Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-186-RC1, 2019

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Calcification and distribution of extant coccolithophores across the Drake Passage during late austral summer 2016" *by* Mariem Saavedra-Pellitero et al.

Anonymous Referee #1

Received and published: 12 June 2019

Summary:

Saavedra-Pellitero and co-authors present coccolithophore measurements from along the western side of the Drake Passage. They quantify coccolithophore species counts and coccolith mass. They present some oceanographic measurements but also use global databases to retrieve nutrients and carbonate chemistry quantities. They find a poleward decrease in both coccolithophore diversity and calcification. They group E. huxleyi morphotypes into two major groups (A and B) and are able to show that the southward decrease in calcification is related to a shift from A morphotypes along the Chilean margin to B morphotypes in the subantarctic and polar front zones. Coccol-

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ithophore calcification is inversely related to alkalinity, dissolved inorganic carbon, and pH. A Principle component analysis reveals three distinct clusters: Chilean coastal, SAZ, and PFZ. Temperature seems to be an important factor in controlling the distribution of coccolithophore species, as well as overall coccolithophore abundance and calcification.

General comments:

This manuscript is well written and of high quality. The authors present a valuable dataset with respect to observed coccolithophores. They present fantastic detailed plots of coccolithophore species in this Southern Ocean transect. This transect is slightly westward of the transects presented Charalampopoulou et al. (2016), which is a similar study. This manuscript offers more information on depth variations in coccolithophore abundances than previous studies in this region, which is great! This study reaches much of the same overall conclusions as previous transects observing coccolithophores in the Southern Ocean, so it is not groundbreaking, but adds to a solid overall conclusion of coccolithophores transitioning from more calcified species/morphotypes in the subtropics to less calcified ones in the ACC region. The conclusion that temperature is a controlling factor on coccolithophore abundance agrees with previous studies (e.g., Charalampopoulou et al., 2016). I think this manuscript is in great shape and only needs minor revisions. One piece that is missing is a bit more specific speculation about how coccolithophore abundance/calcification could change with climate change. The authors say that coccolithophores will be strongly influenced, but not how they will be influenced. I think it's important to hypothesize the direction of change, given current observations and relationships with environmental variables presented in the study. I also think that the positive relationship between temperature and coccolith mass needs to be emphasized a bit more. It is a bit of a shame that nutrients and carbonate chemistry parameters were not measured in situ, but I do not think that having these measurements would have changed the conclusions (it would have just added more strength to them). I also think that the depth

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variations between the three different oceanic region clusters could be more emphasized (especially because this was not as well presented in previous studies, so I find it to be new information): maximum depth of coccolithophores decreases poleward.

Specific comments:

Page 1, Abstract: maybe add in something about the decreasing depth of coccolithophores as you go poleward (as shown in Figure 3a)

Page 2, Line 6: extra “substantial”

Page 2, Lines 11-13: This sentence is awkward and a bit hard to understand. Maybe it would be best rewritten like this: “Coccolithophores produce up to ∼40% of open ocean calcium carbonate (Poulton et al., 2013) and are responsible for ∼20% of global net marine primary production (Malone et al., 2017). Therefore, how coccolithophores respond to changing oceanic conditions is of upmost importance for marine ecology and carbon cycling.”

Page 2, Line 32: I think that it’s important to include that the Beaufort et al (2011) study includes both modern samples and paleodata from the last 40000 years. Maybe just add “over long timescales”: “A known positive correlation exists over long timescales between surface-ocean. . ..”

Page 3, Line 6: replace “actually” with “recently”

Page 3, Line 15: Perhaps replace “species levels” with “overall coccolithophore calcification” since Beaufort et al. (2011) and Freeman and Lovenduski (2015) both have drawn conclusions based on overall coccolithophore calcification. While the Beaufort study has some species level information, the Freeman and Lovenduski study does not.

Page 3, Line 23: Break this sentence up into two sentences for clarity: “Accordingly, we calculated extant coccolithophore species numbers at different stations between 10 and 150 m of the water column and evaluated the coccolith mass variations of

E. huxleyi. We compared these observations with in situ conductivity–temperature–depth (CTD) measurements, carbonate chemistry parameters, as well as to previously published Southern Ocean coccolithophore and calcification datasets."

