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Biogeosciences
Discussions

Interactive comment on "Air–water fluxes and sources of carbon dioxide in the Delaware Estuary: spatial and seasonal variability" *by* A. Joesoef et al.

Anonymous Referee #2

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As the title suggests, this manuscript describes some of the CO2 fluxes and sources for the Delaware Estuary. The message of the paper is clear and consistent throughout: despite high pCO2 supersaturations in the Delaware River relative to the atmosphere, the much larger area and more moderate pCO2 of Delaware Bay lowers the overall annual CO2 flux of the complete estuarine system. The authors systematically explore the biogeochemical influences that lead to these trends by calculating the impacts of temperature, biological activity, and mixing over the six sub-regions, and highlighting the contribution of river-derived CO2 to fluxes in important areas.

This manuscript represents an important contribution to the study of air-water CO2 fluxes on the east coast by providing the first such estimates for the large Delaware

River estuary. As the authors note in their introduction, estuarine fluxes of CO2 can be as important as those for the coastal oceans although many types of estuaries remain understudied. In this vein, the authors point out a paucity of studies particularly for the mid-Atlantic region of the US West Coast. While it is not directly stated, the authors' introductory review also implies a lack of observations of larger estuarine systems. As an expansive, mid-Atlantic estuary, this research in helps to address both of these gaps. The authors also point out in the conclusions of this manuscript that the Delaware Estuary an important industrialized site that may be impacted by future development and activity, and that foundational research is highly important to form a baseline of observations against which future change could be compared.

Though in general well-written, the manuscript could be improved in two areas. Firstly, as a reader I would have appreciated seeing some of the background of the manuscript better consolidated and more clearly state at the start. For example, we do not learn until the concluding sections that this is the first CO2 flux study for the Delaware River estuary. The individual review sections of this manuscript also seem somewhat scattered. By the conclusion of the manuscript, I felt that the broader ideas came through, but the flow of information could be regrouped for better clarity.

The second main weakness of this manuscript is the description of the methods used. The complexity of CO2 calculations and spatiotemporal interpolation can often be a descriptive challenge, but is critical. These methods of calculation often form the backbone of the paper, as the interpretation of the results has been well-established by the wider community. While grounded in recent peer-reviewed research both for estuaries and broader carbon cycling, the methods section here does feel somewhat scattered. A careful reading left me wanting a clearer description of the assumptions, caveats, and choices the authors made in tailoring their calculations.

Overall, I would recommend this manuscript for publication after minor revisions to the manuscript's flow and a major revision to the methods section that can address the concerns I outline below. Additional minor comments are given at the conclusion of this document.

MAJOR COMMENTS

In calculating temperature-normalized pCO2, it is necessary to define an annual mean temperature against which seasonal fluctuations can be measured. Here, the authors use the mean water temperature measured in the Delaware Estuary from 2013-2015, which covers the span of their sampling. Others have adopted a more climatological approach that uses a long-term annual mean temperature, as may be provided by the 10-year average temperature cycle the authors show in Figure 3. Especially since there important seasonal gaps in the authors' analysis (e.g., January and FebruaryâĂŤthe two lowest temperature months), it is my instinct to suggest using a more resolved temperature product for this calculation, such as a USGS record (e.g., that given in Figure 3a if it can be applied to the entire estuary) or a satellite-derived mean. If the authors used a similar temporal interpolation to get an annual temperature despite months without data (as in the calculation of annual CO2 fluxes including months without data, which could also be better shown and described itself), this should be more directly stated. A broad recalculation of this result constitutes a major revision.

Throughout the description of their techniques, the authors take care to point out weaknesses. However, they provide no systematic error analysis. A consolidated summary of methods, assumptions, caveats and gaps at least is integral to the readers understanding, and was missed. For example, I was left wondering how the salinity-binned temperature-normalized pCO2 related to the area-averaged CO2 flux. This seemed out of place next to the other area-based considerations (e.g., Tables 1 and 2, Figures 8 and 9), and the implications of this choice were not well explained either for the method itself or for the interpretation of the data. For additional examples, see the annual mean temperature choice highlighted above, and the absence of discussion about any weakness in the river CO2 contribution calculations, as well as additional points listed in the minor comments below. I consider additional discussion of assumptions and caveats a major revision, whether it involves additional text or additional calculations.

To resolve these comments, I highly recommend that the authors include a conceptual sketch to summarize their calculations, and consolidate the information about their assumptions and caveats in a clear, concise additional section. An excellent reference would be the recent manuscript published by W. Evans and colleagues, which required a similar methodological description and error calculation to underpin their observations of coastal CO2 fluxes. See http://onlinelibrary.wiley.com/wol1/doi/10.1002/2015GB005153/abstract.

