Manuscript: egusphere-2023-2801 - response to reviewers

Dear Jamie,

Thank you for providing an opportunity to respond to the detailed comments by two reviewers. Here, we provide our response to reviewer 2 comments, including the action that will be taken in a revised manuscript (much of which we have already done in preparation, such as the addition of better error analysis). The original reviewer comments are provided in black and italics; our responses are in red.

Best wishes,

Mariem Saavedra-Pellitero and co-authors

REVIEWER #2

Summary evaluation and general comments

I think the study is valuable and, after some major revision, should be published. One overall issue is with the way they focus the study in the Title, Abstract and Intro goes into the weakest result where they do not succeed in resolving things so well. They clearly consider the greatest importance to be the comparison of the in-situ and satellite data, but I think they end up raising more questions in this sense than they succeed in resolving. A major deficiency is the lack of direct discrete measures of PIC by chemical analysis with to which to compare microscopy-based and satellite estimates of PIC. Without such data, they really can't complete a proper comparison. I recognize that it is not always trivial to get or process such samples, and I see some other points of value.

The strongest findings are that

a) coccolithophores are not contributing significantly to the apparent high PIC estimated by satellite in the Antarctic Zone, but are important further north;

b) they strengthen what we know of the biogeographic patterns in the high latitude southern hemisphere, picking two very important but quite distinct part of the circumpolar circulation;

c) there are uncertainties and disagreements between methods for estimating coccolith-PIC using SEM and or polarized microscopy and they provide information that allows evaluating that disagreement.

The first of those three points, that the AZ satellite signal assigned to PIC is not from coccolithophores, is both one of the most important results, but it is also the only part where they do a strong comparison between satellite and microscope-based approaches. In the rest, they do not complete the comparative analysis with a statistical comparison of the different approaches.

Consequently, my recommendation is that the authors should be encouraged to submit a major revision, and I suggest to them to change their focus to the points where they generate robust results that are nevertheless valuable.

We thank Reviewer 2 (R#2) for their helpful suggestions that will lead to a much improved manuscript. We provide our response to each point below.

Major specific comments:

Lines 101-106 and later (generally). This study seems to combine previously published data with new analysis, specifically with new analysis that relies on samples which were analyzed more deeply. It would help to cite that or those previous studies from these transects in this paragraph. I think early in the Methods there should be a first table defining where the samples come from, including noting in which previous studies they have been used in.

A new table will be added that includes this information. Also, we will highlight the already published datasets in figures 3 and 4.

Lines 191-206: 3.3 Satellite-derived PIC data

I think it is interesting to compare in situ data to the output distributed from the NASA group, but then I am not sure what exactly they did. Did they compare a single pixel to each discrete sampling?

Bailey and Wedell (2006) outline the method used by NASA Ocean Biology Processing Group (OBPG) for validating operational ocean colour missions. According to the authors, they would have compared the mean of a 5x5 window, centred on the in situ location. The in situ data utilised was extracted from SeaBASS and the Aerosol Robotic Network - Ocean Color (AERONET-OC). Validation results for MODIS-Aqua PIC are available on NASA's Ocean Color Portal (https://seabass.gsfc.nasa.gov/search/?search_type=Perform%20Validation%20Search&val_sata=1&val_products=9).

Beyond showing later that there is a region of very high satellite signal with low or absent coccolithophores, I suspect this sampling is not really sufficient to do a true comparison of the satellite vs discrete measures.

R#2 is right to point out that the sampling is not sufficient to do a true comparison, particularly because the discrete measurements do not provide analytically-measured concentrations of PIC. Additionally, the satellite-derived PIC data is scarce due to cloud cover. See also reply to Reviewer 1 (R#1) as well as figures R1 and R2 in that response.

Section 4.1.

