The authors have made a commendable effort in addressing the reviewer comments. The manuscript has been improved in a number of aspects, particularly in distinguishing the aims of this paper relative to a similar paper recently published by the same lead author. The manuscript has also been improved in providing more detail as to methodology. But with these additions, some further questions arise concerning the methods by which riverine inputs were derived and whether or not tidal exchange effects were considered (see below). The manuscript would benefit from some additional explanations regarding these issues.

We thank the reviewer for their helpful review of the paper and we have attempted to carefully address the reviewer's comments in the below sections. Please note the line numbers refer to the final document and therefore not the tracked document.

Methods

Section 2.7 (estimation of monthly loads): please specify how frequently DIC and POC are measured for riverine inputs. Also, prior work has shown that C concentrations vary with discharge (POC typically increasing, DIC decreasing), therefore concentration-discharge relationships are commonly used when estimating loads. Was that done here? Lastly, I am not seeing how you are taking into account the effects of tidal exchange on C concentrations in the estuary.

The frequency of sampling was described in section 2.2 on lines 148-149. We edited the sentence to include 'once per month'. To clarify the 'Monthly mean POC and DOC' refers to mean of each zone we included 'for each zone in' on line 231 (text additions are italicized):

Lines	
147-149	Sampling was generally carried out *once per month* in February, May, June, July, August and November (see Rewrie et al. (2023) for a detailed description).
231	Monthly mean POC and DIC for *each zone in* May to August were calculated from 1997 to 2020,

We thank the reviewer for their suggestion to include concentration-discharge relationships. The reasons for calculating DIC and POC loads was to determine if the loads of these 2 parameters were the same magnitude, as well as to compare the internal DIC load during the non-drought and the drought period. This was described on lines 244-251 with 'The statistical differences between internal DIC loads during the recent drought (2014–2020) and non-drought (1997–2013) periods, and the differences between the internal DIC load in the mid-lower estuary and the POC load in the upper estuary (z1), for May to August were tested.' Therefore, we believe the application of concentration-discharge relationships would be beyond the scope for this specific paper.

As the sampling was conducted by helicopter, this permitted sampling at full ebb tide and this allowed the greatest possible synoptic comparability between the samples with regard to the influence of the tides. This means that all samples were collected at the same (or similar) tidal phase, i.e. during the ebb tide reflecting outflow from the estuary. This we consider important for consistency of the monthly sampling in this tidally dominated system. To clarify this we have edited the following sentence (text additions are italicized):

Line	
145-147	The FGG Elbe took ~~synoptic~~ surface water samples from 36 stations in the estuary (Fig. 1a) by helicopter at full ebb current, *which permitted the greatest possible synoptic comparability between the samples with regard to the influence of the tides (ARGE Elbe, 2000).*

Monthly sampling unfortunately does not allow to resolve the tidal impacts (ebb vs flood tide) on carbon dynamics within the framework of this manuscript. Please also see the answer to the last reviewer comment in reference to this.

An explanation of what is AOU and how it is calculated should appear in the main body of the paper given its central role in the author's assessment of changes in productivity.

We thank the reviewer for this suggestion. We have included the AOU equation on line 156-162.

Results

Lines 310-315: I am not understanding how your removal values are not rates (i.e., do not have a time component). Are you simply presenting the difference in concentration between two regions of the estuary as a percentage and calling this removal? Does this take into account changes in DOC and POC in the lower estuary due tidal exchange?

As we followed the method in Amann et al. 2012 we are presenting the difference in percent and concentration. To clarify this we changed the following sentences (text additions are italicized):

Lines	
200-202	To assess the remineralisation of the upper estuary POC and DOC in the Hamburg Harbour and the mid-estuary, *the percentage decrease in organic carbon (OC_D)* ~~the organic carbon removal (OCR), estimated as the percentage decrease in OC~~, was calculated according to the method used by Amann et al. (2012):
207-209	The *POC decrease, compared to zone 1*, ~~OC removal for POC~~ was *only* calculated for zone 2 and 3 due to the influence of the maximum turbidity zone in zone 4 and 5 (Amann et al. 2012).
315-317	*Compared to the upper estuary (z1),* ~~The mean~~ *the late spring and summer* DOC and POC decreased ~~removal~~ *on average by 0.3 ± 21% and 40.6 ± 18%, respectively,* in Hamburg Harbour (*z2-z3*) and mid-estuary (*only* DOC in ~~mid-estuary~~*z4-z5*), f*or the period 1997–2020 (Fig. 3).* ~~estimated as the OC percentage decrease in zones 2 to 5 (DOC in zones 4 and 5) from the initial value in upper estuary (z1), was 0.3 ± 21% and 40.6 ± 18%, respectively (Fig. 3), in late spring and summer from 1997 to 2020.~~
517-520	There was on average 40.6 ± 18% ~~removal~~ *decrease* of z1 POC ~~(Fig. 3)~~, compared to *only* 0.3 ± 21% ~~removal~~ *decrease* of z1 DOC in the estuary *(Fig. 3)*, suggesting that heterotrophic respiration and DIC production was mainly fuelled by remineralisation of POC, which is in agreement with the findings of Amann et al. (2012).

