Replies to reviewers

A boost on the final stretch: intense river metabolism and wetland discharge increase aquatic CO_2 dynamics in the Danube Delta

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Review #1

We greatly appreciate the time and effort invested in reviewing our manuscript and we thank all reviewers for their constructive and insightful comments. In the following sections, the reviewers' comments are reproduced in bold, while our responses are provided in plain text.

This manuscript uses high frequency DO data coupled with high frequency CO2 (derived from pH and alkalinity) to evaluate controls on metabolic processes in the Danube Estuary. This work get at two big questions in limnology: metabolic processes at interface zones like estuaries and deltas. It is much harder (for me at least) to conceptually consider metabolism in a delta versus a single-thread river channel and this paper has great potential to address this problem. This paper also examines the difference between metabolism of DO and CO2 and uses these differences to infer processes; I like this approach very much.

We appreciate the reviewer's positive assessment that the manuscript addresses important questions of limnology and has strong potential to contribute to the understanding of processes governing river metabolism.

The paper has two main areas to improve upon.

1. The paper needs a stronger narrative structure. The end of the introduction does not specify clear goals or questions. The results section displays a lot of data, and the discussion section brings in a lot of new methods and analyses, and most of the figures. Indeed, the discussion seemed like an alternative results section. I suggest this paper needs to follow a much tighter narrative where the intro ends with a set of discrete questions or objectives, the results answers those questions with data, and the discussion interprets, with no new analyses. It will mean a shorter, more focused and more readable paper. Any data that do not address this narrative can safely be put in the SI. I understand the difficulty of interpreting these sorts of data: years of data from multiple sites and multiple data streams enables a lot of insights about an ecosystem. But addressing all of those insights makes for a difficult to read paper.

We followed the constructive suggestion to refocus the paper and use a much tighter narrative. Therefore, we implemented the following changes:

- We rephrased the **title** to outline the main narrative: “A boost on the last stretch: intense river metabolism and wetland discharge increase aquatic CO_2 dynamics in the Danube Delta”
- As suggested by reviewer 2, the abstract and introduction now include three general **hypotheses** that also guide the structure of the paper. In the abstract we write (Lines 35-37): This study tests the following hypotheses: (1) Eco-system metabolism intensifies when river water enters the slower flow paths through channels and lakes of a delta. (2) Wetland discharge from a delta significantly alters the oxygen and carbon dynamics in a river. (3) In such a case, average aquatic CO_2 emissions increase on the final stretch before a river reaches the sea. We repeat these in the introduction on lines 69-71.
- The introduction now ends with the three specific research questions regarding the Danube Delta that guide the structure of the result and discussion sections: “(1) How does the intensity of river metabolism differ between the open waters of the delta compared to the upstream Danube River? (2) How significant is the contribution of lateral inflows from lake systems and reedbeds to the O_2 and CO_2 dynamics of the main river reaches crossing the delta? (3) What are the overall effects of river metabolism and wetland discharge on aquatic CO_2 emission rates in the Danube Delta? (Lines 108-112)

We thank the reviewer for the suggestion to shorten and revise the manuscript into a format where only results are presented that relate to the main questions and where discussion will not introduce new analyses. Therefore, we reorganized the manuscript to first present the results on the intensity of river metabolism (Section 3.1, line 254) followed by the analysis of wetland discharge (Section 3.2, Line 350), and ending with a new section on CO2 emissions (Section 3.3. Lin3 403). To keep the focus, we deleted the old sections 3.2 on covariance analysis
The discussion follows the same general structure and puts the results in a broader context:
Section 4.1 Variability of ecosystem metabolism (Line 433)
Section 4.2 Wetland discharge (Line 494)
Section 4.3 CO_2 emissions by the wetland pump (Line 539)
The conclusions now answer the specific research questions (Line 584)

Specific evidence to this point was that the results section cites only two figures: Fig 3 and Fig 4. The discussion cites Fig 5, 6, 7, 8. All of the results in the paper should show up in the results section, and show only the results needed to answer the specific set of the authors' questions; delete all the rest.

