

Biogeosciences Discuss., author comment AC2
<https://doi.org/10.5194/bg-2021-40-AC2>, 2021
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Reply on RC2

Jens Daniel Müller et al.

Author comment on "Cyanobacteria net community production in the Baltic Sea as inferred from profiling pCO₂ measurements" by Jens Daniel Müller et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-40-AC2>, 2021

Dear Referee 2

Thank you for providing your review, which we considered very helpful to strengthen the presentation of our study. Most of your comments request an extension of the manuscript with additional information, which we are happy to implement. In particular, we aim to add a dedicated discussion section addressing the biogeochemical interpretation of our NCP estimates. Several points you raised were also in agreement with RC1, and we cross reference our answers where this applies.

Please find our detailed answers (bold font) and proposed text edits (bold italic font) next to your comments (normal font) below. Line numbers refer to the initially submitted version of the manuscript.

We hope to have addressed all of your comments appropriately, but welcome additional feedback if required.

Best wishes
Jens Daniel Müller, on behalf of all co-authors

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?

Similar information was also requested in RC1. To our understanding, this question involves two aspects: (1) What is the contribution of cyanobacteria to the annual NCP including the spring bloom and (2) how does NCP relate to deep water deoxygenation.

We intend to address aspect (1) as follows:

Enabling reliable NCP estimates for the mid-summer cyanobacteria bloom is the core aim of this study. A reliable hindcast of the mid-summer NCP will only be possible when the findings of this study are applied to almost two decades of available SOOP pCO₂ data. In the absence of this information, the assessment of the importance of the spring relative to the mid-summer bloom is highly uncertain. Nevertheless, we agree that some more information on the spring-bloom and a rough approximation of its contribution to the annual NCP should be given. Accordingly, we will extend section 1.3 of the introduction with following information:

“The first production event is the spring bloom, which is controlled by the availability of nitrate and shifted from being dominated by diatoms to dinoflagellates in the late 1980s (Wasmund et al., 2017; Spilling et al., 2018). After a so-called bluewater period with close-to-zero NCP rates, the second type of production events are mid-summer blooms of nitrogen-fixing cyanobacteria that develop in most years depending on meteorological conditions. Although cyanobacteria NCP is yet poorly constrained, its relative contribution to the annual NCP in the Eastern Gotland Sea in 2009 was estimated in the order of 40% (Schneider and Müller, 2018; Schneider et al., 2014), though the uncertainty is high. This preliminary estimate further needs to be interpreted with care, as cyanobacterial NCP varies significantly between years and regions.”

We intend to address aspect (2) as follows:

We will add a dedicated section in L399 of the discussion to describe the biogeochemical relevance and interpretation of our NCP estimates. Among others, this section will address the general relation between NCP, organic matter export and deoxygenation as follows:

“Our best-guess of cumulative NCP on July 24 ($\sim 1.2 \text{ mol m}^{-2}$) represents the net amount of organic matter that was produced throughout the bloom event in the surface waters above the compensation depth at 12 m. After subtracting $\sim 20\%$ dissolved organic carbon (DOC) production, our NCP estimate equals the produced particulate organic carbon (POC) that is potentially available for export. [...] However, the potential POC export constraint by our NCP estimate is not equivalent to the supply of organic matter to the deep waters of the Gotland Basin, because POC might be (partly) remineralised before sinking beneath the permanent halocline. Remineralisation of POC that occurs during the bloom event above the compensation depth is – according to our definition of NCP – already included in our estimate. In contrast, any additional remineralisation of POC that occurs between the compensation depth and the halocline, or above the compensation depth after the end of the bloom event, reduces the organic matter supply to the deep waters and thereby mitigates deoxygenation. Indeed, our profiling measurements indicate a steady accumulation of C_T beneath the compensation depth (Fig. 4), likely fueled by the remineralisation of organic matter. However, our measurements do neither allow to constrain the budget of this C_T* accumulation, nor could we attribute the source of organic matter.”*

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.

This issue was also raised by RC1 and we agree that this study itself does not explicitly address the controlling factors of the blooms. However, we expect a major contribution to this question when applying the new NCP reconstruction approach to almost two decades of SOOP observations. In order to clarify this, we will add the following sentence in L62:

"A long-term hindcast of cyanobacteria NCP and the attribution of its strength to prevailing environmental conditions in particular years could improve our understanding of controlling factors and facilitate more reliable predictions of the blooms. However, such a hindcast of cyanobacteria NCP was so far impossible due to missing vertically-resolved observations that would allow to constrain their organic matter production."

