Dear Dr. Marañón,

I find that the manuscript, "Cyanobacteria net community production in the Baltic Sea as inferred from profiling pCO2 measurements", has improved greatly since its original submission. The authors have made effort to clarify the unique contributions that their work has for understanding primary productivity patterns in the Baltic Sea. After reading this second submission, it is now clear to me how their approach to reconstructing NCP from modeled temperature/salinity profiles and opportunistic pCO_2 measurements could improve understanding of eutrophication and hypoxia in this marine environment. In particular, I am impressed by the potential opportunity to reconstruct NCP over past decades using surface pCO_2 measurements from ships of opportunity, and I hope that the authors pursue this research endeavor themselves or help other researchers in the region do so.

Perhaps most importantly, the authors have improved the methods section of the manuscript, providing a thorough explanation of their different approaches to calculating depth- and time-integrated NCP values. I think it is now possible for an external reader to replicate and apply these methods in other contexts.

Overall, I recommend that this manuscript be accepted for publication with minor revisions. The comments in the following letter address some further suggestions and questions I have about the methods and results that I think should be addressed before publication. My comments are separated by section and/or line number, as relevant.

Sect. 2.2.3: How were the C_T* values from discrete measurements used in this study? I suggest that the authors clarify the way these discrete measurements were used. If they were just used for comparison to C_T* values derived from pCO_2 profiles, what were the results of those comparisons?

Section 2.5: It is important to reference Section 2.6.3 in Section 2.5, which explains the calculation of the C_T* drawdown penetration depth.

Section 2.6: For thoroughness, I suggest that the authors write out the calculations for reconstructed NCP, as in Section 2.5. Even though they are similar to the previous set of NCP calculations, it would be useful and complementary to see them written out.

Figure C4: This is so useful for understanding Section 2.6.3. Therefore, I think this figure should be in the main text.

Line 147: Does "below" in this sentence means "less than" 60 m depth? I think the authors should clarify this.

Line 185: Why were discrete samples collected at just two stations?

Line 299: I double-checked the math described in this paragraph (Section 2.6.3), and I think there is a typo here. I think TPD should be defined as the integrated warming signal divided by

the SST increase, instead of the other way around (which is how the sentence is written). That's the only way the example with 10 m provided on line 304 would make sense.

Lines 311-312: Similarly, as in my prior comment, I think CPD is the integrated loss of C_T* divided by the decrease in C_T* at the surface, rather than the other way around, as it is currently written.

Figure 4: I have a number of comments about this figure. First, it is difficult to see the August 16 data (white circles) for panels a1 and b1, so I suggest extending the x-axis on these two sub-plots. In panels a2 and b2, why are there eight vertical profiles for Δtemperature and ΔC_T*? If these values indicate changes between cruise events, there should be seven values rather than eight. It is a bit misleading to plot the July 6 profiles, which have values of 0 °C and $\tilde{}\mu mol\ kg^{-1}$ across depths, as no magnitudes of change could calculated for this first cruise. Finally, why is there just one depth indicated on this figure if the authors allowed the CPD, MLD or TPD values to change throughout the duration of the 8 BloomSail cruises? This contrasts with Figure 6, which indicates variable integration depths.

Line 331: If lateral exchange was important at the northeastern stations, how much did this observed increase in A_T and C_T* impact the best-guess NCP estimates around the July 31 cruise?

Line 451: I recommend the authors cite again here where they acquired the 20% estimate for DOC production?

Line 483: How does the requirement of a mean measured A_T value for the region of study weaken the utility of this surface- and model-based NCP reconstruction approach, considering that A_T is not measured on ships of opportunity?