To follow a more standard format, we separated results and discussions more clearly. We now present Tables 1-3 and Figures 3-7 in the results section. The discussion part is text-only. The old Tables 2 and 3 as well as Figure 6 in the old manuscript were deleted and the new Figure 4 with box plots of indicators for metabolic intensity was added.

2. A core of the paper examines the relationship between DO and CO2 following the Vachon approach. I like the Vachon approach, but I would not use CO2. Instead I would relate O2 with DIC (at equilibrium). The reason is because buffering causes the slope of the DO:CO2 to increase greatly as the CO2 runs out. This point is

evident in figure 2A showing the slope of the cloud steepening at it approaches equilibrium CO2. The solution is simple: use DIC. Because the authors estimated CO2 based on pH and alkalinity, it will be a simple matter to estimate DIC instead (at CO2 equilibrium, so there is still a zero on the plot) and plot that. Shangguan et al. 2025 detail the reasons why to use DIC and not CO2. In short it takes the effect of buffering out of the shape of the CO2-DO cloud so the parameters can be interpreted biologically vs chemically.

Thank you for explaining the merits of using changes in DIC as an alternative to $exCO_2$ (Shangguan et al., 2025). The DIC method indeed eliminates effects of buffering and allows calculating a DIC budget. In our case with important lateral exchange, DIC comes with a main drawback: The variability in the DIC timeseries is large and changes occur rapidly due to hydrological changes such as flood events. In the Appendix we added a section documenting the DIC time-series (Fig. C1) and examples of more stochastic mean 24-h DIC cycles (Fig. C2 as an analogue of Fig 3). We used DIC balancing to calculate the inorganic carbon input on the river stretches (Fig. C3).
To make best use of the $exCO_2$ and DIC approaches in the revised version, we address the strengths and weaknesses of different indicators for metabolic intensity, and we offer a quantitative comparison of the 24-h amplitudes of exO_2, $exCO_2$, DIC and the stretch factor with box plots in Fig. 4.

Specific comments

63 leaf cover

Corrected

74. Many sensor datasets are years, not months
While long-term sensor datasets spanning multiple years do exist, in our experience the reliability of high-frequency aquatic sensor data strongly depends on regular maintenance and recalibration performed at monthly (or even shorter) intervals. Biofouling, sensor drift, membrane aging, and changes in environmental conditions can progressively affect sensor response, leading to biases that may remain undetected without routine servicing and quality control. Therefore, although the temporal coverage of a dataset may extend over years, its accuracy and interpretability depend on frequent maintenance cycles that effectively segment the dataset into shorter periods of verified data quality. This consideration is particularly important in dynamic shallow-water environments such as those investigated in this study.
We added the following sentence: "Seasonal and multi-annual observations can be achieved with appropriate maintenance and calibration." (Line 86)

84. Make the goals (whether they be questions, hypotheses, objectives) clear in a stand alone paragraph at the end of the intro. That list will then define the parallel structure of the results and discussion. This section as written does not clearly define what the study wanted to know, although line 97 indicated some potential questions.

We thank the reviewer for the specific request. As described above, we rewrote the last section of the introduction and provide the following set of specific questions that serves as a road map to the results and discussion section.
"(1) How does the intensity of river metabolism differ between the open waters of the delta compared to the upstream Danube River? (2) How significant is the contribution of lateral inflows from lake systems and reedbeds to the O_2 and CO_2 dynamics of the main river reaches crossing the delta? (3) What are the overall effects of river metabolism and wetland discharge on aquatic CO_2 emission rates in the Danube Delta?" (Lines 108-112)

104 817,000

We changed the style for large numbers and used it consistently.

161. Very nice to have a continuous predictor of alkalinity.

Thank you, following reviewer 2 we also added R^2 values.

