limitations of G. oceanica's niche could relate to the limited number of strains available for this species? These comments are not meant to detract from the present study, but rather emphasize the broader context.

We agree that considering multiple strains, from diverse ocean regions, would benefit our study in describing the fundamental and realised niches for a species in more general terms. Nevertheless, despite the fact that our realised niche projections are based on only one strain for each species, they do generally fit to modern day observations. This indicates that the differences in requirements and sensitivities of the two species as described here are large enough to be revealed by choosing only two representatives.

-Comment 2. The authors use a 'recently proposed metric' for coccolithophore calcification rates (CCPP), but proposed by who? No reference is mentioned in the paper. Could the authors provide more context and information on this new metric?

This metric was proposed by us in a recently published paper. We will add the reference for this metric where it is mentioned in the main body of the paper.

-Specific points -Pg 1, Ln 4 (Ln 29) – Emiliania huxleyi is certainly one of the most abundant species, but not sure if G. oceanica can be classified in the same category. The two are common, though E. huxleyi has such a broad bio-geographical range compared to a narrower one for G. oceanica and generally a tropical range. Maybe relative abundance is not the characteristic to emphasize and either a commonality in many coccolithophore communities or bloom-formation by the two is more relevant (to global PIC production).

We agree and would change to: "the two most common bloom-forming species in present day coccolithophore communities appear adapted to. . ."

-Pg 1, Ln 13 – As well as the R2 of the correlation, it would be good to know what the slope of the line looks like and the p-value in the abstract.

We will add the p-value to the abstract and discuss the slope of the line in the text in more detail. Austral summer p-value= 5.46e-05, slope=0.32, Austral winter p-value= 3.06e-04, slope=1.03.

The reason for the relatively small slope of 0.32 in Austral summer, meaning that we overestimate the total production by a factor of three, are the high values of satellite derived PIC in the Antarctic province (which for several reasons given in the MS were not included in the correlation analysis). To rectify this issue, a simple scaling factor could be introduced.

-Pg 8, Ln 13-15 – Have the authors considered how total cell carbon (PIC+POC) to PON ratios would influence their data? In many ways, the N requirement of a coccolithophore cell is to produce both the PIC and POC. Also, are the PIC:POC ratios of 1 and 2 for E. huxleyi and G. oceanica, respectively, averages of the values given on Lines 23-24? Some justification for the use of the these values, given the ranges known in the literature, is needed.

We have. When calculating maximum supportable carbon production, we first assumed a Redfield ratio of 106:16. This value gave us the maximum POC production from the amount of available nitrate. We then calculated the amount of PIC which would be co-produced, with the POC, based on a mean PIC:POC ratio. So for the calculations both PIC:POC and POC:PON ratios were considered. This will be made more clear in the methods section.

PIC:POC values will be amended and based on average PIC:POC of E. huxleyi and G. oceanica from all treatments between 300-1000 μatm from Sett et al. 2014, Zhang et al. 2015 and this study. This will be mentioned within the methods section.

-Pg 14, Lns 21-23 – Surprised the review article by Monteiro et al. (2016) is not mentioned when considering viral attack and top-down effects as this article concluded that these were key considerations in the ecology of coccolithophores.

The review article will be added.

-Pg 16, Lns 2 and 3 – Rather than citing the PhD thesis of Charalampopoulou (2011), why don't the authors cite the peer-reviewed papers derived from this piece of work that address these points? Charalampopoulou et al. (2011) Irradiance and pH affect coccolithophore community composition on a transect between the North Sea and the Arctic Ocean. Marine Ecology Progress Series 431, 25-43, doi: 10.3354/meps09140. Charalampopoulou et al. (2016) Environmental drivers of coccolithophore abundance and calcification across Drake Passage (Southern Ocean). Biogeosciences 13, 5917-5935, doi: 10.5194/bg-13-5917-2016.

We will adopt the reviewer's suggestion.

-Pg 16, Ln 23 – Consider the use of the term 'benefit' in terms of E. huxleyi outcompeting G. oceanica.

Instead "E. huxleyi will gain further competitive advantage over G. oceanica.

-Pg 16, Ln 27 – What are coccolithophore dominated ecosystems? Please phrase in a more specific way (e.g. where coccolithophores are abundant enough to potentially influence the air-sea CO2 flux (e.g. coccolithophore blooms) or dominate the deep-sea flux of particulate material (e.g. subtropical gyres). Coccolithophores never dominate ecosystems.

We will change to: "Such changes could have significant implications for climate feedback mechanisms, one being the relative strengths of the organic and inorganic carbon pumps in ecosystems where coccolithophores are abundant enough to significantly impact the air-sea CO2 flux (e.g. coccolithophore blooms) and/or dominate the deep-sea flux of particulate material (e.g. subtropical gyres).

-Figures. -Fig 3 - Missing legend that is on Fig 4, consider swapping figures around or reproducing the legend.

We will reproduce the legend on Figure 3.

-Fig 6 – This appears to be a rather unconvincing relationship. Is it possible to plot the 95% CI limits for the relationship? In addition, is there a sampling depth issue here that results in greater amount of data at high temperatures? That is to say, is the distribution of data related to more shallow tropical sediment samples than deep cold sub-polar sediment samples?

We will add 95% prediction bounds for new observations for the global relationship. The fact that only the Atlantic basin does not entirely follow the trend has been mentioned in the text of the paper as well. The data which does not follow the overall pattern (which now will be marked with a different symbol on the plot) is from the south-equatorial to equatorial zone, taken from a study by Boeckel et al. 2006. In this study it appears that G. oceanica abundance is driven more by increasing nutrient concentrations than by temperature. E. huxleyi seems to also be driven by increasing nutrients but it also dominates more in the colder regions. It seems the upwelling in this region is driving a different relationship between E. huxleyi and G. oceanica than in other areas. We shall try to explain this more clearly within the relevant section of the paper.

There is very little sediment sampling data at high latitudes in general let alone which contains these species. There is a disproportionately large amount of sampling in the warmer tropical areas. Also, we selected only samples which were above the lysocline and therefore were not affected by the possible confounding effects of differential dissolution of coccoliths.

-Fig 8 – Would it be more appropriate to plot as scatter plots where each data point is from each province? Maybe this would emphasize better how well the two agree and in which provinces they do not agree?

While using a scatterplot does emphasise that the two do not agree in some provinces, it also makes it more difficult to determine which provinces do and do not agree. For the purpose of clearly comparing each province in each season quickly, and having now included more details and a discussion on the slope of the fits, we feel that the

barplot works best.

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