Response to the comments of reviewer 2

Summary

In this study, Losa et al. present a version of the DARWIN model, which they modified for the Southern Ocean (SO) application presented in this manuscript. In order to better represent the SO phytoplankton community structure, which mainly consists of silicifying diatoms, calcifying coccolithophores, and colony-forming Phaeocystis, the authors have added a second, lightly silicified diatom plankton functional type (PFT) to their model (in addition to a heavily silicified one which was already included in the model before) and have made small modifications to the parametrization of coccolithophores in a first step (their reference simulation).

Subsequently, motivated by problems in keeping both coccolithophores and Phaeocystis alive in their reference simulation, the authors have implemented a life cycle switch (based only on the surrounding iron concentrations) for the Phaeocystis PFT to simulate both solitary and colonial forms of this phytoplankton type (PHAEO simulation). In this manuscript, the authors present a comparison of the simulated phytoplankton community structure to those suggested by satellite-based PFT algorithms and pigment data (the latter for the PHAEO simulation only). In my opinion, the model development study by Losa and co-authors is valuable, as current global models often struggle to correctly represent the SO phytoplankton community. Efforts to improve upon this are needed, given the importance of this ocean basin for global biogeochemistry and climate.

I think the manuscript is in principle suitable for publication in Biogeosciences. However, I cannot recommend the publication in its current form, as I have serious concerns surrounding the model behavior (the extinction of individual PFTs at the end of the reference simulation is worrisome). Furthermore, I think that 1) the chosen PFT parameters and changes done to the model have to be better motivated in the SO context of this study, 2) the used model parameters and parametrizations need to be better documented throughout the manuscript and limitations need to be discussed (especially surrounding the parametrization of the life stages of Phaeocystis), and 3) the impact of the changes and chosen parameters should be more thoroughly assessed by targeted sensitivity simulations. Below, I first summarize my comments into a few general points and then list all my detailed comments, which should be addressed before the manuscript can be accepted for publication.

We thank the reviewer for the constructive comments on the manuscript. Our author's replies are presented in blue, labeled "R:" and follow each reviewer's comment. The changes in the revised manuscript according to the suggestions are presented in blue.

General comments

Below, I will list my general comments, which should be thoroughly addressed before the manuscript can be published:

1) The "extinction" of either coccolithophores or Phaeocystis (Antarctica) in the presented reference simulation deeply worries me. Before this manuscript can be accepted for publication, the authors should understand where this is coming from and fix it, as I currently do not understand how this can happen, given that (based on observational data) their biogeographies in the SO do not overlap completely in space and time (meaning that there should be room for both to exist). Since this model behavior implies a substantial drift in the biomass distributions in the simulations assessed here, it can be expected to lead to a substantial sensitivity of the presented results to the chosen analysis year (see also point 7).

R: As the reviewer mentioned "based on observational data … that there should be room for both to exist". The question addressed in this paper is what exactly makes/provides this room and how well (if ever) this represented in the model. For experiment REF as well as for other sensitivity experiments (overviewed in the Supplementary Material) there were not sufficiently enough differences between the traits assumed for coccolithophores and "other large" (or *Phaeocystis* analogue). As a result, it took longer for the model to get in a quasi-steady state and finally lead to just one of "similar" PFTs survived (taking over for another PFTs). Thus, in experiment REF coccolithophores do not survive and *Phaeocystis*-analogue indeed represents haptophytes in general. Hence, the experiment REF represents diatoms and haptophytes after reaching a quasi-steady state, but cannot distinguish among haptophytes. In original Darwin-2015 model (Dutkiewicz et al. 2015) "other large" did not survive. In this respect, PHAEO configuration with additional differences introduced in the traits of these PFTs, is the fix.

We explained better in the revised manuscript (L346 – 350).

However, we understand the reviewers concern and realize that it was a mistake to show results such as this without fully explaining the point. We now only show results from after the quasi-steady state.

Furthermore, based on the information included in the current version of the manuscript, I don't understand how the subsequent changes made to the parametrization of Phaeocystis (i.e. including life cycle transitions) solved this problem, which should be discussed in more detail by the authors.

R: The additional differences introduced in the parametrization of *Phaeocystis* makes coccolithophores competitive among phytoplankton of larger cell size (or colonies) that requires higher nutrients concentration to grow and/or among PFTs of similar size (small diatoms and *Phaeocystis* solitary cells) that have of higher palatability factor to be grazed.

This is now more clearly stated in the revised version (L 584 – 589)

2) In the method section, a detailed description of the assumptions surrounding the parameter choices of the different PFTs as well as laboratory studies backing up the chosen numbers (Table 1) is currently lacking.

R: Table 1 contains only the parameters used in the parametrizations crucial to drive the differences/diversity in the considered PFTs traits. Most of the biogeochemical model parameters were taken from Dutkiewicz et al. (2015) and from detailed laboratory studies conducted by (Trimborn et al. 2017). We clarified it in the revised version of the manuscript. In the Supplementary Material (Tables S1-S4) we compile information on the parameters chosen for the various model configurations exploited within this study.

We added the following sentence in the text (L153–155):

"Note that most of the biogeochemical model parameters used in our study are taken from the original study by Dutkiewicz et al. (2015) and from detailed laboratory studies conducted by (Trimborn et al. 2017). Hence, Table 1 contains only the parameters used in the parametrizations crucial to drive the differences/diversity in the considered PFTs traits"

Section 2.1.1 and Table 1 are currently incomplete in their description of the parametrizations and parameters used in this study (i.e. e.g. some parameters are missing, no units are given).

R: Units were provided in the text introducing model parameters in the parametrizations (pages 4 and 5, Section 2.1.1). We revised Table 1 and now it also includes the units.

More specifically, regarding the coccolithophores, the authors do currently not motivate why the applied changes to the parametrization (as compared to previous global applications of DARWIN) are justified for the SO (e.g. by relating them to the coccolithophore community in this ocean basin).

R: Indeed, we first mentioned the parameter modifications in lines 93-95 of the original version (supported by references Nejstgaard et al. (1997), Huskin et al. (2000), Paasche, 2001; Iglesias-Rodríguez et al., 2002) and explained them in more detail in the section 3.3, lines 254 – 265 and 272 – 277 of the original submitted manuscript. The discussed changes in the parameters for coccolithophores such as palatability factor and low half-saturation for nutrients are in consistence with what is, generally, known about this PFT. Moreover, in the study by Monteiro et al. 2016 a version of the Darwin model was applied also globally, and the authors reported and justify, for instance, that grazing protection (introduced via palatability factor) appeares to favor coccolithophores in (sub)polar regions.

We improved the text to clarify it (L384-393):

"Our assumptions on low palatability factor of coccolithophores are, nevertheless, backed up by the studies of Nejstgaard et al. (1997), Huskin et al. (2000), Losa et al. (2006) and Monteiro et al. 2016. Based on their laboratory experiments, Nejstgaard et al. (1997) and Huskin et al. (2000) concluded that coccolithophores do not influence the microzooplankton growth due to its "stony" structure. In the study by Losa et al. (2006) on optimized biogeochemical parameters the authors showed that the

coccolithophores bloom was associated with low grazing pressure. While the exact mechanisms of how this PFT use the coccolith to protect itself against grazing is not fully understood (Monteiro et al. 2016), the ability of coccolithophores to escape grazing control has "relatively well-supported evidence" (see Monteiro et al. 2016 for review). In addition, the high affinity of coccolithophores for nutrients (for phosphate and iron to a larger extent than for nitrogen, Paasche 2001) makes them strongly competitive in environmental conditions with declining nutrient concentrations (Paasche, 2001; Iglesias-Rodríguez et al., 2002), for instance under strong ocean stratifications or nutrient consumption by other PFTs."

We know also included references to studies by Krumhardt et al. (2017) and Krumhardt et al. (2019).

In the introduction we also added that (L64-71): "Coccolithophores biogeography was investigated globally by Monteiro et al. (2016), Krumhardt et al. (2017) and Krumhardt et al. (2019) and particularly for the Southern Ocean by Nissen et al. (2018). With respect to specific coccolithophores traits, the study emphasized the high nutrient affinity of the coccolithophores (Krumhardt et al. 2017) and high grazing protection of this PFT (Monteiro et al. 2016). Nissen et al. (2018) reported on higher grazing pressure on coccolithophores relative to those on diatoms. While in the study by Krumhardt et al. (2019), the authors used low grazing pressure on coccolithophores relative to those on diatoms. Krumhardt et al. (2019) related the distribution of coccolithophores to a specific temperature function of its growth rate."

In the introduction we also now explicitly state as one of the hypotheses we test in the study (L84-88):

"Distribution of coccolithophores in the Great Calcite Belt is not necessarily controlled by temperature (Smith et al., 2017) but determined by the ability of this PFT to escape grazing because of their exoskeleton (Nejstgaard et al., 1997; Huskin et al., 2000, Monteiro et al., 2016), and to grow under nutrient depleted conditions (especially phosphate and iron) (Paasche, 2001; Iglesias-Rodríguez et al., 2002, Krumhardt et al., 2017). These characteristics of coccolithophores would make them more competitive among other phytoplankton of larger or similar size, small diatoms and *Phaeocystis*."

Regarding Phaeocystis, the manuscript could be greatly improved by including a more thorough discussion on the limitations of their current parametrization in the model, as important aspects surrounding their life stage transitions (e.g. light) are currently not accounted for.

R: Thank you for the suggestion. In the revised version of the manuscript we introduced a section "Limitation of the study" and extended the discussion on limitations regarding *Phaeocystis* (L528-533), were we state that the light was not considered according the recent findings of Bender et al. (2018):

"*Phaeocystis* colony formation: in this study, we use very simplistic approach to parameterise life cycle transition of *Phaeocystis* given just one model tracer. In our model this transition is triggered only by iron variability (as reported by Bender et al. 2018), but not by light availability (as previously reported by Pererzak, 1993). Since we reported on our first trial, it is worth keeping in mind that the model is expected to be sensitive to the differences we specify for the mortality and grazing

rates and iron uptake for colonial and single cell stage. A careful model calibration of these parameters could further improve the model performance."

Additionally, the authors should comment on the usefulness of simulating both life stages within a single model tracer, as this is important information for those wanting to implement Phaeocystis into their own model.

R: We were motivated by the necessity to prescribe additional differences in the traits assumed for coccolithophores and *Phaeocystis*. It was the simplest approach we came up by following the approach of Popova et al. (2007) and the study by Bender et al. (2018).

We commented on that now in lines 165-168.

"Note that in the model *Phaeocystis,* independent of the life stage – colonial phase or solitary cells, – is considered as one tracer. However, the assumed morphology and, therefore, physiology (mortality rate, rj,k, ksatF e, sinking rate) differ as described above. We have not performed any sensitivity experiments with respect to the new parameters. However, we expect the model to be sensitive to their specification since it will also determine the competition between *Phaeocystis* and small diatoms."

We further commented on limitations (L528-533) as written above.

Nevertheless, the results shown and discussed allowed us to conclude (L582-589):

"This parameterization of morphological shifts indeed allows for co-existence of the two types of haptophytes corroborating our third hypothesis on the dependence of *Phaeocystis sp*. life stages on iron availability. By considering two life stages of *Phaeocystis* we introduce additional differences in the traits, which along with assumed physiological parameters for coccolithophores makes coccolithophores competitive among phytoplankton of larger cell size requiring higher nutrients concentration to grow or/and among PFTs of similar size – small diatoms and *Phaeocystis* solitary cells – but of higher palatability factor to be grazed. These additional differences in the traits of distinct haptophytes, coccolithophores and *Phaeocystis* allows these groups to coexist (e.g. along the Subantarctic and Polar fronts)."

Furthermore, the manuscript currently lacks a sensitivity analysis assessing e.g. the impact of the changes applied to the coccolithophore parametrization (in order to support what is in my view currently largely a speculation on the drivers of their biogeography in their model as important plots are not shown) or the impact of parameter choices (e.g. regarding those of Phaeocystis) on the simulated biogeography.

R: Within the scope of testing the formulated hypothesis (now explicitly written in the introduction – see L79-90), several sensitivity experiments have been performed. In the Supplementary Materials, we only reported on the simulations that most contributed to obtain the concluding results. Moreover, the changes in the coccolithophores physiological parameters are strongly backed up by previous studies (please see our responses to the detailed comments). As about the sensitivity to the

traits (parameter choices) specified for *Phaeocystis,* experiment REF is considered as one of the sensitivity studies. And the comparison of the final PHAEO experiment to REF illustrate allows to infer on the impact of the assumed traits on the simulated PFT biogeography. (Please see our responses to detailed comments)

3) In general, important results (e.g. the change in the simulated phenology when implementing a second diatom PFT or the drivers of the simulated coccolithophore biogeography) are currently getting a bit lost in the manuscript. As these aspects are highly relevant for the modeling community and are the parts for which the manuscript goes beyond a pure model development paper, these aspects deserve more room (in text and figures).

R: We now try to straighten out the presentation and discussion of our results on consequences of including small diatoms, for instance by explicitly showing and discussing the diatom phenological indices in line with Chla distribution of small and large diatoms.

We present figure R.2.10 depicting spatial distribution of small diatoms at lower latitudes and large diatoms at the higher latitudes of the Southern Ocean (as it was also shown in figure 3 of the original manuscript). Figure R2.8 presents phenological indices for the Southern Ocean diatoms showing, for instance, earlier bloom start date and Chla maximum date for small phytoplankton and later bloom start and maximum date for larger diatoms abundant at higher latitudes. When compare the phenological indices with dominance plot, it is seen that the PFT dominance plots, indeed, to some extent reflects the PFT phenology. It is why when discussion on model deficiencies in reproducing PFT phenology (and PFT composition) and reporting on the main results of sensitivity tests (Supplementary Material) we showed PFT dominance plots. The PFT dominance that we show in the main manuscript and agreed (qualitatively) better with the PHYSAT dominance (among different sensitivity experiments) was only possible to obtained by considering two size classes for the diatoms.

We also would like to emphasize more on the results presented in Figure 5 (Figure 6 in the revised version) in support to the the discussion on coccolithophore biogeography. This figure depicts Southern Ocean spatial distribution of diatoms, coccolithophores and *Phaeocystis* along with silica, iron and phosphate for a particular March 2004 (in the revised version we will show March 2008 or February 2008). We chose to show a particular month of a year (could be any after the steady state) but not climatological monthly mean to clearly show patterns of the distributions: 1) the abundance of coccolithophores in the area with very low phosphate; 2) co-existence of this PFT with small diatoms north of the subantarctic front where silica is presented in lower concentrations than in higher latitudes but still sufficient to support the growth of small diatoms and co-existence coccolithophores with *Phaeocystis* solitary cells north of the Southern Antarctic Circumpolar Current Front in the areas with low iron concentration. In first case coccolithophoes can grow due to high affinity of coccolithophores to phosphate and iron. In second case it survives due to lower palatability factor that makes the coccolithophores competitive with small cells of diatoms *Phaeocystis*.

Currently, the conclusions drawn by the authors are not fully backed up by the simulations that are discussed and the plots that are shown in the manuscript, making it often impossible for the reader to evaluate what the authors base their arguments on.

R: We revised the manuscript to make it clearer. See our responses to the specific comments below.

4) Throughout the manuscript, the authors use the term "phenology", which typically refers to the annually reoccurring characteristics of the phytoplankton biomass evolution and can be characterized by the timing of e.g. the phytoplankton bloom start or the bloom peak. However, in the current version of the manuscript, "true" phenology is never presented and often only individual months of the simulated biomass fields are shown and discussed, which gives no information on the phenology (additionally, a definition of "phenology" and how it is assessed is missing in the method section).

R: We opted to remove the term phenology in the paper since we were using it to refer to the dynamic of the PFTs but without explicitly showing phenological metrics. We actually have calculated the metrics but including these results plus discussion would make this manuscript too long and this subject will be explored in another paper.

Figure R2.8 and R2.9 show the phenological indices.

In order to e.g. emphasize the importance of including two diatom PFTs in a SO model (where by the authors claim to have fixed the problem of many models, namely too early blooms), the authors should show the simulated phenology metrics in the revised version of the manuscript (e.g. maps of bloom timing in the "old" model version as compared to the improved setup and those derived from satellites).

R: As mentioned above, the initial idea of the manuscript was also to include information on the timing of the phytoplankton blooms but we realized that the study would be too complex and diverse on topic to be summarized in one manuscript. We plan a dedicated study on the phenology of the PFTs blooms in the Southern Ocean soon.

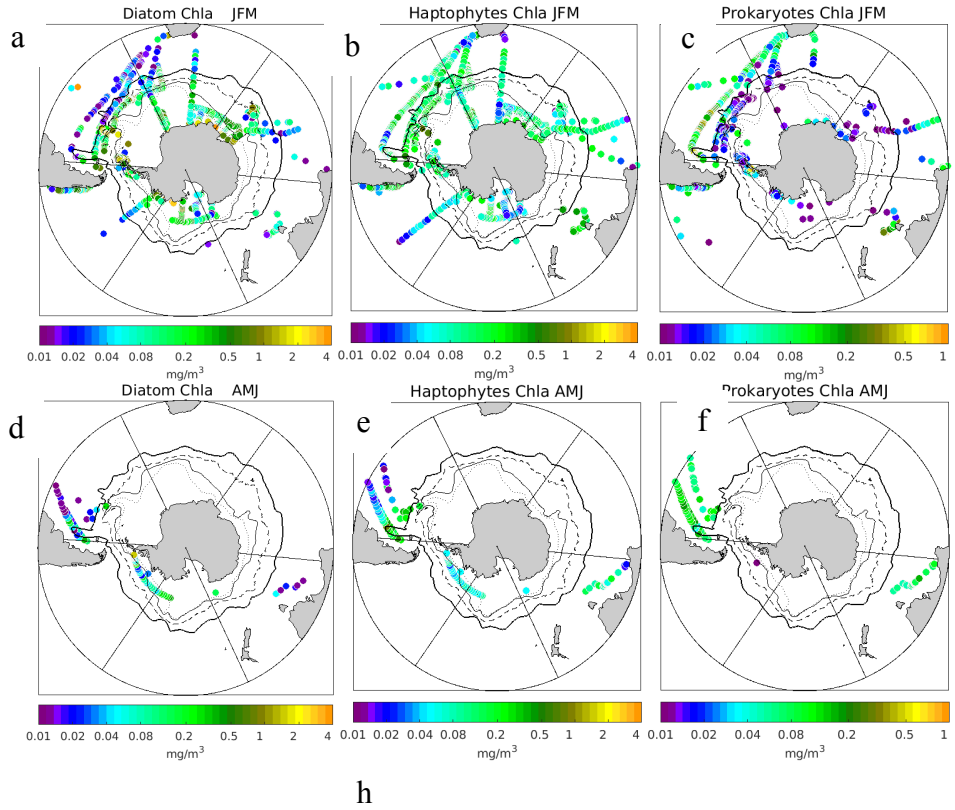
5) Throughout the paper, the authors present very little quantitative evaluation of the simulated phytoplankton distributions, which should be improved in a revised version of the paper. Currently, the included HPLC data are only used for the PHAEO simulation (by plotting the observational data as scattered dots on top of maps, which is very hard to evaluate for the reader), but should also be included for the "old version" of the model and the reference simulation in order to actually show the asserted improvement in model performance.

Additionally, the HPLC data can and should also be used for a discussion of the phytoplankton community structure to complement the satellite-derived products.

R: The assessment of experiment PHAEO is also backed up with the statistics of goodness of model-to-data fit presented in tables 3 – 5 (main manuscript) and tables S9-S11 (Supplementary Material).

We did not include the evaluation of the "old version" (Dutkiewicz et al. 2015) against in situ HPLC-based data because: 1) these results were obtained as climatological mean values only, which makes it difficult to get properly (without large representation error) match-ups between model and in situ data; 2) these simulations did not fulfill one of the evaluation criteria which is the agreement with observational PFT dominance. The reference run (REF) agrees sufficiently well with HPLC based haptophytes and diatoms (statistics can be provided), however it does not distinguish between coccolithophores and *Phaeocystis*, so is not adding more information to it, it is why we do not show the statistics in the manuscript.

However, we do like this excellent idea to emphasize more on the usefulness of the HPLC data. Thus, in the supplementary material, we provide additional figures depicting seasonal composites of the PFT-Chla derived from the HPLC measurements from August 2002 to April 2004 (Figure R2.1, Figure S12 in the Supplementary Material).



h

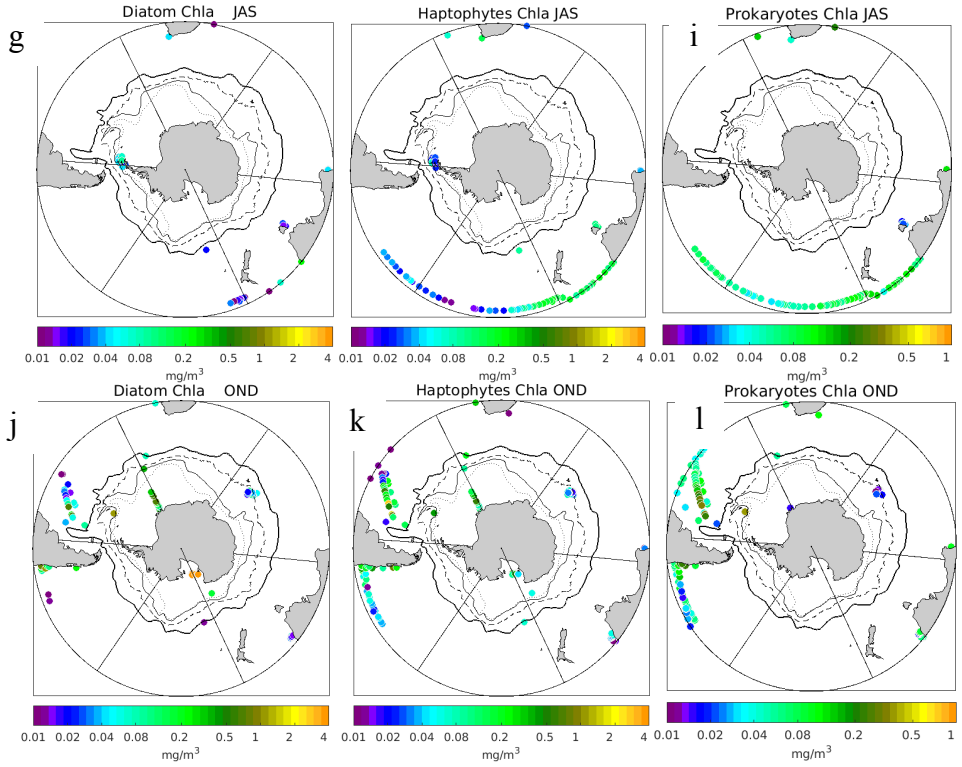


Figure R3.1. Distribution of seasonal composites of HLPC-Chla (Soppa et al.2017) for diatoms, hyptophytes and prokaryotes. Black counters represent Southern Ocean fronts (as white contours in Figure 1 of the manuscript.)

