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Interactive comment on "Relative impacts of global changes and regional watershed changes on the inorganic carbon balance of the Chesapeake Bay" *by* Pierre St-Laurent et al.

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We thank the referee for their careful reading of the manuscript and for providing helpful and thoughtful comments. Referees' comments are italicized while the Authors' responses are not italicized.

General comments: The authors use a linked land-estuarine-ocean model to explore the inorganic carbon balance in Chesapeake Bay. Several sensitivity scenarios are conducted to determine the relative impacts of global changes and regional watershed changes on the inorganic carbon budget. These scenarios include a control experiment

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with realistic forcing of a period of 15 years from 2000 to 2014, an air pCO2 change experiment, a temperature change experiment, a riverine nutrient change experiment, a carbon and alkalinity change experiment and a combined change experiment to represent the period of 1900-1914.

The carbonate system was validated by comparing model outputs against a variety of field observations along the main channel. The model displayed strong spatiotemporal patterns of DIC, Alkalinity, pCO2. This study successfully quantified the contributions of variable anthropogenic stressors on the inorganic carbon balance. The global pCO2 increase has enhanced bay-wide in-gassing, which, however, is mitigated by the temperature increase. Regional nutrient loading increase can enhance the in-gassing by increasing the NEP. Differently, the riverine carbon and alkalinity increase would reduce the in-gassing process. The manuscript is very well written, clear, and should be published with some minor revisions.

Specific comments: Line 118-119. Due to limited observations of DIC and TA, the author use the salinity derived DIC and TA as the forcing at the ocean side. It would be helpful to mention the pH range calculated with these salinity derived DIC/TA, making sure the pH is in a reasonable range.

We agree with the reviewer that this is a valuable sanity check. We propose to include a new sentence in the manuscript that would include this information:

"...are combined with the seasonal climatology used for salinity to prescribe TA and DIC at the model open boundary. The pH at the oceanic model boundary calculated from these TA and DIC values varies seasonally and spatially within the range $7.75 < \mathrm{pH} < 8.05$ with an average value $\mathrm{pH} = 7.89$ (total scale). This range is consistent with the measurements in Wang et al. 2013 (their Figure 8b, transect "MA", $\mathrm{pH} \approx 7.9 \pm 0.1$ where $\pm$ represents one standard deviation). Note that the same

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oceanic conditions. . . ”

Reference cited: Wang, Z.A., R. Wanninkhof, W.J. Cai, R.H. Byrne, X. Hu, T.-H. Peng, W.J. Huang, 2013, The marine inorganic carbon system along the Gulf of Mexico and Atlantic coasts of the United States: Insights from a transregional coastal carbon study, Limnol. Oceanogr., 58(1), 325-342, https://doi.org/10.4319/lo.2013.58.1.0325

Line 124. The 50 anthropogenic DIC might represent a small change to surface/bottom DIC, however, this DIC change could affect the surface water pCO2 a lot and have a much larger impact on the air-sea gas exchange.

We agree with the referee that the original sentence wasn't properly acknowledging the potential impact of anthropogenic DIC on the continental shelf. The referee's comment convinced us that we shouldn't speculate on this matter, and that we should simply state in the text that this component should be considered in future studies. We thus propose to replace the original passage by:

“. . . the same oceanic conditions are used in the 1900-1914 and 2000-2014 experiments since we are primarily interested in historical changes that occurred inside the Bay. The potential impact of the historical change in DIC on the continental shelf (i.e., the anthropogenic DIC) is thus not represented here, but it should be considered in future studies.”

Line 146. Why not use the calculated DIC (from pH and the TA you prepared), which could be more accurate to represent the riverine forcing?

We appreciate the referee's question. The calculated DIC (shown in Figure 3b of the manuscript) exhibits variability on multiple timescales that most likely reflects different processes affecting DIC in rivers. In the context of the present manuscript, we are specifically focusing on the *long-term change* in DIC (which we parameterize as a linear increasing trend). The remaining interannual variations are considered beyond the scope of the present study. We propose to include an additional sentence in the manuscript to clarify this point:

"...TA and DIC followed similar trajectories over these decades (Figure 3b); therefore we also assume linearly increasing DIC in the 2000-2014 experiment (10 mmol-C/m3/yr). The remaining year-to-year variability apparent in Figure 3b is considered beyond the scope of the study and not represented in the model experiments. Finally, a seasonal cycle in TA and DIC..."

Section 3.1.1 Please provide some quantitative measures (e.g. RMSE, relative error) either in Figure 5 or in the texts. It's hard to see the performance of the model in carbonate system.

(N.B. In our response, we assume the referee is referring to Section 3.1.2 ("Evaluation of the modeled inorganic carbon system"), and not Section 3.1.1.)

We followed the referee's advice and computed quantitative measures of the model skill for the carbonate system. We plan to integrate these values in the existing text of Section 3.1.2 during the revision process.

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