Review of “Factors controlling the competition between Phaeocystis and diatoms in the Southern Ocean and implications for carbon export fluxes” (bg-2019-488) by C. Nissen and M. Vogt.

Introduction

In this manuscript, the authors extend previous work reported by Nissen et al. (2018) for the simulation of phytoplankton in the Southern Ocean. Its primary novelty is the separation from other modeled phytoplankton functional types of *Phaeocystis* colonies into a new model component.

In brief, this work involved the use of an existing model, creating a new phytoplankton group, and running the model for the Southern Ocean over 10 years. Analyses with the simulated 5-year daily climatology consist of scrutinizing the relationships between variables in the simulation and comparing the model output to data. Conclusions are drawn about mechanisms driving spatial and temporal patterns and carbon export. Tangentially, seven sensitivity experiments are run to highlight which aspects of the new phytoplankton group have had the greatest affect in distinguishing it from the original no-*Phaeocystis* model.

This work does not include much experimentation with the model. Instead, the simulation outputs of a *baseline* run are most thoroughly examined, starting with general biogeographical patterns. However, the attempt to include or address so many questions in this manuscript (how important is *Phaeocystis* to carbon export; what are the spatial and temporal patterns of *Phaeocystis* and diatom biomass; what are the drivers of *Phaeocystis* and diatoms’ spatio-temporal patterns) makes it feel unfocused. The structure of the paper and section headings only partly help. I am left wondering, which comparisons do the authors feel are most important, most revealing, or most surprising? Although main conclusions are stated in the conclusion section and abstract, the attention paid to each analysis step and their findings in the body does not seem to match these main points.

This work provides a step towards more thorough and comprehensive modeling of Southern Ocean phytoplankton. Therefore, I do think this should be published following textual revisions by which the aims and scope of the research are more clearly represented.

General comments

The section headings do not seem consistent with the scope of what is being assessed, particularly which PFTs are addressed. Section 3.1 includes “phytoplankton” in the heading and assessed all PFTs. In contrast, sections 3.2 and 3.3 include “phytoplankton” in the heading but only addresses *Phaeocystis* and diatoms. Likewise, section 4.2 includes “phytoplankton” in the heading but seems to only discuss

Phaeocystis. I recommend the authors revise the headings or make the content more consistent with the headings.

As I read, I wonder: why is the Ross Sea singled out for evaluation, aside from other coastal areas? Also, in some manuscript sections the Ross Sea is included in the comparisons (e.g. Figure 2, section 3.3 about drivers) and other sections do not include it (e.g. Table 3, section 3.4 about carbon cycle). Why is it only considered for some of the analyses? Without an explanation, these choices make the analysis seem arbitrary. The authors should explain why the Ross Sea is being used as a special study area and why/when it is or is not being included in analyses.

The differences in carbon to chlorophyll ratio may have a substantial impact on some of the conclusions, and yet it seems to have been given little consideration. I refer the authors to several additional papers discussing C:Chl ratios for *Phaeocystis* and diatoms in the Ross Sea: DiTullio and Smith (1996), Smith et al. (1998), Mathot et al. (2000), Kaufman et al. (2018).

I appreciate that calibration is difficult with such a large model, however, this seems to be an important limitation not discussed. I suggest the authors consider addressing it. Moreover, If the authors did train some of the model parameters before picking the 'best' values for their *baseline* run, it should be made clear whether or not model evaluation was done using the same or different data than was used for parameter training/tuning/calibration.

Specific comments

The last paragraph of the introduction does not accurately reflect the organization of the paper. This seems like a great place for the authors to more coherently state the purpose of the analyses.

Line 95: Perhaps it is just me, but I am confused by this sentence. Also, the implication that the model provides "a ***correct*** representation of SO phytoplankton biogeography" (emphasis added) seems very presumptuous.

In section 2.1, the authors refer to a "*baseline*" simulation before it is described. It would be helpful for the authors to refrain from referencing *baseline* before it is defined.

Sect. 2.3: I wonder what the authors mean by "analysis framework" in the section title? To me, growth rate ratios are not an analysis framework, but rather simply a diagnostic variable.

Sect. 2.3.2: The authors should define Betas in the text.

Line 279, and elsewhere: I think "N" is being used to represent both diazotrophs and nutrients. The authors should restrict its meaning to only one or the other.

Lines 318-323: I think the bias could also be due to poor calibration, especially of the newly introduced *Phaeocystis* group.

Line 331: “compared to the 4-PFT”

Line 361-362: “Our model suggests that *Phaeocystis* is an important member of the high-latitude phytoplankton community.” -- I question whether the authors claim that their model suggests something new here is actually in regard to something already known. Furthermore, this is already evidenced by the fact that the authors saw *Phaeocystis* as important enough to include in the model and write a manuscript about.

Figure 6: I believe the “p ratio” should be defined in the caption or removed.

References:

DiTullio, G.R., Smith, W.O., 1996. Spatial patterns in phytoplankton biomass and pigment distributions in the Ross Sea. J. Geophys. Res. Ocean. 101, 18467–18477. https://doi.org/10.1029/96JC00034

Kaufman, D.E., Friedrichs, M.A.M., Hemmings, J.C.P., Smith Jr., W.O., 2018. Assimilating bio-optical glider data during a phytoplankton bloom in the southern Ross Sea. Biogeosciences 15, 73–90. https://doi.org/10.5194/bg-15-73-2018

Mathot, S., Smith, W.O., Carlson, C.A., Garrison, D.L., Gowing, M.M., Vickers, C.L., 2000. Carbon partitioning within Phaeocystis Antarctica (Prymnesiophyceae) colonies in the Ross Sea, Antarctica. J. Phycol. 36, 1049–1056. https://doi.org/10.1046/j.1529-8817.2000.99078.x

Smith Jr., W.O., Carlson, C.A., Ducklow, H.W., Hansell, D.A., 1998. Growth dynamics of Phaeocystis antarctica- dominated plankton assemblages from the Ross Sea. Mar. Ecol. Prog. Ser. 168, 229–244. https://doi.org/10.3354/meps168229