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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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This interesting study by Gafar and Schullz presents a new set of laboratory measurements on the coccolithophore species Emiliania huxleyi and Gephyrocapsa oceanica under a broad spectrum of temperature, light and CO2 levels. The authors use this new data, as well as published data from the same strain of E. huxleyi and G. oceanica, to examine potential differences in the biogeographical ranges of the two species in the present and future ocean. The authors conclude that G. oceanica will suffer a considerable niche (range) contraction under future climate change scenarios, which might be unexpected given it generally favours warmer waters than E. huxleyi. The

conclusions of the study are supported by the data generated and I have only minor comments.

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.

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?

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).

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.

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.

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.

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.

Pg 16, Ln 23 – Consider the use of the term 'benefit' in terms of E. huxleyi outcompeting 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 CO_2 flux (e.g. coccolithophore blooms) or dominate the deep-sea flux of particulate material (e.g. subtropical gyres). Coccolithophores never dominate ecosystems.

Figures.

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

the legend.

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?

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?

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