# Response to the comments of reviewer 1

**Anonymous Referee #1**

In this paper the authors use the Darwin-MITgcm to simulate the phytoplankton composition in the Southern Ocean. The paper
5  is focused on the parametrization of the model to improve coccolithophore abundance, include two sizes of diatoms and two
life stages of Phaeocystis in the Southern Ocean. The paper is an interesting model development, but I am not sure whether
this really fits in the goals of Biogeosciences.

R: We thank the reviewer for the constructive comments on the manuscript. Our author's replies are presented in blue, labelled
"R:" and follow each reviewer's comment.

10

We have revised the paper in response to the insightful comments of all three reviewers. The revised version is clearer why
this is a novel paper and fits the goals of Biogeosciences: it provides unique synthesis (of in situ measurements and satellite
observations) on the Southern Ocean phytoplankton communities, and addresses some of the challenges in modelling them.

15  INTRO: The introduction doesn't provide any context or current challenges of why the work is being done.

R: The introduction has been improved substantially. We have further considered in the introduction the challenges that the
model community is facing despite recent advances and that this has motivated us to adjust a coupled model to improve
representation of the dominating phytoplankton groups in the Southern Ocean.

20  "The Southern Ocean is one of the most important regions in regulating climate via the uptake of about 40% of the global
oceanic anthropogenic $CO_2$ (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
25  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
30  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).

1

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."

3

RESULTS and DISCUSSION: The results and discussion section lacks quantitative assessment of the model.

R: The quantitative assessment of the model comparing simulated PFTs concentration against *in situ* PFTs concentration was provided by three tables in the main manuscript (Table 3 - 5) and three tables (S7-S9) and three figures in the supplementary material. We edited the text to make it more visible now.

"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

135     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 show now the comparison with the MAREDAT data on diatoms, coccolithophores, Phaeocystis, and zooplankton biomass.

140     "The representation error is even larger for the comparison of PHAEO monthly mean climatology of the diatom, coccolithophores and *Phaeocystis* biomass (mgC m$^{-3}$) with monthly composites of in situ PFT biomass measurements from the MAREDAT dataset. Figure S13 shows the distribution of MAREDAT seasonal (summer and spring) composites of diatom, coccolithophores and *Phaeocystis* biomass data vs. model matchups based on monthly MAREDAT and PHAEO monthly climatology for diatoms (panel c), coccolithophores (f) and *Phaeocystis* (i). Because of the poor data coverage and large

145     discrepancies in the represented temporal and spatial scales, differences between the model and in situ data are expected to be large. As a result, correlations between model and data PFT biomass from MAREDAT datasets are 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 *in situ* data. At the end, showing the quantitative estimates of the data and model agreements (MAE = {-0.38, -1.03, -0.61}, RMSE = {0.88, 1.13, 1.04}) for log-transformed biomass of diatoms, coccolithophores and

150     *Phaeocystis*, respectively), we still make a qualitative assessment. MAREDAT measurements are not always collocated for different PFTs, thus, it is not always possible to draw conclusions on the phytoplankton compositions. However, one, can notice, that diatoms, coccolithophores and *Phaeocystis* do co-exist in the areas along the subantarctic and polar fronts (see Figure S13, in the Supplementary Material)."

155     CONCLUSION: Overall, I was left wondering what science advancements or challenges this paper was providing or highlighting.

R: We improved the conclusion to make clear the science advancements provided by our study.

"… Our results support the hypothesis that introducing two size classes of diatoms in biogeochemical models is a prerequisite

160     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). 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

165     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

170 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….”

INTRO: In the introduction the authors seem to switch back and forth between defining PFT as plankton or phytoplankton functional type (see L23 and L 45 for example). The reader is left wondering whether starting at L46, they are talking about

175 phytoplankton or plankton. This is very confusing and I don't think brings any context to the paper.

R: We considered this issue important because of existing mismatch between grouping and "dimension of diversity" used in different observational techniques and models. Moreover, we wanted to emphasize that focus of the study is phatoplankton (not plankton in general). But we agree with the reviewer that we indeed added to much information on how PFTs are defined. In the revised version of the manuscript we removed the discussion on the definition PFTs.