R: HPLC-based PFT-Chla date were used for a quantitative assessment of the PHAEO model. To make the original discussion (lines 329-340) more visible we have edited the text (L467-495):

"We have obtained matchup statistics for the comparison of our PHAEO model results against the in situ HPLC-based PFT Chla observations by Soppa et al. (2017). The mean absolute deviation (mean absolute error, MAE) of collocated model and in situ PFT-Chla over the considered time frame (August 2002 – April 2012) and the entire Southern Ocean is 0.74 mg m^{-3} and 0.22 mg m^{-3} for diatoms and haptophytes, respectively. Tables 4 and 5 present the statistics of model and in situ PFT-Chla comparison at several Longhurst's biogeochemical provinces (Longhurst 1998, see Figure 1). The highest disagreement was obtained for diatoms in the Atlantic Sector of the ANTA province, where the simulated diatom Chla is systematically overestimated by ~0.5 mg m^{-3}. The best agreement with the HPLC based diatom Chla (excluding small provinces, see Figure 1) was obtained at the SSTC and SANT. For the haptophytes, the highest systematic error towards overestimation has been found at two small provinces east of Africa and Australia (EAFR and AUSE) with the bias = 0.57, 0.48 (mg m−3), respectively. The highest random error is (RMSE = 0.62, 0.44 mg m−3) at EAFR and APLR. The lowest differences between predicted and observed haptophytes was at the FKLD, SSTC provinces where haptophytes are mostly presented by coccolithophores, and at

the SANT biogeochemical province, where they both co-exist. As additional information on the agreement between model and observations, Figures S9 and S10 in the Supplementary Material present frequency distributions of diatoms and haptophytes Chla for the simulations and measurements as well as the frequency distribution of the model and data differences. The latter shows that statistical criteria, such as MAE and root mean squared error (RMSE) give statistical meaningful metrics with respect to "model minus in situ Chla data" and the evaluation does not necessarily require a logarithmic transformation, as it is often done in ocean colour product validation (Brewin et al., 2010; Losa et al., 2017).

With respect to the agreement between model and observed in situ HPLC-based Chla for prokaryotic pico-phytoplankton depicted in Figure S11 (in Supplementary Material) one can conclude that the frequency distributions of the simulated and observed pico-phytoplankton are different, and the frequency distribution of the differences confirms that MAE and RMSE given absolute (Table 5) or logarithmically transformed values can hardly provide satisfactory estimates. Nevertheless, it is worth emphasizing that the largest differences between model and observed in situ pico-phytoplankton are located along the Antarctic Peninsula.

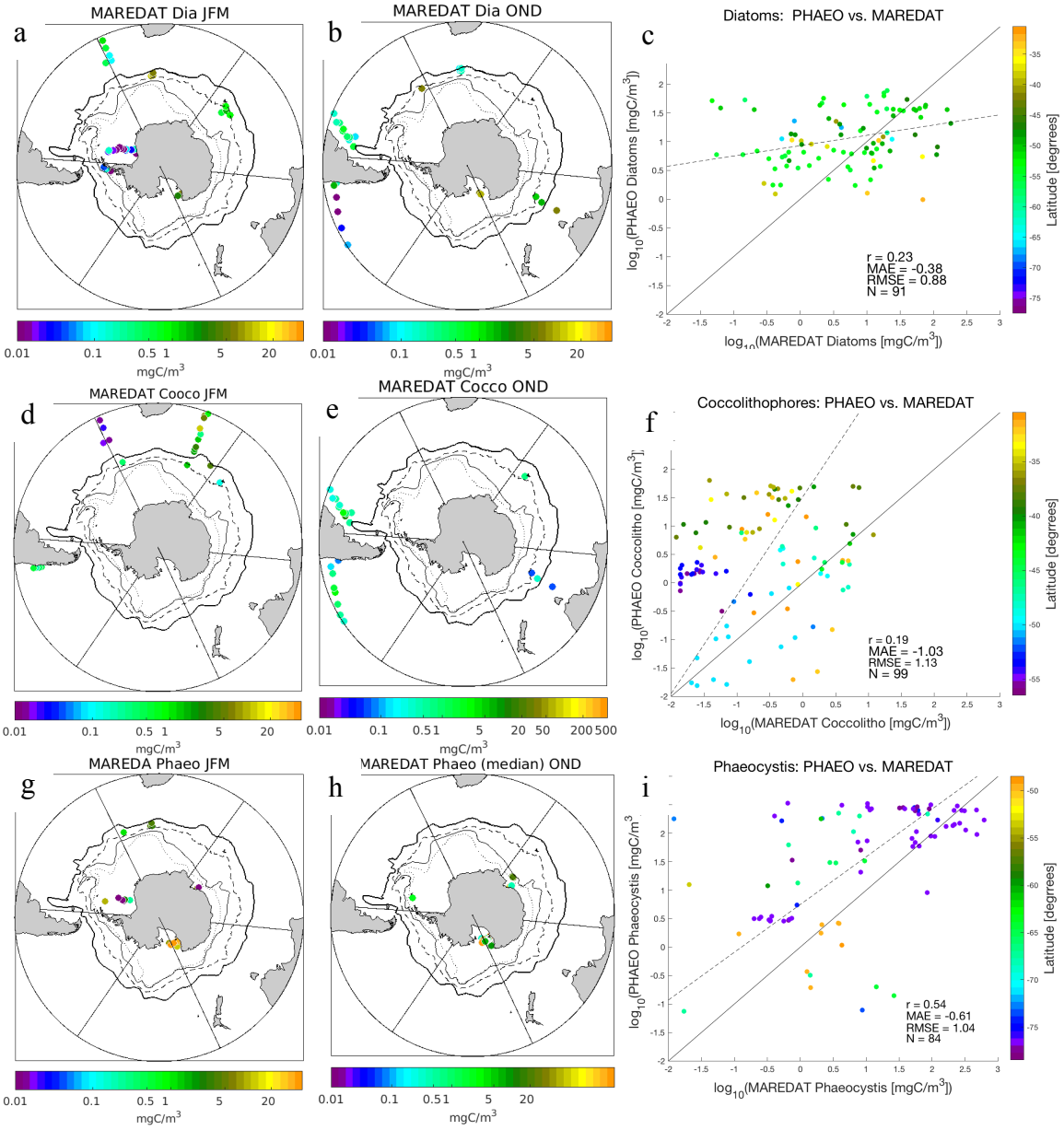
However, it is worth noting that these statistical estimates were obtained based on the model and observation match-ups within ± 1 week. Moreover, the model does not explicitly represent sea-ice algae and, therefore, might work less well in the region around the sea-ice. In this respect, we have to point out that all the statistics are presented for a qualitative assessment of the model rather than for a quantitative estimates of model uncertainties, since the representation error (Janjic et al., 2018) related to the differences in spatial and temporal scales considered and sampled by the model *vs.* observations as well as to the mismatch in grouping phytoplankton (Bracher et al., 2017) are large."

We now also introduce a discussion on model evaluation with MAREDAT PFT biomass dataset.

Even though SO data coverage within the MAREDAT data base is limited, the authors should consider evaluating their model output using these phytoplankton carbon biomass data set to complement the currently included HPLC data.

R: We show the comparison with the MAREDAT data on diatoms, coccolithophores, *Phaeocystis*, and zooplankton biomass. However, the coverage of the Southern Ocean PFT biomass is, indeed, very limited. Figures R2.1 - R2.2 show distribution of MAREDAT seasonal (summer and spring) composites of diatom, coccolithophores and Phaeocystis biomass, and data vs. model matchups based on monthly MAREDAT and PHAEO climatology. Because of the poor data coverage and large discrepancies in representation temporal scales, differences between the model and data (due to the representation error) are expected to be large. As a result, correlation between model and data PFT biomass is weak but significant (0.23, 0.19 and 0.54 for diatoms, coccolithophores and Phaeocystis, respectively). In general, the model overestimates PFT-carbon biomass in comparison with the data. At the end, showing the quantitative estimates of the data and model agreements, we still give a qualitative assessment. Moreover, MAREDAT measurements are not always collocated for different PFTs, thus, it is not

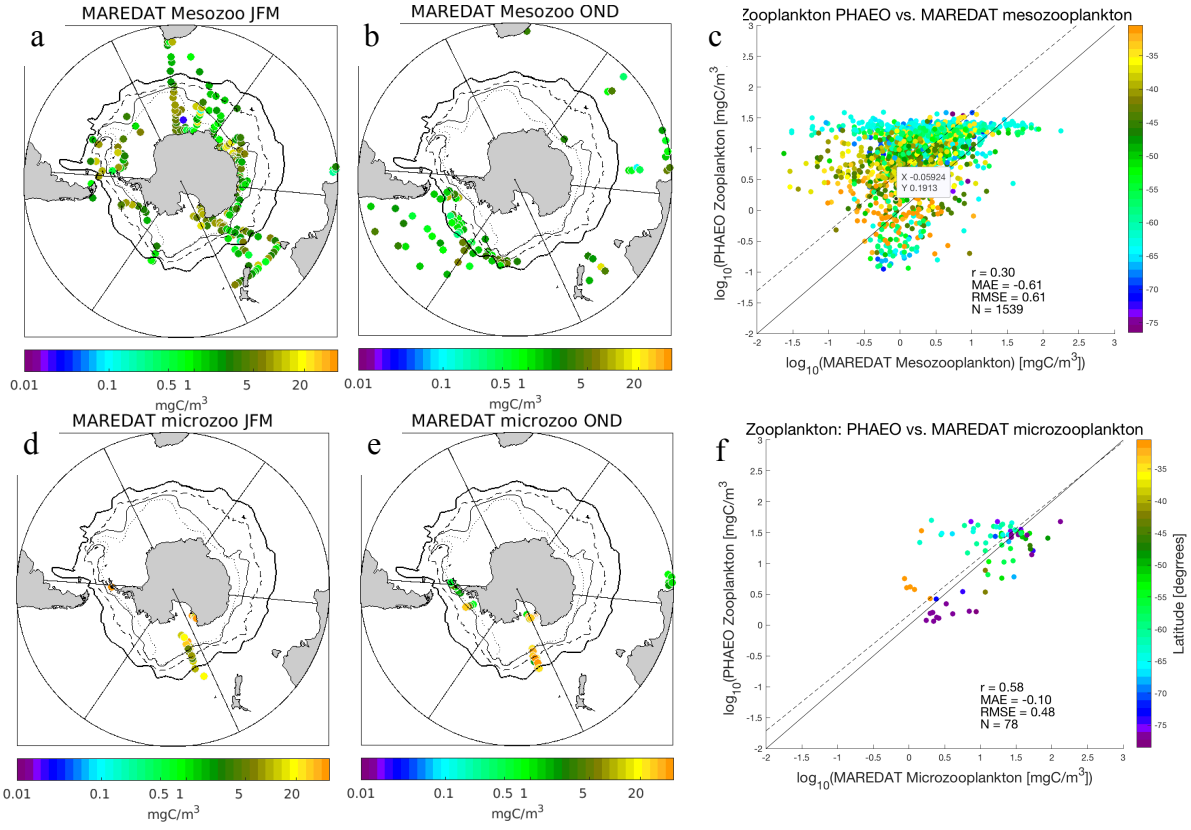
always possible to draw any conclusions on the phytoplankton compositions. However, one, can notice, that diatoms, coccolithophores and *Phaeocysts* do co-exist in the areas along the subantarctic and polar fronts.



(Figure R2.1 is now in the Supplementary Material, Figure S13)

Figure R2.1: Climatological sesonal composites of the MAREDAT surface phytoplankton biomass for diatoms (a: for January – March, b: for October - December), coccolithophores (d: for January – March, e: for October - December) and Phaeocystis (g: for January – March, h: for October - December); scaterplot of the model vs. MAREDAT

matchups based on all surface climatological monthly means: c) for diatoms; f) for coccolithophores; i) for Phaeocystis. (PHAEO model climatology is based on the years 2006 – 2012). Statistics are presented for logtransformed concentrations.



(Figure R2.2 is now in the Supplementary Material, Figure S14)

Figure R2.2: Climatological sesonal composites of the MAREDAT surface mesozooplankton (a: for January – March, b: for October - December) and microzooplankton (d: for January – March, e: for October - December); the model total zooplankton vs. MAREDAT zooplankton matchups based on all climatological monthly means: c) for meso-; f) for micro (PHAEO model climatology is based on the years 2006 – 2012). Statistics are presented for logtransformed concentrations.

Furthermore, in the presentation of the evaluation, the authors often use subjective statements in their description (e.g. "plausible distributions", "skillful enough") which should be avoided as much as possible throughout the manuscript as it is e.g. not clear to me at all when a biomass distribution is "plausible".

R: We removed subjective statements in the revised version of the manuscript.

6) Overall, I think the introduction in its current form misses a clear focus on the focus area, i.e. the SO. From the title of the paper, I would expect a description of the observed SO phytoplankton biogeography somewhere based on available in situ data and satellite algorithms to set up the reader for the assessment of the simulated community structure. Additionally, I would expect a summary on what has been done in terms of PFT modeling in the SO specifically, highlighting what gap is filled with the model used here (for this, see e.g. Lancelot et al. (2009), Wang et al. (2011), Le Quéré et al. (2016), Nissen et al. (2018); Note that the list of available studies is much longer than the examples given here!). The introduction in its current form largely focusses on global modeling approaches without an assessment of how they perform in the SO and is thereby of limited use for the goal of the paper.

R: We modified the introduction accordantly. In the revised manuscript we focused more on the Southern Ocean, added the information of the expected occurrence of the investigated PFTs (in addition to their importance), wrote about current challenges in modeling phytoplankton groups in the Southern Ocean and added a paragraph that explicitly presents the hypotheses tested in our study.

"The Southern Ocean is one of the most important regions in regulating climate via the uptake of about 40% of the global oceanic anthropogenic CO2 (DeVries, 2014) and at the same time, is a region with the dynamics evidently altered by past and present climate change (Stocker et al., 2013). The climatic changes in the Southern Ocean environmental conditions affect the spatial distribution of phytoplankton (Deppeler and Davidson, 2017). The phenology and dominance of different phytoplankton functional types (PFTs) sustaining the marine food web affect the diversity of higher trophic levels (Edwards and Richardson, 2004). Playing distinct roles in biogeochemical cycling, PFTs may determine how and on which spatial and temporal scales the ocean mediates climate (Wilson et al., 2018).

Major bloom-forming PFTs in the Southern Ocean include the silicifying diatoms, calcifying coccolithophores, and colony-forming *Phaeocystis*. Diatoms, the major phytoplankton silicifiers and primary producers in the Southern Ocean (Rousseaux and Gregg, 2014), have high efficiency of carbon export through grazing, direct sinking of single cells, and through mass sedimentation events (Le Quéré et al., 2005; Kemp et al., 2006). They form large spring blooms in the open nutrient-rich waters in the proximity of the Antarctic Circumpolar Current and Polar Front (Smetacek et al., 2002; Kemp et al., 2006). Coccolithophores, the main phytoplanktonic calcifiers in the world ocean, make a major contribution to the total content of particulate inorganic carbon in the oceans (Ackleson et al., 1988; Milliman, 1993; Rost and Riebesell, 2004; Monteiro et al., 2016) through production and release of calcium carbonate plates (coccoliths), and, therefore, also impact the alkalinity of the ocean. This PFT is abundant along the Great Calcite Belt (Balch et al., 2016) and forms massive blooms along the Patagonian shelf break (Signorini et al., 2006). *Phaeocystis* as a dimethyl sulfide producer alters the atmospheric sulfur cycle and can form dense spring blooms in the seasonal ice zone and Antarctic coastal waters as the Ross Sea and Weddell Sea (El-Sayed et al., 1983; Arrigo et al., 1999; DiTullio et al., 2000; Smith et al., 2012), likely supporting export production (Arrigo et al., 2000;

DiTullio et al., 2000; Wang and Moore, 2011). Modeling studies reported the contribution of diatoms to the total primary production in the Southern Ocean of ~89% (Rousseaux and Gregg, 2014), coccolithophores of ~7-16.5% (Rousseaux and Gregg, 2014; Nissen et al., 2018) and *Phaeocystis* of ~13% (P. antarctica) (Wang and Moore, 2011).

Despite the recognized importance of the PFTs, global biogeochemical models struggle to represent the Southern Ocean phytoplankton community accurately. The difficulties primarily originate from uncertain parameters employed in the parametrizations of, e.g., phytoplankton growth and grazing (Anderson, 2005), that define the differences in the phytoplankton traits. On the other hand, the available observational information is still limited in the Southern Ocean to allow to properly constrain the models.

One of the most investigated regions in the Southern Ocean is the Ross Sea, where many in situ observations on diatoms and *Phaeocystis* have been collected and inspired regional coupled ocean-sea ice-ecosystem modeling activities (Arrigo et al., 2003; Worthen and Arrigo, 2003; Kaufman et al., 2017). Several studies that include *Phaeocystis* in the list of simulated PFTs in the frame of global coupled ocean-biogeochemical models have focused on the Southern Ocean (Lancelot et al., 2009; Wang and Moore, 2011; Le Quéré et al., 2016). These studies specified differences in (photo-)physiological parameters between diatoms and *Phaeocystis*, considering *Phaeocystis* in colony form. In a regional study (Popova et al. 2007, Crozet Islands) within the Southern Ocean, *Phaeocystis* was represented by two different life-stages: colonies and solitary cells. This approach was also successfully used by Kaufman et al. (2017) to examine the influence of climatic changes on the Ross Sea phytoplankton.

Nevertheless, an in-depth evaluation of the model simulations of diatoms and *Phaeocystis* with PFT observations either has not been done (e.g. Lancelot et al. 2009) or has been only performed based on a sparse in situ dataset (Wang and Moore, 2011). A more complete evaluation of these PFTs was presented by Le Quéré et al. (2016) by comparing the dominance of the PFTs to satellite-based dominance retrievals, and to a global dataset of in situ-based integrated PFT biomass within upper 200 m of Alvain et al. (2008) and (Buitenhuis et al., 2013), respectively. In general, as compared to the satellite retrievals, the dominance of diatoms and *Phaeocystis* has been overestimated by Le Quéré et al. (2016), while dominance of coccolithophores was underestimated.

Coccolithophore biogeography has recently been investigated globally by Monteiro et al. (2016), Krumhardt et al. (2017) and Krumhardt et al. (2019), and particularly for the Southern Ocean by Nissen et al. (2018). With respect to specific coccolithophore traits, the study by Krumhardt et al. (2017), Monteiro et al. (2016), as well as earlier studies by Paasche (2001) and Iglesias-Rodríguez et al. (2002), emphasized the high nutrient affinity of the coccolithophores and high grazing protection of this PFT (Monteiro et al., 2016). Nissen et al. (2018) reported on higher grazing pressure on coccolithophores than on diatoms. Krumhardt et al. (2019) used lower grazing pressure on coccolithophores than on diatoms and related the distribution

of coccolithophores to a specific temperature function in dependence to its growth rate. However, none of these studies included *Phaeocystis* in their model simulations.

In our study, we improved the representation of key Southern Ocean PFTs, namely diatoms, coccolithophores and *Phaeocystis*, using the Darwin biogeochemical model coupled to the Massachusetts Institute of Technology (MIT) general circulation model (Darwin-MITgcm). In a first step, we modified the Darwin model to account for two distinct size classes of diatoms and for a high affinity for nutrients and an ability to escape grazing control for coccolithophores. Next, the model was extended to include both solitary and colonial forms of *Phaeocystis*. Observational information from in situ and satellite measurements was used to help to define differences in the PFT traits, to constrain the model, as well as to quantitatively evaluate the model performance to overall find a representation of the phytoplankton community in the Southern Ocean that is close to observations. We used the optimized Darwin model to test three hypotheses on the factors controlling the biogeography of Southern Ocean phytoplankton groups:

– Size diversity of the diatoms (Queguiner, 2013; Tréguer et al., 2018) leads to the distribution of small diatoms ("slightly silicified and fast growing") at the lower latitudes and large diatoms ("strongly silicified and slowly growing") at higher latitudes in the Southern Ocean.

– Distribution of coccolithophores in the Great Calcite Belt is not necessarily controlled by temperature (Smith et al., 2017) but determined by the ability of this PFT to escape grazing because of their exoskeleton (Nejstgaard et al., 1997; Huskin et al., 2000; Monteiro et al., 2016), and to grow under nutrient depleted conditions (especially phosphate and iron) (Paasche, 2001; Iglesias-Rodríguez et al., 2002). These characteristics of coccolithophores would make them more competitive among other phytoplankton of larger or similar size, small diatoms and *Phaeocystis*.

– *Phaeocystis* sp. exists in two life stages, solitary cells and colonies, depending on iron availability (Bender et al., 2018). This additional difference in the traits of distinct haptophytes, coccolithophores and *Phaeocystis*, allows them to co-exist.

The paper is organized as follows. Section 2 describes the numerical model set up, experimental design and observations (in situ and satellite retrievals) used for model evaluation, Section 3 presents the results and discussion. Section 4 concludes with summary and outlook."

7) Currently, there is no consistency in the study in what month or even what year is assessed in the different parts of the manuscript (compare e.g. Fig. 3, 4, and 6). In the method section, the authors should clearly state which year(s) and which month(s) of the model output is used in the analysis and why.

R: We provide below (and in the revised version) more details on model evaluation explaining the validation datasets and temporal and spatial representation of the results. A new table (Table 2) is introduced.

"To assess our model results, we compare the simulations to several large in situ and satellite datasets, as detailed below and summarized in Table 2. Where the coverage of the observations is similar in respect to time we use our two-weekly

model outputs. Where only monthly climatological or composite data (often from different time periods) are available we use monthly climatological model results for the period of 2006-2012. Where only results for specific months are available from observations we compare our output to these specific months. Table 3 contains the information about the evaluated phytoplankton groups as classified in the model and observations."

In the subsection(s) describing the observational data used to constrain and evaluate the model, we also clarify how we show corresponding model solution.

Table 2. Datasets used for model evaluation

Dataset	reference	PFT product	units	spatial repr.	time repr.	model output	time repr.
PHYSAT	Alvain et al. (2008)	dominance	unitless	1°x1°	monthly climat. (1998-2006)	dominance	2006–2012**
Darwin-15	Dutkiewicz et al. (2015)	dominance	unitless	1°x1°	monthly climatology	dominance	2006–2012**
SEM	Smith et al. (2017)	dia *vs.* cocco	% cell	in situ	Jan–Feb 2011	dia *vs.* cocco	Jan–Feb 2011
		dominance	counts		Feb-March 2012	% C-biomass	Feb–Mar 2012
SynSenPFT	Losa et al. (2017)	diatom-Chla	mgChla m^{-3}	4x4 km*	March 2012	diatom-Chla	March 2012
		cocco-Chla	mgChla m^{-3}	4x4 km*	March 2012	cocco-Chla	March 2012
PhytoDOAS	Bracher et al. (2017)	diatom-Chla	mgChla m^{-3}	0.5°x0.5°*	March 2012	diatom-Chla	March 2012
HPLC	Soppa et al. (2017)	diatom-Chla	mgChla m^{-3}	in situ	Aug2002 – Apr2012	diatom-Chla	collocated
		hapto-Chla	mgChla m^{-3}	in situ	Aug2002 – Apr2012	hapto-Chla	collocated
		proka-Chla	mgChla m^{-3}	in situ	Aug2002 – Apr2012	*Proch*-Chla	collocated
MAREDAT	Leblanc et al. (2012)	diatom-C	mgC m^{-3}	in situ	1933–2009 climat.	diatom-C	2006–2012**
	O'Brien et al. (2013)	cocco-C	mgC m^{-3}	in situ	1929–2008 climat.	cocco-C	2006–2012**
	Vogt et al. (2013)	*Phaeo*-C	mgC m^{-3}	in situ	1955–2009 climat.	*Phaeo*-C	2006–2012**
	Buitenhuis et al. (2012)	micro-zoo-C	mgC m^{-3}	in situ	climatology	zoo-C	2006–2012**
	Moriarty et al. (2013)	mezo-zoo-C	mgC m^{-3}	in situ	climatology	zoo-C	2006–2012**

$diatom - Chla$ denotes diatom Chla; $cocco - Chla$ is coccolithophore Chla; $hapto - Chla$ is haptophytes Chla; $proka - Chla$ is prokaryotes Chla, $Phaeo - Chla$ is *Phaeocystis* Chla; $Proch - Chla$ is *Prochlorococcus* Chla, extension $-C$ denotes carbon biomass; dia *vs.* cocco is diatom *vs.* coccolithophores; zoo stands for zooplankton; $repr.$ is representation; climat. is climatology.

