Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-110-AC1, 2017

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.

Helen E. K. Smith et al.

helen.eksmith@gmail.com

Received and published: 7 July 2017

Anonymous Referee #1 Comment - This article 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. The study has been carefully conducted and the results are presented clearly and concisely. This work will contribute to improve our knowledge of the factors that control the biogeography of phytoplankton in the Southern Ocean. I support publication of this material in BG, provided the authors address some uncertainties in their analyses and conclusions.

Author Response - We thank the reviewer for their constructive comments and recommendation for publication pending our responses and further development of the manuscript. We address their comments below.

Comment #1 - Reading the description of BGC at the beginning of the Introduction, one may be tempted to infer that the biogeochemical importance of the GCB (e.g. a region of net CO2 uptake) stems from the fact that it is a region of high PIC. However, its importance is probably more related to its being a region of generally increased plankton abundance and productivity. In fact Fig. 1 suggests that the region could be equally defined in terms of enhanced chl a levels.

Author Response #1- We are in agreement with the reviewer – the Great Calcite Belt is an area of both elevated chlorophyll a and particulate inorganic carbon associated with increased seasonal production. The recent confirmation of the GCB as a significant coccolithophore phenomenon leads to this region being of interest in the context of upper ocean biogeochemistry and changing climate. Acknowledging the reviewer's comment we have made the following changes to the introduction to better reflect the generalised increase in plankton abundance and productivity within the GCB.

"The Great Calcite Belt (GCB), defined as an elevated particulate inorganic carbon (PIC) feature occurring alongside seasonally elevated chlorophyll-a in austral spring and summer in the Southern Ocean (Fig. 1; Balch et al., 2005), plays an important role in climate fluctuations. . ."

Comment #2- On a related note, is the PIC to POC ratio actually higher in this region than it is in tropical and subtropical waters?

Author Response #2 - The GCB spatial extent is set by high satellite-detectable PIC concentrations rather than a change in the PIC to POC ratio – both PIC and POC may increase relative to subtropical waters to get the GCB signal, without necessarily changing the ratio between the two.

Comment #3- Some studies have shown that the coccolithophore to diatom biomass ratio actually increases in tropical, unproductive waters (Cermeno et al. PNAS 2008). This study uses abundance to assess dominance of different phytoplankton species. But, due to interspecific differences in cell size, an assesment based on carbon biomass could have been more reliable, as some of the authors have shown before (Daniels et al. MEPS 2016). For instance, section 3.2 starts by noting that nanoplankton tended to be more abundant than microphytoplankton, but this is always to be expected and cannot be directly translated to ecological dominance patterns. The authors should include a statement, and/or provide some sensitivity tests, on how results could change if dominance were assessed by biomass instead of abundance.

Author Response #3 - Indeed, considering biomass would most likely change the picture and decrease the dominance of coccolithophores (in many cases). Whilst, deriving coccolithophore biomass is relatively straightforward (as they are mostly spherical in shape, with no vacuoles or complex cell structures that may include biomass), diatoms are far more morphometrically complex (not spherical, often with setae which may or may not contain cell plasma, and many cells have large internal vacuoles), making direct comparison between the two potentially problematic (especially when the two may be equally abundant) – i.e. small errors in diatom estimates can cause species dominance to radically change. In contrast, comparisons across subtropical waters (no diatoms, some coccolithophores) and upwelling zones (many diatoms, few coccolithophores), as in Cermeno et al. (PNAS 2008), is relatively straightforward.

Furthermore, many potentially significant issues over carbon conversions are not straight forward. Although there are now extensive conversion tables for various phytoplankton carbon content, these come with important caveats, as described in detail (e.g.) in Leblanc et al. (2012), which include (but are not limited to) the effect of preservatives on cell size and content (shrinkage), simplistic bio-volume conversions from cell measurements, time of sampling and age of the community or population, and growth conditions (light, nutrients, temperature). The scope of our manuscript was not

intended to cover full discussion of these issues.

We have now included at statement to show that we recognise the differences in species dominance if biomass was considered, page 7 line 17.

"…not numerically dominant compared to the nanoplankton species at these locations. Consideration of community biomass would potentially reduce the dominance of the nanoplankton relative to microplankton in the GCB. However, converting cell size to biomass is not straightforward for diatoms, as highlighted in Leblanc et al. (2012), and to avoid these potential caveats we have considered species abundance only. Total cell abundances…"

Comment #4 - page 12 line 20. A reference is needed here to support the value of chla content used for Ehux.

Author Response #4 - We do state the appropriate references (i.e. Haxo, 1985 and Poulton et al., 2013 who applied these estimates previously) used to estimate the E. huxleyi chl-a contribution in section 4.2.1 page 12 line 5, and have now restated this in sections 4.2.2 and 4.2.3 to avoid confusion.