180

INTRO: Along those lines, I thought the whole introduction wasn't very helpful in describing the context and problems tackled by this paper.

R: The introduction has been revised accordantly. Please see above the reply to your second comment.

185 INTRO: The intro is mostly about how people have defined phytoplankton functional types when this paper appears to be mostly about the challenges that goes into representing phytoplankton diversity in a model.

R: The introduction has been revised accordantly and in the revised version of the manuscript we removed the discussion on the definition PFTs. Please see above the reply to your second comment (paragraphs 3 to 6).

190 INTRO: The introduction also presents only a very marginal portion of the work that has been done in the modeling of phytoplankton communities. Suggesting that this work started off with the paper from Le Quere et al. (2005) and Follows et al. (2007) when this work had started a lot earlier than this.

R: The introduction has been revised accordantly; please see above the reply to your second comment (paragraphs 4 to 6).

195 INTRO: The authors present all the other models in one sentence (L58) summarizing them as only including 2-3 phyto-plankton groups and mention one other model that has four. This really comes across as a very narrow view of the work that has been done in this area.

R: The introduction has been revised accordantly and we removed this sentence; please see above the reply to your second comment.

200

INTRO: The intro would have benefited from expanding on the work and the challenges that have already been learned from the various models out there instead of the classification of PFTs.

6

R: The introduction has been revised accordantly; please see above the reply to your second comment (now paragraphs 3 to 6).

205

INTRO: Furthermore, the view that the Darwin model 'has the highest potential to simulate globally relevant PFTs' is again narrow minded at best especially considering that the author support this argument by saying that the Darwin allows to represent more than three and up to several thousands of phytoplankton groups (L64). The reader then finds out a few lines later (L85) that the version used here distinguishes only 6 phytoplankton groups (there are several models out there that do

210 this) and not thousands like initially said. This brings the question of why (a) the authors need to state that this model has in fact 'the highest potential' among all models and (b) since they limit their phytoplankton groups to 6 does it really still stand as having the highest potential? The introduction should be focused more on the challenges that the modeling community has faced, the recent advances etc rather than try to convince the reader of why one model is superior to the others (without properly describing their model or the others).

215 R: We did not attend to emphasize the "superiority" of Darwin model (w.r.t. number of simulated PFTs), but rather used it as a good example of a model allowing to consider different aspects of phytoplankton diversity (dimensions of the diversity, Dutkiewicz et al., 2019) or differentiation among phytoplankton groups - biogeochemical role, allometric, photo-physiological and optical parameters. But we agree with the reviewer that the paragraph can be wrongly interpreted and for this reason removed it from the introduction and revised the introduction accordantly; please see above the reply to your second comment.

220

RESULTS and DISCUSSION: this section lack some quantitative assessment of how well the model does compared to the in situ data. Why not report RMSE, bias etc? Everything seems to be based on a few snapshots without a clear description of why authors chose those snapshots and a quantitative assessment.

R: Thank you for the comment. The RMSE and bias estimates between model and in situ HPLC-based PFT data over the

225 period of time Aug 2002 – Apr 2012 were presented in tables 3, 4 and 5, also partitioned for the different Longhurst's provinces. We improved the text so that the readers do not miss out the Tables. The complete set of two-weekly snapshots over the period Aug 2002 – Apr 2012 was/is presented in the video supplement.

It is very hard to know what are the scientific advancements or lessons learned from this paper from the results and discussion section.

230 R: We substantially revised the manuscript to present the results in line with the hypotheses highlighted in the introduction and what we say about these in the conclusion. It is now much clearer summarized in the abstract.

"…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

235 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."

Supplementary material: as detailed in some of my minor comments it appears that some information in the supplementary material would have benefited to be discussed in detail (and potentially included) in the main text. Similarly, the author sometimes refers to a Figure in the paper and compares it to a figure in the supplementary material which is very hard to follow (L334).

R: The paper is kept shorter in order to be more readable and for the readers who are more interested in the technical details we provide more information at the supplement.

