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Interactive comment on "Role of zooplankton dynamics for Southern Ocean phytoplankton biomass and global biogeochemical cycles" *by* C. Le Quéré et al.

Anonymous Referee #2

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This is a very interesting modelling study demonstrating that grazing by various zooplankton functional types (PFTs) could have an important role in controlling primary production in high nitrate-low chlorophyll regions. The authors provide a new model that includes macrozooplankton as an additional heterotrophic plankton type and conclude that trophic cascades induced by macrozooplankton predation on mesozooplankton could control phytoplankton growth in the Southern Ocean. This challenges the present interpretation of dominating processes and will certainly stimulate follow up studies. Thus, the manuscript is timely and relevant. It is generally well written and easy to follow, although some clarifications are required in the methods. Some relevant improvements are nevertheless necessary regarding the suggested role of macrozoo-

plankton. The study potentially underestimates the role of microzooplankton grazing which in turn has implications for the role of macrozooplankton.

The relative contribution of the different heterotrophic PFTs depends the strengths of the trophic linkage between auto- and heterotrophic PFTS which is represented in the model by weighing factors. As outlined in the detailed comments below, I have some problems with the weighing within the microzooplankton group. Heterotroph dinoflagellates are an important grazer in the polar ocean and their role appears to be underestimated.

Another cause for the underestimation of microzooplankton grazing may results from the underestimation of the biomass. The authors compare the geographical distribution of phytoplankton PFTs in models and the field; this is omitted for heterotrophic groups and only global averages are presented. As it looks like, however, there are larger geographic differences in some regions. Most importnat microzooplankton biomass in the Southern ocean appears to be underestimated which likely has important implications in the relative consumption rates of primary production by the different heterotrophic PFTs.

In addition, the manuscript would greatly profit the presentation of model data/field data regarding the stock sizes of the different auto- and heterotrophic PFTs and their growth/grazing rates in the northern and southern ocean. The conclusion that trophic cascades induced by high stocks of macrozooplankton enable phytoplankton blooms in the north and their lack in the south prevents bloom formation is based on these data. However, this is not shown at all. I further miss some representation of how modelled data on the processes reflects real observations.

Specific comments Introduction: p 11938, line 5: The terms small and large zooplankton need to be better defined. Are microzooplankton or small copepods included in the small plankton? Their ecology and function in the ecosystem is very different. On the other hand recycling is also a function of feeding by large zooplankton (particularly of omnivorous zooplankton in the post-spring bloom period (see Banse 1995). p 11938, line 20: 'of' missing. p 11939, line 8: The §is very descriptive. Examples of the inclusion of zooplankton functional types are not given (what are the 'few examples'). The role of zooplankton is only very generally described; what are the insights gained from the inclusion of zooplankton and which PFTs were included, so far? What are the important interactions? It is left open what the more explicit representation should be and what are the expected important clues are.

Methods: p 11941, line 3: Hetero- and mixotrophic dinoflagellates could play an important role as grazers in polar ecosystems (Calbet 2008). Is this group included in the protozooplankton? p 11941, line 5: The different coloring of lines in Figure 1 needs explanation. p 11942, line 20: In some cases too little data was available to parameterize the model (e.g., Chla/C ratio), so that other more arbitrary solutions were used. I would like to know whether authors considered a sensitivity analysis to evaluate the potential error inherent in the model output caused by the lack of data. p 11942, line 29: The procedure to define the nutrient limiting parameters for phytoplankton is very unclear. For instance, was zooplankton biomass, abundance or grazing used to examine co-variation? All PFT or selected PFT? What is meant by 'magnitude of limiting parameters' of zooplankton PFTs? In addition, I thought phytoplankton (growth?) is parameterized here. This chapter needs serious improvement. p 11944: Table 3: What was the scientific basis for the weighing factors? For instance, grazing on Phaeocystis is hampered by its size (at least colonies); so, what justifies weighing them similar to mixed phytoplankton? In addition, considering that Phaeocystis was considered to be colonial, why is this species grazing loss by microzooplankton weighted similar to that of small flagellates? The table further suggests that protozooplankton has a higher preference for colonial Phaeocystis than meso- and macrozooplankton. This does not make sense to me. I would like understand the rationale behind this weighing. Moreover, protozooplankton is a diverse group including a number of heterotrophic dinoflagellates that prey on large plankton such as diatoms and have a high contribution to grazing loses in productive systems (Calbet 2008). The weighing, however, suggest a

focus on flagellates/ciliates as major grazers of microzooplankton. This might strongly influence the model results and I wonder whether the model will be able to depict the apparent shift in dominating trophic pathways in low and high nutrient regimes. Finally, the legend should also clearly indicate whether grazer or prey biomass was weighted. The table suggests links between trophic groups that are not depicted in Fig 1. p 11944, line 25: The reduction in the temperature related mortality of mesozooplankton by a factor of 2 needs explanation.

Results p 11944, line 18: The influence of eco-evolutionary determinants such as reproductive strategy on growth needs explanation. Fig 2 provides empiric maximal growth rates and, therefore, I cannot follow why life cycle strategies should play a role. p 11949, line 26; p 11950, line 12: I miss an evaluation of how well the modeled distribution of micro-, meso- and macrozooplankton compares to observations. As it looks like, there are important differences in all 3 groups regarding the global distribution. Annual mean biomass of the different PFTs should also be presented for the PlankTOM6 model. p 11952, line 19: 'range of observations': Observations of what, biomass or grazing or growth rate?

Discussion: p 11955, line 1: In their results, the authors do not compare the geographical distribution of heterotrophic PFTs with observations. Here, some discrepancies exist – for instance in the underestimation of microzooplankton biomass in the Southern Ocean, the underestimation of mesozooplankton biomass in upwelling and tropical areas or the global pattern of macrozooplankton distribution. p. 11955, line 12: Following the same argument of top down control for the Sothern Ocean, this does not apply for the tropical Pacific in which according to the model results micro- and macrozooplankton are abundant, but not mesozooplankton. However, data of Moriarty and O'Brian (2013) shows also higher mesozooplankton in this area. What is the difference between the areas and mechanisms then? p. 11955, line 14-23: This refers to results (seasonal development) which are not shown. However, they seem to be important for the interpretation of the results and might therefore be presented. With regard to the importance of the conclusions about the role of macrozooplankton, on wonders why the data/model output shown is largely limited to Chla. It would be interesting to see the different autotroph/heterotrophic stocks and related rates (as for instance, grazing rates by the different heterotrophic PFTs, loss rates of the different autotrophic PFTs) and how these compare to in-situ estimates. p. 11956, line 9 following: For me, the underestimation of microzooplankton biomass and the weighing of prey preferences is an important issue to be discussed here. Microzooplankton grazing is a considerable larger loss rate than other zooplankton grazing, and its underestimation has likely a strong influence on the role of macrozooplankton top down control. Why is no modelling data presented regarding the trophic cascades?

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