Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-488-RC1, 2020
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Biogeosciences Discussions

Interactive comment on "Factors controlling the competition between *Phaeocystis* and diatoms in the Southern Ocean" *by* Cara Nissen and Meike Vogt

Anonymous Referee #1

Received and published: 26 February 2020

General Comments

Nissen and Vogt present a model study on the relative importance of the colonial form of *Phaeocystis* for ecosystem processes and biogeochemical fluxes; they evaluate their results with observations from different data sources. A comparable study (Nissen et al 2018) had been performed with a focus on coccolithophores instead of *Phaeocystis* with similar analyses. In that respect this work is not overly innovative nor are original ideas presented. More critical is, however, that there is no thread in this manuscript; a clear goal is missing. A number of topics (e.g. phenology, competition, carbon and DMS-fluxes) are touched but not thoroughly permeated. It is unclear whether the au-

thors would like to study the success of *Phaeocystis* compared to other phytoplankton functional groups or the importance of *Phaeocystis* for carbon export fluxes. Either way, no comprehensible motivation for either of these broad themes is provided. Some aspects of the methodology also need to be revised with consequences for the model analyses. Last but not least, recent work on this topic has been ignored. Overall this manuscript is premature and the authors must clarify their focus before publication. To sharpen the focus maybe it helps to look at the unpublished, recent modelling work on Southern Ocean *Phaeocystis* and PFTs (Losa et al. 2019) that has been put up for discussion in *Biogeosciences Discussion*.

Specific Comments

- title: the title only partly reflects the content of this study
- abstract and entire manuscript: it is unclear which research gap the authors want to fill. What is currently unclear - which open question in this research field are attempted to be answered with ROMS-BEC?
- the manuscript should stand alone. Currently important parts of the model description are missing. The prognostic equation for *Phaeocystis* with all source and sink terms as well as all functional dependencies of rates to environmental drivers need to be provided.
- the newly introduced formulation of the temperature dependent growth for the PFT *Phaeocystis* is fundamentally different from the description of the PFTs of the original BEC model. The former is a "Gauss-like" temperature dependent growth function with a temperature optimum. Any deviation from the optimum is a limitation, varying between 0...1. In contrast, the Q10-approach with different

Q10 values that is applied to the other PFTs denotes the "sensitivity" in the exponential growth towards temperature - in these cases the higher the temperature, the higher the growth. Even if a relatively high reference temperature of 30 degrees Celsius is given (which is likely not reached in the Southern Ocean), there is no such thing as an optimum in the Q10 approach. Thus, the "limitation" values used in the analyses cannot easily be compared.
[Generally the question arises whether the Q10 approach should be applied to PFTs at all. Introduced by Eppley it is valid and a good description for bulk phytoplankton but as soon as the bulk is divided into groups, "Gauss-like functions" with a clear optimum seem to be more adequate.]

- temperature-dependent growth functions of any organism group usually have a negatively skewed thermal reaction norm. This is also true for *Phaeocystis antarctica*. Since there already exists a mathematical description for the temperature-& light-dependent growth function of *Phaeocystis antarctica* (Moisan and Mitchell 2018), I wonder why the authors have not used it. In fact there are more recent observation-based publications on *Phaeocystis antarctica* that may be of interest for this study.

- please specify which atmospheric forcing fields have been used.

- model results: there is a mixture of model results, model evaluation, model comparison with results from previous experiments which makes it difficult to read and to follow the arguments; the entire results section needs to be revised.

  – the sections about the ecological niches, bottom-up and top-down effects are tedious to read and questionable with respect to temperature (see my comments above).

  – the section about carbon cycling arises out of sudden.

- figures: some of the selected figures are not convincing. Why focus sometimes on *Phaeocystis* and diatoms, sometimes on *Phaeocystis*, diatoms and coccolithophores and sometimes on all PFTs?

  – Fig. 2 presents a rather artificial classification of the phytoplankton community. Why is the 25% used for *Phaeocystis* and coccolithophores but 75% for diatoms (Fig 2a)? Is "Mixed" (Fig. 2a) the same as "Others" (Fig. 2b-d)?

  – how does the annual or climatological "relative contribution of the five PFTs" looks like (and not the seasonal contribution as in Fig. 2b-c)? If such a figure were shown the statements in the paragraph l. 348–354 might be more comprehensible.

  – Fig. 4 - why is silicate not chosen as an important factor for diatoms? At least in the northern part of the SO (south of $\sim$40°S) diatoms are limited by silicate.

- the discussion and conclusion sections suffer from what I commented above. The authors must make clear what the paper is about in the first place. I am confident that also the discussion and conclusion section will then be easier to write.

References

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Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-488, 2020.