* the data are presented for a reduced Southern Ocean area as in Smith et al. (2017) and Losa et al. (2018).

** model monthly mean climatology over the years 2006 – 2012.

HPLC: "As we can see there and in Table 2, this large dataset gives us the possibility for a quantitative validation of our model results. Two weekly PHAEO model snapshots from August 2002 to April 2012 have been collocated against in situ HPLC-based Chla observations, if available, within a time window ±1 week. We compare the simulated Chla of diatoms (large + small), haptophytes (coccolithophores + *Phaeocystis*) and prokaryotic pico-phytoplankton against HPLC-derived Chla for diatoms, haptophytes and prokaryotes."

MAREDAT: "These datasets are based on a data collection spanning between 55 to 75 years and are provided as climatological monthly composites. Because of the very sparse distribution of these datasets in the Southern Ocean (except for zooplankton),

which leads to a large representation error when comparing to the model monthly mean climatology (2006 – 2012), only a qualitative assessment was possible." (While quantitative assessment is also shown)

SEM: "Predicted biomass of diatoms and coccolithophores are additionally compared to diatom and coccolithophore measurements (as cell counts) obtained by scanning electron microscopy in the North Atlantic and Indian Ocean sections of the Southern Ocean (the Great Calcite Belt area) during January – February 2011 and February – March 2012 by Smith et al. (2017). For qualitative assessment of the simulated diatom and coccolithophore distributions we compare diatom *vs.* coccolithophore dominance to similar estimates by Smith et al. (2017) collocated in space and time."

SynSenPFT: "We chose only the two groups for comparisons because we are using the SynSenPFT results in addition to the in situ SEM based diatom vs. coccolithophores dominance by Smith et al. (2017). Hence, we only use the same areas and time period as in their study for comparisons to the SynSenPFT results."

PHYSAT: "We compare model climatology of Southern Ocean PFT dominance (averaged over the years 2006 – 2012) to the PHYSAT PFT dominance."

Phenological indices: "These indices are calculated based on the REF Chl simulations for diatoms (including small and large) over the year 2007/2008. We chose this particular year because: 1) with the two-weekly model output the phenological indices can be more precisely calculated than based on the two-weekly or monthly mean climatology; 2) it is a typical year over the period 2006 – 2012 with respect to the simulated PFT distribution (after model reached the quasi-steady state) and climate oscillations (Soppa et al., 2016)."

In this regard, it is e.g. not clear to me why the authors chose to present the ability of the model to represent dominant phytoplankton types in winter, when biomass levels are low.

R: We show the simulations also for the winter because even during this period North of the subtropical front the model simulations show not a negligible biomass (please see supplemented video materials). In addition, we wanted to be consistent with what was shown in the study by Dutkiewics et al. (2015), as well as to explicitly illustrate the disagreement between PHYSAT and "old version" (as well as other tested configurations reviewed in the Supplementary Material) and because this disagreement in winter PFT dominance resulted from incorrectly simulated PFT phenology (e.g. very early bloom of diatoms). This was initially a motivation to consider to size classes of diatoms.

Overall, the figure captions are often incomplete and panel labels are missing entirely. These should be added and referred to in the text to better guide the reader.

R: This has been revised for all figures.

Detailed comments

Abstract:

L. 1: I suggest to make clear in the very first sentence that you're focusing on a single model –otherwise the first sentence sounds like the reader is about to read a review paper on SO PFT modeling. Additionally, I suggest to rephrase to "under past and present climate change".

R: The abstract has been revised accordingly (see comment L. 13).

L. 3: By stating "phenology" so prominently in the abstract, you set up the reader for an assessment of the PFT phenology in your model –which you actually never really do (see comments below). Please rephrase here to have a better representation of the content of the paper and/or adapt the content of the result section (see general comments).

R: The abstract has been revised accordingly (see comment L. 13).

L. 8-9: The new model configuration describes the competition and co-occurrence "best" in what regard and compared to what? Please be precise.

R: Best with respect to considering several "dimensions of phytoplankton diversity" (Dutkiewicz et al., BSD, https://doi.org/10.5194/bg-2019-311)

However, the abstract has been revised accordingly (see comment L. 13).

L. 9-13: Please specify what "older version" you're referring to here, e.g. by explicitly stating "without the above-mentioned changes, but otherwise identical" (if that is the case).

R: The abstract has been revised accordingly (see comment L. 13).

L. 11-13: In the manuscript, you never actually show a quantitative validation of the model output with the SEM data (no plot at all) or the HPLC data (only in maps for the PHAEO simulation, not for the REF simulation), so that it is hard for the reader to evaluate how the model performance improves with your changes (see comments below). Furthermore, I suggest to not overemphasize the SEM data here in the abstract as this comparison is not a major part of your study.

R: A quantitative assessment of the model against HPLC data was provided in three tables (Table 3 – 5) in the main manuscript and 3 Tables in the supplementary material. In Table 2 – 5 we show statistical analysis of the PFT-Chla model-data matchups (RMS, MAE, bias) at several Longhurst's biogeochemical provinces, in table S7-S9 the same but for log-transformed values and table S11 presents more detailed analysis for different sections of the biogeochemical provinces. For qualitative visual evaluation the reader was referred to three supplementary videos. The discussion of the goodness of model to data fit is further extended in the revised version of the manuscript.

In the original version of the manuscript, we compared phytoplankton composition (as meridional distribution of zonally averaged) with respect to co-existing diatoms and coccolithophores with estimates of Smith et al. (2017). Now we show diatom *vs.* coccolithophores dominance collocated in space and time with similar estimates from Smith et al. (2017). Please see figure R2.11 (Figure 5 in the revised version of the manuscript). Nevertheless, the abstract has been revised accordantly, please see below.

L. 13: Please rephrase to "SO PFT dominance patterns". "agrees well" in what regard? Space? Time? Additionally, the abstract in its current form does not represent how much time you spend in the manuscript on the discussion of dominance patterns as opposed to the validation of chlorophyll-a concentrations of the individual PFTs. I suggest to rewrite the abstract to more adequately represent the content of the result section.

R: Thank you for all the comments on the abstract. We have rewritten it based on your comments. We changed the first sentence of the abstract, removed the term phenology and emphasized that the modeled Southern Ocean PFT dominance also agrees well with satellite-based PFT information in terms of spatial and temporal distribution.

"Phytoplankton in the Southern Ocean support important ecosystems and play a key role in the earth's carbon cycle, hence affecting climate. However, current global biogeochemical models struggle to reproduce the dynamics and co-existence of key phytoplankton functional types (PFTs) in this Ocean. Here we explore the traits important to allow three key PFTs (diatoms, coccolithophores and *Phaeocystis*) to have distributions, dominance and composition consistent with observations. In this study we use the Darwin biogeochemical/ecosystem model coupled to the Massachusetts Institute of Technology (MIT) general circulation model (Darwin-MITgcm). We evaluated our model against an extensive synthesis of observations, including in situ microscopy and high-performance liquid chromatography (HPLC), and satellite derived phytoplankton dominance, PFT chlorophyll-a (Chla), and phenology metrics. To capture the regional timing of diatom blooms obtained from satellite required including both a lightly silicified diatom type and a larger and heavy silicified type in the model. To obtain the anticipated distribution of coccolithophores, including the Great Calcite Belt, required accounting for a high affinity for nutrients and an ability to escape grazing control of this PFT. The implementation of two life stages of *Phaeocystis* to simulate both solitary and colonial forms of this PFT (with switching between forms being driven by iron availability) improved the co-existence of coccolithophores and *Phaeocystis* north of the Polar Front. The dual life-stages of *Phaeocystis* allowed it to compete both with other phytoplankton of larger size and/or similar sizes. The evaluation of simulated PFTs showed significant agreement to a large set of matchups with in situ PFT Chl-a data derived from pigment concentrations. Satellite data provided important qualitative comparisons of PFT phenology and PFT dominance. With these newly added traits the model produced the observed >50% coccolithophore contribution to the biomass of biomineralizing PFTs in the Great Calcite Belt. The model together with the large synthesis of observations provides a clearer picture of the Southern Ocean phytoplankton community structure, and new appreciation of the traits that are likely important in setting this structure."

Introduction:

L. 16: Please rephrase "via the sinking of CO2".

R: The corresponding sentence was modified.

L. 17: Please add a reference for the evidence of changes due to on-going climate change.

R: We modified the sentence to:

"The Southern Ocean is one of the most important regions in regulating climate via the uptake of about 40% of the global oceanic anthropogenic CO2 (DeVries, 2014) and at the same time, is a region with the dynamics evidently altered by past and present climate change (Stocker et al., 2013)."

L. 20: Please add a reference for the impact of phytoplankton community structure on the diversity of higher trophic levels.

R: We added Edwards and Richardson (2004).

Edwards, M. and Richardson, A. J.: Impact of climate change on marine pelagic phenology and trophic mismatch, Nature, 430, 881, 2004.

L. 21: Please add a reference for the impact of phytoplankton community structure on climate on different temporal and spatial scales.

R: We added Wilson et al. 2014.

Wilson, J. D., Monteiro, F. M., Schmidt, D. N., Ward, B. A., and Ridgwell, A.: Linking Marine Plankton Ecosystems and Climate: A New Modeling Approach to the Warm Early Eocene Climate, Paleoceanography and Paleoclimatology, 33, 1439–1452,635, https://doi.org/10.1029/2018PA003374, 2018.

L. 32: Please add a reference for the impact of Phaeocystis on SO export production.

R: We added Arrigo et al. (2000), DiTullio et al. (2000) and Wang and Morre (2011).

Arrigo, K. R., DiTullio, G. R., Dunbar, R. B., Robinson, D. H., VanWoert, M., Worthen, D. L., and Lizotte, M. P.: Phytoplankton taxonomic variability in nutrient utilization and primary production in the Ross Sea, Journal of Geophysical Research: Oceans, 105, 8827–8846, https://doi.org/10.1029/1998JC000289, 2000.

DiTullio, G., Grebmeier, J., Arrigo, K., Lizotte, M., Robinson, D., Leventer, A., Barry, J., VanWoert, M., and Dunbar, R.: Rapid and early export of Phaeocystis antarctica blooms in the Ross Sea, Antarctica, Nature, 404, 595, 2000.

Wang, S. and Moore, J. K.: Incorporating Phaeocystis into a Southern Ocean ecosystem model, Journal of Geophysical Research: Oceans, 116, https://doi.org/10.1029/2009JC005817, 2011.

L. 32-35: Why is the description of these types (N2fixers and pico autotrophs) relevant for a modeling study of the SO? I think you can delete this part to have more room to focus on an introduction of the actual topics, such as what is known on the biogeography (from observations and modeling studies) of the most important types in the SO, namely diatoms, Phaeocystis, and coccolithophores.

R: We deleted the description of these types (N2fixers and pico autotrophs). However, distribution of these PFTs impacts the abundance of other PFTs. The more accurate they are modeled (accounted for) the better the distribution of other PFTs is simulated (for instance the north edge of the Great Calcite Belt).

L. 36-39: I suggest to list the three criteria when first mentioning the division by Le Quéré et al. (2005) in e.g. L. 22. The way it is done currently, the 2nd and 3rd criteria come a bit out of the blue for the reader.

R: This part was removed in the revised version of the manuscript (please, check general comments 6).

L. 39: Please give an example that is relevant to the SO application in this study.

R: This part was removed in the revised version of the manuscript (please, check general comments 6).

L. 44-45: I suggest to rephrase to something like "[...] includes also bacteria and zooplankton, but for this study, we use "PFT" to refer to phytoplankton only, in accordance with the definition by the ocean color community".

R: This part was removed in the revised version of the manuscript (please, check general comments 6).

L. 52-55: The relevance of this statement to the study at hand is not clear to me. Please explain.

R: This part was removed in the revised version of the manuscript (please, check general comments 6).

Additionally, you never really use "PG" throughout the text, it is not clear to me why you introduce it here. I suggest to move the information given here to the only place where you actually use it (section 2.2.2).

R: This part was removed in the revised version of the manuscript (please, check general comments 6).

L. 56: It is not clear here why you cite Follows et al. (2007) alongside Le Quéré et al. (2005) after spending almost a page on discussing the latter while not introducing the former. Please make clearer.

R: This part was removed in the revised version of the manuscript (please, check general comments 6).

L. 57: "thee" should be "three"

R: Corrected, but this part was removed in the revised version of the manuscript (please, check general comments 6).

L. 60: Please see also Krumhardt et al. (2019) for a global model with an explicit representation of coccolithophores and consider adding Nissen et al. (2018) here as well as an example of a regional model with explicit coccolithophores to give a more complete overview on what has been done.

R: As suggested, we added these references to the introduction.

L. 66: Please explain more clearly in the text how the Darwin model offers "the highest potential". For example, does this model generally offer "higher potential" than regional modeling approaches? As I am personally not convinced by this (as it will depend on the question you're trying to answer), I suggest to rephrase this statement to explain more clearly.

R: We meant the ability to consider several dimensions of PFT diversity (Dutkiewicz et al. 2019, BSD), but this part was removed in the revised version of the manuscript (please, check general comments 6).

Dutkiewicz, S., Cermeno, P., Jahn, O., Follows, M. J., Hickman, A. E., Taniguchi, D. A. A., and Ward, B. A.: Dimensions of Marine Phytoplankton Diversity, Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-311, in review, 2019.

L. 70-74: In my view, the goals you list here for the study at hand do not match the content of the result section. For example, the manuscript currently lacks a thorough quantitative (!) assessment of the phytoplankton phenology.

R: As mentioned earlier, the initial idea of the manuscript was also to include information on the timing of the phytoplankton blooms but we realized that the study would be too complex and diverse on topic to be summarized in one manuscript. We planed a dedicated study on the phenology of the PFTs blooms in the Southern Ocean soon.

Now, the Chla phenological indices for diatoms are presented in Figure R2.8 (Figure 3a-c in the revised version of the manuscript), Figure R2.9.

What is your conclusion on point 3) here? How can the model complement available in situ observations?

R: The statement was about the model ability to consider different aspects of differentiation among phytoplankton groups – biogeochemical role; allometric, photophysiological and optical parameters; accounting for carbon and Chla decoupling. This ability makes coupled ocean/biogeochemical models a very valuable and skillful instrument that combines the knowledge from in situ measurements and remote sensing by exploiting various PFT retrievals principles used (separately) in these observations and relates it to the environmental conditions. However, the introduction has been rewritten.

L. 71-72: When is a model "skillful enough" in your opinion? When is a simulated distribution "plausible"? Please specify exactly what you mean by this and avoid subjective judgement whenever possible.

R: We removed subjective statements in the revised version of the manuscript.

Please replace "predict" by "simulate" or similar.

R: We replaced the term.

L. 74-75: The statement "When determining [...]" is not clear to me. Please be more precise. What do you mean exactly?

R: Within our study, with the available observational satellite and in situ information we constrain the model with respect to PFT traits specified in the model. Nevertheless, this sentence was removed in the revised version of the manuscript (please, check general comments 6).

Methods

L. 80: I suggest to change the title to include the name of the model used in this study.

R: Changed to "Darwin-MITgcm numerical models"

L. 90: Do you mean lightly silicified?

R: We meant "slightly silicified" (as in Queguiner, 2013).

How was the silicification different between these two classes different in the model? How is silicification parametrized? If you introduce a completely new PFT, you need to give more detail on its characteristics.

R: We did not introduce a complete new PFT but considered small eukaryotes (as in the original Dutkiewicz et al. 2015 paper) being silicified as specified for large diatoms with the parameters listed in Table 1. Thank you, we have now added k_{si} parameter values for large and small diatoms. The level of silification is parameterized by the cellular Si:C ratio.

L. 90-99: Why are these three changes justified for the SO? I suggest to include statements on the reasoning behind e.g. changing the nutrient affinity and grazing parameters for coccolithophores –

R: It was motivated by the following studies Paasche, 2001; Iglesias-Rodríguez et al., 2002; Nejstgaard et al., 1997; Huskin et al., 2000, Losa et. 2006, Krumhardt et al. 2017. Now additionally backed up by Monteiro et al. 2016. We state it now in the introduction as one of the hypotheses tested.

Why does this apply for this SO-focused study and not for global applications of Darwin?

R: Indeed, we think this applies also for global applications of Darwin (this was also shown in Monteiro et al. 2016).

Please add a reference regarding the occurrence of lighter silicified diatoms at lower latitudes.

R: The reference to Queguiner (2013) (w.r.t. "slightly silicified diatoms") were provided (L. 93). We also state it now as a hypothesis we test (with additional reference to Tréguer et al. 2018).

L. 95: Please replace "was presented" by "is represented".

R: We replaced as "has been presented".

L. 95-99: What sensitivity experiments did you perform here? How did you evaluate what a "realistic co-occurrence of coccolithophores and Phaeocystis" is?

R: This sentence has been removed/ was rephrased to: "Other nano-phytoplankton (referred to as "other large" in the original Dutkiewicz et al. 2015) has been presented by *Phaeocystis*. "

I think it is important here to briefly sketch the main characteristics of the parametrizations used for Phaeocystis if you've actually used those from Popova et al. (2007) and Kaufmann et al. (2017), but see also comment further down (on L. 138 in your manuscript).

R: We indeed introduce two phases of the Phaeocystis life stages (colonies and solitary cells) following Popova et al. (2007) and Kaufman et al. (2017). However, these "two *Phaeocystis* life stages were considered as a function of iron availability (Bender et al. 2018)."

L. 101-112: The description of the treatment of light is out of place here as you go back to a description of the PFTs afterwards. Please reorganize the section to make it easier for the reader to follow.

R: Though it was one of the changes/differences with respect to original Dutkiewicz et al. 2015, we have now moved this description to the Supplementary Material.

Additionally, I am not sure this much detail on the parametrizations surrounding light absorption are needed in the main text. Please consider moving this part to the supplement.

R: We have now moved it to the Supplementary Material.

L. 100-117: Here and throughout the text (including e.g. especially Table 1), please make sure you state the units of all variables introduced.

R: Units were provided in the text introducing model parameters in the parametrizations (pages 4 and 5, Section 2.1.1.) To make it clearer, we now include the parameter units in Table 1. We did not provide unites for the function μ_j and alpha (also formulated as a function). As suggested, we have added them now.

L. 113: Please replace “which is presented” by “which are described by” or similar.

R: Replaced.

L. 114: According to Table 1, this parameter only applies to Prochlorococcus. I suggest to state that here.

R: We added “applied to Prochlorococcus”.

L. 115: I find “biomineralizing function” misleading and would rather say “whether or not they form biominerals such as opal or calcite” (or something along these lines).

R: We were following the expression used as in Smith et al. (2017) but we clarify it accordingly as suggested (L126).

L. 115-117: Please rephrase this sentence, it sounds a bit weird to me in its current form.

R: We rephrased to: “These main differences between specified traits alter the growth rate of particular phytoplankton (μ_j, day^{-1}, j = 1, 2,..., 6) and the grazing of phytoplankton by small or micro-zooplanktons (Gr_{jk}, k = 1, 2) given the palatability factor ($r_{j,k}$) and sinking rate (w_{sink}, m day^{-1}).”

L. 118: Please rephrase to “The growth of phytoplankton μj(day-1)[...]”

R: We rephrased as suggested but without the abbreviation and unit since it was introduced earlier in the text.

L. 123: How are the temperature and nutrient limitation terms calculated? Please add the equations.

R: As in the original study by Dutkiewicz et al. (2015), the nutrient limitation is calculated as:

$\gamma_{\eta_{ij}} = \min(\eta_{ij}), \eta_{ij} = \frac{\eta_i}{\eta_i + k_{sat_i}}$.

The temperature limitation is calculated as:

$\gamma_j^T = \tau_T e^{\left(A_T\left(\frac{1}{T+273.15} - \frac{1}{T_O}\right)\right)}$,

given the coefficient $\tau_T = 0.8$ normalized the maximum value (unitless), the temperature coefficient $A_T = -4000K$, and the reference temperature $T_o = 293.15K$“

We have added this and nutrient limitation functions in the revised version of the manuscript (L135).

L. 124: Alpha PI is missing in Table 1.

R: Alpha is not a parameter but it is calculated by equation (5) (eq. 3 in the revised manuscript) given the phytoplankton-specific light absorption and the maximum quantum yield of carbon fixation.

L. 125: The phytoplankton-specific light absorption and the maximum quantum yield of carbon fixation are missing in Table 1.

R: The phytoplankton-specific light absorption is not a parameter but spectra, we have now added the spectrally averaged phytoplankton-specific light absorption values of the maximum quantum yield of carbon fixation into the table.
(Initially in table 1, we opted to specify parameters, which were different from the configuration of Dutkiewicz et al., 2015).

L. 127: Please change to "as opposed to the studies by X andY". However, I don't understand why you refer to two studies here which are based on a different biogeochemical model (NOBM) than the one you're using here (DARWIN). Are you using the same function to calculate the temperature limitation as they do? If yes, state that to make your argument clearer.

R: We use different (to what is used in NOBM) the temprerature limitation function. However, as opposed to studies using distinct temperature function for different PFTs, we use the same function for all considered PFTs. We revised as following: "The γ_j^T function was considered the same for diatom, coccolithophores, *Phaeocystis* and prokaryotes given the coefficient τ_T = 0.8 normalized the maximum value (unitless), the temperature coefficient A_T = -4000 K, and the reference temperature T_0 = 293.15 K." (L142-144).

Furthermore, does your statement mean that the growth of N2fixers is not suppressed at low temperatures?

R: Yes, in this version we did not suppress N2-fixers at low temperatures.

This relates to a comment further down (on Fig. 4) in that I have the impression that your importance of N2fixers for the SO phytoplankton community is way too high if we take into consideration that their growth should be limited to regions of temperatures above a certain threshold (e.g. ~18°C, see e.g. Breitbarth et al. (2007) and Luo et al. (2012)) –even though nitrogen fixers have been found more recently in polar waters, I am just not convinced that they make up such a substantial part of the community in terms of biomass in these latitudes. Are you aware of evidence for this?

R: In PHAEO model simulations, Nfixer shows only north of 45°S.

The Darwin model has typically not imposed temperature limitation on diazotrophy, as model studies suggest that the majority of diazotrophy happens in warmer waters not so much because of the temperature, but because of the nutrient supply ratios of those waters (see e.g. Monteiro et al 2011; Dutkiewicz et al 2012; Ward et al 2013). We feel this is a reasonable approach in this study especially given the discovery of diazotrophs in colder waters (Zehr, 2011, Fernández-Méndez, 2016), also in Baltic Sea.

