

AC2.2: Reply on RC2.2 (second revision)

Provided by Jens Daniel Müller on behalf of all co-authors

Dear Referee 2,

Thank you for taking your time to provide a second careful review of our study, after the comments on the original submission were taken into account. The author team is glad that our previous edits helped to clarify the main scientific progress of our study. We also appreciate your encouragement to apply our NCP reconstruction approach to almost two decades of surface pCO₂ observations. This is of course what we aim for.

Regarding your second revision of our manuscript, the author team considers your suggested minor revisions helpful in order to further clarify methodological details of our study, as well as the interpretation of the results.

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 resubmitted clean 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:

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 pCO₂ profiles, what were the results of those comparisons?

C_T^* directly measured on discrete samples was indeed only used for comparison to C_T^* calculated from pCO₂. We clarified this use of the data in Sect 2.2.3 (l.191) by adding:

The mean observed A_T was used for the calculation of C_T^* from pCO₂ (see Sect. 2.5.2), while measured C_T was only used for comparison to calculated values and not directly included in the NCP calculation.

The outcome of this comparison is illustrated in Fig. 5 showing surface C_T^* from discrete samples along with C_T^* calculated from pCO₂. We further evaluate the agreement between both data sources and added following text in l.327:

Furthermore, we found that C_T^* calculated from pCO_2 agreed with C_T^* derived from discrete samples within the uncertainty range attributed to regional variability (Fig. 5c).

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.

In Sect. 2.5 and 2.6.3 we describe the two fundamental approaches that we applied to calculate NCP, i.e. the best-guess NCP based on complete C_T^* profiles and the reconstructed NCP based on surface C_T^* . However, the C_T^* drawdown penetration depth (CPD) is not required to determine the best-guess as described in Sect. 2.5 and we thus do not agree that a cross-reference is important or helpful here.

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.

Thank you very much for this suggestion. Indeed, the additional equation should make it much easier to clearly distinguish our best-guess and reconstructed NCP estimate. The requested equation was added in I.250 of Sect. 2.6. In order to clearly distinguish the equations for our two types of NCP calculations, the NCP term was labeled with indices “best-guess” and “reconstruction” in equations (2) and (3).

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

Following the bg author guidelines, we placed figures with direct relevance to the results in the main text and figures that illustrate methods or reveal details of supplementary nature in the appendices. According to this criterion, we see Fig. C4 correctly placed in the appendix. Please note that Fig. 4C is referenced in Sect. 2.6.3 and that the appendices (in contrast to supplementary materials) will be part of the manuscript (i.e. be printed in the same pdf). We thus conclude that the figure can easily be found by the interested reader.

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

Yes, the text was changed to “less than”.

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

As the field sampling of this study was performed with a small sailing vessel and only three crew members, it was not possible to collect discrete samples at more than two stations. We explicitly state this constraint by editing the text in l. 185, which now reads:

Discrete samples were collected with a manually released Niskin bottle. Water sampling was restricted to stations 07 and 10 (Fig. 1b) due to logistic constraints.

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.

That is correct. Thank you for spotting the typo. The description was corrected.

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.

That is correct. Thank you for spotting the typo. The description was corrected.

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 Δ temperature 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 $\sim \mu\text{mol kg}^{-1}$ across depths, as no magnitudes of change could be 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.

In order to make the markers for the first and last cruise event visible in a1 and b1, we switched from a vertical line to “+” symbols. We do not extend the x-axis, because we want to avoid the required temporal extrapolation beyond the period covered by observations.

In panels a2 and b2 we removed the July 6 profiles, while keeping the color scale of the remaining profiles consistent with the other figures.

The red vertical line indicates the compensation depth (CD) of the C_T^* drawdown, which we used as a constraint for our NCP best-guess. In contrast to MLD and TPD, which indeed vary over time, we used a constant CD. It is thus correct to draw a single horizontal line in a2 and b2. Please note also the additional information on this topic given in l.349ff:

... the compensation depth located at 12 m. The determined compensation depth reflects the maximum penetration depth of the incremental (i.e. between cruise days), as well as the cumulative (i.e. from July 6 – 24), C_T^* drawdown (Fig. 4).

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?

While the northeastern stations are affected by the lateral exchange and show a temporary increase of C_T^* (see Fig. C3) on July 31, the other stations are not affected and show an almost constant surface C_T^* compared to the previous and the following cruise day. We already addressed this in l.328ff of the main text, which reads:

Between the extremes of pCO₂ and C_T* (minimum on July 24) and SST (maximum on August 3), a noticeable increase of surface C_T* was observed on July 31, which was accompanied by a higher regional variability across the station network (Fig. 5a,c). The temporary C_T* increase was limited to the north-eastern stations 07 – 10 (Fig. C3) and paralleled by a drop in salinity and elevated A_T at the same stations (Fig. B1). It is therefore attributable to the lateral exchange of water masses. All signals of this lateral intrusion vanished within a week. At the other stations (02 – 06 and 11 – 12), no noticeable signs of water mass exchange or C_T* changes were observed between July 24 and August 3, indicating that the production and respiration of organic matter were balanced during this period.