We will further clarify how hindcasts based on our findings will support the disentangling of drivers by adding the following information to our conclusions (L437):

"The application of this approach will allow for the detection and attribution of trends in cyanobacteria NCP across decades. In particular the comparison of NCP estimates of bloom events that occurred under different environmental conditions will provide a better understanding of the controlling factors. Factors to be tested include the environmental parameters used to constrain NCP ($p\text{CO}_2$, SST, and TPD), but also additional observations of nutrients and phytoplankton composition routinely determined on SOOP Finnmaid and in the framework of the Baltic Sea monitoring program. The recently started initiative to deploy biogeochemical ARGO floats in the Baltic Sea will further aid to link surface NCP estimates and deep water deoxygenation, and thereby constrain biogeochemical budgets in the Baltic Sea."

Line 71-73 – It is not clear, in the context of this study, why using oxygen measurements to estimate NCP is inferior to using $p\text{CO}_2$ to estimate NCP, except that $p\text{CO}_2$ 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., $\square\square\text{O}_2/\text{Ar}$ time series) on the time scales of phytoplankton blooms. If O_2 equilibrates more quickly than $p\text{CO}_2$, 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 O_2 and CO_2 in the mixed layer, cumulative NCP estimates based on each parameter should approximate each other over the time scale of a bloom.

We think that an essential piece of information was missing to make our statement understandable. NCP estimates based on either CO_2 or O_2 time series require a correction of the observed concentration changes for the air-sea flux of either gas. The calculation of this air-sea flux is associated with uncertainties. As a consequence, the higher flux rates of O_2 lead to a higher uncertainty in the derived NCP estimate. We will try to clarify this by modifying Line 71-73 as follows:

"In principle, NCP could as well be estimated from O_2 time series. However, the equilibrium reactions of carbon dioxide (CO_2) in seawater result in slower re-equilibration of CO_2 with the atmosphere compared to O_2 (Wanninkhof, 2014). This results in substantially longer preservation of the C_T signal and a

lower uncertainty contribution of required air–sea CO₂ flux corrections, and makes C_T the preferred tracer for NCP.

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.

This information will be included in Line 164 as follows:

“Phytoplankton samples were fixed with Lugol solution, and cyanobacteria community composition and biomass were determined by microscopic counts of the genera Aphanizomenon, Dolichospermum and Nodularia according to the Utermöhl method (HELCOM, 2017)”

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.

In agreement with a comment from R1, we will clarify that our determination of the integration depth aligns with the traditional concept of the compensation depth, i.e. the depth at which primary production and respiration balance out. Accordingly, we will clarify in Line 180 that our NCP best-guess is constrained to the compensation depth:

“... we derive the column inventory of incremental changes of ΔC_T^* ($i\Delta C_T^*$) between two cruise events through vertical integration of ΔC_T^* from the sea surface to the compensation depth (cd), i.e. the depth (z) at which no net drawdown of CO₂ was observed”

This clarification in the methods section will further be supported by two equations summarizing our NCP calculation.

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

The information given in lines 190 - 191 was rephrased and now reads:

“The uncertainty in the determination of changes of C_T* is below 2 μmol kg⁻¹ when the mean A_T is constrained within the observed standard deviation of ±27 μmol kg⁻¹ (see Appendix C1 for a detailed assessment).”

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).

The reference to Figure 3 will be included in line 208. (However, there was no redundant reference to Figure C3 in section 2.4.4. that could be removed)

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.

This is absolutely fair to say. It will be clarified in line 250 that the regional variability of averaged physical parameters from the GETM model was low.

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?

No, for TPD no temperature threshold is required. We will try to clarify this and the approach in general by rephrasing the respective section to:

“TPD characterises the mean penetration depth of surface warming that occurred between two sampling events. TPD was defined as the SST increase divided by the integrated warming signal across the water column, i.e. the sum of all positive temperature changes within 1m depth intervals (for a graphical illustration see Fig. C4A). According to this definition and in contrast to MLD, TPD takes gradual changes of temperature across depth into account and does not require a fixed threshold value. TPD is only applicable when SST increases and has units of metres. To illustrate the TPD concept, it should be noted that a homogeneous warming signal that ceases abruptly at 10 m water depth would result in the same TPD as a warming signal that decreases linearly from the surface to 20 m water depth (TPD is 10 m in both cases)”

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?

