Interactive comment on "A 17-year dataset of surface water fugacity of CO_2, along with calculated pH, Aragonite saturation state, and air-sea CO_2 fluxes in the Northern Caribbean Sea" *by* Rik Wanninkhof et al.

Anonymous Referee #3

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The manuscript describes a 17 year dataset for surface water marine carbonate data collected using multiple ships within the Caribbean and a substantial set of derived data.

The manuscript appears to have been a little rushed. There are instances of unclear statements, inconsistent naming, repetition, use of non-SI units, formatting errors and some structural issues. I have listed all coments referring to these issues under the section 'Minor comments' (See below).

I would suggest that the manuscript is re-considered after revision and my reasoning is explained below within the Major comments.

Major comments: 1. The uncertainty information within the manuscript is inconsistent and/or incomplete. Some information is given for the fCO2 data but nothing is given for the temperature or salinity. No uncertainty information or statements are given for the derived datasets eg pH or aragonite saturation state or gas fluxes. This limited information will limit the use of these data, or could result in users making incorrect assumptions about the uncertainties. It would be good if the authors could follow a standard framework or phrasing for presenting the uncertainty information e.g. BIPM 2008 framework and identifying if uncertainties are Type A or Type B and also identify which components of the uncertainty budget have been considered and which have been ignored. Its clear that the derived datasets are unlikely to be considered to be 'truth' measurements, so the authors need to write some text to explain this, so that users of the dataset don't make the mistake of assuming that these data are truth. It may make sense, and/or make it easier for the reader, if all of the uncertainty information was grouped together into a common location (eg one table?) which can then be referred to within the different sections of the manuscript.

BIPM, 2008 - Guide to the expression of uncertainty https://www.bipm.org/utils/common/documents/jcgm/JCGM_100_2008_E.pdf

2. lines 266 to 268. The text states that the cooler temperatures near the surface could lead to lower fCO2 which can have large impact on the calculated air-sea gas fluxes. But the gas fluxes have been calculated using a version of the bulk flux calculation (equation 4) which ignores all vertical temperature gradients. However the dataset includes OISST data which could be used to perform a more accurate gas flux calculation (e.g. re-calculate pCO2 to a common depth, then perform a more accurate calculation). The authors could either provide the results using a more accurate gas flux calculation or highlight this issue to the user/reader and then refer to them to an example analysis that shows the impact of a lower accuracy gas flux calculation and

the estimate the increased uncertainty within their derived dataset that results from this lower accuracy calculation. To help, see figures 3 and figure 4 of Holding et al., (2019) for an analysis of the impact along single cruise tracks, or panel 1 of Shutler et al., (2019) for the impact over larger spatial and temporal areas.

Holding et al., (2019) https://www.ocean-sci.net/15/1707/2019/ Shutler et al. (2019) https://esajournals.onlinelibrary.wiley.com/doi/pdf/10.1002/fee.2129

3. its not clear why the multi-linear regressions are performed and/or why anyone would want this output. These results and methods should introduced giving an explanation as to why they are useful. I'm not sure that this part of the dataset is needed though.

4. The binning method does not account for the paired nature of the pCO2 and SST datasets (as each parameter is binned individually). Surely the binning will have skewed this relationship and so the paired nature will no longer exist. This issue may be especially true if some bins contain data from multiple cruises (which fig 1 suggests will occur). Can the authors highlight this issue and discuss the implications so that users of the dataset are aware of this problem?

Minor comments: 1. line 30, suggest 'The data and products could be used for determination of ....' as surely the paper is providing data for others to use (rather than presenting their use of these data). 2. the use of the word 'average' throughout the manuscript is ambiguous. do the authors mean a statistical mean, mode or median? (all are averages). suggest that all instances of the word 'average' are replaced with the appropriate statistical name. 3. there are instances of 'month' and 'mo'. the latter I think also means 'month'. I'd suggest that the authors use one throughout, rather than swapping between both. 4. line 99, space needed between 100 and units (m). 5. line 101, no dash needed in '5-m'. similarly three further instances on line 109 and more instances of this on line 191. 6. line 126, the value of 2uatm is twice the size of the value on line 120. are these the same values ie +-1uatm? how has this value of 2uatm been estimated? 7. line 115, I'd suggest '...measurements for the ships with

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air intakes and analysers.' 8. line 124. can you provide the range in values used for the standards? 9. section 1.3.2 the precision, accuracy and sensitivity of these instruments are missing. 10. line 150, use of non scientific phrasing, what is 'bone dry'? 11. line 158, can you define what you mean by infrequently? eg. %age of time. 12. line 160. processing routines are mentioned but no detail is given. could an overview of these processing routines be provided in the appendices? this information would appear fairly important should anyone want to use these data and/or try and follow the same methods for a similar effort somewhere else in the world. 13. line 266, I'd suggest 'While both differences include zero within their uncertainty..' 14. Suggest that section 3.4 (binning procedure) comes before the sections on the calculations (as surely the binning is done first, then the calculations are performed). 15. line 303, I think that CCMP data are available for 2018 eg http://data.remss.com/ccmp/v02.0/ 16. line 327, see 'were compared for 201'. what is 201? 17. line 336, 'insignificant' is a bit subjective and application specific. can you put this into context? 18. table 3, space needed between 12 and (December). 19. table 3, 4, and 5 all contain non-SI unit notation in pH row (mol/kg-SW) 20. line 407, the 'stdev' has previously been used, but not defined. 21. the content in section 4.5 is a bit jumbled. The method for the annual and monthly values needs to be more clearly and sequentially explained. Eg surely the values are first weighted by area and then summed (rather than summed and the mean value area weighted?). 22. line 411 to 414. Can you clarify this paragraph? I'm afraid that I don't really understand this paragraph or the reasoning and why are the data treated differently? 23. line 409, the dash between terra and grams is not needed. 24. section 5 contains repetition (with section 4.1). 25. line 445, I'd suggest '..instrumental in maintaining the science operations.' 26. line 667, month/year notation is different from the main paper. 27. line 676, how is this 'overall uncertainty' determined? 28. line 677, mixing of 'errors' and 'uncertainties' naming. I think that they are all uncertainties (error implies that you know a truth value). What is the 'error' column in table 1? and it appears to be called RMSE in table A1 and A2. 29. line 683, Lat and Lon not defined. 30. line 684, RMS not defined.

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