Interactive comment on "Coccolithophore biodiversity controls carbonate export in the Southern Ocean" *by* Andrés S. Rigual Hernández et al.

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We sincerely thank Dr Griet Neukermans (reviewer 1) for her positive and constructive comments on our manuscript that have helped to improve the paper. We have carefully considered all her comments and addressed each of them as outlined below.

R1-Cx : Referee comment, R1-Rx: authors response.

R1-C1: This paper is a very useful and original contribution to our understanding of how coccolithophore diversity shapes carbonate export in the Southern Ocean based on time series of sediment trap data. The paper is a pleasure to read: very well writ-

ten, well structured, comprehensive, clear, and concise, with high-quality figures, and in-depth discussion. I highly recommend publication of this work in Biogeosciences. Congratulations to the authors for this very nice piece of work. I only have a few very minor comments that may improve the paper.

R1-R1: We sincerely thank reviewer #1 for the careful reading of our manuscript and constructive criticisms and comments that helped to improve the manuscript. We have carefully considered all her comments and have addressed each of her concerns as outlined below.

R1-C2: P3L87-89: replace "satellite reflectance observations" with "ocean color satellite reflectance observations" to precise that it is the fraction of incoming VISIBLE and NEARINFRARED solar radiation that is reflected from the ocean surface. Add reference (Balch et al., 2005) (Gordon et al., 2001) at the end of the sentence. These are the NASA standard algorithms for PIC retrieval.

R1-R2: Corrected according to reviewer 1's suggestion.

R1-C3: P4L111: reference for representativeness is missing

R1-R3: Please note that a detailed explanation of the representativeness of the SOTS and SAM sites was explained later in the text (section 2.2). In the new version of the manuscript we refer to section 2.2 in the line indicated by the reviewer.

R1-C4: P5L132: remove "that".

R1-R4: Corrected according to reviewer 1's suggestion.

R1-C5: P6 Figure 1: STZ not included in legend.

R1-R5: Subtropical Zone - STZ has been included in the legend following reviewer 1's suggestion.

R1-C6: P8L234: Can you briefly explain the method to calculate daily fluxes?

R1-R6: The method employed to estimate coccolith and coccosphere fluxes has been included in the new version of the manuscript (lines 256-263 of the corrected version of the manuscript).

R1-C7:P9L253-255. I strongly appreciate the authors obtained two independent estimates of coccolith fluxes based on the birefringence and morphometric methods, each with their own advantages and disadvantages.

R1-R7: We appreciate reviewer 1's supportive comment. Since both techniques have associated errors, we decided to present both estimates (in the manuscript). Interestingly, in spite of some variability between techniques the general conclusions would remain similar to using any of the techniques individually.

R1-C8:P10L294: Can you briefly explain why you think that the finding of <5% error on DSL estimates from polarization would apply to other species than the one tested?

R1-R8: E. huxleyi overwhelmingly dominated the coccolithophore assemblages in all the samples analysed. Given the very low number of coccoliths of the rest of the coccolithophore species, it was almost impossible to find a representative number of individuals of for most of the "secondary" species in the same sample in order to statistically compare both microscopy techniques. Please note that even when a coccolith of a given species is found under the SEM, it can not always be measured because its position is not always adequate (e.g. they are often tilted or partially covered by other phytoplankton or detritus). Based on this, we decided to measure C. leptoporus because it was the second most abundant species, and therefore, statistical comparison between populations measured under the Light Microscope (LM) and SEM was possible. The subtle differences between coccolith distal length measurements are most likely due to the fact that the peripheral limit of the coccolith shield is not as sharp under the LM as is the case for SEM images. It follows that differences in coccolith measurements between SEM and LM techniques will be probably similar or smaller in the case of larger species. Since the majority of coccolith species identified in the

current study display a similar (e.g. Gephyrocapsa oceanica, Syracosphaera pulchra, Umbellosphaera tenuis and Umbilicosphaera sibogae) or larger size (e.g. Coccolithus pelagicus and Helicosphaera carteri) than C. leptoporus, it can be assumed that the <5% error on DSL estimates for C. leptoporus is applicable to the rest of the species found in the current study.

R1-C9: Materials and Methods section: I think you should add a section on the ocean colour satellite data treatment. Which data did you use? Figure 2 suggests you used weekly data for PIC but monthly for Chla? Why not the same temporal resolution? Did you use multisensor merged products (such as GlobColour?)? Did you do any spatial averaging and how did you compute the weekly averages?