To confirm our interpretation that the production and respiration of organic matter were balanced during this period, we recalculate our NCP time series without the stations affected by lateral exchange of water masses. Our conclusion from this additional analysis were added in l.365:

The temporary drop in the NCP best-guess on July 31 is due to the lateral exchange of water masses as described in Sect. 3.1. Deriving the NCP time series without the stations affected by lateral exchange of water masses (07–10) results in an almost identical NCP estimate on July 24, but a reduced drop on July 31 (data not shown). In both cases, no signs of continued NCP were observed after July 24.

Please find at the end of this document modified versions of Figs. 4 and 5 produced without the observations made at stations 07-10. The modified figures serve for demonstration purposes only in this reply to the reviewer, but are not intended to be included in the manuscript for publication.

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

The references were added.

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?

According to numerous previous studies and our own sensitivity test presented in Appendix C1, changes in C_T* can be calculated from pCO₂ without exact knowledge of A_T, i.e. the bias in ΔC_T* is about 1 μmol kg⁻¹ for a bias in A_T of about 10 μmol kg⁻¹ (Fig. C1). For the Baltic Sea, a rough A_T estimate based on the well known and frequently monitored A_T-S relationship is sufficient to derive C_T* with acceptable uncertainty, i.e. with a conversion uncertainty that is much lower than other sources of uncertainty, such as regional variability. We conclude that the requirement of a mean A_T estimate does not weaken the utility of our approach, except for regions where A_T is very poorly constrained. We clarified this in the main text by adding the following text in l.501:

Likewise, the required mean A_T estimate should not restrict the applicability of our approach even if A_T is not directly measured. For the Baltic Sea, it was demonstrated (Schneider et al., 2003) that A_T estimated from the known A_T-S relationship (Müller et al., 2016) is sufficiently accurate to convert pCO₂ to C_T* (see also Appendix C1).

