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Interactive comment on "Distribution of planktonic biogenic carbonate organisms in the Southern Ocean south of Australia: a baseline for ocean acidification impact assessment" *by* Thomas W. Trull et al.

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Review on: "Distribution of planktonic biogenic carbonate organisms in the Southern Ocean south of Australia: a baseline for ocean acidification impact assessment " by Trull et al. In this study, Trull et al., investigate Diatom and calcifier distribution patterns in the Southern Ocean. Their analysis is based on BSi, POC and size fractionated PIC data. They compare their ground truth data with satellite data and model predictions and report important discrepancies and consistencies. I think their study is very valuable and their paper contains key information to document climate change effects on di-

atoms and calcifiers in the Southern Ocean. I really only have minor comments. Some of these are addressing their methods and some refer to the discussion/conclusion part.

Line 26: Are diatoms really the most abundant phytoplankton? I can understand that they might be dominant in terms of biomass but would intuitively assume that smaller groups (e.g. picoeukaryotes such as Micromonas) are more abundant than diatoms. (I may be wrong here but just to double check.)

Line 56: I am not sure that the under-saturation is primarily due to low TA. I would assume that it is due to the low temperature that leads to generally low carbonate ion concentration.

Line 63: It is a bit weird that you say that their relative importance is poorly known but then in the same sentence say that they will have an influence ecosystem health. The second part of the sentence implicitly contradicts the first part. Furthermore, I did not understand how the "importance" will "influence of the overall impact. . .". This sentence could perhaps be rephrased.

Line 80: Aren't these results? Perhaps move this sentence to results part. Furthermore, I do not understand the use of the second "suggested" in this sentence. Please check.

Line 91: In this context it may also be useful to remind the reader that the PIC50 fraction could also contain aggregated coccolithophore calcite (e.g. within fecal pellets).

Line 152: It would be helpful to know whether or not you expect a loss of CaCO3 by sieving the samples. Are there large quantities of CaCO3 expected in the >1000 μm size fraction?

Line 152: What do you mean by "ship clean"? Please clarify.

Line 154: Can you provide any information if the 50 μm filter tended to block when such a large volume is filtered? I am asking because it could be that towards the end of the filtration process also smaller particles might have been retained on the filter due to clogging. I know this is difficult to reconstruct, but in case you have any further information it would be useful to provide them. I have personally made bad experiences with sequential filtrations.

Line 158: I am a bit nervous about the PIC filter cleaning procedure. Omega is 0 in the deionized water and the pH is (probably) low. Does the deionized water have the potential to dissolve CaCO3?

Line 336: "as resulting" twice.

Line 402: I do not understand why mesoscale variability makes the comparison difficult. If you are at a certain location with a ship and sample PIC and you have satellite data for the very same time, you could easily compare these values, couldn't you?

Line 405: What is "e-folding"? The term has not been introduced.

Line 469: Dominant in terms of abundance? Dominant in terms of biomass would probably be the more important metric here.

Lines 470 and 473 : These results imply that diatoms (and to a limited extent coccolithophores) more or less exclusively contribute to the bulk POC in Antarctic waters. I am not so experienced with the plankton communites in the Southern Ocean but would intuitively disagree. Is it really possible that diatoms are so dominant? What about grazers? Did the analysis include e.g. copepod as a POC source or were these not captured on the filters? I think the result of bulk POC = diatom POC in the Antarctic is very interesting.

Line 480: Abundance of calcifiers or concentration of CaCO3? I think you should stick to the latter term to be more precise.

Line 482: You argue that PIC/POC is low which leads to little influence on the TA-mediated reduction of atmospheric CO2 uptake. I agree with that. However, PIC can induce biogeochemical feedbacks in other ways e.g. through ballasting (as you men-

tion in the paragraph before). So I think that it is not really valid to say that coccolithophores had a limited influence on the uptake capacity of atmospheric CO2 if you neglect other feedback mechanisms than TA reduction.

Section 3.4: In section 3.4 you compare model predictions with field data to test whether they predict meaningful trends. I think this is extremely valuable. I have, however, two comments.

1) You first use the Bach et al., 2015 and Langdon et al. 2000 models. These models only consider carbonate chemistry conditions and no other environmental parameter to predict calcification rates. Your data nicely shows that carbonate chemistry is obviously not the driving factor behind the latitudinal trend in the Southern Ocean because model prediction and latitudinal patterns are inconsistent. The Bach et al., model basically predicts that the carbonate chemistry conditions are close to ideal throughout the Southern Ocean. The Langdon et al., model predicts a decline which reflects the trend in Omega. Both models describe calcification response to carbonate chemistry and not distribution patterns of calcifiers. The reason why I mention this is because at the end of this part of the paper you state: "Thus, and unsurprisingly, coccolithophore abundances are clearly not controlled by inorganic carbon chemistry alone" (Lines 603-604). I could not agree more with this statement. However, the way this is formulated implies to some extent that your finding contradicts what we have concluded in our study. But this is not the case. In Bach et al. (2015) we wrote: "great care must be taken when correlating carbonate chemistry with coccolithophore dispersal because this is by no means the only parameter controlling it. Physical (e.g. temperature), other chemical (e.g. nutrient concentrations), or ecological (e.g. grazing pressure) factors will under many if not most circumstances outweigh the influence of carbonate chemistry conditions, unless differences in the latter are extreme. We will therefore focus the discussion on those cases where differences in carbonate chemistry conditions are rather extreme." Thus, our valuation is very similar to that of the authors of this manuscript. I would appreciate if you could point out that your main conclusion in this

paragraph (that carbonate chemistry is probably not the key factor controlling coccolithophore distribution) is also in line with (and not conflicting with) what we assumed in our studies.

2) I am a bit skeptical about the growth rate vs. temperature argument based on the Norberg model. The model predicts a decline of coccolithophore growth rates due to decreasing temperature. This in itself is not convincing because the decrease of growth rate would apply for every other phytoplankton group as well. What you would really have to look at is if the growth rate of coccolithophores decreases over-proportionally relative to other phytoplankton species. If this was then case, then you could argue that coccolithophores become less competitive the further South you go.

Line 653: In this concluding remark you only consider the bottom-up control on diatom vs. coccolithophore distribution. Have you also considered if top-down mechanisms could have played a role here? Even though there may not be appropriate data available to test this in the present study, it may still be useful to remind the reader that this mechanism exists and could also have played a role. I think the Assmy et al., (2013) study nicely made the case that predators may have an important influence on phytoplankton composition in the Southern Ocean.

Table 1: I think the uppercase 3 also needs to be added to PIC01, POC, and BSi.

Figure 1: It would be helpful to add full names and abbreviations of the various fronts to the figure caption.

Figure 3: One particularly interesting finding presented in Figure 3 is that PIC50 (foraminifera) concentrations are considerably lower than PIC01 (coccolithophores) concentrations except for maybe the most Southern stretch of the transects. Sometimes the discrepancies are orders of magnitude. This implies that coccolithophores are the much more important pelagic calcifiers in the Southern Ocean than foraminifera and pteropods. Is this conclusion valid? If so, I think this finding definitely deserves more attention in your paper. Furthermore, it would be interesting to compare this with

the results from Broecker and Clark (2009) who found roughly equal contribution of coccolithophores and foraminifera to the sediment CaCO3 (although their most southern sample came from 40° South).

Figure 4: What was the rationale of showing the POC/PIC ratio? I think readers will generally be more familiar with PIC/POC ratios.

I hope my comments help to further improve the manuscript.

With kind regards,

Lennart Bach

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