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Biogeosciences Discussions

Interactive comment on "Element budgets in an Arctic mesocosm CO_2 perturbation study" *by* J. Czerny et al.

Anonymous Referee #1

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General comments In the main frame of a mesocosm ocean acidification experiment performed in the Arctic the manuscript focuses on the difficult task to identify elemental stoichiometry dynamics through a complete elemental budget. Even in closed systems like mesocosms, the estimation of elementals budget is usually hampered by the difficulties to quantify gas exchange between the sea surface and the atmosphere and to take into account the fraction of nutrients and carbon which is channeled in the sediments and in the bacteria/algae community growing on the mesocosm walls. Thanks to innovative improvements to their experimental setup the authors affirm in the abstract the intention to measure "all relevant element pools and fluxes of carbon, nitrogen and phosphorus" to be able "to close the gap in element budget". They also observed a CO2 driven enhancement of the autotrophic community with no overall effect on the vertical export due to a shift of the system towards a retention-type microbial food

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web. The manuscript contains new data on ecosystem functioning which are of great interest not only for the scientific community working on the effect of climate change on the aquatic ecosystem. Moreover the numerous parallel studies performed during the same experiment constitutes an invaluable opportunity to understand this particular ecosystem functioning. It is therefore disappointing to read through the MS and to discover that the authors did not profit of such opportunity building their conclusions more on assumptions than on solid data. Thus, although in the abstract they define the intention of considering all the elemental pools, they were not able to measure with sufficient accuracy all the dissolved elements (DOC, DON and DOP), they did not measure zooplankton contribution and the flux of particulate organic matter was only partially considered. Therefore to close the gap for each element budget the authors use a set of "Pool Xs" which should include the dissolved pools, part of the sedimentation, and the larger zooplankton. They use the variation of these poolXs for most of their conclusions assuming such pools mainly constituted by dissolved elements (DOC, DON and DOP). This assumption is not supported by any results presented in this or other studies. The results of parallel studies during the same experiment are only used when they confirm the author conclusions. The results are generally well presented but the MS is difficult to read because of a lack of consistency in the use of the terminology (see specific comments) and because of a poor English. Concluding although the MS presents results which are important for the general understanding of the effect of increased CO2 in the arctic ecosystem and that are necessary for the understanding of the results obtained in other studies of the same experiment I believe that, in this form, this MS is not suitable for publication in Biogeosciences.

Abstract Specific comments (page 11886, lines 10-13) The sentence "all relevant element pools and fluxes of carbon, nitrogen and phosphorus were measured, using an improved experimental design intended to narrow down some of the mentioned uncertainties" set the reader expectation very high. (page 11886, lines 19-20) "Enhanced carbon consumption appears to result in accumulation of dissolved organic compounds under nutrient recycling summer conditions" This is not supported by data

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(page 11886, lines 22-24) “The out-competing of large diatoms by comparatively small algae in nutrient uptake caused reduced production rates under future ocean CO2 conditions in the end of the experiment” The authors only assume that they were competing for nutrients. Introduction Specific comments (page 11888, lines 3-5) This is not entirely correct. Pteropods are important only in limited area of the ocean as also reported by the cited author. Technical comments: (page 11888, line 20) “but surface ocean warming” can be removed since is a consequence of the increasing temperature. (page 11888, line 6) Remove “often” and change “is keeping” with “may keep” (page 11888, line 11) “in global carbon flux models” add “some” and add more references. (page 11888, line 28 continuing at page 11889 lines 1-2) remove Material and methods Specific comments It is not clear why the mesocosms were closed at day 7 (page 11890 line 10). Please explain (page 11891 line 13) Should I understand that the authors performed two salt additions during the experiment? When was the first addition performed? Please clarify. (page 11891 line 26) Here a passage is missing. Please indicate how the water was transferred inside the carboys. There is some confusion, at least for me, in the total duration of the experiment and the duration of the analysis. Please clarify The text is difficult to follow because of the use of different terminology: e.g. “Dissolved substances” (page 11890 line 22) “Dissolved and particulate parameters” (page 11891 line 21) “Particulate and dissolved substances” (page 11891 line 26) “Particulate matter” (page 11892 line 1) “sediment” (page 11892 line 3) “particles” (page 11892 line 5) Please be more precise and consistent. If the sampling device reach 12m depth and the sediment collector is at 15m depth this means that there is about 4.2 m3 of water, 8.4-5.6% of the total volume which should be sampled daily to “measure all the relevant element pools and fluxes” It is not clear which is the volume of water sampled every day from the sedimentation trap. Please clarify In the results is reported the presence of cirripeda larvae in the sediment (page 11900 line 6). I presume therefore that the authors performed microscopic analysis of the sediments but it is not explained. Please clarify. (page 11893 lines 13-15) It is acceptable to cite another source for describing a technique but the short description left should

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be understandable....I am not sure to get what " implementing measured CO2 gradients". Please explain better. (page 11894 lines 1-2) Of which equilibrium are you talking about and how was it measured? Do you mean the CT concentration between the water column and the dead volume? Please explain it better.