228 µM or µmol L-1?

We use the chemical notation, mM, throughout the manuscript

237 methods in results

We moved the timing details to methods, section 2.1 (Lines 157).

250. Here is an example of providing data for which the question was not clear to me. What is the finding and what was the question?

Thank you for this example. In short, the question here was: Are the channels of the delta acting merely as a conduit for Danube River water or are they behaving as reactors for processing biomass from terrestrial biomes? The question regarding spatial differences in metabolic intensity is now raised explicitly in the introduction, results are summarized in section 3.1 and the discussed in section 4.1.

267. Here is a key sentence. Yes, in an ideal case. But instead buffering in part defines the relative change in CO2 and DO in water. By plotting DIC, then this sentence gets closer to being correct.

Thank you for flagging this sentence. We agree that the statement refers to an ideal case. In order to focus the paper, we decided to discard detailed discussion of slopes of correlation analyses and mean 24-h cycles. The key sentence has been deleted together with the old subsection 3.2 on covariance.

272. Correlation analysis needs to be in the methods.

Agreed. In the spirit of focusing the paper on the main research questions, we deleted the section on correlation analysis and former Table 2 from the results part. In addition methodological digressions on slopes and offsets were deleted from the Appendix to make room for results of the DIC analysis.

275 This slope does not relate well to ecosystem stoichiometry when pCO2 approaches atmospheric equilibrium

Agreed, but only 3 out of the 14 examples shown in the old Figure 4 cases were close to equilibrium. As we focused on the main questions of metabolic intensity, wetland discharge, and CO_2-emissions, we now use the old Figure 4 (new Figure 6) as an illustration for wetland discharge. As mentioned, we restrict the discussion of slopes and focus instead on 24-h amplitudes and stretch as indicators for metabolic intensity and on centroid position and offsets as indicators for wetland discharge.

281 George

We deleted the covariance section.

295. Not sure what is meant by "leaving its marks"

The paragraph has been rewritten and is now part of the section 3.2 on wetland discharge: “The delta stations showed smaller seasonal differences in stretch between June and November but marked shifts in the position within the exO_2-$exCO_2$ diagram. While the Puiu-Rosu data indicated intense photosynthesis with O_2 supersaturation and equilibrium-level CO_2, Busurca represented an extreme case of wetland discharge from the surrounding reed stands with CO_2-rich and O_2-depleted water masses. Balanova located downstream of Puiu-Rosu reflected a mixture of lake- and wetland water. Its monthly fingerprints resembled those of Puiu-Rosu but with a downward shift along the theoretical line of slope -1. “ Lines 358 – 363.

303. State that difference here in the topic sentence of the paragraph

Thank you for the suggestion. We rephrased the topic sentence as follows “In contrast to the rather uniform covariance plots at the four Danube stations, the three delta stations show very different behavior between June and November (Figure 4). Here is a summary of the characteristics and potential drivers for these local differences: “

Fig 4. Are the slope values stated anywhere?

The slopes are documented in the data repository https://doi.org/10.3929/ethz-c-000786936 , but following the suggestion to refocus the paper, we restrict the discussion of slopes to a brief statement in the methods section: “The time-of-day averages reduced the influence of outliers and resulted in slopes s_{24} that may reflect EQs more closely than the tilt of the monthly covariance ellipse (s_{cov}) (Fig. 2b). The following analysis, however, focuses on amplitudes and stretch as indicators of metabolic intensity and on the offset and centroid positions as indicators of wetland discharge.” (Lines 232 -235).

329. 7 y ago does not seem recent

Correct, we deleted the sentence.

340. New findings in the discussion. In fact most of this paragraph is describing a new, set of results and should be in the results section (if indeed it is one of the core questions in the paper)

We agreed and moved this section together with old Figure 5 (new Figure 7) to the results section 3.2 on Wetland discharge. See Lines 388 and following.