Monteiro, F., Dutkiewicz, S., Follows, M. J.: Biogeographical controls on the marine nitrogen fixers. *Global Biogeochemical Cycles*, 25, GB2003, doi:10.1029/2010GB003902, 2011

Dutkiewicz, S., Ward, B.A., Monteiro, F., Follows, M. J.: Interconnection between nitrogen fixers and iron in the Pacfic Ocean: Theory and numerical model. *Global Biogeochemical Cycles* , 26, GB1012, doi:10.1029/2011GB004039, 2012

Ward, B.A., Dutkiewicz, S., Moore, C.M., Follows, M.J. : Iron, phosphorus and nitrogen supply ratios define the biogeography of nitrogen fixation. *Limnology and Oceanography*, 58, 2059-2075, 2013

Zehr, P.: Nitrogen fixation by marine cyanobacteria, *Trends in Microbiology,* 19 (4), 162 – 173, doi:10.1016/j.tim.2010.12.004, 2011

Fernández-Méndez, M., Turk-Kubo, K. A, Buttigieg, P. L., Rapp, J. Z., Krumpen, T, Zehr, J. P., Boetius, A.: Diazotroph Diversity in the Sea Ice, Melt Ponds, and Surface Waters of the Eurasian Basin of the Central Arctic Ocean.Front. Microbiol. 7:1884. doi: 10.3389/fmicb. 2016. 01884, 2016.

L. 131: gmax and ksat are missing in Table 1. Furthermore, the equation you give has a Holling Type III ingestion term. Are you using Holling Type II or III? Please double-check.

R: g_{max} and k_{sat} are used unchanged as in Dutkiewicz et al. (2015). We use Holling Type III to formulate grazing, thank you for pointing out the typing error.

L. 138-145: I have some concerns regarding the way you parametrize Phaeocystis here.

- First of all: are you following the parametrizations of Popova et al. (2007) and Kaufmann et al. (2017) or not? You state this in L. 99, but according to what you state here, I don't think you can say that you use their parametrizations. In both the cited studies, the transition of Phaeocystis from single cell to colonies (and back) is a function of a specified maximum colony formation rate, a maximum single cell liberation rate, the single cell biomass concentration(using a threshold concentration to allow for colony formation), the position in the water column (i.e. light availability, see also Peperzak (1993)), and the nutrient limitation–as opposed to just a fixed iron concentration threshold you seem to have used here (if I understood this correctly). Differences to the cited literature need to be made very clear here as your parametrization appears distinctly different.

  R: We do not use exactly the same parametrizations as in Popova et al. (2007) and Kaufmann et al. (2017). We state that we introduce two phases of the *Phaeocystis* life stages (colonies and solitary cells) following Popova et al. (2007) and Kaufman et al. (2017), but with different implementation: 1) these two *Phaeocystis* life stages were considered only as a function of iron availability as shown in the study by Bender et al. 2018; 2) just one tracer was considered (l. 144 – 145).

The effect of neglecting certain aspects and the potential impact on the simulated biogeography should then be at least discussed somewhere in the manuscript.

R: In this respect we provide the reference to a recent study by Bender et al. 2018 who reported on the role of iron "as a trigger" for colony formation and now state in the introduction that we test the hypothesis that the transition in the *Phaeocystis* life cycle is determined by iron availability.

Besides, Becquevort et al. (2007) and Hassler & Schoemann (2009) also showed that Fe addition had an effect on the morphotype dominance (colonies vs. solitary cells) of *Phaeocystis antarctica* with proportionally more solitary cells under low Fe conditions.

Becquevort, S., Lancelot, C., Schoemann, V.: The role of iron inthe bacterial degradation of organic matter derived from *Phaeocystis antarctica*. Biogeochemistry 83:119–135, doi 2007

Hassler, C. S. and Schoemann, V.: Bioavailability of organically bound Fe to model phytoplankton of the Southern Ocean, Biogeosciences, 6, 2281–2296, https://doi.org/10.5194/bg-6-2281-2009, 2009.

With respect to the influence of light:

Colonial *P. globosa* cells were found to be more effective competitors under high light conditions due to mucus formation, which was suggested to act as an energy drain mechanism storing fixed carbon in the form of polysaccharides inside the mucoid matrix (Riegman and von Boekel 1996). In line with this, colony formation of *P. antarctica* within a natural phytoplankton assemblage of the Ross Sea was favored under a high (52–276 µmol photons m^{-2} s^{-1}) relative to a low natural light regime (11–58 µmol photons m^{-2} s^{-1}, Feng et al. 2010). Based on Heiden et al. (2019), cell abundance of solitary relative to colonial *P. antarctica* cells as well as the number of colonies was similar between medium and elevated light treatments a, pointing toward a high light tolerance also of the single-celled *P. antarctica*. Similar findings were previously made for a single celled strain when exposing it to increasing irradiances (Trimborn et al. 2017).

Riegman, R., Van Boekel, W. : The ecophysiology of Phaeocystis globose, A review, Journal of Sea Research, 35 (4), 235-242, doi: 10.1016/S1385-1101(96)90750-9, 1996

Feng et al.: Interactive effects of iron, irradiance and CO2on Ross Sea phytoplankton, Deep-Sea Research I, 57, 368–383 doi:10.1016/j.dsr.2009.10.013, 2010

Heiden, J.P., Völkner, C., Jones, E.M., van de, Poll, W.H., Buma, A.G.J., Meredith, M.P., de, Baar, H.J.W., Bischof, K., Wolf-Gladrow, D. and Trimborn, S.: Impact of ocean acidification and high solar radiation on productivity and species composition of a late summer phytoplankton community of the coastal Western Antarctic Peninsula. Limnol. Oceanogr., 64: 1716-1736. doi:10.1002/lno.11147, 2019

Trimborn, S., Thoms, S., Brenneis, T., Heiden, J.P., Beszteri, S. and Bischof, K.: Two Southern Ocean diatoms are more sensitive to ocean acidification and changes in irradiance than the prymnesiophyte *Phaeocystis antarctica*, Physiol Plantarum, 160: 155-170. doi:10.1111/ppl.12539, 2017

- One vs two tracers for Phaeocystis: Have I understood correctly that your whole Phaeocystis biomass pool just switches back and forth between single cells and colonies based on the iron concentration threshold?
  R: Yes, we consider just one tracer (L144 original version). In the revised manuscript (L165) we write:
  "Note that in the model *Phaeocystis*, independent of the life stage – colonial phase or solitary cells, – is considered as one tracer."

  I understand that this makes it computationally more efficient, but this might be too simplistic (I am not sure myself). Assuming I understood this correctly, are you tracking in space and time what "Phaeocystis state" the model tracer is in? Based on this tracking: are you confident that you capture the transitions well enough with just the dependency on iron to justify neglecting the other dependencies that have been suggested to be important (such as light levels), meaning that one model tracer is enough to simulate both life cycle stages simultaneously? This would be an important piece of information for other people wanting to implement Phaeocystis into their model. Please discuss this in the manuscript.
  R: We did not track the "*Phaeocystis* state" for the present. We agree that this approach is quite simplistic, but nevertheless it agrees with the recent study by Bender et al. 2018 and allows Phaeo and coccolithophores to co-exist, which was our prior goal. However, we state "there is still room for improvement. For instance, for specifying more precisely the differences in photophysiology and related optical imprints (Moisan and Mitchell, 2018) for *Phaeocystis* in cells and colonies phases." (L355-356 in the original version, L589-591 in the revised manuscript).

- Sensitivity to chosen parameters: I would be curious to see how sensitive your simulated biogeography is to how long Phaeocystisis in the colonial form during summer. Have you looked at the sensitivity to the chosen threshold? Additionally, what are the changes in parameters based on (30% and 25% higher mortality and grazing rate, respectively, as well as 20% lower kFe in single-cell-state, choices seem random)? How sensitive is the simulated biogeography to these choices?

R: We expect the model to be sensitive to the parameters since it will determine the competition/co-existing also between small diatoms and *Phaeocystis*, which will result in some changes of the PFT distribution. However, at first, we had to test whether this additional differences in the traits for haptophytes would help to get stable solution allowing both (coccolithophores and *Phaeocystis*) to co-exist. Careful calibration of the model with respect to these parameters could further improve the model performance.

L. 148: What is the horizontal resolution across the SO in the setup you're using here?

R: 18 km (L148, Subsection 2.1.2).

L. 151: If you state that your setup was similar to the one in Taylor et al. (2013), I am immediately wondering what is different. Please state this clearly.

R: With respect to the differences we state: "Starting on January 1st, 1992, the model with biogeochemistry was forced until 2012 by 3-hourly atmospheric surface fields of the Japanese 55-year reanalysis (JRA55, Kobayashi et al. 2015). Initially, the model time step had to be decreased to 10 min because of the higher forcing frequency, this constraint was slowly relaxed to 20 min by January 1st 1996. The change in forcing also required an adjustment of some the sea-ice model parameters. The albedos for dry ice, wet ice, dry snow, and wet snow were set to 0.75, 0.71, 0.87, and 0.81, respectively; the simulation did not use the replacement pressure method (Kimmritz et al. 2017)." (L180-185)

L. 160-165: Do you spin up the model in the coupled physical-biogeochemical setup immediately or do you spin up the physics first and only coupled once the circulation in spun up (or close to that)? This is not clear to me right now. I am wondering what impact spinning up both together (what it sounds like based on your manuscript) would have on the simulated biogeographies. Have you looked into this?

R: We do first spin up the physical model and then perform a spinup in the coupled physical-biogeochemical mode.

We now explicitly provide the details on the initialisations (L176-180):

"Initial conditions of the physical model were obtained from a spinup simulation initialised in January 1979 from rest and from temperature and salinity fields derived from the Polar Science Center Hydrographic Climatology (PHC) 3.0 (Steele et al., 2001). In the spinup phase, the model forced until the end of 1991 by 6-hourly atmospheric surface fields derived from the European Centre for Medium-Range Weather Forecasts (ECMWF) 40 year re-analysis (ERA-40) (Uppala et al., 2005). For more details see Losch et al. (2010, Section 3)."

And further (L185-187):

"After spinning up the biogeochemistry for six years (from 1992), during which also the physical simulation is adjusted further to the new forcing, the years 1999 – 2012 are integrated and the period of Aug 2002 – Apr 2012 is used for analysis."

After reaching a guasi-steady state, seasonal cycles of nutrients and PFT biomass are repeated with some interannual variability but no significant drift. Model nutrients climatology agreed with the World Ocean Atlas given correlation coefficient r = {0.92, 0.90, 0.97} and normalized standard deviation STD = {0.67, 1.27, 1.13} for silica, nitrate and phosphate, respectively.

L. 160: Please replace "evolved" by "involved".

R: Changed

L. 163-165: How does using model output from a different model compare to initializing with e.g. WOA and satellite derived chlorophyll concentrations (making some further assumptions on C:Chl ratios and the depth profiles)? Do you introduce biases?

R: With respect to chlorophyll, the model that, actually, has decoupled C and Chla (as well as in REcoM used in Taylor et al., 2013) forgets the initial state within the time from several hours up to several days. There is no need to do any assumptions on C:Chl ratios or the depth profiles. Thus, you do not introduce biases.

How does the model used in Taylor et al. (2013) perform in the SO?

R: The model simulations from Taylor et al. (2013) were evaluated and validated exactly for the Southern Ocean. As stated in Taylor et al. (2013):

„Generally, simulated mean monthly chlorophyll a concentrations (log-transformed) correlated to remote-sensing data at R = 0.62 globally and R = 0.23 for the Southern Ocean. These global correlation values are higher than those presented by other coupled general circulation model (GCM) studies [Schneider et al., 2008; Doney et al., 2009]. Lower correlations appear to be common for polar regions. In a review of both GCM and remote-sensing algorithm models of primary production, the Southern Ocean was found to be an area of highest divergence of estimates [Carr et al., 2006]."

L. 168-184: In this section, I am currently lacking a description of what model output you're comparing to the observations. Climatological? Single years? Co-located? Surface only? Please state here, what you're going to present in the result section, as this will help the reader to follow your structure.

R: We write in Section 3.4.2 "For a more precise comparison of the PHAEO model simulations with in situ information, we collected a series of 2 weekly model snapshots from August 2002 to April 2012 and considered the spatial distribution of Chla for diatoms (large + small), haptophytes and prokaryotes against in situ observations, if available, within a time window ± 1 week."

In general, Section 2.2. has been revised with respect to details on how the model is evaluated given particular data or information. Below we clarify what model output we take (and why) for the evaluation. Additionally, we have introduced a

table summarizing the datasets used for PFT evaluation (including spatial and temporal representation) and related model output.

As for the comparison with the data by Smith et al. (2017), you need to be clearer here as it is not obvious how you compare the "simulated PFTs" (do you mean the simulated biomass concentrations? Please be precise) to SEM observations (cell counts). Again, do you co-locate? Do you use single year model output? Climatological model output?

R: we revised this part as following:

"Predicted PFTs were additionally compared to diatom and coccolithophores measurements (as cell counts) reported by Smith et al. (2017). These data were obtained by scanning electron microscopy in the North Atlantic and Indian Ocean sections of the Southern Ocean (the Great Calcite Belt area) during January – February 2011 and February – March 2012. For qualitative assessment of the agreement of the simulated diatom and coccolithophore distributions to these data, we provide estimates of the diatom vs. coccolithophores dominance to compare to the similar estimates by Smith et al. (2017) collocated in space and time."

The evaluation of the coupled model skill with respect to predicted PFT Chla was performed given in situ HPLC-based Chla retrievals for diatoms, haptophytes and prokaryotes (2166, 2388 and 1425 matchups, respectively) over the time period of August 2002 – April 2012 (Soppa et al. 2017). Quantitative assessment of the agreement between model and data collocated matchups were/are provided for several biogeochemical provinces (Tables 3 – 5 of manuscript and Tables S7 – S9 of the Supplementary Material). For the matchups we collected available in situ observations co-located with 2 weekly model snapshots within a time window ± 1 week (as originally stated in Section 3.4.2).

The qualitative assessment for simulated PFTs was performed when the observational data either from satellite or in situ are available in different units as the simulated data. Thus, the qualitative assessment for simulated PFTs was carried out for:

1) the simulated Southern Ocean PFT dominance by comparing to the PHYSAT PFT dominance climatological data product over the years of 1998 – 2006 (Alvain et al. 2008);
2) the diatoms *vs.* coccolithophores dominance (based on biomass, mmolC m^{-3}) in the Great Calcite Belt by comparing with collocated in situ cell counts by Smith et al. 2017 for January – February 2011 and February – March 2012 (keeping in mind that dominance by cell number and dominance by biomass are not equivalent and there is large uncertainty of converting cell counts to biomass);
3) PhytoDOAS coccolithophore fit (Losa et. 2018) and SynSenPFT Chla retrievals for diatoms and haptophytes(coccolithophores) over the same area and time period as shown in Smith et al. (2017);
4) Southern Ocean Diatom phenological indices (see Figure R2.8, added into the revised version of the manuscript as Figure 3a,b,c) compared to Soppa et al. (2016).

Spatial distributions of the simulated nutrients were compared with World Ocean Atlas (Garcia et al. 2014). Originally, the spatial distribution of the simulated surface phosphate, silica and iron were provided for March (2004) in support to discussion on drivers of coccolithophores biogeography (Section 3.3). We added the following information in the manuscript: "In general, the simulated surface nutrient climatology agrees well with the World Ocean Atlas given correlation coefficient of 0.90, 0.97 and normalised standard deviation of 1.27, 1.13 for silicon and phosphate, respectively (see also the Supplementary Material)."

L. 186: Similar to above: Please state very clearly what model output you take (and why) for the evaluation. As stated in the comments further down, I find it very confusing as a reader that you currently pick what seems like random months of a random year and are additionally not consistent across the different simulations (compare Fig. 2, which shows July & January, to Fig. 7, which shows June-August and December-February; compare Fig. 4, which shows February 2008, to Fig. 5, which shows March 2004, or to Fig. 8, which shows March 2012). Please rewrite this section accordingly and double-check how you can be consistent in the use of the years.

R: We apologies, as we now realise how confusing it was to have multiple months/years presented without providing explanations. We now compare to climatologies where possible, but given the importance of interannual variability or specific patterns in the discussed distribution of PFTs and nutrients we still consider a particular month and include comparison of PFT to HPLC matchups and SEM observations from the actual month/year. To make this less confusing, we explicitly state in the text why we chose specific months/years.

In the beginning of Section 2 we now introduce the following overview:

"To assess our model results, we compare the simulations to several large in situ and satellite datasets, as detailed below and summarized in Table 2. Where the coverage of the observations is similar in respect to time we use our two-weekly model outputs. Where only monthly climatological or composite data (often from different time periods) are available we use monthly climatological model results for the period of 2006-2012. Where only results for specific months are available from observations we compare our output to these specific months. Table 3 contains the information about the evaluated phytoplankton groups as classified in the model and observations."

We extended Section 2.2.2 with the following text: "For qualitative assessment of simulated PFTs, we compare model climatology of Southern Ocean PFT dominance (averaged over the years 2006 – 2012) to the PHYSAT PFT dominance climatological data product (1998-2006, Alvain et al. 2008). Comparison of predicted Chla for diatoms and coccolithophores with PhytoDOAS coccolithophore fit (Losa et. 2018) and SynSenPFT Chla retrievals for diatoms and haptophytes (coccolithophores) were carried out for the same area and time period as shown in Smith et al. (2017)".

L. 196: You state "only 0.5°" –how does this compare to your model resolution? (You give an average resolution of 18km, but it wasn't clear to me over what area that is averaged, see further up)

R: The discussion in line 196 refers to the spatial and temporal resolution of PhytoDOAS product as initial input information in the SynSenPFT algorithms, which derived PFT at a daily 4 km resolution (see Losa et al. 2017 for more details). There was no averaging nor projection done with respect to model grid, since it was only foreseen as qualitative evaluation w.r.t the satellite products.

We slightly reformulated this section to improve it:

"Model results are compared to phytoplankton dominating groups from the climatological monthly mean satellite derived product PHYSAT (1998-2006, Alvain et al., 2008). PHYSAT is based on the analysis of normalized water-leaving radiance anomalies, computed after removing the impact of chlorophyll-a variations. Specific water-leaving radiance spectra anomalies (in terms of spectral shapes and amplitudes) have been empirically associated to the presence of dominant phytoplankton groups, based on in situ diagnostic pigment observations. This product is based on the multispectral Sea-Viewing Wide Field-of-View Sensor (SeaWiFS) information and available in http://log.cnrs.fr/Physat-2?lang=fr.

We also evaluated the model simulations (mg m^{-3}) against the satellite PFT Chla (mg m^{-3}) product SynSenPFT (Losa et al. 2017, https://doi.org/10.1594/PANGAEA.875873). The SynSenPFT product combines the information of two satellite PFT Chla products: one retrieved with the differential optical absorption spectroscopy method (PhytoDOAS, Bracher et al. 2009; Sadeghi et al. 2012) applied to hyperspectral information from the Scanning Imaging Absorption Spectrometer for Atmospheric Chartography (SCIAMACHY, Bracher et al. 2017; https://doi.org/10.1594/ PANGAEA.870486) and the OC-PFT abundance-based approach (Hirata et al. 2011 and refined in Losa et al. 2017) applied to multi-spectral satellite total Chla data from the Ocean Colour Climate Change Initiative (OC-CCI). While the PhytoDOAS products from the SCIAMACHY sensor are only available at 0.5° spatial resolution and monthly means, OC-PFT applied to OC-CCI Chla products can be obtained daily and at 4 km resolution.

PhytoDOAS and PHYSAT satellite products are derived based on phytoplankton absorption properties captured by the satellite sensors and distinguished by the retrieval algorithms either as a particular PFT optical imprint ("finger print") in case of available hyperspectral information (in PhytoDOAS) or as anomalies in a multispectral signal (in PHYSAT). Thus, the PhytoDOAS allows to retrieve quantitatively major PFTs (coccolithophores, diatoms, cyanobacteria), while PHYSAT provides information about five dominant phytoplankton groups: prokaryotes (presented by *Prochloroccocus* and *Synechococcus*-like SCL), diatoms, haptophytes in general and *Phaeocystis* in particular.

We compare model climatology of Southern Ocean PFT dominance (averaged over the years 2006 – 2012) to the PHYSAT PFT dominance (dominance of the modeled PFT is defined if its Chla fraction is more than 55% of the total Chla). In line with the evaluation against the PHYSAT PFT dominance, the simulated PFT dominance are compared to similar estimates obtained in the study by Dutkiewicz et al. (2015). Two SynSenPFT products (at 4 km and daily) – diatoms Chla that combines diatoms

Chla from PhytoDOAS and OC-PFT, and coccolithophores Chla that combines coccolithophores Chla from PhytoDOAS with haptophytes Chla from OC-PFT – are used in addition to the in situ based diatom vs. coccolithophores dominance by Smith et al. (2017). Hence, we only use the same areas and time period as in their study for comparisons to the SynSenPFT results. Here as well the comparison is qualitative as the SynSenPFT products are mostly based on OC-PFT in our study region and the global relationships between Chla and the fraction of PFTs from the OC-PFT algorithm might differ in the Southern Ocean, as shown by Soppa et al. (2014) for diatoms."

Results & Discussion

L. 205: "Improved" compared to what?

R: We changed the title of the subsection to "Diversity within diatoms".

L. 206: From the title of the section, the reader expects a discussion of phytoplankton phenology here (i.e. e.g. bloom timing, bloom peak timing, bloom duration), but instead you discuss dominance patterns. Please choose a more appropriate title. In fact, I would suggest to not use "phenology" throughout the text as you currently do not really assess it in a quantitative sense. If you want to keep it (and there is value to that!), you need to introduce this in the method section, where the definition of bloom start etc. is currently missing, and present the simulated phytoplankton phenology and the comparison with e.g. satellite derived phytoplankton phenology.

R: We agree and have changed the title of the subsection to "Diversity within diatoms".

L. 206-223: You never state in the method section that you will compare model output from a version without the listed changes to the setup which includes the changes. Please add this to the method section.

R: We have extended the method section w.r.t. comparison to the version of Dutkiewicz et al. (2015):

"In line with the evaluation against the PHYSAT PFT dominance, the simulated PFT dominance are compared to similar estimates obtained in the study by Dutkiewicz et al. (2015)"

L. 207: "were misrepresented" –please rephrase to state more clearly what model version/setup/simulation you're referring to here.

R: We have added the reference to the Darwin-MITgcm version of Dutkiewicz et al. (2015).

L. 208-209: How confident are you in the satellite-derived dominance pattern in austral winter (July)? Additionally, do you really think that for a region like the SO, it is critical how well the model simulates the dominance patterns in winter? Personally, I would have preferred to see the agreement for all summer months (December-February or even March) to additionally get a better feeling for how the model is doing in terms of seasonality.

R: We now show the PFT dominance for all twelve months in the Supplementary Material. As mentioned before, we show the simulations also for the austral winter because even during this period north of the subtropical front the model simulations show not a negligible biomass (please see supplemented video materials). In addition, we wanted to be consistent with what was shown in the study by Dutkiewics et al. (2015), as well as to explicitly illustrate the disagreement between PHYSAT and "old version" (as well as other tested configurations reviewed in the Supplementary Material) and because this disagreement in winter PFT dominance resulted from incorrectly simulated PFT phenology (e.g. very early bloom of diatoms). This was initially a motivation to consider to size classes of diatoms.

