

14 June 2016

Dear Editor

thank you for the re-review of our manuscript, and please find attached the revised version.

Reviewer's comments: In their revised version the authors have largely responded to the questions and remarks of the referees and revised the manuscript additional information/figures accordingly. However, there is still little justification given for the choice of weighing factors used to describe the preferences of the different zooplankton functional types in the model. I generally agree that in the absence of sufficient data, arbitrary predator-prey ratios could be used to weigh the preferences. However, they should at least match the current understanding of grazing losses by various PFTs. In case of the well-studied *Phaeocystis* spp, for instance, this is not the case as the weighing contradicts the size-dependent relationship recently reviewed by Nejstgaard et al. (2007). It is therefore difficult to accept that the colonial form is preyed on with the same preference like picophytoplankton or with a similar weighing to larger zooplankton. Similar discrepancies exists in other PFTs and might be one factor for stronger coupling between phytoplankton and zooplankton biomass as suggested in the conclusion. While this procedure might be acceptable in the present manuscript as it is based on a published source, the authors could generally be more critical with their results and stronger highlight the lacking knowledge (e.g., on trophic relationships, not represented PFTs) to improve the understanding of the role of macrozooplankton in controlling the phytoplankton stocks because this might be critical for the improvement of future models. Apart from this criticism, I find the manuscript well in the focus of BG, stimulating and acceptable for publication.

Hirst & Kiorboe (2002) is missing in the references.

Revision: We clarified the text on p. 6, lines 4-5 to say that preference data is not available across the range of zooplankton and phytoplankton considered here, and added a sentence specific to *Phaeocystis*: "Although some data was available to characterise grazing on *Phaeocystis* spp, (Nejstgaard et al. 2007), it is not used specifically here because it required knowledge on the life forms of *Phaeocystis* in situ.". We generally agree with the reviewer that we should use all available information, although *Phaeocystis* has been particularly difficult to represent because of its presence in single cells versus colonies. This specific predator-prey relationship needs to be explored much further in a separate analysis. We also added a sentence in the conclusion (p. 13 lines 31-32): "In addition, the lack in knowledge of trophic relationships means that semi-arbitrary choices have to be made to characterise the predator-prey relationships based on size. ". We hope this is sufficient to address the remaining concerns of the reviewer.

We added the missing reference as well as the reference to Nejstgaard et al 2007 paper mentioned by the reviewer.

Corinne Le Quéré