Minor comments:

L16: this sentence needs a reference

R: We added DeVries (2014) and Stocker et al. (2013).

"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)."

DeVries, T.: The oceanic anthropogenic CO2 sink: Storage, air-sea fluxes, and transports over the industrial era, Global Biogeochemical Cycles, 28, 631–647, https://doi.org/10.1002/2013GB004739, 2014.

Stocker, T., Qin, D., Plattner, G.-K., Tignor, M., Allen, S., Boschung, J., Nauels, A., Xia, Y., Bex, V., and Midgley, P. e.: Climate change 2013: the physical science basis: Working Group I contribution to the Fifth assessment report of the Intergovernmental Panel on Climate Change,

270 Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, https://doi.org/doi:10.1017/CBO9781107415324, 2013.

L20: needs a reference

R: We added Wilson et al. (2018).

275

"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)."

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

L57: three no thee

R: The sentence was removed in the revised version of the manuscript.

285

L84: 'The version of the Darwin model used in our study simulates, among total 42 biogeochemical compartments..' change to '...among a total of 42...'.

R: Changed.

290 Btw what do you mean by compartments? As in variables?

R: "Compartments", "Components", "model variables", we changed to components.

W.r.t. model components: except for the reduced number of phytoplankton groups (from nine to six) as biomass and Chla, model variables are identical to those in Dutkiewicz et al. (2015).

L103-104 in the revised version we state: "Starting from this reduced with respect to the number of PFTs Dutkiewicz et al.
295 (2015) Darwin configuration…"

L85: earlier on this paper it said that the Darwin model had several thousands of phytoplankton groups? How did we end up with 6 only?

300 R: We used a version with six PFTs since it handles the PFTs we are interested in and at the same time it has an "affordable complexity" given the spatial and temporal resolution integration time period and, therefore, computational expanses.

The model Darwin can be considered as a biogeochemical model framework allowing to simulate different number of phytoplankton types given specific applications. We have removed the sentence about 1000 plankton as this is not germane to
305  the paper.

Methods: what is the spatial resolution of the biogeochemical model? Same as the circulation model?

R: Yes, the Darwin biogeochemical model is coupled to MITgcm and running on the same grid. The spatial resolution is 18 km as mentioned in Subsection 2.1.2. We made it clearer in the revised manuscript (Lines 90-91).
310

L111: are the CDOM spectral slope used constant values?

R: Yes, in lines 110-111 (in the original version), we wrote that "$s_{cdom}$ is the CDOM spectral slope (the value is taken from the study by Kitidis et al., 2006), but in the revised paper we omitted the part of the light penetration in the ocean.

315  L128: why do the authors compared to this other model? Seems like a random comparison

R: The list of the reference to the studies using distinct temperature limitation function for different PFTs (to suppress the PFT distribution to particular latitudes) could be extended. However, in the revised manuscript, we delete the sentence.

L150: so the model was spinned up for 6 years only?

320  R: The model was spun up for more than 6 years. We improved the text to explicitly explain how the model initialization in 1992 was carried out, including details on the initial physical model spin up starting in 1979 (L173-187).

"The simulation includes a dynamic sea-ice model with a viscous-plastic rheology and a zero-layer thermodynamic submodel (Losch et al., 2010). Penetrating light is attenuated within sea ice with an exponential law (Taylor et al. 2013).
325  Initial conditions of the physical model were obtained from a short spin-up 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 spin-up phase, the model is 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). More details can be found in Losch et al. 2010 (section 3). Starting on January 1st, 1992, the model with
330  biogeochemistry is 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 $1^{st}$ 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 does not use the replacement pressure method (Kimmritz et al., 2017). After spinning up the
335  biogeochemistry for six years, during which also the physical simulation adjusts to the new forcing, the years 1999 – 2012 are integrated and the period of Aug 2002 – Apr 2012 is used for analysis."

Was that enough to get stable conditions for the biogeochemistry? Did the authors check for that and if so how?