371. Effects of flooding is new results in discussion.

We deleted old Table 3 and old Figure 6 to keep the narrative focused as the reviewer suggested.

420. Methods in discussion, followed by new results

We edited this section and moved it together with old Figure 7 to Line 304 of the section on "Intensity of river metabolism". Old Figure 7 now appears as Figure 5

495. I would cite Aho et al. LO Letters (2021)

Agreed, we added this reference on Line 98.

537 What is "complementary perspective for a system"?

In the theory of knowledge *complementarity* means that one way of looking at a system may not be enough. But our phrasing seems to be too philosophical and we deleted the sentence.

653. The O2 and CO2 data were very easy to find and look over, thanks to the authors for that. Is there a plan to release the sensor data to enable replicating the results of this study, i.e. temperature, conductivity , pH ?

Yes, we are updating the data repository to include the DIC data and the original sensor data. The link will be ready for the final version of the manuscript.

Review #2

Please note that this review has been provided directly by the Associate Editor to expedite the review process delayed by an overdue reviewer.

We thank the reviewer for this constructive review which helped expediting the review process. In the following sections we copied the reviewer's comments in bold and added our answers in plain text.

General comments
The manuscript presents high-resolution, paired O2-CO2 measurements in river reaches and channels of the Danube Delta spanning three years. Given the scarcity of O2-CO2 measurements in estuaries and deltas of large rivers, these invaluable monitoring data offer rare insight into river metabolism and CO2 dynamics in the large river delta, as highlighted by a two-order-of-magnitude difference in diel CO2 amplitudes. While the findings from this well-designed study are novel and invite further study, the current version shows some weakness in methodology and structuring that must be addressed.

Thank you for the positive remarks regarding value of the data, the study design and the novelty of this research.

1. Key hypotheses and messages
As another reviewer noted, explicitly articulating the key hypotheses and weaving a consistent narrative throughout the manuscript would significantly strengthen its impact. Please articulate the hypothesis hidden in the Discussion (L 374-376) in the Abstract and Introduction. Furthermore, as detailed in the minor comments below, additional site-specific information (Methods) and expanded discussion are required to explain why certain Danube branches are more sensitive to lateral flows from adjacent wetlands and lakes.
Drawing from my own experience with CO2 measurements in the lower Ganges (Haque et al.), I highly value this rare dataset. The limited existing literature tends to suggest relatively low-amplitude CO2 fluctuations in the lower reaches of large rivers. However, your study demonstrates a pronounced influence from deltaic wetlands. This novel finding merits a clearer articulation of its significance, specifically regarding your underlying hypotheses and the implications for carbon fluxes at regional to global scales.

Thank you for the constructive remarks and suggestions on how to improve abstract and introduction by inserting the key hypotheses and research questions of the study. We realize that we did not adequately justify the choice of CO2 over DIC in our analysis and we missed the opportunity to estimate average CO2 emission rates and their regional differences. In response to both reviews, we therefore outline the following main hypotheses in the abstract and repeat them in modified form in the introduction:

> "This study addresses the following general hypotheses: (1) Ecosystem metabolism intensifies when river water enters the slower flow paths through channels and lakes of a delta. (2) Wetland discharge from a delta significantly alters the oxygen and carbon dynamics in a river. (3) In such a case, average

aquatic CO_2 emissions increase on the final stretch before a river reaches the sea..” Lines 34 – 37 in the Abstract.

Following the suggestions of Reviewer 1 we also rewrote the last section of the introduction to outline the main research questions that serve as a road map for the results and discussion sections:

“… allowed us to address the following specific research questions: 1) How does the intensity of river metabolism differ between the open waters of the delta compared to the upstream Danube River? (2) How significant is the contribution of lateral inflows from lake systems and reedbeds to the O_2 and CO_2 dynamics of the main river reaches crossing the delta? (3) What are the overall effects of river metabolism and wetland discharge on aquatic CO_2 emission rates in the Danube Delta? Lines 108-112.