Here (in Figures R2.4 – R2.7) we compile all climatological monthly mean PFT dominance for PHYSAT, Darwin-15, REF and PHAEO (REF and PHAEO climatologies are calculated over the years 2006 - 2012). These four figures are now in the Supplementary Material.

In the revised version of the manuscript (Figure 2) we present PFT dominance for climatological December – January – February and July obtained from PHYSAT, Darwin-2015 (Dutkiewicz et al., 2015) and REF experiment (blue boxes in Figures R2.4 – R2.6).

Note: as seen from the comparison of 2003/2004 monthly mean PFT dominance (presented in the original version of the manuscript for REF and PHAEO) to the climatology, the year 2003/2004 is typical with respect PFT dominance. The outputs are similar to climatology but showing finer spatial structures.

Figure 8 (in the revised manuscript) depicts PHAEO climatological December – January – February and July PFT dominance (blue boxes in Figure R2.7).

In the revised manuscript we edited the text as following (L306-317):

"For complete 12 monthly mean climatologies for PFT dominance as retrieved by PHYSAT and predicted in Dutkiewicz et al. (2015) and REF experiment, the reader is referred to the Supplementary Material (Figures S15 – S17, respectively). In general, the PHYSAT Southern Ocean PFT dominance climatology (over the years 1998 – 2006) shows a strong seasonal variability of PFT compositions and contributions of PFTs to TChla (Alvain et al., 2008). From November to January south of 40°S, the diatom contribution is higher than 50%. This high diatom contribution during the austral spring and summer is associated with large diatom blooms starting in October at lower latitudes and moving towards higher latitudes in December – January. The nano- non-silicified phytoplankton is dominating during the time period from March to October. The Southern Ocean PFT dominance obtained in Dutkiewicz et al. (2015) disagrees with PHYSAT observations: diatoms are underrepresented in comparison to PHYSAT in circumpolar Southern Ocean during January and February, while in July they are over-represented in the Atlantic section of the Subantarctic Zone which is also opposed to the observed dominance of haptophytes. Generally, the model version Dutkiewicz et al. (2015) overestimate the dominance of small non-silicified phytoplankton. These results clearly indicate deficiencies in the Dutkiewicz et al. (2015) model setup and motivated a series of Darwin-MITgcm experiments, with different model configurations with respect to assumed PFTs and their traits described by various physiological parameters."

120

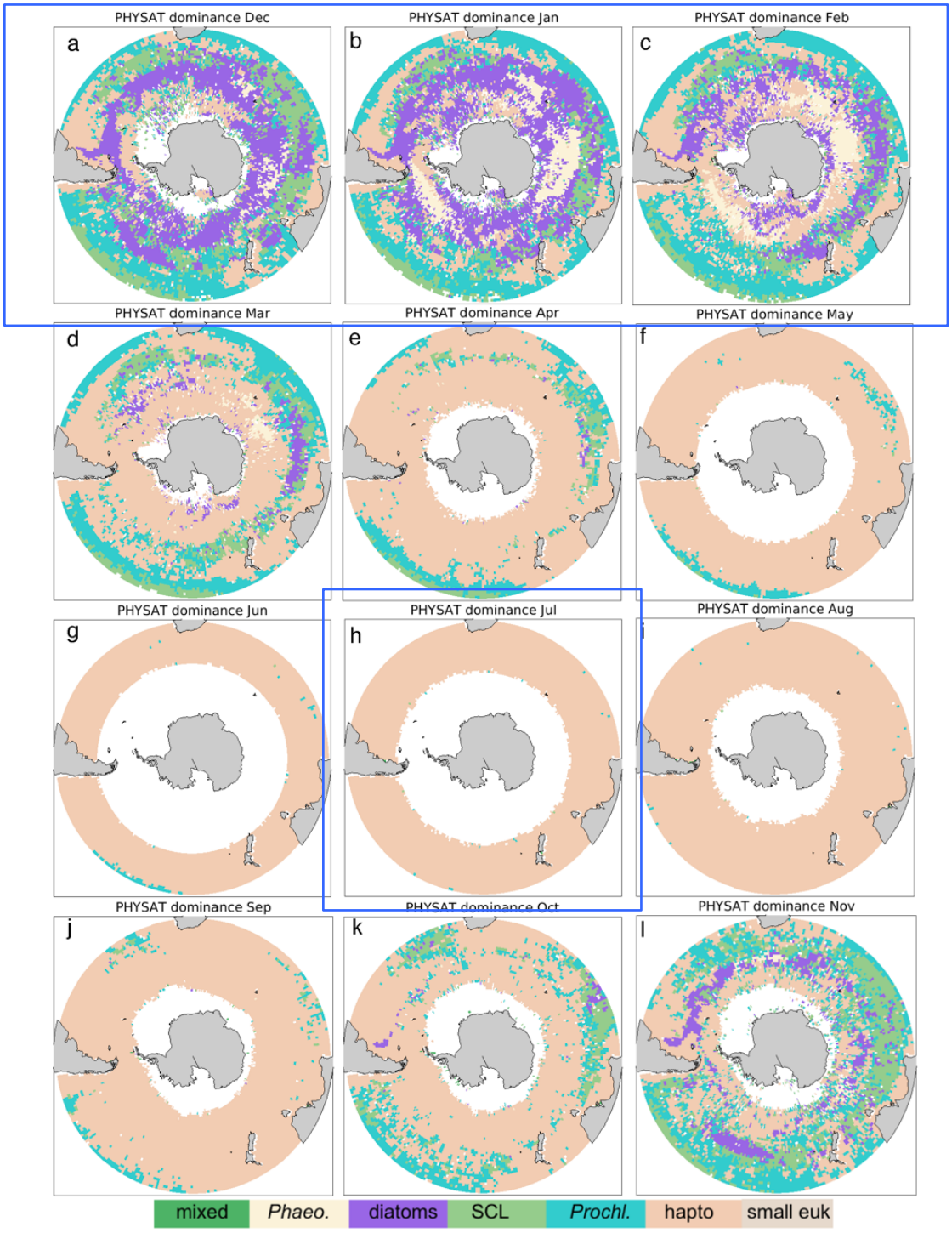
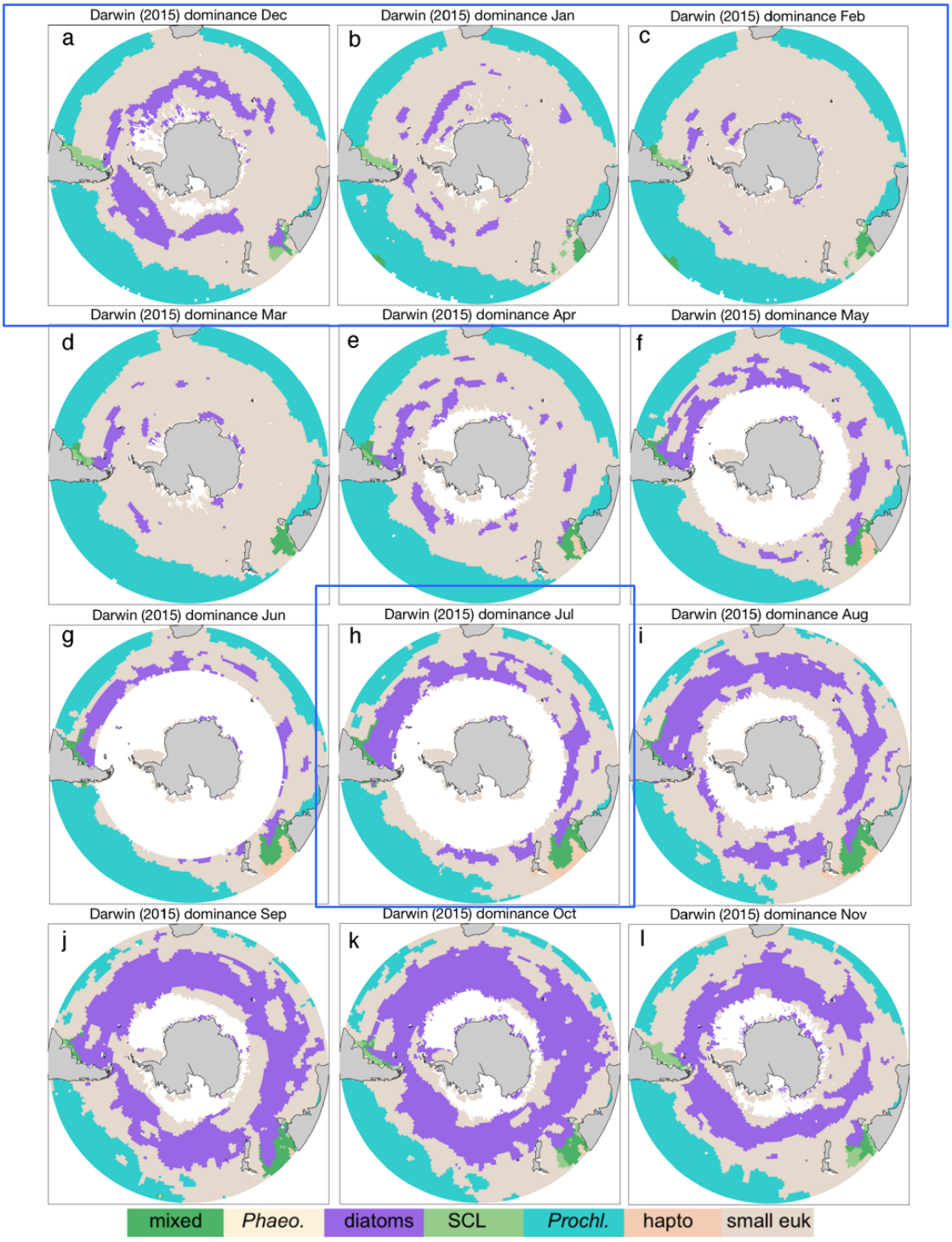


Figure R2.4: PHYSAT dominance climatology over 1998 – 2006 (Figure S15 in Supplementary Material).



125

Figure R2.5: DARWIN-2015 (Dutkiewicz et al. 2015) dominance climatology, masked by PHYSAT missing values (Figure S16 in Supplementary Material).

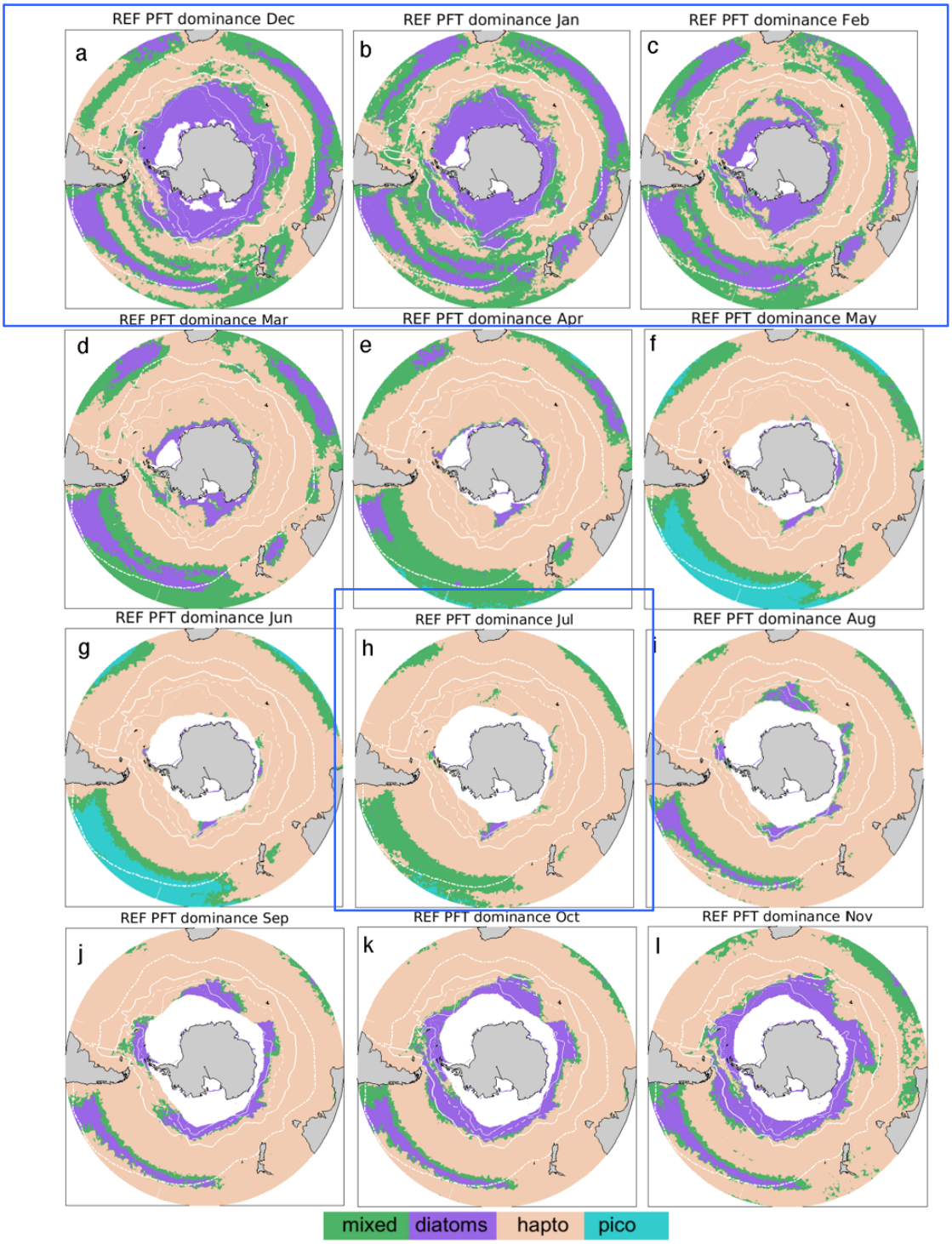


Figure R2.6: REF dominance climatology over 2006 – 2012 (Figure S17 in the Supplementary Material). The model output is masked by the area with sea ice concentration > 75% during respective month. White contours denote the Southern Ocean fronts (Orsi et al., 1995; Orsi and Harris, 2001).

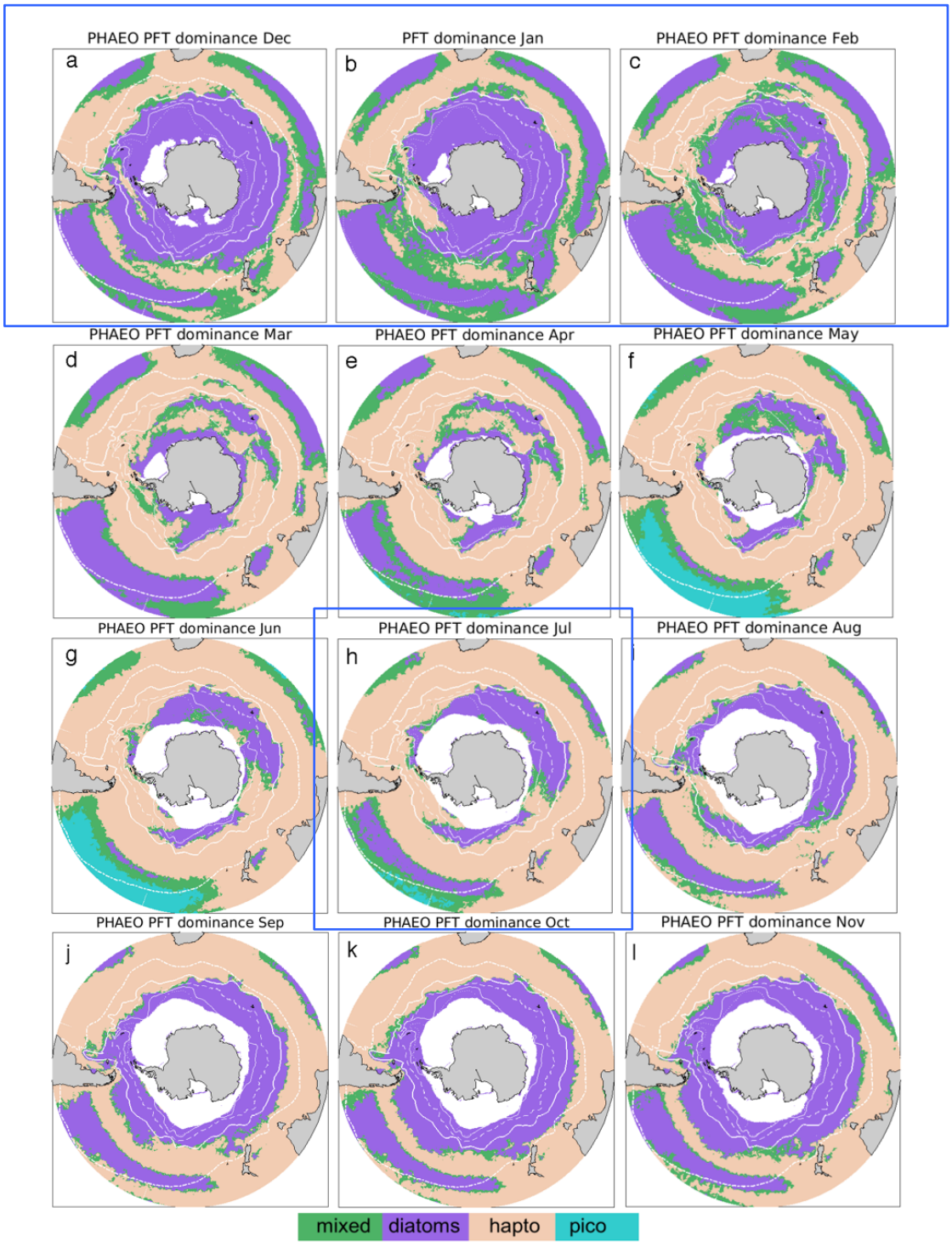


Figure R2.7: PHAEO dominance climatology over 2006 – 2012 (Figure S18 in Supplementary Material). The model output is masked by the area with sea ice concentration > 75% during respective month. White contours denote the Southern Ocean fronts (Orsi et al., 1995; Orsi and Harris, 2001).

L. 208: The transition between sentences is confusing for the reader: "[...] in austral summer. In July, [...]" First, you set the reader up for hearing more about the summer and then you jump to talk about July. Please rewrite.

R: We have rewritten as following: "…, while in July simulated diatoms were dominant in the"

L. 210: Related to above, looking at the model performance in a single month does not tell you much about how the model is doing in terms of simulating phenology. Please rephrase.

R: Changed to "indicates model deficiencies in presenting PFT distribution."

L. 210-211: Which model are you referring to here?

R: We added a reference to Darwin-MITgcm – 2015 version (Dutkiewicz et al., 2015).

Throughout the text, please add references to panels of the Figures (these need to be added to each Figure!), as this will be very helpful for the reader.

R: In the revised version we added the references to Figure panels.

Maybe refer also to the HPLC data here? These should support the discussed bias in the community at high latitudes.

R: The REF experiment was not compared to the HPLC derived PFT Chla since it was not our "final" model (did not differentiate among haptophytes).

L. 211-214: This information belongs into the method section. What exactly do you mean by "in terms of agreement with observed phytoplankton composition"?

R: Here we reported (as a summary) on the experiment showing best results among a number of sensitivity experiments (some of them listed in the Supplementary Material) with respect to diatom phenology and general PFT dominance (that reflects also the phenology) but not allowing to distinguish among haptophytes. It is why we just briefly report on the results with the kind of motivation for experiment PHAEO that is evaluated more thoroughly (and also contains small and large diatoms).

How did you evaluate this?

R: The simulated composition/dominance was qualitatively evaluated against PHYSAT and Trimborn et al. (2015) observations.

For completeness, consider adding the reference Trimborn et al. (2015) to the method section

R: The reference to the study by Trimborn et al. (2015) has been added.

2.2.1. Where do you show the diatom phenology of the model?

R: Phenology itself (not phenological indices) can be seen from the supplemented videos showing the dynamics and distribution of the Southern Ocean diatoms, haptophytes and prokaryotes simulated with experiment PHAEO over the time period August 2002 – April 2012.

L. 218-220: I am curious to what extent the improvement of the model in the SO is at the expense of the model performance on the global scale. Are the simulated patterns still reasonable?

R: we have not thoroughly evaluated model performance globally. Some assessments have been performed for the Arctic Ocean. Results showed satisfactory results with respect to TChla, bloom development and nutrients. On a global scale the simulated PFTs show expected distribution of prokaryotic pico-phytoplankton at low latitudes (from 40°N to 40°S) and abundance of diatom and haptophytes at high latitudes.

L. 220-223: I cannot follow what you base this conclusion on given the plots you're showing in the manuscript, but I think this is an important point to make.

If you really significantly improve the simulated phenology by including two types of diatoms instead of one, this aspect deserves a lot more room than it currently gets in the manuscript in my opinion.

R: Indeed, we planned a separate paper focusing on the analysis of phenological indices. The dominance plots are a part of model evaluation with available satellite information, but nevertheless reflects the PFTs dynamics (or phenology, if the term "phenology" is used as PFT Chla dynamics in general).

We added the figure below on REF phenology of diatoms (Figure 3 in the revised version of the manuscript). The figure shows the spatial distribution of the REF diatom phenological indices in 2007/2008: bloom start date, chlorophyll-a maximum date, and bloom end date. "We chose this particular year because: 1) with the two-weekly model output the phenological indices can be more precisely calculated than based on the two-weekly or monthly mean climatology; 2) it is a typical year over the period 2006 – 2012 with respect to the simulated PFT distribution (after model reached the quasi-steady state) and climate oscillations (Soppa et al., 2016)."

Consider including plots of the simulated phenology (e.g. bloom start and bloom peak of total chlorophyll and diatom chlorophyll in "old version", REF and PHAEO) as compared to those derived from satellite products.

R: Figures R2.8 and R2.9 depict diatom phenological indices – bloom start date (BSD), chlorophyll maximum date (CMD) and bloom end date (BED) – for experiment REF and PHAEO, respectively. The phenological indices were calculated following (Siegel et al. 2002, Soppa et al. 2016) based on model snapshots of the year 2007/2008. For the "old version" (Dutkiewicz et al. 2015) the outputs are stored only as monthly means, which makes it hardly possible to estimate accurately the phenological indices.

Figure R2.10 show Chla spatial distributions for large and small diatoms for three months of the year 2007/2008. This figure demonstrates clearly that south of the Polar front diatom is mostly represented by large cells, while north of the Polar front the diatom is abundant in small cells showing distinct phenology.