R: Yes, with these years (since January 1992 – August 2002) we obtained enough stable conditions for the biogeochemistry
340 as we checked the model solution with respect to reaching the quasi-steady (as example see the figure below depicting the temporal evolution of PFT Chla averaged over Longhurst's biogeochemical provinces). In general, the model forgets the biogeochemical initial conditions quite fast, however it takes from 3 to 5 years for the model to adjust/adapt to the specified biogeochemical parameters.

345 We have also edited the text of the manuscript to state the following (L198-202):
"As in previous studies using the ecosystem model (e.g. Dutkiewicz et al. 2015; Clayton et al. 2017) the phytoplankton establish a repeating seasonal cycle after about 3 years such that we can assume a "quasi-steady state" by year 2002. Surface nutrients are also in quasi-steady state. Longer term drift in deep nutrient distributions do not significantly change the results for the rest of the period that we consider here. It is not computationally possible to reach a totally adjusted system, and the
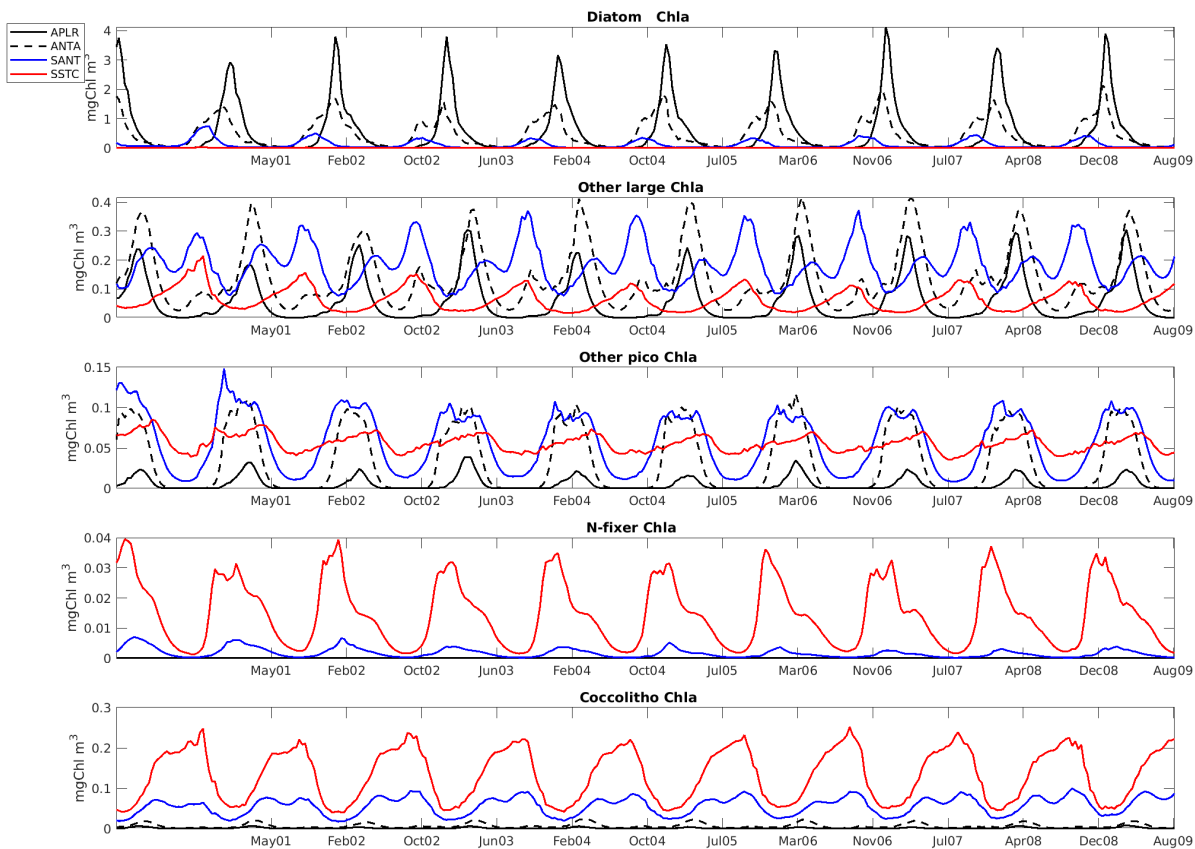350 ecological questions we address in this paper do not require such adjustments."