We added more site-specific information to the methods part to facilitate the discussion of the contrasting results at the different monitoring stations. As mentioned in our reply to Reviewer 1, we now present the results in three sections on 1) the intensity of river metabolism, 2) wetland discharge and 3) CO2 emissions. The discussion follows the same structure and the new section on CO2 emissions allows us to address the implications of intense deltaic processes for regional carbon fluxes.

2. DIC calculation
Both the initial and community reviewers indicated the limitations of CO2 data in high-alkalinity systems. While I recognize that adding new measurements to this already comprehensive manuscript would be challenging, calculating DIC to complement the CO2 data would directly address these concerns.

As outlined in our response to Reviewer 1, we collected DIC results in the Appendix C in the form of the complete DIC time series (Fig. C1), an example of 24-h cycles (Fig C2), and the cumulative increase of DIC along the Danube reaches (Fig. C3). For a direct comparison included the DIC 24-h amplitudes in the box plots of Fig 4 in the main text. To keep the paper focused, we omitted the old Tables 2 and 3 and old Figure 6 and we deleted the old sections 3.2 (covariance), 4.2 (flooding and flood recession) and the old Appendix section C on offset and slope. These changes allowed us to address the merits and limitations of exCO2 and DIC without expanding the manuscript.

3. Alkalinity estimation
Given the pivotal role of alkalinity in estimating CO2, the empirical relationship used here has not been adequately justified. Please provide a citation or theoretical basis for this approach (refer also to a recent Biogeosciences paper that employed a similar approach: https://bg.copernicus.org/articles/22/4923/2025/).

Thank you for the suggestion to cite the Ngueyen et al. study. We added the following sentence. “The same approach has been applied derive multidecadal trends for CO2 emissions and river metabolism in the Loire River (Nguyen et al. 2025) and to estimate global CO2 emission rates from river systems {Raymond, et al. 2013).” Lines 193 -194

Please articulate statistical significance in the following:
L 93 “a correlation of conductivity with alkalinity” and Appendix B:
L 161-163: Please provide an R2 value.

Agreed, we omitted methodological details the introduction, but documented R^2 values for the conductivity-alkalinity correlation ($R^2 = 0.7$) and for the correlation between measured and calculated CO_2 ($R^2 = 0.82$) and in the Methods section and in Appendix B, Lines 191 and 199 and 654-663.

Specific comments:

- **Lines (L) 43-44 "strong contributions of methane oxidation and/or nitrification": The O2-CO2 plot can only indicate potential sinks of O2. Please reformulate the sentence.**
- We agree, and we deleted the sentence because the abstract is now focused on metabolic intensity, wetland discharge and CO2 emissions.
- **L 39-40: Don't you need to mention (and later discuss the limitation of this approach) that the two types of water were monitored separately in two different phases?**
- We agree and now specify the deployment times as follows: "We tested these hypotheses based on measurements of the oxygen and carbon dynamics at 15-minute time resolution obtained from sensor packages. They were deployed for two years in the three main river reaches of the Danube Delta in Romania and for an additional year in three channels within the delta." Lines 38 and following.
- **L 46-48: This seems too general as a concluding remark. Please articulate the key implications in relation to the underlying hypotheses mentioned above.**
- We agree and in the revised version we deleted the more speculative discussion section 4.4 on pathways of carbon turnover. Therefore we deleted the discussion of calcite precipitation and uptake of HCO_3^-.
- **L 88 "wetland water": Do you mean "wetland discharge (drainage)"?**
- Yes, thank you for this suggestion. It helped us to refocus the paper, including the title and the structure of the results and discussion.
- **L 90: "commercially available multiprobe sensors (YSI EXO2?)"**
- Thank you for the suggestion, which explains the abbreviation. The sentence now reads: "…from a two-year deployment of commercially available multiprobe sensors (YSI EXO2) recording every 15 minutes… Line 105.
- **L 104 and throughout the manuscript: Please correct the apostrophe used in numbers (817'000).**
- Apologies for the wrong style. We checked and corrected the apostrophe use in the whole manuscript.
- **L 122-125: Given the importance of wetlands as a primary source of CO2 in the delta, it would be helpful if you provide more detailed information on the area and basic ecosystem characteristics (like wetland types and dominant vegetation).**
- Agreed, we added a short description of the wetland vegetation and its distribution as follows:
- Reed (*Phargmites australis*) is the dominant species in the Delta covering ~ 1,600 km^2 or almost 40% of the study area (Oosterberg et al., 2000). Large stretches of the channels are bordered by riparian forests composed mainly of black alder (*Alnus glutinosa*) and silkvine (*Periploca graeca*) (Oprea et al., 2024). Most of the lakes are eutrophic and classified into three types: (1) lakes at short distance to the river with high flushing rates and dense submerged vegetation, (2) large,