MINOR COMMENTS

Page 10900 Line 24: Nice note about industrialization and future change at the end of this manuscript that could be included up front.

Page 10902 Line 5/6: Great statement. Lead this section with that, and then clearly state the gaps that contribute to this concern (spatially, not a lot done in Mid-Atlantic; theoretically, not a lot done in large, fast-moving estuaries), and how the Delaware Estuary could help address these issues. That would lead well into the review of the Delaware R. Estuary in the next section.

Page 10902 Line 9/10: Later you point out that your manuscript represents the first carbon work, so it is important here to state that too.

Page 10902 Line 14: Why does industrialization matter? It is better stated at the conclusion of the paper, but should be explained here too.

Page 10903 Line 3: Sampling bias implied by lack of Jan/Feb data. This should be discussed in reorganized methods section

Page 10904 Line 3/4: Sampling method reference for filtration?

Page 10904 Line 5: There is some internal discussion among the carbon community about use of mercuric chloride as a preservative in low-salinity samples. The challenge is that the mercury salt impacts alkalinity concentrations at salinities less than 10. The excess alkalinity from the HgCl2 may have lowered calculated CO2 concentrations, and as a result these flux estimates are likely conservative. Important to point this out in new assumptions/caveats section.

Page 10904 Line 26: Starting out this section strong by trying stating these challenges directly, but need a clearer description or summary of how they were addressed.

Page 10906 Line 14: Why split the upper and mid-bay regions? Are these salinity-binned designations, or geographically…?

Page 10907 Line 24: stating the temperature constant here is redundant

Page 10907 Line 25-27: Address with other caveats in new section

Page 10908 Line 13: Not sure what "stationary" signifies here. The salinity bin rationale needs more detail, and more description of how salinity-bins and geographic bins relate to each other in the data interpretation.

Page 10908 Line 25/26: Why use this for the annual mean temperature?

Page 10909 Line 5: Change "normalized temperature" to "normalized the pCO2 at in-situ temperature"

Page 10909 Line 20-22: Clever way of calculating this, but is it the ideal way? What are the challenges here? Good thing to discuss in caveats section.

Page 10910 Line 15: Very interesting comment in the caption for Figure 2 concerning panel C, but panel C is not discussed here. Another important point for the assumptions and caveats section, especially since CO2SYS was again used for the calculation of riverine contribution to fluxes (Section 2.4; Page 10909, line 20-22).

Page 10910 Line 21-25: Why different time ranges for discharge and temperature? Directly mention differences in the data record.

Page 10911 Line 1: Change "The Delaware River discharged…" to "The Delaware River discharge…"

Page 10912 Line 10: Difficult to see any DIC drawdown relative to salinity in Figure 2a that would also indicate a bloom… unusual for such a strong bloom. If entire water column/estuary was affected by the bloom, how does this affect your calculation of the DIC endmember? Since much of the seasonal cycle you describe relies on this point, the biological production should be better shown or explained.

Page 10913 Section 3.4: Temporal questions here could be better addressed, such as definition of seasons as well as the temporal averaging for the annual estimates.

Page 10914 Line 6: Why is the salinity interval more important than the geographical interval? I'm still confused by the salinity binning as decribed in Section 2.3 and the results here are not helping clarify why this was important or what it showed.

Page 10914 Line 13/14: How does this bloom timing relate to other observations of the seasonal biological cycles in the Delaware Estuary?

Page 10915 Appendix B could be expanded and would be a worthy inclusion in the main paper.

Page 10917 Section 4.3: Figure 9 is a good summary figure.

Page 10919 Line 6: Great statement! Belongs at the top of the paper.

Page 10919 Line 6-16: This section needs to be broadly expanded to turn it into an appropriate assumptions and caveats section (consider it section 4.4). Some of the points made here could then receive more attention, such as some review of how nearby marshes might influence pCO2.

Figures and Tables:

Table 1. Area-weighted average flux in an additional column.

Figures 1-9. In general, sizing here could be made a little more uniform. In some cases text sizes were extremely difficult to read (see especially the legends in Figures 7 and 9). Increasing the size of Figure 2 might also be able to highlight any potential DIC drawdown influencing biological production.

Figure 2. Panel c not discussed in manuscript.

Figure 4, 5. ODV stamp required on figures generated using that program, as well as a Schlitzer citation.

Figure 7. Not sure this is the best way to show differences between temperature and biological forcing, salinity binning aside. Firstly, the variables need better labels (use T and B as in Appendix B) and the pCO_2(obs) line should be the boldest. I would also consider plotting T and B as anomalies from pCO_2(obs), or showing a vector diagram, or using color to highlight times when warming, cooling, production, and respiration are clearly factors so that the interpretation of this figure immediately jumps out.

Interactive comment on Biogeosciences Discuss., 12, 10899, 2015.