We did actually calculate NCP based on a density difference criterion (MLD) and results are displayed in Fig. 6c (left panel). One could argue that this estimate is based on surface CO₂ observations only, and is therefore a reconstruction and not a best-guess. However, the vertical variability of the C_T* profiles above the MLD is very small (compare Fig 4, a2) and accordingly there is no significant difference between integrating the surface values or vertically resolved C_T* values across the MLD. We therefore argue that the requested comparison is already covered in Fig. 6c.

Figure 5 – I am used to reporting NCP as positive if C_T* decreases, and negative if C_T* 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.

This comment is in agreement with a remark by R1. To clarify the issue we will indicate in Figure 5 and the corresponding caption, that the sign of NCP is indeed the opposite of the changes in C_T^* . We will also include the equation for NCP calculation in Sect. 2 and explicitly mention the interpretation of the sign of the three components (observed C_T^* changes, air-sea fluxes, and mixing).

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?

The contribution of cumulative NCP estimates to the better understanding of hypoxia in the Baltic Sea will be addressed in a dedicated section in the discussion. Please refer to our answer to your comment on Line 46 above and also the reply to RC1.

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.

This sentence was indeed unclear. Our intention was to point out that our presentation of the results and their discussion and interpretation will focus on the period until the NCP peak on July 24, not that the reconstruction approach is technically limited to this period. To avoid this misunderstanding, we will rephrase the respective sentence to:

“Accordingly, our interpretation of the reconstructed NCP based on surface pCO_2 observations will focus on the NCP peak value on July 24.”

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)?

We already pointed out that the mean rate of $4.4 \mu\text{mol kg}^{-1} \text{d}^{-1}$ determined in this study refers to the average C_T^* drawdown of $\sim 90 \mu\text{mol kg}^{-1}$ over 12 days (Line 377). To make this mean rate directly comparable to the values from Wasmund et al. (2001), we will further clarify that:

“Wasmund et al. (2001) conducted ^{14}C incubation experiments at different water depths to determine instantaneous rates of daytime primary production during a cyanobacteria bloom. The reported carbon fixation rates in surface waters ($0.4 - 0.8 \text{ mmol C m}^{-3} \text{ h}^{-1}$, equivalent to $9-19 \mu\text{mol kg}^{-1} \text{ d}^{-1}$) are in the same order of magnitude as the mean rate found in this study ($4.4 \mu\text{mol kg}^{-1} \text{ d}^{-1}$). The lower rate found in this study might be attributable to our averaging of the C_T^* drawdown across day- and nighttime.”

However, it should also be emphasized that the comparison to the findings of Wasmund et al. (2001) focuses on the depth distribution of NCP rather than the absolute values, for which the unit conversion is irrelevant. This aspect will be highlighted as:

“More important than the agreement between the sea surface fixation rates, is the fact that Wasmund et al. (2001) also found significantly lower fixation rates below 10 m water depth ($< 0.2 \text{ mmol-C m}^{-3} \text{ h}^{-1}$), which agrees well with the depth distribution of NCP observed in this study.”

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?

We will clarify in the methods sections that the model run we used “... covers the entire Baltic Sea and the period 1961 - 2019.”

With respect to the applicability outside the Baltic Sea, it should be noted that in general GETM is a state of the art ocean model as it is ROMS or NEMO. As long as good enough forcing data are available (atmospheric data, boundary data) and the models spatial and vertical resolution is sufficient to resolve the important scales, we do not see any show stopper in applying GETM (or other models) to other regions of the coastal or global ocean. At present GETM is used for studies in the North Sea / Celtic Sea, the Mediterranean Sea, the Black Sea, Persian Gulf, and along the west coast of Australia. However, we deem it beyond the scope of this study to provide such detailed information on the availability/applicability of GETM or other ocean models. We would even be afraid that too much information to this end distracts from the core findings of this study or creates the impression that we consider our reconstruction approach ready-to-go for other ocean regions. Therefore, we prefer to refrain from adding this information to the manuscript.