Technical comments: (page 11890 line 15) “were unfolding themselves” Please rephrase. (page 11890 line 12) “mesh” not “mash”. (page 11890 lines 22-24) “nonetheless. . ..” Please rephrase. (page 11891 lines 5-7) “ The replicate measured volume. . ..” This sentence is really unclear, please rephrase it. (page 11891 line 13) Please remove “early” (page 11891 line 21) “dissolved and particulate parameters” Please be more precise. (page 11891 line 22) “Which is the IWS total volume? (page 11891 line 26) The word “substances” is too vague. Please be more precise. (page 11892 lines 1-3) “Installed . . ..” Please rephrase (page 11892 lines 3-4) “Sediment suspended. . .” Please rephrase with something like: ?? L of water was sampled inside a glass bottles applying a vacuum pressure to the end of the tube. (page 11892 line 8) “Particulate matter” please change with “water” Please indicate the volume filtered for each analysis. (page 11893 line 1) Dissolved inorganic carbon should be indicated with “DIC” (page 11893 line 6) I am not sure to understand what "slowly freeze dried" means. Are this the POM pellets which were stored in the -80 freezer?..I do not understand. Please explain better and rephrase the entire paragraph. (page 11893 line 12) Please change “deliberate tracer” with “gas tracer” (page 11893 line 18) Please remove “on” and change “days” with “day”

Results Specific comments (page 11896 line 16) Isn’t it “t-7” the day in which mesocosms were closed (page 11890 line 10)? (page 11896 lines 16-18) This sentence should be in “discussion” since does not refer to reported data (page 11897 lines 7-9) What ratio was used for phase three? (page 11897 lines 9-11) I am surprise to not see an increase in chl a in day 30 after the brushing of the wall. Should I assume that the brushing was performed after the Chl a measurement? Please clarify. (page 11897 lines 14-15) Sedimentation does not mirror the Chl a bloom development. (page 11898

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line 12) “averaged over all mesocosms” Does this mean that on average on each bag walls there were 7.3 μmol C kg-1. In this case it would be interesting to know the standard deviation or at least the range. (page 11899 lines 22-23) Figure 5d is PoolX for phase one...I do not understand? (page 11900 line 1) Growth of what? Phytoplankton? (page 11900 lines 5-10) If this is reported in "results" then the zooplankton analysis method should be reported in M&M (page 11900 lines 11-14) Or it goes to Discussion or has to be described in M&M (page 11900 lines 21-23) Why does it seem possible if it is not indicated by the data? (page 11900 lines 25-28) From Shulz et al 2012 I can not detect this difference in N and P concentration between High CO2 and low CO2 in phase III (page 11901 lines 1-3) How did you estimate the net autotrophic growth rate? Figure 3 (page 11920) If the wall brushing was performed at day 30, why the dot is plotted at day 27? Discussion Specific comments (page 11902 lines 13-14) Viral lyses of what? Grazing by whom? (page 11902 lines 19-22) I am not able to find the base of these assumptions. Please explain better (page 11903 lines 21-24) This hypothesis is not supported by the data of Brussard et al 2012. (page 11904 line 15) How the authors can assert that Pool X was mainly constituted by DOC. It could be entirely due to the "undetermined pools" and not to DOC (page 11904 lines 23-25) I do not see any differences in nutrient concentration between the treatments.

Synthesis Specific comments (page 11907 lines 5-7) I am not able to find the bases for this statement. (page 11907 lines 11-13) The authors can not state this on the base of their data. I am not able to see how pH controlled the ecosystem productivity. (page 11907 lines 16-17) What the authors mean with "characteristic effect of C02"? Please explain better. (page 11907 lines 17-19) This, in my opinion, means that are nutrients more than the Co2 to control the productivity of the system. (page 11908 lines 4-6) I have not seen evidence of increased DOM due to CO2 and I have not seen any data regarding the size of phytoplankton. (page 11908 lines 19-21) May be Brussard et al did it but the data are not presented in this study. (page 11908 line 23) What are follow up effects? Please be more precise.

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Interactive comment on Biogeosciences Discuss., 9, 11885, 2012.