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Discussions

Interactive comment on "A ^{13}C labelling study on carbon fluxes in Arctic plankton communities under elevated CO_2 levels" *by* A. de Kluijver et al.

Anonymous Referee #2

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This is a very interesting study that aims at describing carbon flow in a planktonic food web during a mesocosm study with CO2 perturbation. The study uses 13C organic and inorganic carbon pools (DIC, DOC, POC) to follow the transfer of 13C added to the mesocosm system at the start of the experiment. 13C labeled biomarker (polar lipid fatty acids) were used to discriminate between plankton groups from bacteria to zooplankton. Results obtained from the 13C analyses are compared to a NPZD-model, the technical details of which are described in a separate manuscript (van Engeland et al, BGD, submitted).

It has to be acknowledged that the authors attempt to draw a holistic picture of the very complex development of a planktonic ecosystem. However, the 13C approach as well as the applied model include many assumptions and derive knowledge from an

indirect approach. The study would clearly benefit from a more detailed comparison to data on carbon cycling and ecosystem development that were directly obtained during the same mesocosm study (e.g. Leu et al., Czerny et al., Brussaard et al., Engel et al., Piontek et al., Niehoff et al.). I therefore cannot recommend publication of the present study in BG without a major revision.

Detailed comments: Introduction: the authors should give more information on the suitability of PLFA as biomarker in general and for the chosen taxa in particular; what is the variability of conversion factors applied to calculate organic carbon from PFLA biomass? Since the chosen groups Phyto I and II include a variety of species, I would assume that the PFLA:OC ratio is highly variable. How is this accounted for when estimating production rates?

Results: The authors observed no CO2 effect on Phyto I including autotrophic dinoflagellates (as derived from PFLA). Leu et al (same issue) observed a positive effect on autotrophic dinoflagellates when using polyunsaturated fatty acids; how can this be explained? Page 8583, line 10-13: DOC production was <0.06 μmol C L-1 but estimated to be <6.2 μmol C L-1 during the first 11 days and <11 μmol C L-1 during d14-28: According to the estimated production rate, DOC production during the two phases should be much lower, shouldn't it?

Discussion: page 8590 line 16-17 'The addition of nutrients did not increase phytoplankton and bacterial biomass in the mesocosms.' This statement seems to be inconsistent with the findings of Leu et al, Brussaard et al., Czerny et al., (BGD, same special issue). Again, a more direct comparison to other data of the same study would be necessary.

Model: I do not see the benefit of this model in the present study. The assumptions of the model seem to conflict with the scientific findings of the study (e.g. constant bacterial biomass, no substrate limitation). The sensitivity of the model towards variations in parameter setting was not tested. Moreover, the use of a fixed stoichiometry

to derive nitrogen fluxes from carbon fluxes surely is inaccurate (see Silyakova et al., same issue) and falls short on the actual data on nitrogen cycling obtained in the frame of the study.

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