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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 #2**

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General Comments The study addresses a current and important topic of ocean ecosystem responses to ocean acidification caused by anthropogenic CO2 emissions and the subsequent increase in atmospheric CO2 levels. This scenario is also well documented in ms Introduction. The overall logistic and experimental design of the study is of extraordinary quality, even meeting the harsh Arctic conditions. The Czerny et al. ms is evidently important for the overall aims of the study, as a key research question concerns evaluation of the changes of biogeochemical properties of the ocean caused by acidification. The CO2 treatment gradient allowed statistical extraction of the acidification effects with linear regression analysis of distinct growth phases. However, nutrients (inorganic N and P) were added in equal amounts to all mesocosms on Day 13 to boost phytoplankton growth, and this complicates extrapolation of the results to natural conditions, as CO2 and nutrient treatment effects cannot be statis-



tically differentiated with this design. This has also caused considerable confusion in the interpretation of the results after Day 13 in the Czerny et al. ms. Altogether this ms contains exceptionally comprehensive data and several relevant comments and interpretations. Unfortunately, the authors do not stick to the observations, but create a dubious pool X, which is not directly measured, but estimated by difference assuming mass balance in arbitrarily selected variables.

Specific Comments 1) The ms title "Element budgets in an Arctic mesocosm CO2 perturbation study" is misleading, since only chlorophyll a results are shown in absolute values, all particulate and dissolved nutrient pools being presented as treatment responses or temporal changes in 3 growth phases vs. (subtracted) initial reference value. 2) It is not clear why the response of the indigenous plankton community from Day 0 to Day 8 is left out of the treatment (CO2 perturbation and "bag effect") examinations, when CO2 treatment started already on Day -1 (and continued until Day 4)? 3) I strongly oppose the way of introducing 'Pool X', where the authors aim to assign the measured changes in inorganic C, N and P that can not be accounted for by the combined changes in pools of dissolved and particulate organics, cumulative gas exchange and sedimentation. However, dissolved organic C and N measurements as well as particulate organic P were excluded from the corresponding mass balance calculations because "measurement uncertainties of these parameters were larger than the size of Pool X and would therefore compromise mass balance calculations." From statistical (quantitative) point of view this approach is unacceptable. For example, the authors justify in Discussion the exclusion of direct DOC measurements from respective Pool X estimates by contamination of DOC samples. However, temporal development of DOC observations (given in Schulz et al, Fig. 8D, Biogeosciences Discuss., 9, 12543–12592, 2012) does not support the contamination argument. 4) The authors state in Abstract that "CO2 treatments induced a shift away from diatoms towards smaller phytoplankton and enhanced cycling of dissolved organics was pushing the system towards a retention type food chain with overall negative effects on export potential." However, virtually no diatoms were found in mesocosms, until the major part

of nutrient additions were depleted near the end of the experiment, so diatoms could not be outcompeted by smaller algae. Moreover, the phytoplankton succession in the mesocosms seemed to be mainly governed by the combination of nutrient availability and cascading grazing effects, which were then mostly positively modified (but not controlled or induced) by elevated pCO2.

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