relatively deep lakes with longer residence time and high *Potamogeton*-cover that collapses during winter, and (3) small, isolated lakes receiving "black-water" inflows from floating reedbeds and dominated by *Ceratophyllum demersum* and *Nitellopsis obtusa* (Coops et al., 2008). A high diversity of submerged and floating macrophytes is present in the canals and old meanders (Oosterberg et al., 2000). Line 143 and following.

- **L 124 "lead": led**
- Corrected. See Line 139.
- **L 178 "data-cleanup": Do you mean "data processing" or "data filtering"?**
- We believe that "data filtering" is a more precise term for removing conductivity spikes, The sentence now reads: "Some data-filtering was necessary for the time series of Chilia (2016)... " Line 207.
- **L 223: "supersaturation with respect to atmospheric CO2 equilibrium"**
- Agreed, your suggested phrasing is more precise. Corrected see Line 254.
- **L 224-225 (Fig. 3): Showing examples from a mainstem site (e.g., Tulcea), together with the displayed delta site, would help readers compare the general patterns of the two monitoring groups.**
- We liked this idea and plotted the new Figure 3 with two panels (Tulcea and Balanova). For comparison we added the same plots with DIC to Appendix Fig C2.
- **L 230-232: Isn't it noteworthy that fluctuations at Chilia were relatively small compared to those of the other near-shore Danube sites? Later discussion (of limited wetland influences ?) would be help readers figure out the observed inter-branch differences.**
- Correct, we now discuss the effects of lateral connectivity in more detail in on Lines 470 and following and with the DIC balance of lateral inflows (Fig C3) on Lines 515 and following.
- **L 272: Just to double check: Pearson's rho or r?**
- Table 1 now lists both rho and R^2. Line 345
- **L 310: Indicating distance from the adjacent lakes would be helpful.**

Unfortunately, for this complex aquatic system, proximity is not a direct indicator for hydrological connectivity in the Danube Delta, but we agree that the role of the hydrological regime needs to be better explained. We introduce the delta stations as follows "Puiu-Rosu is influenced by outflow of a large and relatively deep lake complex, Balanova receives mixed input from lakes wetlands while Busurca is mainly connected to reedbeds with negligible lake influence." (Lines 161 – 163)

- **L 321: What about differences between the three near-shore sites?**
- Good point. We discuss the hydrological differences of the main sections as follows: "The significant differences between the three river reaches, can likely be attributed to their morphology and hydrological connectivity. Patterns exO_2-$exCO_2$ pairs within covariance ellipses retained close similarity between Tulcea and the downstream stations of Sulina and Chilia, whereas they appeared "randomized" at St. George (Fig. 6). The Sulina channel has been strongly modified for navigation with straight geometry and dykes that reduce lateral exchange, while the Chilia branch retains a more natural meandering geometry. The St. George reach consist of natural meanders and straight cuts for

navigation, resulting in a broader distribution of water travel times and more intense exchange with adjacent wetlands and aquifers. This close interaction with wetlands at St. George likely superimposes multiple water masses with different metabolic histories, thereby disrupting the coherent diel covariance patterns that were preserved at Sulina and Chilia. Consequently, the 24-h amplitudes of exO_2, $exCO_2$ and DIC all follow the sequence St. George > Chilia > Sulina consistent with decreasing lateral connectivity (Fig. 4). " Lines 463-473

