

Review: „On modeling the Southern Ocean phytoplankton functional types“ (Losa et al., 2019)

Summary

5 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).

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20 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.

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30 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.

General comments

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

- 40 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). 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.
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50 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. 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).

55 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). 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. 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.

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65 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.

70 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). 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.

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80 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). In order to e.g. emphasize the importance of including two diatom PFTs in a SO model (whereby 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).

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90 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. 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. 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”.

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- 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.
- 110 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
- 115 assessment of how they perform in the SO and is thereby of limited use for the goal of the paper.
- 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,
- 120 the authors should clearly state which year(s) and which month(s) of the model output is used in the analysis and why. 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. 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
- 125 the reader.

Detailed comments

130 **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*”.

135 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).

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

145 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).

150 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.

155 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

concentrations of the individual PFTs. I suggest to rewrite the abstract to more adequately represent the content of the result section.

Introduction:

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L. 16: Please rephrase “via the sinking of CO₂”.

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

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

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

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

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L. 32-35: Why is the description of these types (N₂ fixers 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.

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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.

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

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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”.

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

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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.

L. 57: “thee” should be “three”

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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.

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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.

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

215 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. Please replace “predict” by “simulate” or similar.

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

220 **Methods**

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

225 L. 90: Do you mean lightly silicified? 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.

230 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 – why does this apply for this SO-focused study and not for global applications of Darwin? Please add a reference regarding the occurrence of lighter silicified diatoms at lower latitudes.

L. 95: Please replace “was presented” by “is represented”.

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

240 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. 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.

245 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.

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

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

255 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).

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

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

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

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

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

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L. 127: Please change to “as opposed to the studies by X and Y”. 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. Furthermore, does your statement mean that the growth of N₂ fixers is not suppressed at low temperatures? This relates to a comment further down (on Fig. 4) in that I have the impression that your importance of N₂ fixers 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?

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L. 131: g_{\max} and k_{sat} 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.

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

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

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- 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? 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.
- Sensitivity to chosen parameters: I would be curious to see how sensitive your simulated biogeography is to how long *Phaeocystis* is 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 k_{Fe} in single-cell-state, choices seem random)? How sensitive is the simulated biogeography to these choices?

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- 315 L. 148: What is the horizontal resolution across the SO in the setup you're using here?
- 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.
- 320 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 is 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?
- 325 L. 160: Please replace "evolved" by "involved".
- 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? How does the model used in Taylor et al. (2013) perform in the SO?
- 330 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.
- 335 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?
- 340 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.
- 345 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)
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Results & Discussion

- 355 L. 205: "Improved" compared to what?
- 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.
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- 365 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.
- L. 207: “were misrepresented” – please rephrase to state more clearly what model version/setup/simulation you’re referring to here.
- 370 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.
- 375 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.
- 380 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.
- L. 210-211: Which model are you referring to here? 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. Maybe refer also to the HPLC data here? These should support the discussed bias in the community at high latitudes.
- 385 L. 211-214: This information belongs into the method section. What exactly do you mean by “in terms of agreement with observed phytoplankton composition”? How did you evaluate this? For completeness, consider adding the reference Trimborn et al. (2015) to the method section 2.2.1. Where do you show the diatom phenology of the model?
- 390 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?
- 395 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. 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. 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.
- 400 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.
- 405 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?
- 410 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 [...]”. What “small changes in the Darwin model physiological parameters” are you referring to here exactly?
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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? Am I understanding it correctly that by the end of your reference simulation, coccolithophores go extinct in your model?

420 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? 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.

425 In observations, the biogeographies of coccolithophores (mainly in the subantarctic) and *Phaeocystis* (only *P. Antarctica* in 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.

430 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.

L. 245: Why this exact month?

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

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

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L. 251: I think this statement needs to be rephrased. Smith et al. (2017) state that based on their measurements, coccolithophores made up *maximum* 20% 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.

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

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

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. 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. 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.

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Furthermore, I am wondering how high your simulated coccolithophore carbon biomass concentrations are compared to e.g. MAREDAT observations. Taking your ~30% contribution of

470 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
475 extent controlled by differences in assumptions surrounding the grazing formulations. 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.
480 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. Additionally, without the relative grazing advantage of coccolithophores relative to
485 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.

L. 260: Do you assume the drivers to be the same globally? In my view, one could very well imagine a
490 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. 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.

495 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.

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

500 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.

505 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.

510 L. 274: Replace “depleting” by “depleted”.

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.

515 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.

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

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

L. 281: Again, why March 2004?

530 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 is not thoroughly introduced. 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*? 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. 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. Similarly, for PIC, you nowhere state in the method section how calcification by coccolithophores is described in the model. Please add this information.
535
540 The cited papers by Balch et al. do not comment on POC concentrations, as far as I could see. Please double-check.

L. 288-291: Similar to above, how do you define the "much better agreement" or "even larger agreement"? Try to be quantitative whenever possible. 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. 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.
545

550 L. 293: "of monthly means"

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.
555

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?

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. Same is true for the discussion of the diatom distributions.
560

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

L. 315: What is "less accurate" in this case? Please be precise.
565

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

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.
570

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

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

575

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.

580 **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?

585

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

590

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.

595

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

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.

600

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

605

L. 355; Please check the grammar.

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. 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.

610

L. 362: Please include this information on assumptions surrounding α_{PI} in the method section and in Table 1.

615

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)? 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). 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.

620

625

L. 370: And is it a problem if one had to choose different α_{PI} for different PFTs?

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

635 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?

640 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. 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.

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

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

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

Figures/Tables

660 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). Furthermore, please add the units and a short description of each variable to the table. What temperature is the maximum growth rate at? This needs to be specified.

665 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?

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

670 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 $0.19 \cdot 10^{-3}$ - $1.14 \cdot 10^{-3} \text{ mmol m}^{-3}$). Regarding the N_2 fixers, *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 k_{Fe} for the N_2 fixers in your model impacts their relative contribution to the SO phytoplankton community, which is currently quite high (see Fig. 4)?

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

680 All Figures & Tables:

- Please add panel labels (Figures) and use these in the captions and the text.
- Please double-check that you clearly state in the captions, which year of which simulation you're assessing and what year (Tables 3-5)
- Please make sure you include units for *all* variables in the captions.

685

Fig. 1: You only plot HPLC observations. Please be precise in caption. Replace "curve" by "contour".

690

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 *Phaeocystis* as a separate class in the Figure legend. Please clarify in the Figure legend as well as in the result section 3.1. Why do you combine the two for the model output? "The our model output" -> please rephrase. What is the basis of choosing 55% as the dominance threshold? This seems random to me. 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? If I look at the transects in Fig. 4, it does not necessarily look like it. Please double-check.

695

Fig. 3: What are the white contours? Please add info to caption.

700

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.

Fig. 5: panes -> panel. Please change the order of "phosphate" and "iron" in caption to match the order with that in the figure. What are the white contours?

705

Fig. 6: I suggest to add "REF" and "PHAEO" directly in the Figure to make clearer immediately what the two columns are.

Fig. 7: Please correct "the our model output". Please add a reference to Fig. 2.

710

Fig. 9: Is "diatoms" large + small here? Be more precise. Add "in situ HPLC observations". Which simulation?

Fig. 10 & 11: Please say explicitly "coccolithophores and *Phaeocystis*" (Fig 10). Please state which simulation is shown.

715

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