Section 2 has been extended by additional Subsection 2.3 "Diatom phenological indices" with the following text:
"Following Soppa et al. (2016) we evaluate the diatom phenology by calculating phenological indices based on a threshold method proposed and initially applied for assessing the TChla phenology by Siegel et al. (2002). In particular, we use the following indices: the Chla maximum date, the bloom start date, and the bloom end date. These indices are calculated based on the REF Chl simulations for diatoms (including small and large) over the year 2007/2008…"

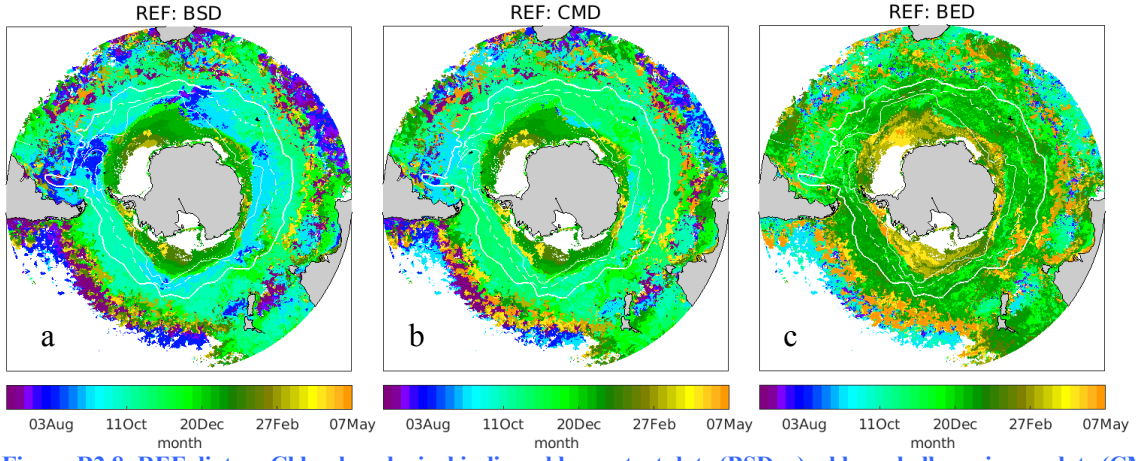


Figure R2.8: REF diatom Chla phenological indices: bloom start date (BSD, a), chlorophyll maximum date (CMD, b), bloom end date (BED, c) (Figure 3(a,b,c) **in ther revised version of the manuscript).**

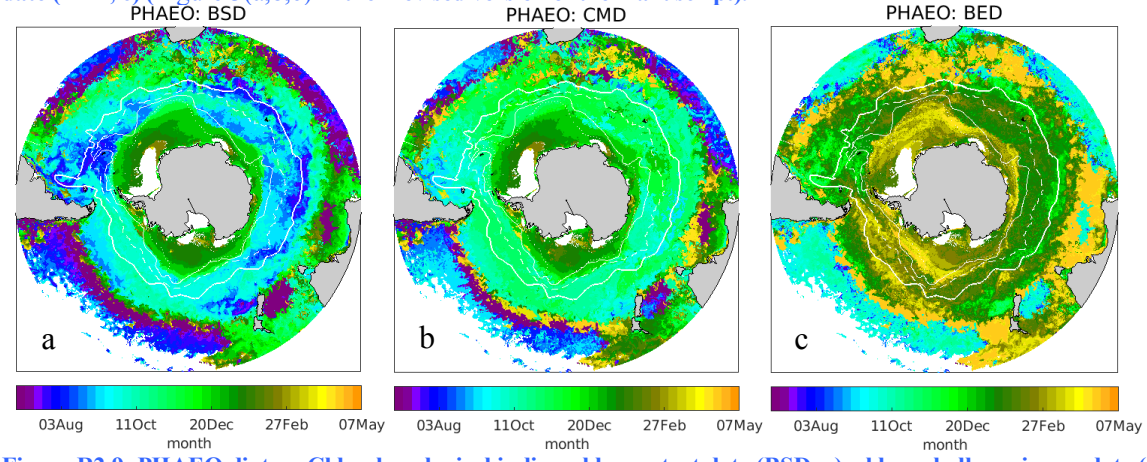


Figure R2.9: PHAEO diatom Chla phenological indices: bloom start date (BSD, a), chlorophyll maximum date (CMD, b), bloom end date (BED, c).

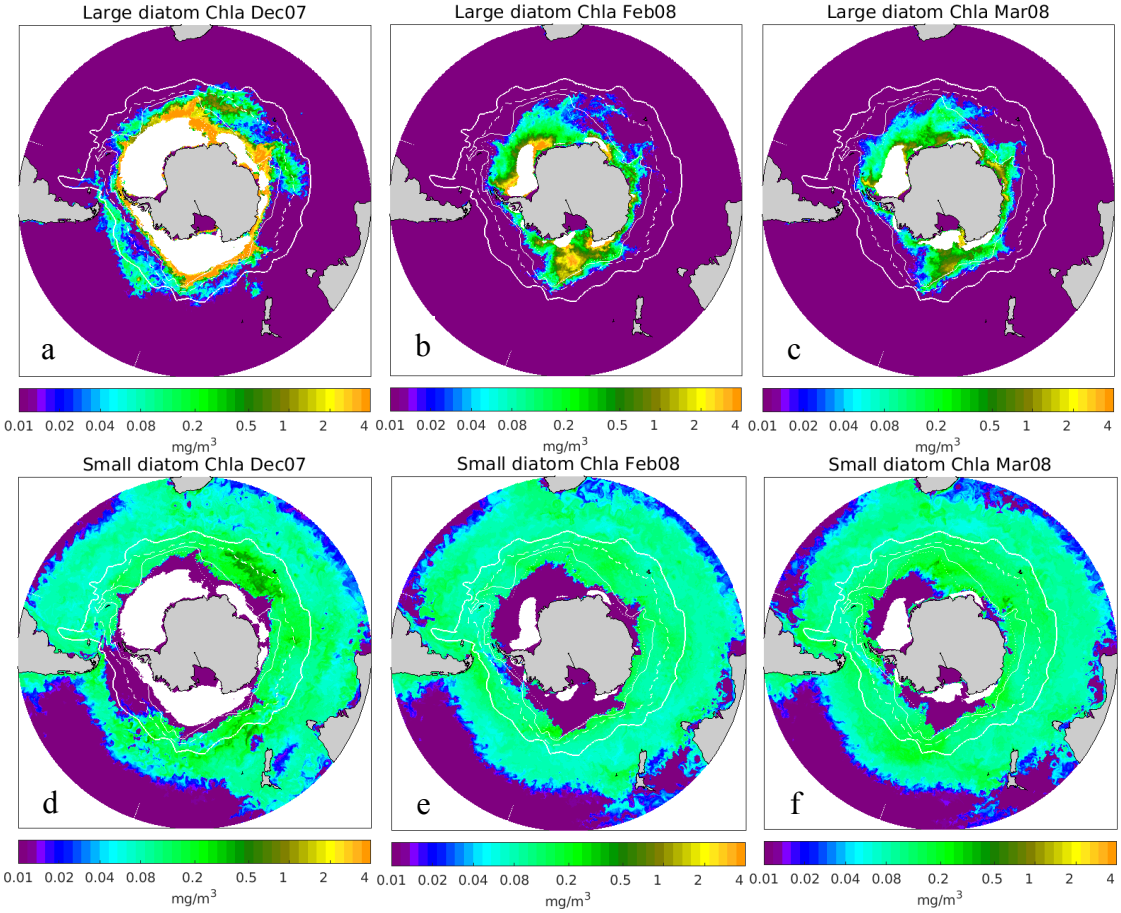


Figure R2.10: REF: spatial distribution of Chla for large diatoms (upper panels) and small diatoms (low panels) for December 2007 (a and d), February (b and e) and March (c and f).

Figure R.210 is similar to Figure 3(d,e,f,g,h,i) of the revised version of the manuscript. (Mind that in the manuscript the figure panels - middle and low – present October 2007, December 2007 and February 2008).

Consider also adding a reference to the regional SO model used in Nissen et al. (2018) here, as this model simulates too early total chlorophyll/diatom blooms as well, demonstrating that this issue is not restricted to global models.

R: We now include the reference to the study by Nissen et al. (2018).

L. 225-226: Where is this seen? You don't show the biomass patterns for the run without the two diatom classes in the current form of the manuscript.

R: We refer to the runs without considering two diatom classes in the supplementary material. The runs shown in the main manuscript clearly show the distribution of large diatoms at high latitudes and small diatoms at low latitudes (see Figure 3 and

Figure R2.10). Figure 3 has been replotted for the year 2007/2008 and also includes now the phenological indices for the same time period (see also Figure R.2.8).

L: 228-229: In what way is the simulated pattern in agreement with the cited studies? Please be more precise here. Related to earlier comments, how did you evaluate this exactly?

R: We evaluate this qualitatively by comparing our plots to those presented by Signorini et al. (2006), Balch et al. (2016) and Smith et al.(2017) in their papers. We removed this sentence in the revised version of the manuscript.

L. 233-236: Consider rephrasing "the model representation of co-existence/competition within the haptophyte group" to something like "the simulated biomass distributions of both coccolithophores and Phaeocystis were very sensitive to chosen model parameters, and small changes in [...]".

R: Thanks, rephrased as suggested.

What "small changes in the Darwin model physiological parameters" are you referring to here exactly? What is a small change in this context? And which parameters are you referring to?

Can you include more information on these in the supplementary material?

R: 5% (or less) changes in palatability factor or mortality rate of coccolithophores or *Phaeosystis* could lead to the situation when one overcompetes the other. This was documented in the sensitivity tests overviewed in the Supplementary Material (S1.1.2 – S1.1.4).

Am I understanding it correctly that by the end of your reference simulation, coccolithophores go extinct in your model? If this is indeed what you mean, I am not entirely sure I understand why this happens, but I certainly find it very worrisome for the evaluation of your reference simulation, as this implies that you have significant drift in your PFT biomass concentrations and/or distributions. Is this the case?

R: In revised version we write that "after reaching a quasi-steady state, in experiment REF" and show REF results after reaching this quasi-steady state. In the revise version of the manuscript we more carefully state the issues of the long-term decline in coccolithophores.

"For instance, in experiment REF after reaching a quasi-steady state, coccolithophores did not survive. It happened because there were not sufficient differences between the traits assumed for coccolithophores and "other large" (or *Phaeocystis*-analogue). As a result, it took longer for the model to get in a quasi-steady state and finally lead to just one of the haptophytes survived (taking over for another). Hence, the experiment REF represents diatoms and haptophytes after reaching a quasi-steady state, but cannot distinguish among haptophytes. In original Darwin-2015 model (Dutkiewicz et al. 2015) "other large" did not survive"

This also worries me in that your choice of showing different time periods in the different figures of the manuscript will then have a possibly considerable impact on the biogeographies you show.

R: All the figure shown in the original version of the manuscript (except for figure 3 and 6, the panels depicting exp. REF related to distinguishing among haptophytes) were representative for the considered period of evaluation time period August 2002 – April 2012 (see figure R1.1 in our response to reviewer 1). In the revised version of the manuscript we do not show the year 2003/2004.

The issue we stressed here with REF results is about the model behavior in the presence of the biochemical module structure with two similar (with respect to the assumed traits) PFTs leading to that just one of these two exists. In experiment REF we did not differentiate sufficiently enough between the traits assumed for coccolithophores and "other large" (or Phaeo), which lead to a longer integration period of time to reach a quasi-steady state and to just one of "similar" PFTs survived. It means that in experiment REF *Phaeo*-analogue indeed represents haptophytes in general (taking over for coccolithophores). In the original Darwin-2015 model (Dutkiewicz et al. 2015) "other large" did not survive.

We make this clearer in the text as mentioned above.

In observations, the biogeographies of coccolithophores (mainly in the subantarctic) and Phaeocystis (only P. Antarcticain the SO, mainly in the high-latitude SO) do generally not fully overlap, so I don't understand how competitive exclusion between these two types of phytoplankton leads to the extinction of one in the model, as I don't see these two types exclusively competing for nutrients.

R: In model configuration REF (as in Darwin-2015), however, the traits initially considered in the way that "other large" and coccolithophores compete for the same resources. By considering two life stages of *Phaeocystis* we introduce additional differences in the traits, which along with changed physiological parameters for coccolithophores makes coccolithophores competitive among phytoplankton of larger cell size (or colonies) that requires higher nutrients concentration to grow and/or among PFTs of similar size – small diatoms and *Phaeocystis* solitary cells – that have higher palatability factor to be grazed.

We make this clearer in the text (L582-587).

L. 240: Please clarify: Does the reference simulation already have the changes listed in the method section (in the nutrient affinity and the grazing pressure)? In the method section it sounds like it, here in the result section it does not, I got confused.

R: The reference simulation (REF) contains the changes with respect to the nutrient affinity and the grazing pressure as listed in the Method section. The discussed in line 240 is the introduced "two distinct life stages of Phaeocystis ant. (colonies and solitary cells) in which its morphological features and physiology depend on iron availability (Bender et al., 2018)."

L. 245: Why this exact month?

R: February is chosen as one of the austral summer months (in Figure S8 of the supplementary material, we present also results for different months). The year 2008 is typical (as explained above). We clarify in the text:

"Figure 4 presents these meridional PFT distributions of the different PFTs in February 2008 (one of the months discussed in the previous subsection, Figure 3)"

L. 247: Please clarify: By "other large", you mean large diatoms and Phaeocystis together?

R: "Other large", in terms of the study by Dutkiewicz et al. (2015), means non-silicified nano-phytoplankton, in our case *Phaeocystis*-analogue, not strictly however (see table 2 of the original version, table 3in the revised version).

The text is revised accordingly:

"One can see that in experiment REF, "other large" (Dutkiewicz et al. 2015, in our case non-silicified nano-phytoplankton including Phaeocystis, but not strictly) outcompetes coccolithophores leading to too low concentrations of coccolithophores north of the Polar Front…"

Also, your statement "too low concentrations of coccolithophores south of the PF" is based on what? This statement confuses me due to two reasons: First, Fig. 4 only shows relative contributions to total phytoplankton biomass and does not give any information on absolute biomass levels. Second, I am not aware that one would expect significant concentrations of coccolithophores south of the PF (see e.g. Balch et al., 2016). So what exactly are you referring to here?

R: Thank you for this comment. The correct statement should be "too low concentrations of coccolithophores **north** of the PF" for experiment REF based on what is known from observations in the Great Calcite Belt (Smith et al.,2017; Balch et al., 2016)

L. 249: Similar to above, what do you mean by "more plausible" here? Compared to what? Please be more precise and avoid subjective judgement.

R: "more plausible" with respect to the coccolithophores fractions gradually increasing in the direction to the north of the subantarctic front, compared to experiment REF that could not simulate specifically coccolithophores in the Great Calcite Belt (Balch et al. 2016, Smith et al. 2017) since hardly distinguished between coccolithophores and Phaeocystis.

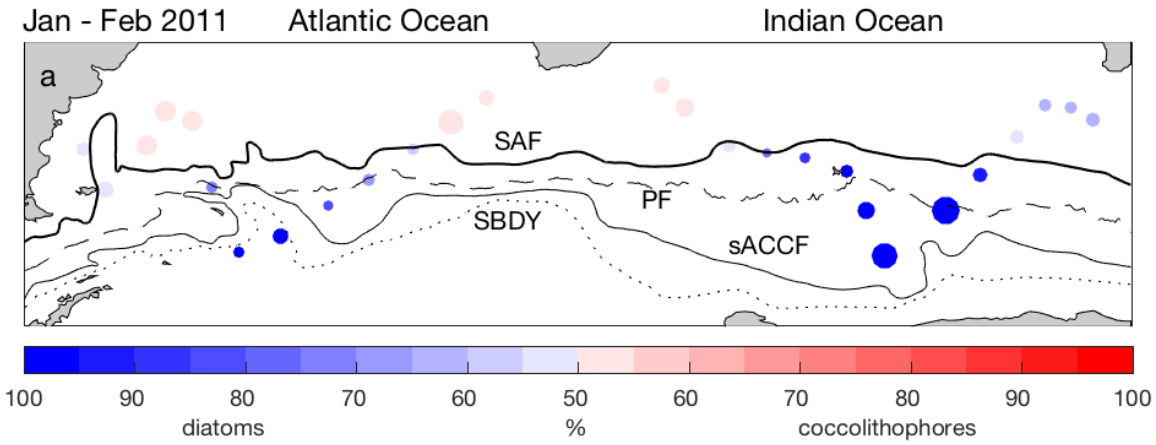
However, we have changed the text as to not using subjective statements.

"One can see that in experiment REF, "other large" (Dutkiewicz et al. 2015, in our case non-silicified nano-phytoplankton including *Phaeocystis,* but not strictly) outcompetes coccolithophores leading to too low concentrations of coccolithophores north of the Polar Front, while small diatoms exist in both experiments (however, in different percentages). In experiment PHAEO, meridional distributions of the phytoplankton composition reveal that the coccolithophores fraction gradually increases to the north of the Subantarctic Front… This result is comparable to the estimates of Smith et al. (2017) obtained in AOS and IOS for late summer (January – February – March) of the years 2011 and 2012."

L. 251: I think this statement needs to be rephrased. Smith et al. (2017) state that based on their measurements, coccolithophores made up maximum20% of total chlorophyll concentrations locally, but generally contributed less than 5%. Consequently, I would phrase it more conservatively than saying that simulating 30% of total biomass is in agreement with Smith et al. (2017), which it clearly isn't.

R: We agree, our estimates of PHAEO coccolithophores contribution to total biomass exceed 20% reported by Smith et al. 2017 based on *in situ* SEM (but in much better agreement than those from experiment REF). Moreover, in general, our PHAEO results confirm what is clearly stated in this study by Smith et al. (2017): in the Great Calcite Belt, coccolithophores are "an important contributor to phytoplankton biomass" and can contribute more than 50% into the biomineralizing phytoplankton. To avoid any further confusions, we now present diatom *vs.* coccolithophores dominance as shown in Smith et al. 2017. We now provide the following figure (Figure R2.8, Figure 5 in the revised version of the manuscript) depicting diatom *vs.* coccolithophores dominance obtained in experiment PHAEO…

"…collocated in space and time with observations of Smith et al. (2017). Although our estimates have been obtained based on phytoplankton biomass (mmolC m^{-3}), but not on cell counts as in Smith et al. (2017), our results agree to their higher concentrations and dominance of diatoms in the SBDY and SACCF, while north of the Polar Front coccolithophores become more abundant. However, as compared with Smith et al. 2017 (their figure 2), in the Atlantic section, the dominance of simulated coccolithophores (55%) is shifted northward of the subantarctic front leading to underestimation of the coccolithophore dominance along the polar front and south of SAF front and overestimation north of SAF."



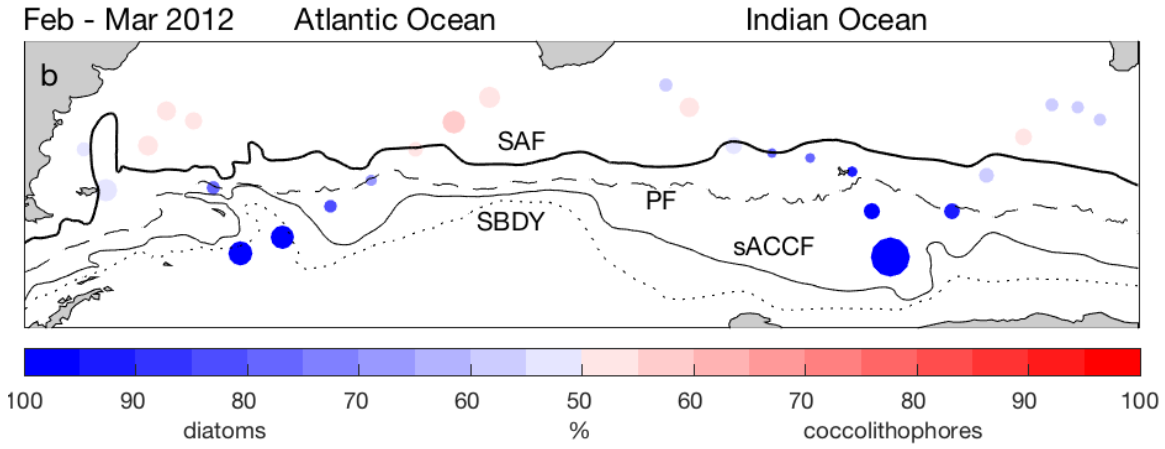


Figure R2.11: PHAEO Diatom *vs.* coccolithophores dominance averaged over January-February 2011 (a) February - March 2012 (b). The size of the circles is relative to phytoplankton carbon content (mmolC/m^3). The largest size of the circle corresponds to 3.12 (mmolC/ m^3). (Figure 5 in the revised version of the manuscript)

L. 253: I think you're referring to Section S3 here.

R: Right we refer to Section S3.

Do I expect the fraction of coccolithophores to be higher in winter? How is this backed up by observations (e.g. HPLC)? And how relevant is the community structure in SO winter, when biomass levels are generally very low?

R: North to subtropical front the model simulations show not negligible biomass (please see supplemented video materials). However, we cannot confirm that it is expected that fraction of coccolithophores to be higher in winter, since there is no *in situ* HPLC observation except for August 2006. However, based on satellite retrievals Alvain et al. (2008) reported on decreased fractions of diatom from March to September and mostly non-silicified nano-phytoplankton contributing to the TChla.

L. 254-255: This is an obvious statement. What is the reader to take away from the distribution of zooplankton biomass?

R: Sorry, in a pre-version, there were a following sentence on comparison of the distribution of zooplankton biomass (mmolC m^{-3}) to the MAREDAT data (in the context of shown ranges). Thus, we wanted first to comment on the agreement with the MAREDAT zooplankton data and continue with zooplankton grazing pressure.

In the revised version we write:

"The largest differences between REF and PHAEO zooplankton is shown between 75°S and 50°S. However, for both experiments, REF and PHAEO, simulated zooplankton is within 0 to 20 mgC m^{-3}, which agrees with in situ observations reported by Moriarty and O'Brien (2013) and shown in Dutkiewicz et al. (2015) and in Supplementary Material (Section 5.2)"

L. 255-260: Here again, what is the "realistic distribution" for you? What are the "other circumstances"? This is a very vague statement. Please be more precise.

R: Rephrased as following:

"The discussed distribution of coccolithophores have been obtained under the assumption of lower palatability function (leading to lower grazing pressure) in comparison with what is assumed for other PFTs"

By "other circumstances" we meant other assumptions on the PFT traits made in the current model set up. We delete the words in the revised manuscript

Have you done a sensitivity simulation in which coccolithophores could not escape the grazing pressure to assess the impact on the biomass distributions and community structure? This would be very interesting to back up your statement.

R: Indeed, our choice of particular model configuration is based on several sensitivity tests with the assumed PFT palatability factor. However, it was also supported by literature.

Related to above, in this context it will matter a great deal how different you choose e.g. the maximum grazing rates of zooplankton grazing on coccolithophores as compared to grazing on e.g. diatoms in the model, which is related to what assumptions you make regarding the coccolithophore community you're simulating (all coccolithophore species? E. huxleyi only?

Please see also comment further up and prey preferences of the zooplankton PFTs.

R: In other words, depends on assumed palatability factor for diatoms, *Phaeocystis* and coccolithophores (0.8, 0.78, 0.58, respectively) regarding to coccolithophores community but considering still *E. huxleyi* as dominant (Krumhardt et al. 2017, Smith et al. 2017).

The "palatability matrix" defines the zooplankton preferences for different phytoplankton types. For most phytoplankton this is 1. Maximum grazing rate is multiplied by this factor.

Furthermore, I am wondering how high your simulated coccolithophore carbon biomass concentrations are compared to e.g. MAREDAT observations.

