

***Interactive comment on* “The Influence of Environmental Variability on the Biogeography of Coccolithophores and Diatoms in the Great Calcite Belt” by Helen E. K. Smith et al.**

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Reviewer #3 Reviewer Comment (RC) - This work adds incremental knowledge about the environmental forcing of coccolithophores and diatoms distribution in the southern ocean. In the future, the importance of this work may be that it serves as a base line study. The paper is well written and substantial with many references, although I believe it could be shortened by about 25% and still say the same. Based on the abstract and conclusions there is not much new insight except that the authors are looking at diatoms and coccolithophores at the same time. There is a host of environmental data and they are discussed at length but few significant patterns emerge which is often the

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case in beyond control “ships of opportunity studies” where the research is constrained by circumstances, timing, and sampling strategy.

Author Response (AR) - We consider that this study presents a comprehensive analysis of environmental forcing upon the distribution and abundance of dominant diatoms and coccolithophores in the Great Calcite Belt, a region of high importance for marine biogeochemical cycles. This work will contribute to improve our knowledge of the factors that control the biogeography of phytoplankton in the Southern Ocean. It may well form a baseline for the standard of analysis required for future studies, in that they will require a comprehensive investigation over a wide suite of environmental data when considering phytoplankton biogeography in the Southern Ocean - there is a need to move beyond single factor analysis.

RC1 - This brings up the next point: The collections design was perhaps not ideal. In the paper (page 4 line 25) it says that water was collected from the upper 30 m. Apparently, only one liter of water was sampled that integrates the entire 30 m? This is really precious little water unless I am reading this incorrectly in which case it needs to be explained. It is well known that phytoplankton biomass can occur below this level (e.g. Hegseth and Sundfjord, 2008). Why was the collection limited to 30m?

AR1 - The focus of this study was on the upper mixed layer in the Southern Ocean, rather than deeper waters below the productive euphotic zone and noting that few subsurface chlorophyll maxima (SCM) were encountered (limited to sub-tropical waters). Sampling at 30 m is hence suitable for characterising variability in upper ocean phytoplankton communities. Sampling 1 litre of water is standard procedure for SEM identification of phytoplankton on a 25 mm filter area. Higher volumes lead to clogging of the filter and loss of useable filter area for enumeration when cells are covered in additional organic matter and/or other phytoplankton cells.

RC2 - Also the method of identification is not really suited to a detailed morphological analysis of *E.hux* which is important especially in the southern oceans where there

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exist various morpho/phenotypes of this species.

AR2 - We acknowledge that there are various morphotypes of *E. huxleyi* in the Southern Ocean, however morphological examination of *E. huxleyi* was not performed as part of this study. Further, differentiating *E. huxleyi* morphotypes for the statistical analysis was not our specific focus, which was on differentiating different coccolithophore and small diatom species.

RC3 - What were the reasons for the magnifications differing at 5kx and 3kx?

AR3 - The difference in magnification for the two transects reflects the overall lower cell densities found in the Indian Ocean versus the Atlantic Ocean and our requirement to enable sufficient filter area for identification and enumeration.

RC4 - What about all the other material on the filter?

AR4 - There were occasionally other material present on the filter, but these were not straightforward to identify and therefore were not quantified. Additional material beyond coccolithophores and diatoms were not the focus of the study and so were not included in the manuscript.

RC - There are many generalities in the paper that could use more explanation. Some of these are defined by G below;

RC5 - G: Page 2 line 5: Takahashi wrote many papers on CO₂ sequestration of CO₂. How do we know whether the CO₂ that is being taken up by areas of the ocean is anthropogenic or natural? Also the North Pacific is also such an area.

AR5 - We have included the North Pacific in this sentence as follows:

“The region between 30-50oS is recognized as having the highest uptake of anthropogenic carbon dioxide (CO₂) alongside the North Atlantic Ocean and North Pacific Ocean (Sabine et al., 2004).”

Also, following back to the original work the anthropogenic uptake was estimated from

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a carbon tracer technique (Gruber et al, 1996).

Gruber, N., Sarmiento, J. L., & Stocker, T. (1996). An improved method for detecting anthropogenic CO₂ in the oceans. *Global Biogeochemical Cycles*, 10(4), 809–837.