R1-R9: Corrected according to reviewer 1's suggestion. A new subsection called "2.8 Remotely sensed chlorophyll-a and PIC concentrations" has been included in the new version of the manuscript describing how we obtained and processed the Chl-a and PIC satellite data used in the manuscript. Weekly Chl-a data is now plotted in the graphs. Additionally, in order to support our statements in section 4.1 of the discussion, CaCO3 fluxes registered by the traps have also been included in Figure 2.

R1-C10: P12 Figure legend: specify "ocean color satellite-derived". Panel b, please add Chla data for October/November to see the potential rise in Austral spring. Can you present PIC and Chla data at the same temporal resolution? That would make sense. R1-R10: Corrected according to reviewer 1's suggestion. As mentioned in the previous comment weekly Chl-a data is now plotted in Figure 2. Moreover, data for the month of November is now included in figure 2.

R1-C11: P12 Figure 2: panel c at 61S is missing.

R1-R11: The figure caption erroneously mentioned the 61°S site (the figure caption corresponds to an earlier version of the manuscript where data from the 61°S was presented in the graph). In the new version of the manuscript this information is not required. Therefore, the reference to the 61°S site in the caption of Figure 2 has been

deleted.

R1-C12:P16L429: the secondary maximum of satellite PIC might be an artefact of satellite data treatment, but it's hard to say, since that critical information is missing from the manuscript Materials and Methods. . .

R1-R12: As mentioned above (see R1-R9), a new subsection called "2.8 Remotely sensed chlorophyll-a and PIC concentrations" has been included in the manuscript. It is important to note that the PIC satellite signal for the grid area considered representative of the SAM station (coordinates 47-45° S and 171°E-179°W) was almost identical to that of a smaller area over the SAM site (47-45° S, 177.5-179.5°E). An alternative explanation of the secondary PIC maximum (i.e. possibility of storm-induced bubbles) has been included in the text. See section 4.1 of the new version of the manuscript.

R1-C13: P14L377: Not clear what you mean with total CaCO3 export in Fig. 5. Is this the combined export of coccos and forams? If yes, how did you quantify foram export? I suggest you also explain total CaCO3 in the Figure legend. R1-R13: Both figure and figure caption have been modified in order to make clear that annual total CaCO3 export (represented by yellow bars in Figure 5) refers to the total amount of CaCO3 collected by the traps determined chemically (as explained in section 2.4) while the clear and dark blue bars represent the two different estimates of the contribution of CaCO3 based on birefringence and morphometric techniques, respectively.

R1-C14: P17L436: it may also be a foraminiferan signal, see for example (Rembauville et al., 2016).

R1-R14: We appreciate reviewer 1's suggestion. Indeed, we did consider the possibility that heterotrophic calcifying plankton such as planktonic foraminifera or pteropods could account for the secondary maximum in February-early-March. However, total CaCO3 fluxes recorded in the trap do not reflect an increase during this interval. Therefore, we believe this possibility is unlikely. Please note that in the new version of the manuscript total CaCO3 fluxes have been included in figure 2 (see also R1-R9).

R1-C15: P18L497 etc.: The satellite PIC algorithm has indeed been calibrated in Northern hemisphere waters, where E. huxleyi greatly outnumbered other coccolithophore species, which is also the case in your study areas. In fact, the satellite signal (which is proportional to the particulate backscattering coefficient) is more sensitive to the concentration of E. huxelyi-sized particles, compared to larger, less abundant cocco species. Indeed, if larger, much heavier species are more prevalent in the Northern hemisphere waters, where the conversion factor for backscatter to PIC is calibrated, then this would lead to an overestimation of PIC in any waters where larger species are less prevalent. Put in other words, the conversion factor of backscatter to PIC is dependant on the size of the calcite particles. An alternative explanation for the overestimation of PIC is Southern Ocean waters is the contribution of bubbles to the backscattering coefficient.

R1-R15: Our intention was to highlight that the different composition of coccolithophore assemblages between the Northern Hemisphere and Southern Ocean may contribute (only one factor among probably many) to the overestimation of PIC concentration in the Southern Ocean. In the new version of the manuscript this has been clarified and the possible influence of microbubbles to the backscattering coefficient has also been included.

R1-C16: P23L654: poleward expansion of E. huxleyi to the Arctic has also been demonstrated by (Neukermans et al., 2018)

R1-R16: We appreciate the new reference provided by reviewer 1. This study is now mentioned in the new version of the manuscript.

R1-C17: P24L664 etc.: see also recent review in (Krumhardt et al., 2017)

R1-R17: The reference mentioned by reviewer 1 has been included in the discussion (section 4.5).