Lines 233-237: "Weekly and monthly MODIS-derived PIC at the sampling locations consistently overestimate PIC values… with respect to in-situ values calculated from coccolith mass" and "there is a relatively good agreement in the latitudinal satellite and coccolith-PIC trends in the SAZ and PFZ" and again at lines 292-293: "In the studied transects, the calculated coccolith-PIC and the satellite-derived PIC trends show quite good agreement in the SAZ and PFZ,"

This is a possibly highly subjective, evaluation of correspondence has been made. What is "good agreement"? Sometimes when coccolith-estimated PIC is low, satellite-estimated PIC is also low, and when coccolith-estimated PIC is higher, satellite-estimated PIC is also higher, but, even in the SAZ and PFZ they do not track precisely. I do not think they could track precisely even if all PIC was from coccoliths and the estimations were completely faithful, because one is a very discreet estimate at a single sample in a single Niskin bottle, @and the satellite value is an average over at least many days. So they can't correlate perfectly. Nevertheless, to make the statement it would be nice to see a direct analysis of the correlation between coccolith-PIC (calculated) and satellite-estimated PIC. That is, I'd like to see figure, combining both datasets, plotting one against the other.

This point was also raised by R#1. This analysis will now be included in the supplementary material.

Also, the phrasing could be more careful in lines 233-244, because "consistently overestimate PIC values" is only correct if one knows that most PIC comes from coccolithophores instead of other sources, which might not be true. Also, it is not clear if there is a way of knowing the error on the coccolithophore PIC estimation.

We will reword these sentences and add information about potential errors relating to other sources of PIC as well as standard deviation in the text, figures and tables. This was also raised by R#1.

There are several reasons why the estimates could not agree. Here are just some principal possibilities:

Perhaps the satellite estimates in fact are right, but a lot of coccolith PIC is in the form of coccolith fragments that are too fragmented to be recognized and counted, so microscopy estimates would be lower than the "true" value

Perhaps there is another PIC producer (although that possibility was implicitly considered in the Intro)

Perhaps the assumptions in the coccolith-PIC calculations are actually wrong (e.g., the shape factor is different than estimated, or the number of coccoliths per cell assumed is wearing)

In the Discussion, the authors do touch on some of these and other possibilities, but I would like to suggest they might try a modest reorganization so as to make it clear to go one by one through the possibilities to consider.

We are very grateful for those suggestions. We will reorganise the discussion to incorporate these points.

Line 270: "The relative tube width' (an index for calcification; Young et al., 2014), calculated using equation 2, varies from…"

We will amend (now *relative tube width).*

How do you deal with the issue that the "overcalcification" is variable? The tube width in the "overcalcified" forms tends to be very irregular, and sometimes the central area is covered nearly fully. This should be made explicit, perhaps with a new panel in Fig. 2 or a supplementary figure associated with the Methods section to show how this complicated situation (like the case in Fig. 6 bottom left) is dealt with.

We agree that the calcification of the central area is variable, just as all measured parameters of the placoliths are highly variable. "Overcalcified" specimens were in general rather rare, but as we have assumed an average value for all other parameters, we have calculated a mean ks value that is higher for this morphotype than for the other morphotypes (see Table 1).

This data is available in the supplementary material submitted to Biogeosciences (Tables S1 and S2). It is also stored in Pangaea: https://doi.pangaea.de/10.1594/PANGAEA.964672 and https://doi.pangaea.de/10.1594/PANGAEA.964674 -but still under moratorium-.

We did not add any extra panel or supplementary figure, because the other morphotypes are more dominant (see cells/L in figures 3 and 4).

I have some concerns about the PCA and how it is discussed.

First, the PCA does not seem to be very successful at separating the coccoliths based on morphometric parameters. Except for the overcalcified forms, which separate only based on tube width (needs to be clearer how that is really measured, as discussed above), the other forms overlap a lot. In fact, component 2 in the PCA, which correlates mostly with other morphometric characters quantified, does not appear to separate the morphotypes at all.

*Some of the characters that have been used for separating types A and B are not analyzed, and might be quite difficult to analyze automatically (such as central area type, or whether the distal shield elements or straight or not). I think it is definitely valid to do and report the PCA, but it is important to discuss the fact that this analysis, using the current state-of-the-art quantification, seems to fail at what are visually quite striking differences is very notable. This suggests that perhaps it is time to try new approaches to disting*uishing these types.

Also following a comment from R#1, we will remove the PCA section from the revised version. We will incorporate the other very interesting points raised here in the discussion of the revised manuscript.