Figure R1: Temporal evolution of PFT chlorophyll-a concentrations averaged over the APLR, ANTA, SANT and SSTC biogeochemical provinces (Longhurst, 1998): ("other large" – is a *Phaeo* - analogue).

L168: define Chla

R: Chla is the chlorophyll-*a* concentration, was defined earlier (L83 of the original version)

L175: why were these groups not included if they have the observations for it and the model allows to discriminate for them?

R: We decided to focus on diatoms and haptophytes because overall these are the most important phytoplankton groups in the Southern Ocean/Antarctic and there are *in situ* data for evaluation of our simulations. There were no *in situ* data of PFT-Chla derived from HPLC pigments for these PFTs apart from prokaryotes (L211-212, 221-225). The same for the satellite SynSenPFT product.

"It is worth mentioning that DPA allows also to retrieve other PFTs – like dinoflagellates, cryptophytes and green algae – however, they were not included in this referenced dataset, originally generated for the evaluation of satellite retrievals of diatoms, coccolithophores (haptophytes) and prokaryotes"

Section2.2.1: there should be a 1-sentence description of how they went from pigments to phytoplankton classification.

R: We extended the methods including the following information (L216-221): "The phytoplankton groups were derived using the Diagnostic Pigment Analysis (DPA) following Vidussi et al. (2001) and Uitz et al. (2006) and modified as in Hirata et al. (2011) and Brewin et al. (2015) and adapted to a much larger data set. Briefly, different PFTs have different and specific pigments (marker pigments, e.g. fucoxanthin – diatoms) that allow to distinguish the different phytoplankton groups. The biomass of a specific PFT can be quantified by determining the contribution of the corresponding diagnostic pigment to total phytoplankton biomass (represented by the weighting sum of the diagnostic pigment)".

Section 2.2.2: The results present some snapshot from various month and year. Why did the authors not compare just a whole climatology for 1999-2012? Or annuals? How did they decide which year to compare?

R: Originally years 2004 and 2008 (January and March) were shown because we considered them as typical with respect to the obtained dominance climatology and we wanted to demonstrate the consistency of model simulations covering the time period of interest. Several two-weekly model PFT Chla snapshots overlaid by collocated HPLC-derived estimates were shown as an example of model matchups to these observational dataset (Soppa et al. 2017) available over the time period of August 2002 – April 2012. (The statistics of all available model to HPLC-based observations matchups – compiled in three video supplements – were presented in Tables 3-5 S7-S9).

In the revised version we reiterated the presentations of the datasets used for evaluation and corresponded model outputs (its temporal and spatial representation). 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."

Thus, in the revised version we present the PFT dominance in the climatological context, but the agreement with in situ HPLC derived observations (Soppa et al. 2017) and diatom and coccolithophores cell counts (Smith et al. 2017) observations is assessed given collocated matchups over the time period of August 2002 – April 2012 (HPLC) and over particularly January – February 2011 and February – March 2012 (the time period of available cell counts for diatoms and coccolithophores by Smith et al., 2017) . We have further extended the model evaluation with comparison of model PFT and zooplankton (carbon) biomass with available MAREDAT datasets. Since the MAREDAT data are presented as climatological monthly composites, this evaluation has been carried out based on model climatology (over the years 2006 – 2012).

**Table R1.1 (Table 2 in the revised manuscript): Data sets 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 dominance | % cell counts | in situ | Jan–Feb 2011 Feb-March 2012 | dia *vs.* cocco % C-biomass | Jan–Feb 2011 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.

405 L20: Did the authors look at the full seasonal cycle to conclude this or just the two months that they presented?

R: We looked at the full seasonal cycle, more precisely, a series of seasonal cycles. In figure 2 we show the PFT dominance plots for four months but in the supplement we show the whole climatology for each dataset: PHYSAT (Fig. S15), Darwin-15 (Fig. S16), REF (Fig. S17) and PHAEO (Fig. S18).