RC6 - G: >Page 2 line 7-9: vague sentences. Poorly constrained, critical? Why

AR6 - We have rephrased this paragraph to read as follows:

“Our knowledge of the impact of interacting environmental influences on phytoplankton distribution in the Southern Ocean is limited, for example how light and iron availability or temperature and pH may interact to control phytoplankton biogeography (Boyd et al., 2010, 2012; Charalampopoulou et al., 2016). Hence, if model parameterizations are to improve (Boyd and Newton, 1999), and provide more accurate predictions of future biogeochemical change, a multivariate approach is required.”

RC7 - G: >Page 2 line 28-30: Why important

AR7 - We have added context at the beginning of the paragraph that highlights the importance of studying mineralizing phytoplankton.

“In the context of climate change and future ecosystem function, the distribution of mineralizing phytoplankton is important to define when considering phytoplankton interactions with carbonate chemistry (e.g., Langer et al., 2006; Tortell et al., 2008) and ocean biogeochemistry (e.g., Baines et al., 2010; Assmy et al., 2013; Poulton et al., 2013).”

RC8 - Page 3 line 1....”south of ~30°S and extends to ~60°” (This has already been stated.

AR8 - This text has now been removed.

RC9 - G: Page 3 line 25 “uncertainties” why?

AR9 - To clarify we have altered the sentence to read as follows:

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“... remains a significant issue when considering the impact of climate change on natural phytoplankton communities.”

RC10 - Page 5 line 11: Why were individual coccoliths not counted? This can also say a lot about the age of the community.

AR10 - Our focus in the present study was on comparative biogeography of coccolithophores and small diatoms rather than coccolithophore growth dynamics. Hence coccolith counts were not included.

RC11 - Page 7 line 5-13. Can all these parameters be displayed graphically?

AR11 - These parameters could be displayed graphically, however, this would look confusing given the north-south and east-west cruise tracks and irregular distances covered between stations. It was decided that retaining the original data in table format also allowed better access to the parameter values.

RC12 - Page 7 line 29. Maybe I missed it but what were ALL of the 28 coccolithophore species?

AR12 - This information will be available as a Pangea dataset, combining the 28 coccolithophore species and 76 diatom species and their abundances.

RC13 - Page 11 line 28 “occurrences” instead of “features”

AR13 - This has now been altered.

RC14 - Page 12 line 7. Where is the rest of the Chl coming from? This section (lines 7-9) is not clear

AR14 - The remaining fraction of the Chl-a is most likely to represent phytoplankton not enumerated in this study such as small picoplankton, non-mineralising nanoplankton (e.g. naked flagellates), dinoflagellates and other diatoms.

RC15 - Page 13 line 4 “coccolithophores IN this region”

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AR15 - We have noted this and corrected.

RC16 - Page 14 line 10 What is the “theoretical species abundance”?

AR16 - We have removed this comment and amended the sentence as follows.

“Nanoplankton are subject to high grazing pressure (Schmoker et al., 2013), with the growth and mortality of a species both directly influencing cell abundances (Poulton et al., 2010), which could result in nanoplankton abundance patchiness additional to the influence of temperature or other environmental gradients.”

RC17 - Page 15 line 12. “However A FEW non-blooming

AR17 - We have noted this and corrected it.

RC18 - Page 15 line 13 “conspicuously absent” why is this conspicuous?

AR18 - We have removed conspicuously from this sentence.

RC19 - Page 15 line 14-15 “to be a *E. huxleyi* specific rather than a coccolithophore-wide phenomena” not clear what the authors meant to say. I don’t agree.

AR19 - We have altered the sentence to read as follows

“Therefore, a low silicic acid concentration in the surface waters of the GCB may negatively impact coccolithophore species that do have a silicic acid requirement, such as *Calcidiscus leptoporus*, and favour bloom-forming species that do not require silicic acid such as *E. huxleyi*.”

RC20 - Page 16 lines 25-27...There are many studies con and pro for this sentence

AR20 - We are not sure what the reviewer means here. We have removed part of the sentence for clarification.

“In our study, there was no significant correlation between *E. huxleyi* and Ω_{calcite} (Pearson’s product moment = 0.093). However, the waters of the GCB remained oversaturated ($\Omega_{\text{calcite}} > 2$) throughout, and furthermore the relationship between coccol-

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ithophores, calcification and carbonate chemistry is now recognized as being complex and non-linear...”

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