Page 3, Line 28: no need to capitalize "stations"

Page 6, Line 7: instead of "a taxon" say "one taxon"

Page 7: Line 22: Add references to Figures 4 and 5: "..., grouped into A (Figure 4) and B (Figure 5) according to Young et al. (2019). Also, by "Young et al., 2019" do you mean Nanotax3 website? It is unclear what reference this is referring to in the bibliography.

Page 7, Line 30: Type A overcalcified and Type R seem very similar to me. How are they different exactly?

Page 8, Line 23: When you say that Syracosphaera dominates in the SAZ, do you mean that it dominates among the rare coccolithophore assemblage or among coccolithophores overall? Please modify to be more specific.

Page 9, Line 7: Take out the extraneous "the" before 77.4%.

Page 10, Lines 25-29: Could silica be becoming more limiting north of the PF, opening a niche for coccolithophores? Perhaps competition among phytoplankton is another possibility of coccolithophores increasing in abundance at the PF.

Page 11, Line 10: rather than saying "up to 61.7°S", maybe it's more appropriate to say "down to 61.7°S"

Page 11, Line 21: misspelling of the word "coastal"; and instead of saying "... increasing towards oceanic regions" maybe say "... increasing towards open ocean regions"

Page 12, Line 12: change "communities" to "community"

Page 12, Line 19: The widely used E. huxleyi strain NZEH (morphotype R) and E.

huxleyi strain RCC1216 (morphotype R) were both isolated from around New Zealand so I believe that would count as “observing” it there too. For example, see Methods in Iglesias-Rodriguez et al. (2017) and Langer et al., (2009):

Iglesias-Rodriguez, Maria Debora, Bethan M. Jones, Sonia Blanco-Ameijeiras, Mervyn Greaves, Maria Huete-Ortega, and Mario Lebrato. "Physiological responses of coccolithophores to abrupt exposure of naturally low pH deep seawater." PloS one 12, no. 7 (2017): e0181713.

Langer, Gerald, Gernot Nehrke, Ian Probert, J. Ly, and Patrizia Ziveri. "Strain-specific responses of Emiliania huxleyi to changing seawater carbonate chemistry." Biogeosciences 6, no. 11 (2009): 2637-2646.

Page 12, Line 30: change “up to ca. 6°C” to “down to ca. 6°C”

Page 13, Line 20: change “decreases” to “decrease”

Page 14, section 4.3 in general: I think that the temperature as a controlling factor needs to be discussed more. It’s in the abstract (Page 1, Line 28/29) as a greater limiting factor than carbonate chemistry, which I totally agree with, but I think it needs more discussion in the paper. Are colder temperatures in the poleward direction selecting for lightly calcified species/morphotypes? Or could it be a physiological change induced by colder temperatures? I think a bit more speculation (perhaps bringing in some laboratory experiments) would be nice in this section. There’s a summary of the effects of temperature on coccolithophore calcification in Krumhardt et al. (2017): Krumhardt, Kristen M., Nicole S. Lovenduski, M. Debora Iglesias-Rodriguez, and Joan A. Kleypas. "Coccolithophore growth and calcification in a changing ocean." Progress in oceanography 159 (2017): 276-295.

Page 14, Line 20: misspelled the word “mass”

Page 14, Lines 20 – 25: It’s good that you pointed out the fact that the carbonate chemistry parameters have been estimated, rather than measured. However, the latitudinal

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gradients in carbonate chemistry parameters are pretty well established and I don't think it would affect the relationships you're seeing.

Page 15, Line 3: misspelled the word "Striking"

Page 15, Line 10. Here is where it would be good to speculate on the direction of change in coccolithophore abundance/calcification (or latitudinal species/morphotype shifts) with ongoing climate change. You could bring up the positive correlation with temperature shown in Table 4 and the PC analysis.

Figure 13: I like that you included this comparison to the Charalampopoulou et al. (2016) paper. Is the direction arrow on the right hand side of the figure supposed to say "East" rather than "West"?. I thought the Charalampopoulou transects were to the east of the present study. . .

Table 1 and Plate 1: I like that you grouped the E hux morphotypes into 2 main groups. There seems to be a fluidity between all these morphotypes and grouping into only 2 groups makes the information much more digestible.

Table 4: misspelling on the line HCO3- CO2Sys

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