R: Compared to MAREDAT observation, we overestimate coccolithophores carbon biomass (Figure R2.1). However, it is worth keeping in mind, that estimamted uncertainties for MAREDAT coccolithophores due to the convertion from cell counts to biomass are several 100%.

Taking your ~30% contribution of coccolithophores to total biomass (which seems a bit higher than that suggested by Smith et al. (2017), see above) and a maximum of ~20% in austral summer between 40-50°S in Nissen et al. (2018; their Figure 3),

in my view, it is very conceivable to assume that this difference is to a large extent controlled by differences in assumptions surrounding the grazing formulations.

R: We agree, our PHAEO estimates of the coccolithophores contribution to total biomass a bit higher than 20% estimates reported by Smith et al. 2017 (but in much better agreement than those from experiment REF). However, in general, our PHAEO results confirm what is clearly stated in this study by Smith et al. (2017): in the Great Calcite Belt, coccolithophores are "an important contributor to phytoplankton biomass" and can contribute more than 50% (up to 78%) into the biomineralizing phytoplankton. To be more consistence with the study by (Smith et al. 2017) we now present diatom *vs.* coccolithophores dominance (Figure R2.11). We agree, that we could still calibrate the model and get lower value with slightly enlarged grazing pressure on coccolithophores, but it should be still less than for diatom and *Phaeocystis*.

Monteiro et al. (2016) reported that, in subpolar regions, the cocccolithophore contribution to total phytoplankton biomass can increase up to 40% under bloom conditions.

Additionally, if one looks at the discussion in e.g. Monteiro et al. (2016), there is a lot that is still not understood with respect to the coccosphere and grazing pressure from zooplankton, which is why I don't think one can per se say that coccolithophores should always escape grazing pressure in models –in the same way as I don't think the reverse can be stated (will be highly dependent on the ecosystem structure at a given location). Therefore, I think it is important to point that out in the manuscript.

R: We agree with the reviewer, that the distribution of coccolithophores is not always explained only by grazing protection but a combination of different factors (e. g. immune to light, high affinity to nutrients) in different area, we emphasize this in the manuscript, we now include the references to the study by Krumhardt et al. (2017) and Monteiro et al. (2016).

It is worth mentioning, that in the study by Monteiro et al. (2016), the authors stated that

- "the reduction in grazing pressure might have been the likely initial reason for why coccolithophores calcify" and there are other benefits (associated with calcification)
- "grazing protection appears to favor coccolithophores in (sub)polar, coastal, and equatorial areas" (their Fig. 4)
- the initial benefits "associated with grazing protections have relatively well-supported evidence"

W.r.t. what is still not understood, the authors of the study by Monteiro et al. (2016) mentioned that studies investigating the protective role of coccolith based on comparison of direct grazing on different clones (calcified and noncalcified, representing different phase) of same coccolithophore species reported on slower, the same or faster grazing on calcified cells in comparison with non-calcified coccolithophore cells, but these results might be obtained because of other independent of calcification conditions.

In the text we write:

"The discussed distribution of coccolithophores have been obtained under the assumption of lower palatability function (leading to lower grazing pressure) in comparison with what is assumed for other PFTs. This contradicts the study by Nissen et al. (2018), who reported on an increased (relative to diatoms) grazing of coccolithophores as a factor controlling the coccolithophore biogeography in the Southern Ocean. Our assumption on low palatability factor of coccolithophores are, nevertheless, backed up by studies by Nejstgaard et al. (1997), Huskin et al. (2000), Losa et al. (2006) and Monteiro et al. 2016. In the study by Losa et al. (2006) on optimized biogeochemical parameters, it was shown that coccolithophore blooms are associated with low grazing pressure. Based on laboratory experiments, Nejstgaard et al. (1997) and Huskin et al. (2000) concluded that coccolithophores (due to its "stony" structure) do not influence the microzooplankton growth. While the exact mechanisms of how this PFT use the coccolith to protect itself against grazing is not fully understood (Monteiro et al. 2016), the ability of coccolithophores to escape grazing control has "relatively well-supported evidence" (see Monteiro et al. 2016 for review)."

Additionally, note that Nissen et al. (2018) state that grazing is a major control on the simulated coccolithophore biogeography and their biomass concentrations relative to those of diatoms, but they do not comment on the effect of the assumed grazing difference between diatoms and coccolithophores on the simulated phenology of the two in the subantarctic. Please rephrase L. 258 accordingly.

R: Rephrased accordingly (the word "phenology" has been removed).

Additionally, without the relative grazing advantage of coccolithophores relative to diatoms, the simulated coccolithophore biomass levels in Nissen et al. (2018) increase three-fold between 40-50°S (see their Figure 7), pushing the simulated coccolithophore biomass levels way beyond what MAREDAT observations suggest for this area.

R: We are not sure it is a good argument. It could be just a compensation for other model deficiencies. Moreover, the aforementioned representation error in the MAREDAT data are large.

Besides, in the studies by Krumhardt et al. (2019), Monteiro et al. (2016) the grazing pressure (palatability factor) for coccolithophores was also considered lower than for diatoms.

L. 260: Do you assume the drivers to be the same globally? In my view, one could very well imagine a difference in the relative importance of grazing in controlling coccolithophore bloom phenology, as the competitive success of coccolithophores will largely depend on 1) which coccolithophores are present (and hence simulated), 2) which other phytoplankton are present, and 3) which grazers are present.

R: We agree with the reviewer, moreover, in our discussion we provide two examples.

1) "The simulated abundance of coccolithophores north of the Subtropical Front (STF) – where phosphate occurs in very low concentrations – is explained by the introduced high affinity of this PFT to phosphate (small half-saturation rate) allowing coccolithophores to grow in nutrient depleted conditions.

2) "in the region between the Subtropical and Subantarctic Fronts the occurrence of coccolithophores is more evidently linked to low grazing pressure on this PFT, due to its much lower palatibility for zooplankton in comparison with small diatoms or *Phaeocystis* presented by single solitary cells."

I suggest to point this out as a potential limitation of the comparison of a study focusing on the North Atlantic to the one here.

R: Our assumptions are based not only on studies focusing on the North Atlantic, they are supported also by the study by Paasche, 2001, Iglesias-Rodríguez et al (2002), Rost, B. and Riebesell (2004), Krumhardt et al. (2017) provided global overview on coccolithophores as well as backed up by the study by (Monteiro et al. 2016).

L. 269: Please rephrase in order to avoid subjective statements like "agreed well". Additionally, where is this seen? I suggest to add validation plots to the supplementary material.

R: We provide the statistics and we rephrase the text as following.

"In general, the simulated surface nutrient climatology agrees well with the World Ocean Atlas (Garcia et al., 2014) with correlation coefficient of 0.90 and 0.97 and normalised standard deviation of 1.27 and 1.13 for silicon and phosphate, respectively".

L. 266-278: Why do you show March of 2004 now?

R: In the revised version we show the year 2008 (a typical year, it does not change anything with respect to discussion and conclusion). However, it worth emphasizing that we show a particular summer month (of the year 2008) March (or February as in the revised version of the manuscript) but not a climatological month to see much clearly patterns of the depicted distributions. We make the reason behind this choice clear in the revised text so as not to cause the confusion that the original version caused.

"We present this particular summer month of a typical year to show much clearly the patterns of the depicted and discussed distribution which could not be obviously seen on seasonal or climatological mean maps"

L. 271: Why "potential existence in colony form"? Does that mean you did not track when and where Phaeocystis was present in the colonial form in your simulations? I think this information would be a useful output to assess where and when the chosen parametrization leads to colony formation and to assess/discuss/speculate what impact neglecting further dependencies of colony formation (light etc., see above) have on the simulated biogeography.

R: We did not trace it explicitly for the outputs, since the priority was to get *Phaeocystis* and coccolithophores co-existing. However, we agree that this would be interesting and we will do this in future studies.

L. 272-274: I don't see in Figure 5 how the introduction of the high nutrient affinity of coccolithophores causes what you claim here. For that, you would need to show the original biogeography before applying the changes.

R: Indeed, it is seen from the depicted distribution of PFT Chla (e.g. coccolithophores) in line with obtained distribution of nutrients (e.g. phosphate). We write,

"The spatial distribution of silicon, dissolved iron and phosphate is discussed in line with the simulated PFT Chla biogeography. … Thus Figure 6 shows that the simulated abundance of coccolithophores north of the South Subtropical Convergence province (SSTC) – where phosphate occurs in very low concentrations – is explained by the introduced high affinity of this PFT to phosphate (small half-saturation rate in the γ_η function) allowing coccolithophores to grow in nutrient depleted conditions. … "

L. 274: Replace "depleting" by "depleted".

R: Replaced.

L. 275: Where is the Subtropical Front in the plot? The STF is not introduced and the caption of Fig. 5 does not include a definition of the white contours either. Please include this information somewhere.

R: We now show the position of the Subtropical Front in Figure 1.

L. 275-277: Similar to comment on L. 272, I don't see how Fig. 5 shows this. Again, one would need the plot before the change – otherwise I don't understand how it is possible for the reader to see this. Please clarify.

R: Please see our response to the comments on L. 272-274, here we continue

"…However, between the Subtropical and Subantarctic Fronts, where there are still nutrients (other than phosphate) to support the growth of small diatoms and *Phaeocystis,* the occurrence of coccolithophores is more evidently linked to low grazing pressure on this PFT due to its much lower palatibility for zooplankton in comparison with small diatoms or *Phaeocystis* presented by single solitary cells."

L. 277-278: Please rephrase "the simulated coccolithophores". This sentence does currently not make a lot of sense.

R: Thank you. Rephrased as "As in the study by Smith et al. 2017 reported on observed coccolithophores biogeography in the Great Calcite Belt, our simulated coccolithophore Chla is distributed in the silica-depleted area, where small diatom cells, if could still compete for other nutrients, have higher palatability for grazers."

What do you conclude from the fact that you find highest coccolithophore biomass levels (I assume that is what you mean here) where/when silicic acid is depleted? Please discuss shortly what this implies for the competition with diatoms.

R: Thank you for the comment, indeed, it implies for the co-existence/competition with diatoms presented in small cells, since, even if small diatom could compete on phosphate, nutrients or iron, it can be easier grazed than coccolithophores. Coccolithophores do not compete with small diatoms on silica resources and might survive due to its lower palatability factor.

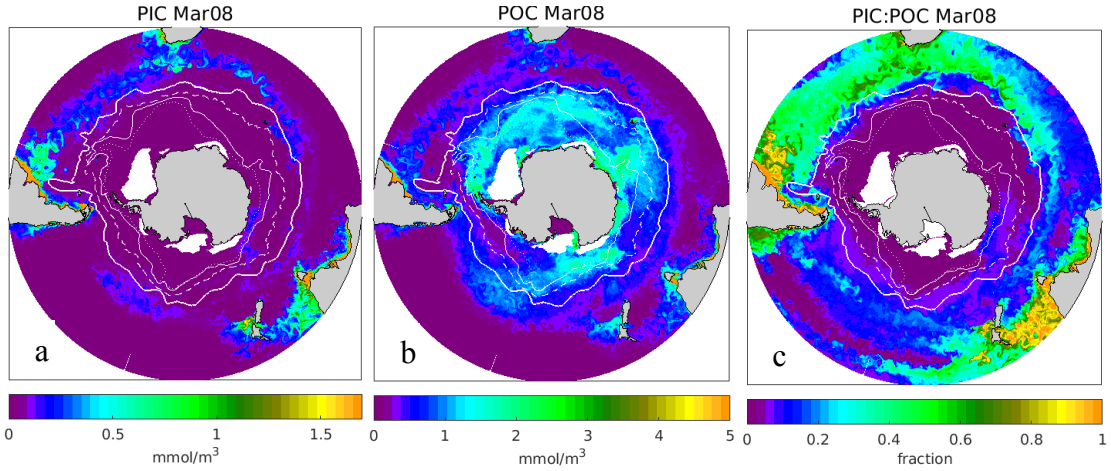
Moreover, in this area silica limited diatoms can grow slower allowing coccolithophores for early access to other not used by diatoms macronutrients and iron (Dutkiewicz et al. 2019). We edited the text as written above (L412-416).

555 L. 279-280: Please revise the grammar of this sentence.

R: Revised as following "Figure 7 illustrates the implication of the differences among the haptophytes on the carbon cycle as carbon contributes differently into inorganic and organic, particulate and, consequently, dissolved pools."

L. 281: Again, why March 2004?

560 We can show climatological March or February (summer month) or March/February 2008 (summer month of a typical year, as discussed above). See figures R2.13 and R2.14. It does not change the point. By showing a particular month we just benefit with respect to finely shown and resolved patterns of the distribution.



565 **Figure R2.13: PHAEO surface distribution of the PIC, POC and PIC:POC ratio for March 2008**

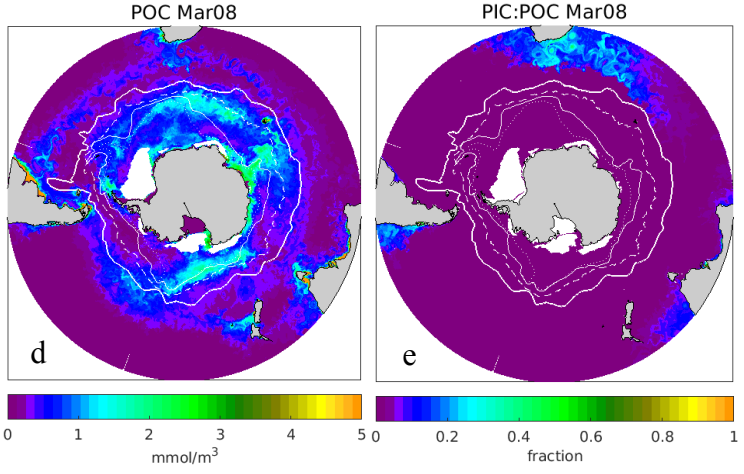


Figure R2.14: REF surface distribution of the POC and PIC:POC ratio for March 2008

L. 279-284: This whole paragraph is too superficial and lacks the build-up from the introduction and method section, as the impact of different phytoplankton types on POC production/availability in not thoroughly introduced.

R: The purpose of presenting the figures was to emphasize the importance of distinguishing among haptophytes when providing any carbon stock estimates. We did not introduce POC equations/parameterisations since for the full set of Darwin equations we refer(red) the reader to the study by Dutkiewicz et al. (2015).

Additionally, you nowhere state what assumptions you make in DARWIN regarding the routing of biomass losses to POC by the different PFTs. What do you assume for coccolithophores, diatoms, and Phaeocystis?

R: As in the study by Dutkiewicz et al. (2015, their equation A12), POC is a prognostic variable presenting integrated total particulate dead organic carbon (that makes it different from what is measured).

The model parameterizes a fraction of dead cells, and non-assimilated grazed cells to either a DOC or a detrital (i.e. POC) pool. This fraction (0.5) is the same for large diatom, coccolithophores and *Phaecystis*. For *Prochlorococcus*-like prokaryotic pico-phytoplankton and Nfixer the fraction is 0.2.

Why are the POC concentrations south of the SACCF higher in the PHAEO simulation? I suggest to relate this back to changes in phytoplankton community structure and assumptions in the model, so that the reader can take something away from your statement.

R: Experiment PHAEO reveal different distribution and composition of PFTs (higher biomass of diatoms, for instance), which results in higher POC concentrations (see Figure 4, original version).

Are you showing POC resulting from haptophytes only or from all phytoplankton? You state that you're looking at the impact of haptophytes, but possibly, you're showing all phytoplankton. Please double-check and clarify.

R: We show total particular dead organic carbon. We clarify it in the text.

Similarly, for PIC, you nowhere state in the method section how calcification by coccolithophores is described in the model. Please add this information.

R: As in Dutkiewicz et al. (2015), PIC is produced by coccolithophores (no other type produces PIC) in accordance to their equation A15 given the PIC dissolution rate, PIC sinking rate and ratio of inorganic carbon of organic phosphate specified as $0.0033(day^{-1})$, 10 $(mday^{-1})$ and 0.8 (mmolC/mmolP).

We did not introduce this parameterization in the method, since did not opt to focus too much on the PIC/POC but rather on PFTs and provide an (obvious probably) example how the PFT distribution and composition alter the particulate carbon pool.

The cited papers by Balch et al. do not comment on POC concentrations, as far as I could see. Please double-check.

R: Balch et al. (2005) presented PIC, Balch et al. (2016) looked at PIC/POC.

L. 288-291: Similar to above, how do you define the "much better agreement" or "even larger agreement"? Try to be quantitative whenever possible.

R: we apologies for not being quantitative enough. We now tried to reduce the number of subjective terms used. However, in this particular case, only qualitative assessment (in terms of distribution) was performed, partly because of the mismatch in definition of POC in the model and observations.

We changed the text as following:

"Figure 7 illustrates the importance of distinguishing among haptophytes on the carbon cycling as carbon distributed into different inorganic and organic, particulate and, consequently, dissolved pools. Shown are the particulate inorganic carbon (PIC, panel a) produced by coccolithophores (see Dutkiewicz et al. 2015, their eq. A15) and ratio of PIC to total particulate dead organic matter (POC, Dutkiewicz et al. 2015, their eq. A12), PIC:POC (panel b), for the experiments PHAEO in February 2008. Due to the improved representation of the coccolithophores and, therefore PIC (see Balch et al. 2005) in the experiment PHAEO, the depicted PHAEO PIC:POC ratio (opposed to those in REF, Figure 7c) clearly indicates that north of the SAF the value can be from 0.4 up to 1 (on the Patagonian Shelf) which is comparable with PIC:POC export ratio presented in Balch et al. (2016), even though there is a mismatch in how POC is presented in the model and how it is measured. As in the study by Balch et al. (2016) the PIC:POC ratio is lower than 0.05 south of the Polar front."

Additionally, in Fig 2 you only show July & January for PHYSAT and the "old" model version, here you make a statement for the months June-August and December-February. Please show all months for PHYSAT and the "old" model version somewhere.

R: Here we show all months for PHYSAT and "old" model version (Figure R2.6 and Figure R2.7). The figures have been added to the Supplementary Material (Figure S15 and S16).

And again, I don't understand why you decide on these months now, when before you focused on March 2004. This is very confusing for the reader.

R: We just wanted to provide more information about PFT dominance obtained for experiment PHAEO that is evaluated in more details. It is why we showed December, January and February. That was actually what the reviewer expressed in reviewer's comment on 208 – 209: "Personally, I would have preferred to see the agreement for all summer months (December-February or even March) to additionally get a better feeling for how the model is doing in terms of seasonality". The results could be compared with the monthly PHYSAT dominance climatology published in (Alvain et al. 2008).

We apologies once again for the confusion. We have remedied this by showing now PHAEO monthly mean climatology (over 2006 - 2012) for December – January – February to compare with the revised version of figure 2. All climatological monthly mean PFT dominance obtained in PHAEO are shown in the supplementary material (Figure S18)

L. 293: "of monthly means"

R: Corrected

L. 293-298: Why do you reduce the plot to the Atlantic and Indian sector based on Smith et al. (2017)? Why 2012 now? You don't actually show any data from their study so it is not clear to me why you reduce the area shown in the Figure and why you chose a different year all of a sudden.

R: The observations shown and discussed in Smith et al. (2017) were collected in the year 2011 (January - February) and 2012 (February - March). We do not show any data from Smith et al (2017). However, our results could be compared against their data as well as to hyperspectral satellite coccolithophore retrievals shown in Losa et al. (2018, for the same domain as in Smith et al. 2017). It is why we reduce the area and chose 2012 (could be also 2011). We clarify it in the revised manuscript.

L. 296: Where is the "smaller belt"? Be precise in your description. What is the latitudinal extent in the model output and the satellite product?

R: We write:

"Figure 9 presents the monthly mean spatial distribution of simulated surface Chla for coccolithophores and diatoms over the region from 30°S to 70°S and from 70°W to 120°E as shown in the study by Smith et al. (2017). These model results are compared with Chla obtained for the same domain and time with SynSenPFT algorithm (Losa et al., 2017). The simulated coccolithophore distribution reveals the calcite belt around 35°S to <50°S, which in comparison with SynSenPFT is well agreeing considering the northern boundary. The results are supported by the PhytoDOAS PFT retrievals from hyper-spectral information presented in the study by Losa et al. (2018, https://oceanopticsconference.org/extended/Losa_Svetlana.pdf) for the related region and time frame. But opposed to these satellite products the predicted calcite belt is not extending further south of the Polar Front."

L. 298-306: This is a very nice discussion, but please link it back more explicitly to the "smaller belt" to make the take away message clearer.

R: We now continue:

"In this respect, it is worth emphasizing that SynSenPFT product at the latitudes higher than 60°S is mostly influenced by OC-PFT estimates because of much less available SCIAMACHY information (see Supplementary Material, Section S2) and the OC-PFT retrievals (Losa et al., 2017) contain information generally on haptophytes (not specifically on coccolithophores). Moreover, PhytoDOAS coccolithophore retrievals are based on coccolithophore specific absorption spectrum that is, indeed,

very similar to the specific absorption spectrum of *Phaeocystis*. Model simulations, as seen from Figures 4 and 6, support the evidence of *Phaeocystis* dominance among haptophytes at these latitudes. Thus, SynSenPFT more likely overestimates coccolithophore Chla at the latitudes higher than 60°S".

(The result is, somehow, in line with independent study by Holligan et al. (2010) concluded that current satellite algorithms may significantly overestimate PIC in cold waters of the Southern Ocean.)

Holligan, P.M. Charalampopoulou, A., Hutson, R.: Seasonal distributions of the coccolithophore, *Emiliania huxleyi*, and of particulate inorganic carbon in surface waters of the Scotia Sea, *Journal of Marine Systems*, 82 (4), 195 – 205, doi: 10.1016/j.jmarsys.2010.05.007, 2010.

Same is true for the discussion of the diatom distributions.

R: We edited the text as following:

"For diatoms, modeled Chla exceeds SynSenPFT estimates south of the Antarctic Circumpolar Current Front. However, SynSenPFT diatom Chla is known to be underestimated for the Antarctic Province (see Losa et al. 2017). At the same time, diatom Chla estimates obtained with PhytoDOAS are higher (see Supplementary Material, Section S2) despite the low coverage of the product, which can indicate that predicted diatom Chla could be a bit less overestimated than it is suggested by comparison with SynSenPFT."

L. 310: How were the days of the snapshots chosen?

R: Two-weekly snapshots over the period of time from August 2002 to April 2012.

L. 315: What is "less accurate" in this case? Please be precise.

R: We clarify: "our simulated Chla for Phaeocystis as haptophytes in Ross Sea are underrepresented in comparison with HPLC-derived estimates."

"However, the comparison of *Phaeocystis* biomass to the MAREDAT dataset (Vogt et al., 2012) revealed quite a good agreement (see Subsection 3.3.3)": coefficient correlation r = 0.54, MAE = -0.61 for log-transformed biomass (see Figure S13, in the Supplementary Material)

L. 318: Why "see Vogt et al. (2012)"? This citation here is not obvious to me. Can you clarify for me?

R: We edited the text as mentioned above and clarify in subsection 3.3.3.

L: 324: Does Fig. S9 only include model output that was collocated with the observations? Please clarify in the text and/or the Figure caption.

R: It is clarified now that Figure S9 shows collocated with the observations model matchups.

L. 331-332: Is a systematic overestimating by 0.5 mg chl m-3 really that bad in your view? That's what the writing currently makes it sound like to me.

