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Discussions

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

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The manuscript by Rigual Hernandez et al. represents a comprehensive study of species-specific fluxes of coccolithophore-derived CaCO3 fluxes to the deep-sea in the Southern Ocean. The manuscript is well written and easy to follow, and provides several new insights into the important role of numerically rare coccolithophore species with high relative coccolith and cellular CaCO3 content. Such understanding has been well recorded in terms of production and export in northern polar and sub-polar waters, but the manuscript by these authors reveals the importance of this processes in the Australian-New Zealand sector of the Southern Ocean. There are no significant is-

sues with the methods or conclusions, and only a few points that need clarity or further referencing.

Ln 39: 'E. huxleyi dominates remote sensing images as a result of higher cell abundance and detachment of its small coccoliths.' This is an oversimplification and ignores the vital role of the characteristic light-scattering properties and size of E. huxleyi coccoliths, in addition to its tendency to shed coccoliths and characteristic bloom formation.

Ln 56-57: 'decline in saturation state of carbonate minerals in seawater makes the biological precipitation of carbonate difficult and increases dissolution rates of their shells or skeletons'. Current theoretical consensus of the response of coccolithophores to carbonate chemistry (e.g. Bach et al., 2015) specifically relates their internal calcification to substrate availability (HCO3-) and inhibition by proton (H+) concentrations; i.e. different carbonate chemistry parameters than inferred in the text (i.e. CaCO3 saturation state).

Bach et al. (2015). A unifying concept of coccolithophore sensitivity to changing carbonate chemistry embedded in an ecological framework. Progress in Oceanography, 135, 125-138.

Ln 92-95: As well as recent work by Trull et al. (2018) showing that satellite ocean-colour based PIC estimates can be unreliable in Antarctic waters, should also cite Holligan et al. (2010) which came to the same conclusion earlier.

Ln 131-132: 'which that', delete one or the other, both not necessary.

Ln 294-295: 'For the ks value of each taxa, data from the literature were (Table 1)' – sentence not finished.

Ln 329: Missing word – 'later' at end of sentence 'i.e. approximately eight months <later> (Fig. 2).'

Fig. 2. Would it not be better to make the y-axis on these plots the same scale?

Ln 417-419: This is an interesting point, as it is similar to loss terms found specifically for coccolithophores from microzooplankton grazing in the temperate N Atlantic setting (60-80%; Mayers et al., 2019).

Mayers et al. (2019). Growth and mortality of coccolithophores during spring in a temperate Shelf Sea (Celtic Sea, April 2015). Progress in Oceanography 177, 1010928.

Ln 490-492: Again, although Trull et al. (2018) recently identified over-estimate of coccolithophore PIC in the Southern Ocean by the NASA satellite ocean-colour-based PIC algorithm, this was examined earlier by Holligan et al. (2010). In the case of Holligan et al. (2010), the difference was attributed to the lower coccolith and cell $CaCO_3$ content of E huxleyi found in the S Atlantic (Scotia Sea). This is in general agreement with the reasoning suggested here (i.e. issues over the coccolith specific-area:mass ratios for the dominant reflective particles), though differs over whether this is considered a problem with E huxleyi or C pelagicus (or other species with high coccolith $CaCO_3$ content).

Ln 570: Should the units not be 0.4 Tmol C yr-1?

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