Additional figures for review purpose only

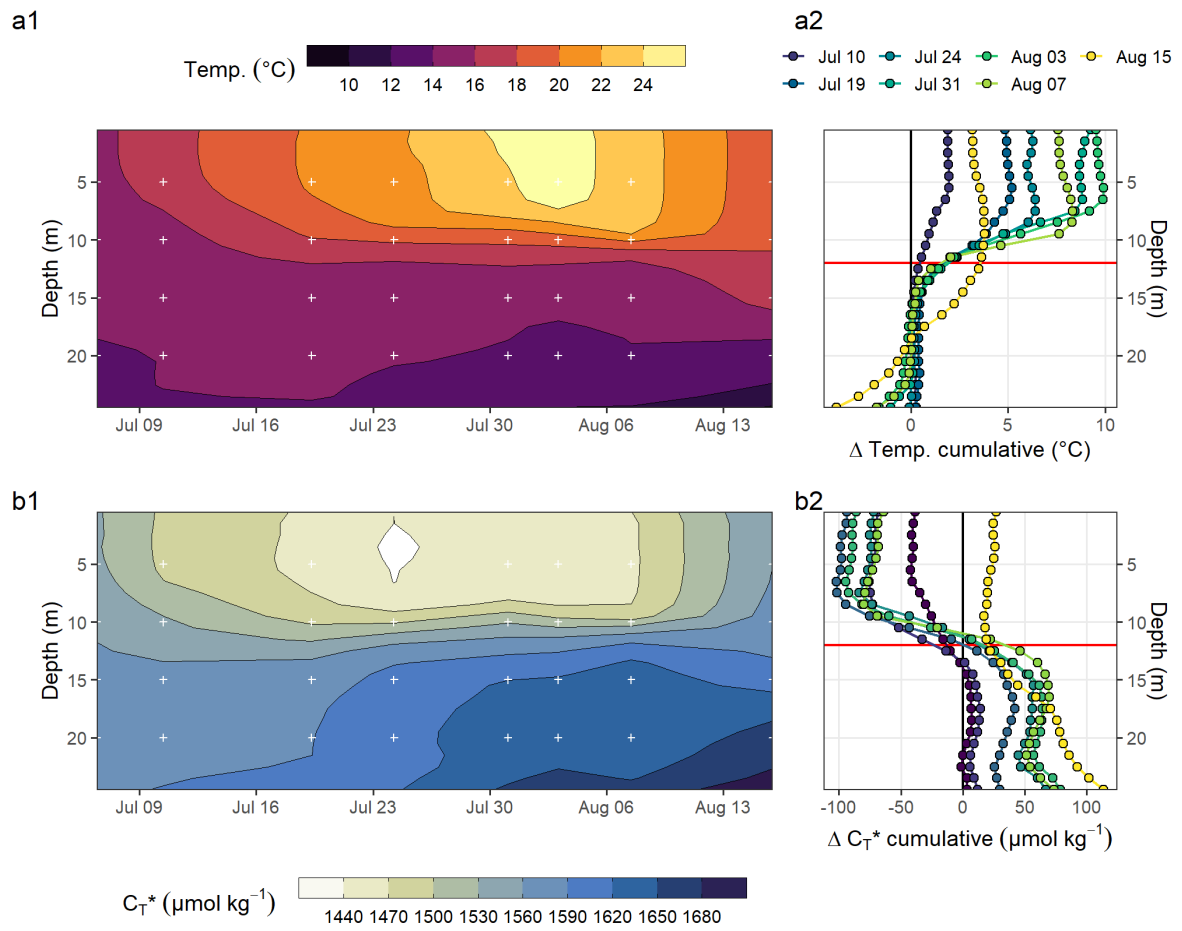


Fig. 4 as in the manuscript, but without data from stations 07-10.

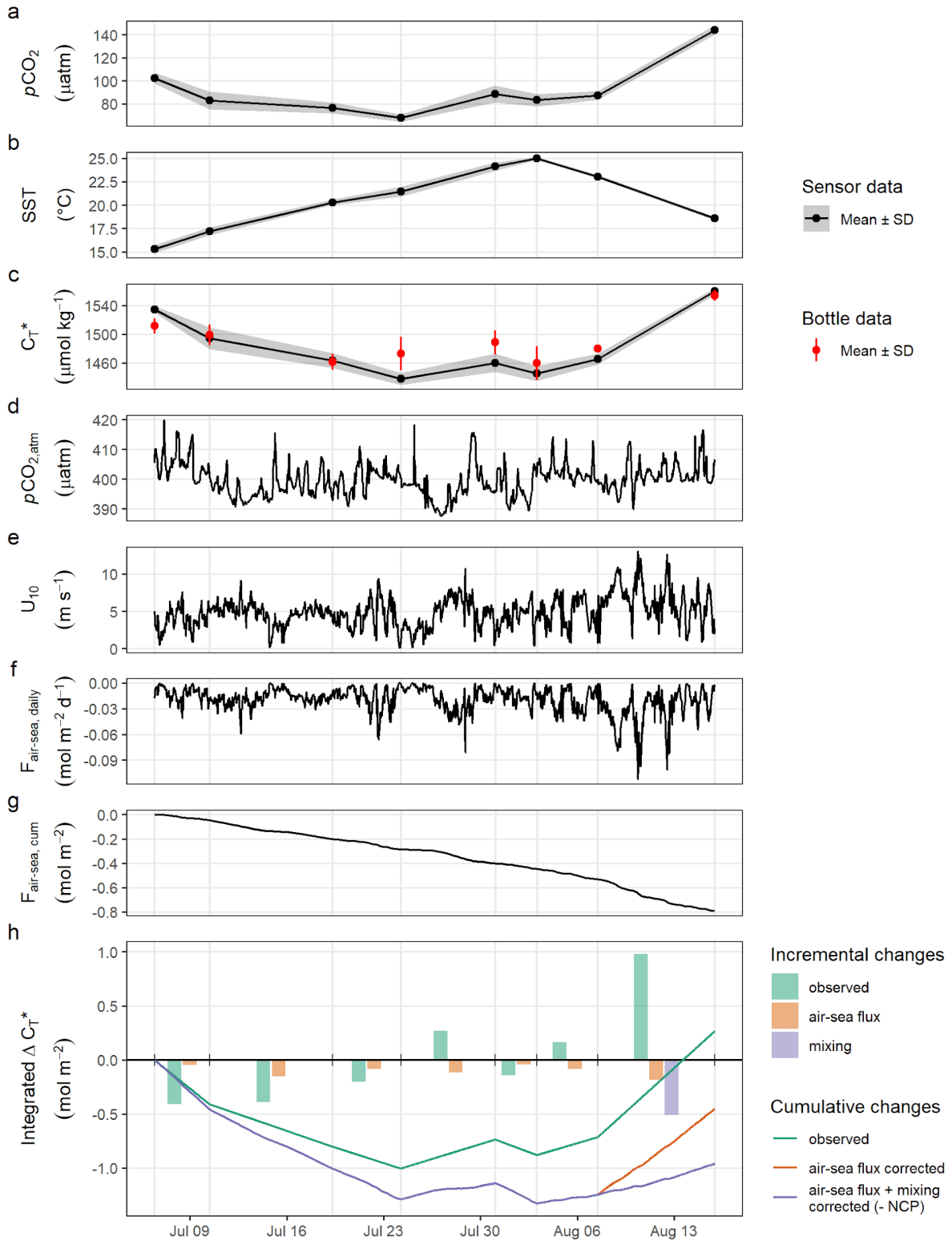


Fig. 5 as in the manuscript, but without data from stations 07-10.