

Interactive comment on “A17-year dataset of surface water fugacity of CO₂, along with calculated pH, Aragonite saturation state, and air-sea CO₂ fluxes in the Northern Caribbean Sea” by Rik Wanninkhof et al.

Anonymous Referee #1

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A 17-year dataset of surface water fugacity of CO₂, along with calculated pH, Aragonite saturation state, and air-sea CO₂ fluxes in the Northern Caribbean Sea.

This manuscript presents a straightforward assessment of an impressive dataset taken in the Caribbean Sea aboard two cruise ships outfitted with state-of-the-art CO₂ measurement equipment. The CO₂ data along with modeled salinity (used to estimate TA), MLD and satellite SST were used in a year by year algorithm to estimate pH and Omega-aragonite. A statistical binning process is used to aggregate these data into 1-degree bins.

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This work should be good to be published with minor revisions assuming the comments are addressed.

My most pressing concern centers on using separate fitting algorithms for each year. The authors need to justify this. A quick glance reveals that the differences in the coefficients are quite large year by year. Why wouldn't it make more sense to do this by aggregating seasonal data? When considering the time series data: what would be the effect of the use of annual estimates rather than a single fitting? For example, I wouldn't expect a smooth transition between December and the subsequent January that uses a different algorithm.

Also, I do not see good description of how the derived parameters pH and Omega Ar were validated.

Minor concerns: Line 121. A Licor 6262 was used. While this is a fine instrument, it has not been production since 2005. I'm not worried about the CO₂ measurements since they do a good job with standards, but how was the H₂O channel calibrated or standardized? How accurate is your pH₂O?

Section 2.4. What information do the MLD estimates convey? They mention what they could be used for e.g. inventories, but nothing about how they help the estimates. Please include your ideas on this.

Line 150. The term "bone dry" is not very scientific and should have been hyphenated. Plus, bones are not that dry. See, Timmins, P.A., Wall, J.C. Bone water. Calc. Tis Res. 23, 1–5 (1977). <https://doi.org/10.1007/BF02012759>

Line 150. Mentions "analyses typically had a humidity of 10% or less", but earlier (L. 120) mentioned that the headspace was dried >75%. I think I get the distinction, but it was confusing to me.

Line 267. Mentions that cooler near surface temperatures could lead to lower fCO₂ values and that this has a "large impact on the calculated air sea fluxes". Please use a

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citation or constrain the “Large impact” with some stats.

Line 273-275. Where does the steady state increase of 2.13 uatm/yr come from? Is this from the ship’s atm data, or is it Mauna Loa, carbon tracker or something else. Also, over a 17-year time series, one would expect a changing rate of CO₂atm. Is “steady state” referring to the linear increase over that time period? Clarify.

Line 324 For Boron, I though the Lee at al, 2009 algorithm was more commonly used these days.

Line 343. Use a reference or two for the use of Omega as a biological indicator.

Equation 9. If this is right out of CO₂sys, I see no reason for an equation.

Figure 1. It’s unfortunate that there’s little data where the salinity variability is presumably the highest (i.e. in the Southern Caribbean where the large South American Rivers affect the region). What is the effect of this on the gridded data?

Acknowledgments: should mention the cruise line that made this possible.

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