R: What we wanted to point out that this level of systematic model-from-observation deviation was the highest in comparison with bias estimates obtained for other biogeochemical provinces.

We make this clearer:

"The highest disagreement was obtained for diatoms in the Atlantic Sector of the ANTA province, where the simulated diatom Chla is systematically overestimated by ~0.5 mg m^{-3}. The best agreement with the HPLC based diatom Chla (excluding small provinces, see Figure 1) was obtained at the SSTC and SANT. For the haptophytes, the highest systematic error towards overestimation has been found at two small provinces east of Africa and Australia (EAFR and AUSE) with the bias = 0.57, 0.48 (mg m^{-3}), respectively. The highest random error is (RMSE = 0.62, 0.44 mg m^{-3}) at EAFR and APLR. The lowest differences between predicted and observed haptophytes was at the FKLD, SSTC provinces where haptophytes are mostly presented by coccolithophores, and at the SANT biogeochemical province, where both coccolithophores and *Phaeocystis* co-exist…"

L. 334: Differ in what way? This is a vague statement.

R: Rephrased: "…frequency distributions of the simulated and observed prokaryotic pico-plankton are different…"

L. 285-340: Personally, I would suggest to present the validation earlier in the manuscript. I find it a bit unfortunate to have the evaluation as the last result section.

R: The story we wanted initially to tell the reader is how leveraging satellite estimates and in situ observations allowed us to define the trait requirements for capturing phytoplankton biogeography in the Southern Ocean. And then evaluated the model set up with the specified traits allowing also to test the hypothesis … We now lay this out in the introduction so that this process is clearer.

Conclusions

L. 342-343: I don't understand the first sentence. How did satellite-derived estimates and in situ observations help to define trait requirements (characteristics? Or simply traits?) of phytoplankton? Can you rephrase?

R: We clarify that "an extensive synthesis of available observations on the Southern Ocean PFTs allowed us to better understand their biogeography. This information was used to infer which types should coexist in which regions, and, therefore, to constrain the model." In other words, this gave us a basis on which to define which traits would allow these regional co-existences.

L. 347-348: The necessity of the inclusion of two diatom classes and the changes to the coccolithophore parametrization have not been sufficiently motivated and the subsequent improvement of the model has not been sufficiently demonstrated, please see comments above (e.g. on L. 220-223 and on L. 275-277 of your manuscript).

R: Please see above our response to the aforementioned comments. Nevertheless, we only change parameter values for coccolihophers (as listed in Table 1), not the underlying parameterization.

This part of the conclusions has been revised as following:

"Our results support the hypothesis that introducing two size classes of diatoms in biogeochemical models is a prerequisite to simulate the observed diatom phenology and PFT distribution in general. We have also shown that the simulated biogeography of coccolithophores is not controlled by temperature itself as reported by Smith et al. (2017), since we did not use a specific for coccolithophores temperature limitation function. It was directly explained by phosphate depleting as well as by low palatability of this PFT for grazers. This confirms our second hypothesis. Nevertheless, we found that the simulation of co-occurrence of coccolithophores and *Phaeocystis* required additional model developments to account for changes in assumed life stage of Phaeocystis (Popova et al., 2007; Kaufman et al., 2017) subject to iron availability (Bender et al., 2018). This parameterization of morphological shifts indeed allows for co-existence of the two types of haptophytes corroborating our third hypothesis on the dependence of *Phaeocystis sp.* life stages on iron availability. By considering two life stages of *Phaeocystis* we introduce additional differences in the traits, which along with assumed physiological parameters for coccolithophores makes coccolithophores competitive among phytoplankton of larger cell size requiring higher nutrients concentration to grow or/and among PFTs of similar size – small diatoms and *Phaeocystis* solitary cells – but of higher palatability factor to be grazed. These additional differences in the traits of distinct haptophytes, coccolithophores and Phaeocystis allows these groups to co-exist (e.g. along the Subantarctic and Polar Fronts). However, there is still room for improvement,…"

Furthermore, I don't understand the logic in the sentence in parentheses. Please rephrase to clarify.

R: The sentence in parentheses has been removed.

L. 349: That temperature is not a driver of the coccolithophore biogeography in your model has not been shown/discussed in your result section. Please include it there or adjust the conclusion section.

R: In a way we refer to the fact that we do not use a specific for coccolithophores temperature limitation function.

L. 350: Please revise the grammar of this sentence ("Neither[...]").

R: Revised

L. 350-355: Again, please double-check carefully what in your conclusion section are results that you've actually presented in this manuscript and what are speculations or work not included here. Currently, a lot of the things you say here do strictly not follow from what you've shown.

R: We wrote that "The simulation of co-occurrence of coccolithophores and *Phaeocystis* required additional model developments. Thus, as a first trial, the Darwin model was augmented to account for changes in assumed life stage of *Phaeocystis ant.* (Popova et al., 2007; Kaufman et al., 2017) subject to iron availability (Bender et al., 2018). This parameterization of morphological shifts did indeed allow for co-existence of the two types of haptophytes."

That is what we have done and shown.

Additionally, including life stages of Phaeocystis allowed for co-existence of the two types where and/or when?

R: in the revised version we have extended the sentence mentioned above as following: "(e.g. along the Subantarctic and Polar fronts)."

Going back to L. 234-236, I think you're referring to the fact that one goes extinct when not accounting for these. I still think this is worrisome and I do not understand at all how the changes to the model then prevent this from happening.

R: As we explain above: we are dealing with model behavior in the presence of the biochemical module structure including two similar (with respect to the assumed traits) PFTs leading to that just one of these two exists. In experiment REF we did not differentiate sufficiently enough between the traits assumed for coccolithophores and "other large" (or *Phaeocystis*), which lead to a longer integration period of time to reach a quasi-steady state and to just one of "similar" PFTs survived. It means that in experiment REF *Phaeocystis*-analogue indeed represents haptophytes in general (taking over for coccolithophores). In the original Darwin-2015 model (Dutkiewicz et al. 2015) "other large" did not survive. Thus, the introduction of additional diversity/differences in the PFT traits is crucial.

It helps to make coccolithophores competitive among phytoplankton of larger cell size (or colonies) requiring higher nutrients concentration to grow or/and among PFTs of similar size – small diatoms and *Phaeocystis* solitary cells – but of higher palatability factor to be grazed. We make this clearer in the conclusion of the revised text.

L. 355; Please check the grammar.

R: Thanks. Checked and corrected

L. 359-362: Is this really the case? I would expect the nutrient limitation terms to have a big influence on differences between PFTs as well, given the differences in their half-saturation constants (Table 1). Please double-check.

R: We agree that the nutrient limitation terms also affect differences between PFTs. The discussed was (should be) eq. 3 and 5 in particular (equation number is corrected in the revised version, it is now 1 and 3) with respect to photophysiological parameters P_{max}^{c} and α.

The "realized" growth rate (specific growth rate) is a result of all environmental factors it depends on and the non-linearity of the functions might lead to unexpected results with regards to their impact on the specific growth rate at a given point and time.

R: We agree, what we wanted to emphasize is the importance of careful specification of α.

L. 362: Please include this information on assumptions surrounding alphaPI in the method section and in Table 1.

R: We do not prescribe alpha by a number explicitly, it is calculated in eq. 5 (see also Dutkiewicz et al. 2015). We now add this reference.

L. 367: If your maximum growth rates are likely too high, this should be discussed/mentioned somewhere in the manuscript. Can you plot how your temperature-limited growth rate in the model for Phaeocystis, diatoms, and coccolithophores relates to laboratory measurements (see e.g. supplementary material in Le Quéré et al. (2016) for a compilation)?

R: We did not use the approach used in Le Quéré et al. (2016) for deriving a temperature limiting functions and allowing to suppress the PFT distribution to particular latitudes. All phytoplankton types have the same temperature function (unitless) as done in Dutkiewicz et al. (2015).

The Tables S5 & S6 do currently not include information on what temperature the reported growth rates are measured at (and you don't specify the temperature dependence used in your model).

R: We included in the tables information extracted from the literature (including growth rate at specific T if available). In our model as in Darwin-2015 (Dutkiewicz et al. 2015), we use the following formulation:

$\gamma_j^T = \tau_T e^{\left(A_T\left(\frac{1}{T+273.15}-\frac{1}{T_O}\right)\right)}$,

given the coefficient $\tau_T = 0.8$ normalized the maximum value (unitless), the temperature coefficient $A_T = -4000\ K$, and the optimal temperature $T_o = 293.15\ K$.

Plotting the function that is actually used in the model over a range of temperatures together with a range of measurements will help to understand in what temperature ranges the temperature-limited growth rates in your model is too high/too low.

R: In this respect we would like to state the following: 1) we started from the parameter values used in the original study by Dutkievicz et al. (2015) changed them not too much, mostly to account for relative differences reported from the lab experiments (Tables S5, S6); 2) and there is still importance of alpha (Losa et al. 2004, 2006); 3) as mentioned above we did not use the approach used in Le Quéré et al. (2016) for deriving PFT specific temperature limiting functions and allowing to suppress the PFT distribution to particular latitudes; 4) in equation 4, there is another function $\gamma_\eta = f(k_{sat_\eta}, \eta)$, in our case k_{sat_η} depends on the assumed phytoplankton size (Ward et al. 2012, 2017).

L. 370: And is it a problem if one had to choose different alphaPI for different PFTs?

R: It is not a problem, the alpha should be different (Losa et al. 2004, 2006). The discussed is eq. (5) – (6), used to calculate the alpha (Dutkiewicz et al. 2015) given the Phytoplankton specific absorption spectra and $\phi_{\max}$.

L. 371-373: Again, this has not actually been shown in your study. Can you back this up with some references?

R: It is a result of one of the sensitivity experiments included in the Supplementary Material (Figure S4, Section S1.1.4)

Try to make your language more accurate by including words like "potentially", "possibly", then you would immediately avoid misunderstandings regarding where you speculate and where you refer to things you have actually shown.

R: Thank you for pointing this out. In the revised manuscript we are careful to use more accurate words for things we have actually shown, and these less specific ones for speculation.

L. 379: But you have included CDOM in the model simulations you discuss here, haven't you (see Equation 2)? Then I don't understand what you mean here exactly, as you're talking about possible improvements. Please be precise. What would need to be improved and how?

R: We pointed out on results of our sensitivity test with respect to including/excluding CDOM in/from the model. Results of this experiment indicate that the model is sensitive to parameterisations of remineralization processes. We state this as a study limitation.

L. 380: Similar to above: Please be precise on what you think should be improved regarding the algae-sea ice interactions and how you think this would impact the study at hand.

R: We raise this issue for the discussion because current set up of the light penetration module (as in Taylor et al. 2013) equation (12) and (13) of the original version of the manuscript leads to diatom blooming if the sea ice concentration is less 90%. With no sea-ice-algae specified in the model it might lead to overestimation of the simulated diatom Chla in the marginal ice zone. At least we would like to mention this issue as a limitation of the study.

Please try to always relate your suggested improvement back to this study –there is possibly an endless list of things one could improve in your model (and in any other model for that matter), but not all of those things are relevant for modeling PFTs on a basin scale in the SO. Please make very clear why you think the things you suggest to improve are important and how you think they would impact the study at hand.

R: All discussed aspects for improvement were proposed based only on the experience/sensitivity tests with the model that were carried out but not yet shown in the manuscript since required more thorough evaluation and presentations. Nevertheless, a sensitivity experiment with changed ϕ leading to changing α (different for diatom and *Phaeocystis*) showed that it is possible to further improve representation of diatom and *Phaeocystis* in the Ross Sea. Experiments with excluding or including CDOM effected PFT compositions (via altering the remineralisation processes and, therefore, nutrient distributions).

L. 384-385: Please delete the statement about green algae and dinoflagellates, as this is not relevant here.

R: Deleted as suggested.

L. 386: The information becomes closer? To what? Please revise the logic.

R: we deleted this expression.

L. 382-403: In my opinion, this whole paragraph is misplaced in the conclusion section. Overall, I think the conclusion section is way too long right now. I would instead suggest to include and "limitations & caveats" section between the results and the conclusions. In such as a section, you can then discuss the difficulties described here, as well as the limitations surrounding the PFT parametrizations and the suggested improvements (L. 355-381).

R: As suggested, we have introduced a new subsection: "Limitations of the study"

Please focus the conclusion section on the main take away messages from your paper.

R: Thank you, we have done this now in the revised version.

Figures/Tables Table 1: As mentioned in the detailed comments above, the table is currently incomplete. Please add the missing variables (even if they are the same for the different PFTs, it is important to state that here for important variables such as alphaPI and the maximum grazing rate).

R: In the original version we did not include the alphaPI in Table 1 as a parameter since it was considered as a function calculated with eq. 3, and maximum grazing rate we kept unchanged as in Dutkiewicz et al. (2015): for large zooplankton - $gmax_{jk} = \{1; 0.1\}$ on large and small phytoplankton respectively; for small zooplankton - $gmax_{jk} = \{0.1; 1\}$ on large and small phytoplankton respectively. We added the reference in the text (L139, L148, L153).

Furthermore, please add the units and a short description of each variable to the table.

R: Added.

What temperature is the maximum growth rate at? This needs to be specified.

R: at 30°, it is now specified in table 1.

I am also irritated by the three digits of the half-saturation constants of e.g. N –is the model that sensitive to changes in this number? Have you tested this?

R: These parameters we calculated using empirical allometric relationships (Ward et al. 2012. 2017, Dutkiewicz et al, BGD, in review). It is why the values are so precise. We have not performed a sensitivity test with respect to the precision required.

Dutkiewicz, S., Cermeno, P., Jahn, O., Follows, M. J., Hickman, A. E., Taniguchi, D. A. A., and Ward, B. A.: Dimensions of Marine Phytoplankton Diversity, Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-311, in review, 2019.

Please also add the half-saturation constant of silicic acid by diatoms.

R: We added the half-saturation constant of silicic acid in table 1.

I am also slightly confused by your half-saturation constants of iron. Assuming your reported numbers are in mmol m-3, your value for large diatoms (0.028) is e.g. an order of magnitude smaller than those suggested for the SO in Timmermans et al. (2004; 0.19-1.14 nmol L-1 or 0.19*10-3-6701.14*10-3mmol m-3). Regarding the N2fixers, Trichodesmium is typically considered to have a higher iron requirement and half-saturation constant of iron than other phytoplankton PFTs (I suggest you to have a look in e.g. Berman-Frank et al. (2001) and Ward et al. (2013) and check references therein, as I am not an expert myself in nitrogen fixers). Have you tested how your low kFef or the N2fixers in your model impacts their relative contribution to the SO phytoplankton community, which is currently quite high (see Fig. 4)?

R: In our study half-saturation constant of iron for N-fixers is higher than for all other PFTs (except for large diatoms). In the version of the model in Ward et al. (2013), Monod kinetic are used. As such growth is given as $\mu_{max} * \frac{R}{R-K_R}$, where R is nutrient, and K_R is the half-saturation for growth. In general, most lab/field observations are given in terms of nutrient uptake (not growth):

$v_{max} * \frac{R}{R-k_R}$, where k_R is half saturation for nutrient uptake.

K_R and k_R are related but not the same. In fact, K_R is often about an order of magnitude lower than k_R. (See discussions in Ward et al, JPR, 2014; Verdy et al, L&O, 2009; Dutkiewicz et al, BGD, in review). In our study as well as in Dutkiewicz et al. 2015, the growth half saturations were parameterized based on a nominal size for each phytoplankton and using empirical allometeric relationships for k_R and the other relevant parameters needed to calculate K_R. These are more fully explained in Dutkiewicz et al (BGD, in review).

Table 2: What do “PSC” and “SCM” stand for?

R: These stand to specify size class as referred in the observations (Phytoplankton Size Class, PSC) and Darwin-2015 model (Size Class in Model, SCM), explained in the caption.

All Figures& Tables:

Please add panel labels (Figures) and use these in the captions and the text.

R: The panel labels have been added

Please double-check that you clearly state in the captions, which year of which simulation you’re assessing and what year (Tables 3-5)

R: Checked.

Please make sure you include units for all variables in the captions.

R: Checked. Included

Fig. 1: You only plot HPLC observations. Please be precise in caption.

R: We modified the caption.

Replace “curve” by “contour”.

R: Replaced

Fig. 2

When you say “Haptophytes” here, do you mean coccolithophores? Or the combination of coccolithophores and Phaeocystis? I am confused because both PHYSAT and the Darwin model do discriminate between the two and you list Phaeocystisas a separate class in the Figure legend. Please clarify in the Figure legend as well as in the result section 3.1.

R: “Haptophytes” is a sum of coccolithophores and *Phaeocystis*. We clarify it in the figure caption.

Why do you combine the two for the model output?

R: Experiment REF as well as Darwin-2015 presented in Figure 2 could not distinguish between coccolithophores and *Phaeocystis*. We emphasize this in section 3.1.

"The our model output" -> please rephrase.

R: Thank you, rephrased.

What is the basis of choosing 55% as the dominance threshold? This seems random to me.

R: Indeed, it was just our choice.

I am surprised to see that there is no area of coexistence, so this means at every grid cell there is always one PFT that contributes more than 55% to biomass?

R: The area of co-existence is depicted in the green colour (mixed).

If I look at the transects in Fig. 4, it does not necessarily look like it. Please double-check.

R: In the transect, small and large diatoms, coccolithophores and *Phaeocystis* are shown separately. In the dominance plot, they are combined in diatoms and haptophytes, respectively.

Fig. 3

What are the white contours? Please add info to caption.

R: We have added that the white contours represent the Southern Ocean fronts (Orsi at al., 2005, as in Figure 1): the Sub-Antarctic Front (SAF, thick contour); the Polar Front (PF, dashed), the Southern Antarctic Circumpolar Current Front (SACCF, thin contour) and the Southern Boundary of ACC (SBDY, dotted).

Fig. 4

The caption is incomplete: explain Nfix, Proc, UML... I also suggest to add "REF" and "PHAEO" directly in the Figure to make clearer immediately what the two columns are.

R: The caption is complete now, explaining 'UML' for the upper mixed layer, 'Nfix' for nitrogen fixers, 'Proc' for Prochlorococcus. We have added "REF" and "PHAEO" directly in the Figure.

Fig. 5:

panes ->panel.

R: Corrected.

Please change the order of "phosphate" and "iron" in caption to match the order with that in the figure.

R: Changed, thank you for pointing this out (now figure 6).

What are the white contours?

R: We have added that the white contours represent the Southern Ocean fronts (Orsi at al., 2005, as in Figure 1).

Fig. 6:

I suggest to add "REF" and "PHAEO" directly in the Figure to make clearer immediately what the two columns are.

R: Added as suggested.

Fig. 7:

Please correct "the our model output".

R: Corrected. Now figure 8 in the revised version of the manuscript.

Please add a reference to Fig. 2.

R: Added.

"Figure 8. Surface PFT dominance simulated with Darwin-MITgcm for 2007/2008 for experiment PHAEO (see figure 2 for comparison). Model PFT is considered dominant if its Chla fraction of total Chla is more than 55%. The model output is masked by the area with sea ice concentration > 75% during respective month."

Fig. 9:

Is "diatoms" large + small here? Be more precise.

R: Figure 9 is replaced by a reference to a supplemented video. The description is provided with link to the supplementary videos.

https://doi.org/10.5446/42871

Add "in situ HPLC observations".

R: Added when providing the link to the supplementary videos.

Which simulation?

R: "PHAEO", it is stated in the section title and when providing the link to the supplementary videos.

Fig. 10 & 11

Please say explicitly "coccolithophores and Phaeocystis" (Fig 10).

R: Done with the link provided to video, since, as for diatoms, these figures were removed in the revised version of the manuscript.

Link for haptophytes (coccolithophores and Phaeocystis):
https://av.tib.eu/media/42873

Link for haptophytes prokaryotes:
https://av.tib.eu/media/42872

Please state which simulation is shown.

R: The shown is PHAEO. The figures are replaced by a reference by a video.

References

Berman-Frank, I., Cullen, J. T., Shaked, Y., Sherrell, R. M., & Falkowski, P. G.: Iron availability, cellular iron quotas, and nitrogen fixation in Trichodesmium. Limnology and Oceanography, 46(6), 1249–1260, https://doi.org/10.4319/lo.2001.46.6.1249720, 2001.

Breitbarth, E., Oschlies, A., & LaRoche, J.: Physiological constraints on the global distribution of Trichodesmium– effect of temperature on diazotrophy. Biogeosciences, 4(1), 53–61, https://doi.org/10.5194/bg-4-53-2007, 2007

Krumhardt, K. M., Lovenduski, N. S., Long, M. C., Levy, M., Lindsay, K., Moore, J. K., & Nissen, C.: Coccolithophore Growth and Calcification in an Acidified Ocean: Insights From Community Earth System Model Simulations. Journal of Advances in Modeling Earth Systems, 11, 2018MS001483, https://doi.org/10.1029/2018MS001483, 2019.

Lancelot, C., de Montety, A., Goosse, H., Becquevort, S., Schoemann, V., Pasquer, B., & Vancoppenolle, M.: Spatial distribution of the iron supply to phytoplankton in the Southern Ocean: a model study. Biogeosciences, 6(12), 2861–2878, https://doi.org/10.5194/bg-6-2861-2009730, 2009

Le Quéré, C., Buitenhuis, E. T., Moriarty, R., Alvain, S., Aumont, O., Bopp, L., ... Vallina, S. M.: Role of zooplankton dynamics for Southern Ocean phytoplankton biomass and global biogeochemical cycles. Biogeosciences, 13(14), 4111–4133, https://doi.org/10.5194/bg-13-4111-2016, 2016.

Luo, Y.-W., Doney, S. C., Anderson, L. A., Benavides, M., Berman-Frank, I., Bode, A., ... Zehr, J. P.: Database of diazotrophs in global ocean: abundance, biomass and nitrogen fixation rates. Earth System Science Data, 4(1), 47–73, https://doi.org/10.5194/essd-4-47-2012, 2012

Monteiro, F. M., Bach, L. T., Brownlee, C., Bown, P., Rickaby, R. E. M., Poulton, A. J., ... Ridgwell, A.: Why marine phytoplankton calcify. Science Advances, 2(7), e1501822, https://doi.org/10.1126/sciadv.1501822, 2016.

Peperzak, L.: Daily irradiance governs growth rate and colony formation of Phaeocystis (Prymnesiophyceae). Journal of Plankton Research, 15(7), 809–821, https://doi.org/10.1093/plankt/15.7, 1993

Timmermans, K. R., Wagt, B. Van Der, & de Baar, H. J. W.: Growth rates, half saturation constants, and silicate, nitrate, and phosphate depletion in relation to iron availability of four large open-ocean diatoms from the Southern Ocean. Limnology and Oceanography, 49(6), 2141–2151, https://doi.org/10.4319/lo.2004.49.6.2141745, 2004

Wang, S., & Moore, J. K.: Incorporating Phaeocystis into a Southern Ocean ecosystem model. Journal of Geophysical Research, 116(C1), C01019, https://doi.org/10.1029/2009JC005817, 2011

Ward, B. A., Dutkiewicz, S., Moore, C. M., & Follows, M. J.: Iron, phosphorus, and nitrogen supply ratios define the biogeography of nitrogen fixation. Limnology and Oceanography, 58(6), 2059–2075, https://doi.org/10.4319/lo.2013.58.6.2059, 2014.