On lines 208- 210 we described that we calculated the difference for zones 2-3 for POC and zones 2-5 for DOC and therefore did not calculate the difference for the lower estuary. This was done because the lower estuary was identified as a region, which is predominately influenced by OC produced in the German Bight (coastal North Sea), as discussed in the '4.2 Controls on inorganic carbon in the lower-outer estuary' section on lines 643-690.

Lines 335-340: I am not following this argument. Are you saying that the increase in DIC was due to greater internal production of CO2? How does the TA:DIC ratio support that view?

Yes. To clarify the point that the TA:DIC ratio can indicate 'the additional DIC input was in the form of pCO2' we changed the following sentence:

Lines	

339-342	In addition to this evidence, the ratio of TA to DIC can serve as an indicator of the source of carbon, *and specifically when < 1 this can reflect DIC input in the form of* CO_2 (Joesoef et al., 2017)*, which was observed in this temperate estuary*.

Lines 405-410: As per my earlier comment, how is the non-/conservative behavior of DIC (i.e., effects of tidal exchange) taken into account when estimating the internal load?

Tidal exchange unfortunately cannot be resolved with the monthly samples we have used in this study. However, based on other studies with high-frequency time series (Cox et al. 2015; Voynova et al. 2015) and studies describing the net transport and loading of organic carbon (van Beusekom et al., 2009) we can deduce that tidal exchange is most likely an important modulator of organic carbon, especially in the lower and outer estuary, where organic carbon produced in the German Bight is an important factor (van Beusekom et al., 1999; Schulz et al., 2023). As discussed and referenced in the '4.2 Controls on inorganic carbon in the lower-outer estuary' section, specifically on lines 662-668 and 673-674.

The non-conservative behaviour of DIC in the estuary (z1-z5) has been accounted for in Figure 4 and in the SOM in Figures S4-S7 (lines 348-352 (results) and 665-668 (discussion)), as well as through the DIC:TA ratio (Fig. 4 and on lines 339-344 in the results).

Also, we would like highlight that in the submitted manuscript, we suggested that a follow-up study should concentrate on the nearshore waters, whereby both flood and ebb tide should be considered, on lines 761-763.

References

Cox, T. J., Maris, T., Soetaert, K., Kromkamp, J. C., Meire, P., & Meysman, F.: Estimating primary production from oxygen time series: A novel approach in the frequency domain. Limnology and Oceanography: Methods, 13(10), 529-552, https://doi.org/10.1016/j.seares.2008.06.005, 2015

Schulz, G., Sanders, T., Voynova, Y. G., Bange, H. W., & Dähnke, K.: Seasonal variability of nitrous oxide concentrations and emissions in a temperate estuary. Biogeosciences, 20(15), 3229-3247, https://doi.org/10.5194/bg-20-3229-2023, 2023.

Van Beusekom, J. E. E., Brockmann, U. H., Hesse, K. J., Hickel, W., Poremba, K., & Tillmann, U.: The importance of sediments in the transformation and turnover of nutrients and organic matter in the Wadden Sea and German Bight. German Journal of Hydrography, 51(2-3), 245-266, DOI: 10.1007/BF02764176, 1999.

Van Beusekom, J. E., Loebl, M., & Martens, P.: Distant riverine nutrient supply and local temperature drive the long-term phytoplankton development in a temperate coastal basin. Journal of Sea Research, 61(1-2), 26-33, https://doi.org/10.1016/j.seares.2008.06.005, 2009.

Voynova, Y. G., Lebaron, K. C., Barnes, R. T., & Ullman, W. J.: In situ response of bay productivity to nutrient loading from a small tributary: The Delaware Bay-Murderkill Estuary tidally-coupled biogeochemical reactor. Estuarine, Coastal and Shelf Science, 160, 33-48, https://doi.org/10.1016/j.ecss.2015.03.027, 2015.