Biogeosciences Discuss.,
doi:10.5194/bg-2016-56-RC2, 2016

Biogeosciences
Discussions
Open Access

Interactive comment on "Effects of ocean acidification on pelagic carbon fluxes in a mesocosm experiment" *by* K. Spilling et al.

Anonymous Referee #2

Received and published: 24 April 2016

The manuscript of Spilling et al. reports on a mesocosm experiment conducted in the Baltic Sea to test for effects of increased CO2 on plankton carbon fluxes. This manuscript is part of a special issue and specifically reports on estimated net community rates and their variation due to increased pCO2. Although I am convinced of the scientific relevance of this study, I am not convinced considering this budgeting exercice as a separate manuscript is highly relevant. Spilling et al. under revision in this special issue already reports on decreasing respiration rates at high CO2, causing higher Chla, TPC and DOC concentrations in the high CO2 treatments. The added value of the present manuscript is to estimate plankton rates that have not been directly measured (NPPe, but see later comment on this term; GPPi, but again see later comment; BR; DOC production). I would definitely recommend merging the two Spilling et al. papers to provide a more comprehensive overview of what happened during this

Printer-friendly version

Discussion paper

experiment.

If this suggestion is not followed, this manuscript, in my opinion, needs major revisions in order to improve its clarity and to discuss and criticise more deeply what has been found.

Estimates of DOC, TPC and DIC pools in mol C m-2: I was wondering for quite a while how these initial pools have been calculated and how the authors could provide an error estimate on a single sampling. I saw in the other Spilling et al. that these pools were actually averages of 3 sampling dates at the start of each phase. This must be clarified in the present manuscript. Also, how were integrated pools estimated: it is mentioned (and only for DIC, L136) in the ms that volumetric concentrations in per kg were converted using seawater density. Obviously, they were further multiplied by the considered depth. Please clarify.

Estimates of DOC, DIC and TPC rates of change: no information is provided on how these rates have been calculated. I believe these were calculated through linear regressions of each stock evolution during the considered phase. This must be clarified. Looking at Table 1 of the other Spilling et al., there are some discrepancies with rates presented here (e.g. Exp TPC of 7.4 in the first mesocosm compared to 6.6 in this paper, but this is also the case for other rates). Looking at the important errors associated with these rate estimates, it does seem like many slopes are not significantly different from 0. Please comment. In that case, how is it possible to compare these rates between the different mesocosms. Were these differences actually tested?

Estimates of NPPe and GPPi: Based on observed variations of TPC, DOC and DIC, the authors further calculated biological carbon fluxes. Net primary production measured by the 14C method (over 24h incubations) were compared to, what the authors refer to as NPPe being the missing process closing the organic budget: NPPe = Export + net variation in TPC + net variation in DOC). As the authors correctly mention, NPPe does incorporate total respiration and not only autotrophic respiration, as does the 14C method (this is actually clearly doubtful considering the long incubations that have been performed)). Anyway, this is incorrect to refer to this process as Net Primary production, this is misleading and you really should consider using the proper term: Net Community Production, and as it is based on an organic budget, you should use NCPo. The authors further use an inorganic budget (based on DIC net fluxes, and estimated CO_2 fluxes) to estimate Gross Primary Production. I would strongly recommend for clarity to reconsider this part and to calculate NCPi, being the Net Community Production based on the inorganic budget. This is, I believe, what is shown in Fig. 3 and termed as Biological release or uptake. The authors have thus two estimates of the plankton community metabolism that do provide different outputs. While it seems like the inorganic budget shows that the community was heterotrophic in ambient mesocosms (Biological release of DIC), the organic budget suggests the opposite for all phases. This must be discussed. The paper as it stands is highly confusing with respect to this metabolic aspect. i.e. In the abstract is mentioned that during phase 1, the community under ambient and high CO_2 treatments was autotrophic (i.e. more production than respiration, with capacity to export to the sediment traps and export to the DOC pool). However, it is clearly stated that the community was heterotrophic during the entire experiment under ambient CO_2 conditions. Again, this must be clarified.

Comparison between the inorganic and organic budget: I already mentioned this, but I would like to insist on the fact that this paper reports on budgets based on both inorganic and organic constituents. Since they do not really agree, this must be deeply discussed. A recommendation on which type of budget is the most relevant and associated with the lowest uncertainties should be further proposed.

CO_2 effects on estimated rates: I do not see how differences between estimated rates between low and high CO_2 treatments have been tested. It is mentioned on L345 that “an effect of the different CO_2 treatments was noticeable in the NPPe but not in the NPP14C”, how was it tested?

Comparison between NPP14C and NPPe: as correctly stated by the authors, NPP14C

Printer-friendly version

Discussion paper

rates should provide equal or higher estimates than NPPe (NCPo, see above). This is not the case and attributed (on top of potential errors on one control mesocosm) to “changed parameterisation during in incubation in small volumes”. Based on my experience, we usually observe higher rates in small incubations vs large ones, not really in accordance with the lower rates of NPP14C during phase 1. Alternatively, this offset could be attributed to errors associated with NPPe estimates, since the TPC pool was clearly underestimated. Could you comment on this?

Estimates of biological pools: I do not really get what is the added value of calculating and presenting pools of meso- and microzooplankton, micro- and nanophytoplankton, picophytoplankton, bacteria and viruses. I would guess that these informations are already available in other manuscripts from the special issue. Is this not the case? This makes a small paragraph of the Results and Discussion and, apart from showing that measured TPC is much much lower than the cumulated stocks of these biological compartments, I do not see what valuable information it brings.

Estimates of variability: I would recommend the authors to mention the sample size each time SE are provided. Furthermore, I do not really see (as this is not explained) how SEs have been calculated for estimated rates (error propagation). e.g. as NPPe rates are based on DOC net fluxes, therefore the associated errors should be at least equal to the errors associated with DOC net fluxes right? This is not the case, and this must be clarified further.

Minor issues: Abstract: L57: did not transfer, please correct L58: revealed a clear effect of increasing CO2 on carbon production. I don’t think this is correct. Carbon production does not seem impacted, while carbon loss is.

Materials and Methods: L104: I understood that more mesocosms were initially deployed, I do not see why this is not mentioned here. Everyone must also know how hard such experiment is. L159: Grossart et al. (2006), please correct L185: according to: , please correct L194: Cherny et al. (2013b), please correct. L205: organic carbon pool, please add dissolved + particulate for clarity L207: Direct measurements using . . ., please correct

Results and Discussion: L265: While some indication on temporal evolution is provided for the other measured variables, this is not the case for bacterial biomass, please add this information. L280: Spilling et al. 2016), please correct L286: in e.g., please remove e.g. L287: have pointed at, please correct L297: p<= 0.01 I believe, please correct. L325: (Paul et al. 2015 (a or b)), please correct and clarify L353: Spilling et al. 2016), please correct

Figures Fig. 3. As mentioned earlier, for clarity, Biological release or uptake should be referred to as NCPi (based on the inorganic budget). Values are not in mol C m-2 but in mol C m-2 d-1 I think, please correct.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-56, 2016.