Biogeosciences Discussions

Biogeosciences Discuss., referee comment RC2
https://doi.org/10.5194/bg-2021-40-RC2, 2021

Comment on bg-2021-40

Anonymous Referee #2

Referee comment on "Cyanobacteria net community production in the Baltic Sea as inferred from profiling *p*CO_2 measurements" by Jens Daniel Müller et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-40-RC2, 2021

General comments:

In their manuscript, "Cyanobacteria net community production in the Baltic Sea as inferred from profiling pCO2 measurements", the authors establish a method for deriving depth-integrated net community production (NCP) from underway surface pCO_2 measurements on ships of opportunity in the Baltic Sea. The authors have successfully communicated the relevance of their data set in the context of the Baltic Sea, one of the most extensive anthropogenic hypoxic zones in the world. Their efforts make an important scientific contribution to primary productivity and biological pump studies in general, as their proof of concept offers potential to scale the availability of NCP estimates using a parameter, pCO_2, that is relatively common and requires low effort to measure.

While I find the approach of this study robust, particularly the multiple comparisons of NCP and the treatment of lateral/vertical mixing effects on NCP, there are some important details of the Methods missing. As I imagine an important goal of the authors is for their NCP methodology to be replicated and tested elsewhere, it is important that they clarify these details. Below, I have offered some specific suggestions towards this end, referencing line numbers of the submitted manuscript when relevant.

Specific comments:

Line 46 – It would be interesting and relevant to the study background if the authors could clarify what is the relative contribution of the second, summertime cyanobacteria to the Baltic Sea hypoxia?

Section 1.3 – It is not very clear how this study will "disentangle" the multiple stressors on cyanobacteria blooms in the Baltic Sea. The way this section is written promises the reader that the results and discussion will address this challenge, even though it does not clearly do so. I would instead focus the background here more on the relationship among cyanobacteria blooms, hypoxia and NCP, which better relates to the study's aims.

Line 71-73 – It is not clear, in the context of this study, why using oxygen measurements to estimate NCP is inferior to using pCO2 to estimate NCP, except that pCO2 data are perhaps more readily available from ships of opportunity. There are multiple examples of

NCP being estimated using dissolved oxygen time series (e.g., â□□O_2/Ar time series) on the time scales of phytoplankton blooms. If O2 equilibrates more quickly than pCO2, would it not be preferable for studying Baltic Sea dynamics over a few weeks? In any case, in this study, the authors report cumulative NCP estimates over time, which suggests that, despite the different equilibration time scales for O2 and CO2 in the mixed layer, cumulative NCP estimates based on each parameter should approximate each other over the time scale of a bloom.

Section 2.2.3 – It would be useful here to state the different phytoplankton that were collected by name, rather than in Appendix B2 only.

Section 2.4 – It would be very helpful to provide the equations used to calculate the "best-guess" NCP values, including how the integration depth was determined in this approach. In the results, the authors imply that they used a depth of 12 m (lines 308-310), but this should be explained in the Methods rather than in the results.

Lines 184-191 – It would be useful to clarify here that applying an average alkalinity to derive Ct* is valid because, as the results will show, the biogeochemical variability across stations of interest was low.

Section 2.4.4. – This section about vertical mixing should reference Figure 3 because the vertical mixing event is very clear when looking at all the profiles at once. Referencing Figure C3 seems redundant (see my technical comment below).

Line 249 – Is it fair to say that, because the BloomSail data exhibit little regional biogeochemical and physical variability across the stations of interest, the cruise tracks encompassing this same region should not be variable as well? If so, perhaps the authors could clarify that here, as well.

Lines 259-265 - While the general idea is there, the authors need to better explain how they obtain TPD values. It was unclear in this paragraph and in Fig. C4A how they choose an actual depth. Is there a threshold for change in temperature between cruises that helps one select the right depth (e.g., the depth when change in temperature is 0.2 °C, according to Fig. C4A?), analogous to using a density difference criterion for deriving mixed layer depth?

Lines 308-310 – How does this choice of integration depth compare to a best-guess NCP estimate calculated using a mixed layer depth based on a density difference criterion? Should the "best-guess" NCP estimate also be calculated using this density difference approach so that it is more comparable to the reconstructed NCP calculations later on in the paper?

Figure 5 – I am used to reporting NCP as positive if Ct* decreases, and negative if Ct* increases. Thus, I do not understand how NCP was negative, until August 6, and positive from August 6-13, unless the authors are using an opposite sign? (I would expect the opposite because inorganic carbon drawdown would indicate more production over respiration.) I suggest the readers reevaluate the sign of the NCP values they report here. This is another reason that it would be important for them to share their NCP calculation equations in Section 2, as well.

Lines 321-322 – It would be interesting for the authors to discuss this peak cumulative NCP value more in the discussion. How does resolving the change in cumulative NCP over the course of a bloom improve understanding around hypoxia in the Baltic Sea?

Lines 323-324 – If the authors focused on reconstructing NCP over July 6 to July 24, why does Figure 6 show reconstructed values beyond July 24? The figure is not consistent with the text in this respect.

Section 4.1 – Since different NCP units are being reported throughout this section, the authors should make sure to always translate units explicitly in the text to show how they are comparable. For example, how would the authors convert their NCP units to be directly comparable to the values from Wasmund et al. (2001)?

Section 4.2 - Scaling this study's method depends on the availability of models like the GETM for other locations. Can the authors comment on the availability and applicability of this model (or other similar models) in other ocean regions?

Line 405 – That a mean regional alkalinity could be used to normalize dissolved inorganic carbon seems to be an important output of the study. Even though this is not the main objective, perhaps this outcome should be acknowledged as a goal in the introduction, as well.

Technical comments:

Line 39-40 – It is better to paraphrase this quotation than quote it directly from the source.

Lines 216-217 and 220-222 – I suggest writing out these bullet points into full sentences to be stylistically consistent with the rest of the manuscript.

Figure 2 – The authors do not need color here to convey the date. Mean cruise date could just be written into the x-axis labels (with labels rotated so that the text fits).

Line 330-331 – This sentence, "For both data sets CT* time series were calculated based on the same mean AT", should go in the methods, Section 2.5.

Line 359-361 – This sentence is grammatically confusing and should be rephrased.

Line 421- I suggest using a different word than "planktological".

Figure C3 – This is not necessary because the same information is conveyed in Figure 3 (therefore this figure seems redundant).