In the revised manuscript we write (L306-317):

410 "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

415 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

420 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…"

The seasonal cycle(s) over the time period Aug 2002 – Apr 2012 are also shown in the three added videos (Simulated

425 distribution of diatom (small + large) chlorophyll concentration in the Southern Ocean, https://doi.org/10.5446/42871; Simulated distribution of haptophytes chlorophyll concentration in the Southern Ocean, https://doi.org/10.5446/42873; Simulated distribution of prokaryotes chlorophyll concentration in the Southern Ocean, https://doi.org/10.5446/42872).

L236: 'to the end of the considered period of time'. What period of time is that?

430 R: To avoid any confusion, we now write "after reaching a quasi-steady state" (L349 in revised manuscript).

L247-249: how long does it take for the large Phaeocystis to outcompete coccolithophores?

R: Four to five years.

435 L278: 'in the model world'. What does that mean? As in in your model?

R: Yes, we changed to "in our model".

L287: 'Similar to Figure 2, Figure 7...' how is Figure 2 similar to Figure 7? One shows all 3 methods while the other compares July and January output from the model.

440 R: Similar in terms of depicting the PFTs dominance. We removed the term "Similar to Figure 2" to avoid confusion.

L294: instead of referring throughout the paper to the study by smith et al (2017) refer to it as the in situ dataset. Otherwise the reader is left wondering here for example what that paper is and why we are taking the same area. The first time I read the paper I didn't make the connection that this was the in situ dataset used for comparison.

445 R: Thank you for the comment. We indeed referred to the Smith et al. (2017) study and results they presented given their data. We have now rephrased the sentence: "For a more precise evaluation of the PHAEO results by comparing to the study by Smith et al. (2017), we show diatom vs. coccolithophores dominance collocated in space and time with observations of Smith et al. (2017) which agrees well to their higher concentrations and dominance of diatoms in the SBDY and SACCF, while north of the Polar Front coccolithophores become more abundant (better seen in Fig. 9). …"

450

L324-329: this paragraph seems random, doesn't report any of the results yet seems like it should be discussed in the main paper (not appendix) since it contains some quantitative assessment of how well the model does.

R: As pointed out above (where? I think it is in the third comment), we now more clearly present the quantitative assessment of our model configuration.

455

L331: 'the worst statistics..' use a different word than 'the worst..'

R: We replaced by "the highest disagreement".

L348: "...which indeed might support a biochemical/physiological hypothesis on the coccolithophore distribution...". Which
460 hypothesis is this? Why does this come up for the first time in the conclusion section? This hypothesis wasn't mention anywhere else and it's unclear what it is referring to.

R: In the original text the hypothesis was mentioned in Sections 3.1, 3.2, 3.3. In the revised version of the manuscript we state it clearer in the Introduction.

465 "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.

15

470 – 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*.

475 – *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."

L351: how do you define palatability? How did you conclude this from the results presented here?

R: Palatability is one of the model parameters (see eq. 7 in the original version and table 1). When specifying the PFT traits,
480 we assume lower palatability for coccolithophores than for diatoms, other small PFT and *Phaeocystis* (solitary cells), as well as lower half saturation nutrient (phosphate) uptake. The results shown were obtained under these assumptions. From Figure 5 (6 in the revised version) it is seen that (L407-412) "… 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 in $\gamma_\eta$ function) allowing coccolithophores to grow in nutrient depleted conditions.
485 However, 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 palatability for zooplankton in comparison with small diatoms or *Phaeocystis* presented by single solitary cells."

L369: phytoplankton not phytoplankton
490 R: Corrected.

L387: "..the information from these different sources becomes closer..." how can you say it becomes closer? In what way?

R: We have deleted this expression.

495 L396: not only are cruises carried out close to the shelf but they are also mostly during spring and summer introducing another bias.

R: We agree and also pointed out in the manuscript the bias due to the fact the expeditions are carried out usually during spring and summer months in the Southern Ocean.

500 Figure 2: the first column uses the method as the title but the third column's title describes the variable instead

R: We modified the titles of panels to: PHYSAT dominance, Darwin (2015) dominance and REF PFT dominance.