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Discussions

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.

Anonymous Referee #1

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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. 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 im-

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portance 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 chla leveles. On a related note, is the PIC to POC ratio actually higher in this region than it is in tropical and subtropical waters? 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 nanoplanton 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. page 12 line 20. A reference is needed here to support the value of chla content used for Ehux. 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. 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. 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. Minor point 'TOxN' is awkward and seems to suggest organic nitrogen. Better use 'NOx' or just nitrate (indicating in methods that nitrate acually refers to nitrate+nitrite). In any event nitrite concentrations are likely to be negligible, in comparison with nitrate, in these waters.

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