Second, the issue also makes me wonder if we have independent estimates of how well the approaches of trying to estimate coccolith PIC by morphometry or by polarized microscopy work when comparing different morphotypes. It seems that, while there are many studies that directly quantity PIC in culture (either as acid-labile particulate carbon or as particulate calcium), it is hard to find any that also count the number of coccoliths to measure PIC/coccolith. It might be worth highlighting this need. I should mention, and not parenthetically, that I appreciated a lot the analysis they showed in Fig. 8, which goes directly to this point. I think their data suggests we are still lacking precision in the way we measure coccolith PIC, and I would hope that, while their study might not resolve it, at least it helps identify the problem.

Good points, and we will elaborate further on this topic in the new version of the manuscript.

Third, in lines 346-347 they say: "The PCA performed on the E. huxleyi morphometric dataset shows that those heavily calcified type A coccospheres occupy a relatively restricted ecological niche offshore of Chile" The PCA is only based on morphometric characters, regardless of the location or oceanographic conditions in which they are found. How can such a PCA, which does not include any environmental information, indicate anything about whether the ecological niche of one form is restricted or not?

Good point. We know that just because previous published papers dealt with the different morphotypes and the environmental preferences (add citations here), but they were not included here. That is why we decided to (initially) focus on the PIC and leave the PCA out of the main story. Ultimately, and in response to a R#1 comment too, we have decided to remove the PCA aspect.

For Figure 1, and the corresponding description in the Methods (lines 108-122), I could not tell if the white lines correspond to average positions of the fronts or to the positions the fronts occupied at the time of the study.

We will include a modified figure that uses a colour blind friendly palette and make clear that they are average positions (based on Orsi and Harris, 2019).

Minor technical and comments and corrections

Abstract: Eliminate page breaks within abstract

It will be done

Line 84: Perhaps reserve "concentration" for chemicals (such as PIC) and "abundance" for cell or coccolith numbers/volume. It's not strictly necessary, but it might help to be clearer

It will be done

Line 88-89 : " available coccolithophore concentrations" maybe clearer "available measurements of coccolithophore abundances"

It will be done

Line 98: "have targeted areas of coccolithophore bloom". Should be "blooms" (plural)

It will be done

172-173: "and modified for E. huxleyi according to the degree of calcification obtained for each morphotype (see Table 1)" How is this modification of KS according to calcification performed? Is there a reference?

The different shape factors used were based on the identified morphotype following Young and Ziveri (2000): ks = 0.02 for morphotypes A and B/C and ks = 0.04 for morphotype A overcalcified. The shape factor for morphotype O (ks = 0.015) was introduced by Poulton et al. (2011) in a plankton study along the Patagonian Shelf for a morphotype with a central area described as an "open or thin plate" which the authors called type B/C but that we identified as morphotype O based on the published images and description of Hagino et al. (2011).

The dataset linked to this answer is available in the supplementary material submitted to Biogeosciences (Tables S1 and S2). It is also stored in Pangaea: https://doi.pangaea.de/10.1594/PANGAEA.964672 and https://doi.pangaea.de/10.1594/PANGAEA.964674 -but still under moratorium-.

The reference that compiles that information is Vollmar et al. (2022) and it has been included in the new version.

Vollmar, N. M., Baumann, K.-H., Saavedra-Pellitero, M., and Hernández-Almeida, I.: Distribution of coccoliths in surface sediments across the Drake Passage and calcification of Emiliania huxleyi morphotypes, Biogeosciences, 19, 585–612, https://doi.org/10.5194/bg-19-585-2022, 2022.

See also our response to R#1 for further details.

Line 178: "Measurements of the distal shield diameters of Calcidiscus leptoporus the second most abundant species" Need a comma after "leptoporus"

It will be done

Line 265-266: "Note that this data is shown in Figures 3 and 4, but the coccolith-PIC was calculated in this work using equation 1 and the average lengths mentioned in Table 1 " Not clear.

All the data is shown in Figure 3c and 4 c, but to estimate the coccolith PIC, the average length was considered. The sentence will be modified to make this clearer.

Line 270: We observed that some coccoliths are clearly overcalcified (see Figure 5 for an example),…" Do you mean Fig. 6? Fig. 5 doesn't have any photos so can't see that observation.

Yes, that is a mistake. We meant Figure 6. Thanks for spotting it.