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.

Although this study confirms that a mean regional alkalinity can be used to quantify changes in normalized dissolved inorganic carbon concentration over time and provides a detailed uncertainty assessment for the underlying conversion from $p\text{CO}_2$, this approach is not a new outcome or intellectual achievement of this study. In contrast, we rely on this previously established method, which is explicitly stated in the introduction as:

“... it was demonstrated that highly accurate time series of changes (not absolute values) in C_T can be derived from $p\text{CO}_2$ observations (Schneider et al., 2006). The conversion from $p\text{CO}_2$ to C_T relies on a fixed alkalinity (A_T) estimate and is applicable under the condition that internal sources of A_T can be excluded, which is the case in the Baltic Sea due to the absence of calcifying plankton (Tyrrell et al., 2008).”

and:

“This study builds upon the previous success to determine NCP based on pCO₂ time series, but extends the approach to vertically resolved observations for the first time.”

Accordingly, we do not intend to introduce this outcome as a goal of this study.

Technical comments:

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

The quotation will be paraphrased.

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.

We appreciate this suggestion, but feel that the bullet points help to present the various reconstruction approaches in a structured fashion. Furthermore, we do not believe that this is stylistically inconsistent with the rest of the manuscript, as there are no similar lists of approaches, which are not set as bullet points. Therefore, we prefer to keep the formatting as is.

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).

We agree that in Figure 2 itself, using color to convey mean cruise dates is somewhat redundant with the date on the x-axis. However, the intention of using color in Figure 2 is to directly link it to Figures 3, 4, and B1, in which we use the exact same color scale. Furthermore, writing mean cruise dates directly on the x-axis would result in unequal breaks of the time axis, which we want to avoid. Therefore, we intend to keep Figure 2 as is.

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.

The sentence will be relocated.

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

We agree that this sentence was confusing and combined two statements, each of which deserves its own sentence. The sentence will be split and rephrased to:

“Still, this lateral variability is small compared to the signal to be resolved (i.e. the C_T^* drawdown of $\sim 90 \mu\text{mol kg}^{-1}$). However, on a relative scale the lateral C_T^* variability is about as large as the difference between the best-guess and the TPD-based NCP reconstruction ($\sim 10\%$), suggesting that the bias of the reconstruction falls within the uncertainty range of the best-guess.”

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

“planktological findings” will be replaced with “findings from the long-term cyanobacteria monitoring program”

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

While it is true that the same data are shown in Figure 3 and C3, the information that should be conveyed differs. While Figure 3 is intended as a first, compact overview relating all measured profiles to each other on one plot, Figure C3 seeks to resolve the variability across stations on one cruise day. This higher resolution is critical to underpin some of our statements in the results section (e.g. “During this period of intense primary production, the regional variability of SST, $p\text{CO}_2$, and C_T^* across stations was low compared to their temporal change”) as well as in the discussion (e.g. “The temporary C_T^* increase was limited to the north-eastern stations 07 – 10”).

We therefore intend to keep both figures as they are.

Additional references used in this reply

Spilling, K., Olli, K., Lehtoranta, J., Kremp, A., Tedesco, L., Tamelander, T., Klais, R., Peltonen, H., and Tamminen, T.: Shifting Diatom—Dinoflagellate Dominance During Spring Bloom in the Baltic Sea and its Potential Effects on Biogeochemical Cycling, *Front. Mar. Sci.*, 5, <https://doi.org/10.3389/fmars.2018.00327>, 2018.

Wanninkhof, R.: Relationship between wind speed and gas exchange over the ocean revisited: Gas exchange and wind speed over the ocean, *Limnol. Oceanogr. Methods*, 12, 351–362, <https://doi.org/10.4319/lom.2014.12.351>, 2014.

Wasmund, N., Kownacka, J., Göbel, J., Jaanus, A., Johansen, M., Jurgensone, I., Lehtinen, S., and Powilleit, M.: The Diatom/Dinoflagellate Index as an Indicator of Ecosystem Changes in the Baltic Sea 1. Principle and Handling Instruction, *Front. Mar. Sci.*, 4, <https://doi.org/10.3389/fmars.2017.00022>, 2017.