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Interactive comment on "High-frequency variability of CO_2 in Grand Passage, Bay of Fundy, Nova Scotia" *by* Rachel M. Horwitz et al.

Anonymous Referee #2

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Review of Horwitz et al., High-frequency variability of CO2 in Grand Passage, Bay of Fundy, Nova Scotia.

This manuscript presents an interesting analysis of the complex physical and biogeochemical interactions that control the marine CO2 system in a region with extreme tidal currents. It deserves publication even if the relevance for global change issues such as ocean acidification is low, but this should not be the criterion for the scientific quality of a paper. Still, I suggest some revisions which can improve the readability, intelligibility and scientific correctness of the manuscript.

Overall structure: I suggest to include a short paragraph that characterizes the hydrography of the Bay of Fundy. Readers outside of North America may not be familiar with the hydrographic biogeochemical conditions in that region. The "Discussion" contains

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two rather short sections (4.2 and 4.3). My suggestion is to merge these with “Results” which is then named “Results and Discussion”. The CO2 flux estimate (4.1) is of no relevance for the science in your manuscript. To satisfy those who are always hunting for such numbers, move it to the Appendix.

Comments: 2.1 Time series measurements: The pCO2 measurements are the basis of your study. There-fore it is necessary to give some more information about the measuring principle, preci-sion/accuracy, response time, calibration procedure, etc. . What is meant with “flushing”?

2.4 Estimating alkalinity from salinity, p.4/line 23 “ Grand passage measurements are not expected . . .”, unclear sentence, one can only guess what the meaning is. Has the intercept any biogeochemical meaning?

2.5 Calculating DIC, p.4/last line: The “system of equations” is not created by Lewis and Wallace (1998), but represents the well-known thermodynamics of acid-base equilibria. Ko from Weiss (1974)?

3.1 Seasonal evolution of measured and equilibrium solution variables “equilibrium solution variables”, bad term in the headline.

p. 5/line 17: Tidal alkalinity variation of 718 μmol/kg??? Check. p.5/last paragraph: How are the first two sentences logically related to each other? DIC is a conservative variable and does not depend on temperature. The observed DIC decrease refers to different water masses, it is therefore not allowed to make any conclusions about effect of “photosynthesis and respiration”. Such changes occur on a background DIC level which is directly related to the alkalinity. In order to identify any biogeochemical DIC changes, it is thus necessary to remove the effect of the differing back-ground DIC as you have done it by calculating DICex.

3.2 Unravelling daily and tidal cycles of biogeochemical driven changes in DIC Subscript “mix” is somewhat misleading, I prefer to call it “background DIC”. Please make

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clear that the zero level in Fig. 4 has no biogeochemical meaning since it de-pends on the choice of the reference pCO2. You could have taken also the atmospheric pCO2 and spring SST as reference because in many ocean regions the spring bloom starts when the surface water is approximately at equilibrium with the atmosphere. In that case the zero DICex means “zero ” biology. p.6/2nd paragraph: Your interpretation of the seasonal DICex pattern is confined to biologi-cal effects and ignores the effect of gas exchange. Can you roughly estimate the relative im-portance of the two processes?

3.3 Tidal phasing The calculation of H+ex makes no sense because H+ is a non-conservative variable and does follow conservation of mass. This is also reflected in a strong dependency of ΔH+/ΔTA and thus of H+ex on the choice of the reference pCO2.

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