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Interactive comment on "Seasonal variability of the inorganic carbon system in a large coastal plain estuary" *by* Andrew Joesoef et al.

Anonymous Referee #3

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Joesoef et al. present new data on inorganic carbon seasonal dynamics and land-ocean connectivity from a large North American East Coast estuary that had not previously benefited from significant study of carbonate chemistry (other than the authors' prior publication in this journal a few years ago). Focusing on biogeochemistry, this manuscript is clearly within the scope of BG and will make a nice contribution to the literature after significant revisions to clarify and highlight major concepts and conclusions in the paper.

One newer idea presented in the paper is that the authors draw a distinction between the carbon cycle behavior of larger estuaries compared to the smaller ones that have received more attention in the literature, highlighting the role of different types of habitats (e.g. intertidal wetlands vs. open estuarine water column) in driving the overall

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carbon balance of the estuary system. This may benefit from a conceptual diagram if the authors want to argue this is a generalizable phenomenon they are describing – to show the relative influence of different biogeochemical processes and pathways.

While the title is clear and appropriate and the paper is generally well-structured, there are quite a few places where the language is not as clear as it could be. In particular, the abstract could use a fairly substantial rewrite in that the authors' wording is often vague. For example, they say "Our data further suggest that DIC in the Schuylkill River can be substantially different from DIC in the Delaware River, and thus in any river system, tributary contributions must be considered when addressing DIC inputs to the estuary" – the second half of this sentence is so broad as to be essentially meaningless. I'm not sure they ever made a really compelling argument for why this might be important. I am sure there are several reasons why it could be important, but the authors should articulate their reasons for believing this to be important.

The language is fluent and mostly clear, save in a few places where the language becomes vague/imprecise. It may seem redundant to the writer, but there are numerous places where a few more words added would make the difference between vagueness and clarity.

Some examples of these are:

P1, L7: "widely understudied" seems like a bit of a non sequitur/oxymoron.

P1, L26: You might want to say "flux to the coastal ocean" instead of just "flux to the ocean" as in coastal carbon cycle circles, we also discuss export from coastal oceans to the open ocean.

P2, L2: "majority of the DIC produced" – in the estuary, I presume? Clarify.

P2, L10: land-to-ocean continuum?

P2, L 25: "The supply of inorganic carbon by rivers. . ." – to the coastal ocean?

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One part of the interpretation of the data that was never really explained to my satisfaction was why DIC and TA wouldn't both be diluted under higher discharge and thus why the DIC:TA ratio would change with discharge. I suspect that there's a role of temperature in biotic production of CO2 in soils that has a different slope than the temperature dependence of weathering, or something along these lines. The authors' could do a more complete job of illuminating readers on the various factors contributing to the seasonal changes of DIC vs. TA to create a fuller picture and narrative about why they observe a changing DIC:TA ratio through the seasons. The importance of temperature in driving these changes is critical if this work is to have any bearing on predictive studies under future climate change. The discussion of the lithology in the watersheds of each of the study rivers was a bit more detailed than needed, so some of this could be placed into supplemental material, or the text could just be shortened, with the same references. I don't think the detail adds anything to the understanding that one part of the watershed and its tributary contain more carbonate rocks than other parts. Again, why does this matter? (I'm not saying it doesn't, but tell us why you find it important.)

Further comments:

P2, L 23-24: I thought it was just for carbonate minerals that the CO2 is eventually released back to the atmosphere via oceanic carbonate sedimentation, stoichiometrically speaking. Please verify that this statement is correct.

In several places, the authors use the word "impact" when "affect" would be more appropriate. "Impact" is often used to convey negative connotations.

P3, L3: "by weathering and decomposition"

P3, L12: "more large bay systems" would be clearer

P4, L 18: replace "ongoing" with "underway"

P4, L27-28: does this method of filtering samples affect the DIC values? I presume the

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references given address this, but if not, it would be good for these authors to address whether filtering samples introduces any artifacts or bias. Filtering DIC samples is not typical (e.g., per the Dickson et al. 2007 SOPs for the CO2 system), but can be done without introducing bias with adequate care (e.g. Bockmon and Dickson 2015? L&O).

P5, L4: Are you sure it's precision that is +/- 2 umol/kg? Vs. some overall uncertainty or average offset from CRMs?

P5, L 5-7: Need to state pH scale is NBS.

P6, L 10: "northernmost"

P6, L21-22: might be good to clarify that this is from the rivers where measurements were taken (vs. the scaled up estimate presented later on).

P6, L27-28: re-cite figure here? Here and just below, it seems like there are a few steps left out of your description of how you did the calculations.

P7, L5: Not enough info given about what this data set is and how the data compare to yours. Put in methods or otherwise describe.

P7, L7: This seasonality doesn't agree with what you described above (summer+fall vs. spring+summer, etc.).

P7, L16: "strong" correlations, not "high" (or "highly correlated")

P7, L20: "dilution of weathering products" (vs. production)

P8, L3: could be faster transport or lower surface area to volume ratio (i.e. deeper)

P9, L1: I am not sure what you mean by "physiographic"

P9, L7: not totally clear what "the historical record" refers to - all USGS data? Just a subset?

P9, L8: closer to 3 decades, at 26 years – maybe "over recent decades" is better?

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P9, L9-11: to facilitate reading the paper, it may be best to stick to river names rather than mixing in city names for those readers outside your region

P9, L 16-18: After too much detail on watershed lithology at the start of this section, a bunch of things are summarily mentioned without discussing how these processes might contribute to TA change sufficiently (e.g. would these processes individually increase or decrease TA, and how?).

P9, 26-31: See previous – superficial treatment of these factors (also "can also have huge effects" on following page). Be more specific about the relative roles each of these factors would play if they are important.

P10, L18-20: this is very qualitative. Can you be more quantitative about this?

P11, L12-17: This sentence seems circular to me – how are you defining the difference between input and inflow? If you mean to consider groundwater inputs too, you need to be more concrete and specific with your wording. (Also, there was the roughly 10% from wastewater treatment plants [WWTPs] from up top not mentioned here. Intentional?)

P11, L26: Do you mean water column/internal estuarine CO2 production (per top of next page, I think you do)? Calling it "production" without further clarification of what is being produced gets confusing when primary/community production may also be involved.

P12, L5: near the top, you had a figure for 30 mˆ3/s from WWTPs – this seemed like not a trivial part of the total input

P12, L27: "intertidal" instead of "internal," yes?

P13, L9: "Here" – where are you referring to?

P14, L1: "compared to total DIC input flux" – suggest adding "from rivers"

P26: Seems to ignore interannual variability to list cruises by month w/o noting they

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occurred in different years. Do you have enough data on interannual variability to justify that this makes more sense than an alternative? (I don't feel strongly that this shouldn't be done but am curious about the choice to do it this way – would be nice to have some explanation – but doesn't need to be extensive.)

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