Comment #5- However, the chla content of algal cells is highly dependent on temperature, light, nutrients, etc. which makes this calculation very uncertain. Carbon biomass is a more reliable metric to estimate relative importance of different species, because the C cellular content is less variable.

Author Response #5 - Cell chl-a content is indeed variable with physiological growth conditions. Carbon biomass is possibly a more reliable metric, however this would rely on two requirements: (1) that the entire phytoplankton community (pico-plankton to micro-plankton) be assessed in terms of cell carbon (which few studies undertake), and (2) that there are few errors in estimates of cell carbon from cell size and biovolume (see earlier comment). Literature biovolume to carbon conversions are often generalist across multiple species and many (though not all) are based on culture values under optimum growth conditions rather than realistic in situ conditions (temperature, light, nutrients). Hence, there are also large potentials for cell carbon estimates to be as variable with physiological growth conditions than a rough conversion of cell numbers to chl-a. We have now added an appropriate caveat to the text to acknowledge potential issues over variable cell chl-a content and the estimates derived from them.

“It should be noted that the cell Chl-a content from Haxo (1985) falls at the lower end of the range of measurements for E. huxleyi cell Chl-a content (e.g., 0.24-0.38 pg Chl-a per cell; Daniels et al., 2014) and leads to conservative estimates of Chl-a contributions from this species.”

Comment #6 - The conclusion in the Abstract that temperature is the main driver of nanoplankton distribution should be qualified, as it may well be that temperature is co-varying with other factors that are the actual, ultimate drivers.

Author Response #6 - We agree with the reviewer and have now rewritten the final line of the abstract to better reflect the results of the multivariate analysis.

“After statistical consideration of the influence of spatial variability in a diverse suite of environmental factors on the distribution of nanoplankton in the GCB, we identify a combination of carbonate chemistry and macro-nutrients, co-varying with temperature, as the dominant drivers of species biogeography in a large proportion of the modern Southern Ocean.”

Comment #7 - On p. 10 line 10, what is the basis for statement that nanophytoplankton contribute 40% of total PP? The references provided do not have that kind of evidence (they are reviews on the ecology and biogeochemical role of diatoms). The authors should use instead remote sensing studies (e.g. Uitz et al. 2010 GBC) to support the statement that nanophytoplankton are the largest contributors to global marine PP.

Author Response #7 - We actually refer in the text to the micro-phytoplankton contribution, in order to highlight that the majority of studies in the Southern Ocean have

focused on large phytoplankton species (i.e. most often diatoms). We have now inserted the Uitz et al. (2010) reference in the relevant section to further highlight the contribution of micro-phytoplankton, but also the contribution of nano-phytoplankton, as discussed in the next sentence (starting p.10 line 11).

“Studies of Southern Ocean phytoplankton productivity have generally focused on the micro-phytoplankton (Barber and Hiscock, 2006) as these species contribute around 40% to total oceanic primary production (Sarthou et al., 2005; Uitz et al., 2010). However, nanoplankton and picoplankton are becoming increasingly recognised as important contributors to total phytoplankton biomass, productivity and export in the Southern Ocean (e.g., Boyd, 2002; Uitz et al., 2010; Hinz et al., 2012). . .”

Comment #8 - Minor point ‘TOxN’ is awkward and seems to suggest organic nitrogen. Better use ‘NOx’ or just nitrate (indicating in methods that nitrate actually refers to nitrate+nitrite). In any event nitrite concentrations are likely to be negligible, in comparison with nitrate, in these waters.

Author Response #8 - The notation for nitrate+nitrite has now been changed to NOx throughout the manuscript.

Additional references used in responses

Cermeño, P., Dutkiewicz, S., Harris, R.P., Follows, M., Schofield, O. and Falkowski, P.G.. The role of nutricline depth in regulating the ocean carbon cycle. P. Natl. Acad. Sci. USA, 105(51), 20344-20349, 2008.

Daniels, C.J., Sheward, R.M. and Poulton, A.J.. Biogeochemical implications of comparative growth rates of Emiliania huxleyi and Coccolithus species. Biogeosciences, 11(23), 6915-6925, 2014.

Leblanc, K., Arístegui, J., Kopczynska, E., Marshall, H., Peloquin, J., Piontkovski, S., Poulton, A.J., Quéguiner, B., Schiebel, R., Shipe, R. and Stefels, J.. A global diatom database–abundance, biovolume and biomass in the world ocean. Earth Syst. Sci.

Data, 4, 149-165, 2012.

Uitz, J., H. Claustre, B. Gentili, and Stramski D. Phytoplankton class-specific primary production in the world's oceans: Seasonal and interannual variability from satellite observations, Global Biogeochem. Cy., 24, GB3016, doi:10.1029/2009GB003680, 2010.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-110, 2017.