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- **336-339: Please elaborate more as to how these branches drain different source areas in terms of riverine CO2?**
- We use the DIC balance (Fig C3) to constrain lateral inputs as follows "A quantitative assessment of the different lateral interactions affecting the three Danube River reaches was based on monitoring the average monthly difference in DIC concentrations between the downstream stations and the inflow at Tulcea (Fig. C3). Integrating these differences from April to December 2016 reveals an annual DIC addition of 0.5 and 1.2 mol m^{-3} yr^{-1} at Sulina and St. George, respectively. The observations in 2017 provided a similar result for St. George, but a higher accumulation rate for the Sulina stations. The seasonal balance for the Chilia branch, however, resulted in a negative value (-0.1 mol m^{-3} yr^{-1}). These contrasting observations can be explained by differences in hydrological connectivity and the origin of lateral inflows. Sulina and St. George are receiving carbon-rich wetland water. The important northern inflow to Chilia, however, originates from Lake Yuluph, the largest freshwater lake in Ukraine. Water from this system is transported via Lake Kuhurlui and connecting channels to the Chilia branch and appears to deliver DIC-depleted water. (Lines 515 and following).
- **L 365: How is the trophic status of the lake? And are phytoplankton blooms rare or frequent?**
- We added a short characterization of the delta lakes as follows: "Most of the lakes are eutrophic and classified into three types: (1) lakes at short distance to the river with high flushing rates and dense submerged vegetation, (2) large, relatively deep lakes like Puiu and Rosu with longer residence time and high *Potamogeton*-cover that collapses during winter, and (3) small, isolated lakes receiving "black-water" inflows from floating reedbeds and dominated by *Ceratophyllum demersum* and *Nitellopsis obtusa* (Coops et al., 2008). A high diversity of submerged and floating macrophytes is present in the canals and old meanders (Oosterberg et al., 2000). (Lines 145 – 150)
- **L 451: Was the relationship with water temperature significant for all sites?**
- Yes we added the levels of significance at lines 335 and 338.
- **L 480: Specifying methane and DO levels would be helpful.**
- We now discuss the methane levels measured in a previous survey (Canning et al, 221) in more detail starting on Line 527.
- **L 522: I wondered what implications of the "lateral inflow of O2-depleted and CO2 oversaturated water from adjacent wetlands" you would like to highlight as a key take-home message. For example, any implication in terms of regional- or global-scale carbon fluxes?**
- Thank you for this excellent suggestion which motivated us to add the specific sections on CO_2 emissions to the results and discussion part. Table 3 now

compares the aquatic emissions with estimates of potential CO_2 drawdown by the wetland vegetation. We conclude: "Overall, the downstream stations near the Black Sea exhibited $exCO_2$ values which were 1.7 to 2.1 times higher than at Tulcea. The estimated annual diffuse CO_2 emissions from water surface in the delta of 49 Gg C yr^{-1} were compatible with an earlier estimate based on more stations but less frequent sampling of 60 Gg C yr^{-1}. Estimates of the landscape-level CO_2 sink by the highly productive wetlands, however, are ~ 5 times higher. " Lines 603-607.

- **Tables & figures: Please pay attention to the initial "upper-case" letters of the first words, like "P"arameter and "T"ime of day**
- Thank you for this observation, we changed the labels in figures and tables to initial upper case.

May 19, 2026