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Discussions

Interactive comment on "A high-frequency atmospheric and seawater $p\mathrm{CO}_2$ data set from 14 open ocean sites using a moored autonomous system" *by* A. J. Sutton et al.

Anonymous Referee #1

Received and published: 1 July 2014

This paper describes new mooring technology and data sets. It is a significant contribution in a few ways. First, it provides a description and comprehensive assessment of MAPCO2 sensors, which have been developed over the past decade and allow high frequency CO2 measurements on moorings for extended periods. The only other published assessment of sensors capable of long deployments on moorings, including this sensor type, has been made through the Alliance of Coastal Technologies. However these were focused on short-term field measurements (days-week). The text does mention another comparison in Japan that indicates the sensors are capable of high accuracy and precision measurements on short time scales, but the results of the

Japan intercomparison are not easy to find. Second, the paper describes a major new data set of high frequency measurements made at fourteen ocean sites, particularly in the equatorial Pacific and will be a significant contribution to improving estimates of air-sea CO2 exchange. Third, the paper shows clearly the MAPCO2 sensors provide data of similar quality to underway observations and are of acceptable standard for constraining air-sea fluxes.

The data described will have widespread use for determining air-sea CO2 exchange, testing of models, and for assessing controls on ocean-atmosphere CO2 exchange and ocean acidification. I believe the paper should be published with some revisions suggested below.

The data are only reported to 2011, and this is related to submission of data to the Carbon Dioxide Information Analysis Center. It is a pity more recent data is not included.

The text mentions that two of the moorings have been removed. A statement in the table 1 caption on what sites are no longer operating would be useful.

Introduction: pg 388, A reference to the accuracy of CO2 measurements required to constrain regional air-sea CO2 fluxes to 0.2PgC/yr should be included. Why 0.2 PgC/yr?

Methods:

Page 390: Has the linearity of the Licor 820 sensor response over the range of CO2 (e.g. 0-500 ppm) been determined? This will have an impact on the precision and accuracy and is not considered.

Page 391: The methods description mentions the drying of the gas before a measurement. No detail is given. Is this a chemical dryer and what substance? Data reduction:

Page 393: Give the range of the four standard reference gases used on the 8050 underway system.

Has the comparison between the General Oceanics 8050 and the MAPCO2 system pre-deployment been done at a variety of pCO2 values and temperatures, or at one value - ie the value in the seawater tank at room temperature? Are checks also made when the system is returned to the laboratory after recovery from the moorings?

Page 393, line 23: The description of data quality control mentions a post calibration curve is used for the final calculation. Is this a curve or a straight-line fit (only zero and span are measured for calibration)? How is it possible to generate a curve from two points?

Equations 2 and 3 should be described more thoroughly. Equation 2 needs a reference, including where the coefficients are taken from, as only the 1.004 value appears to be referenced. The text describing equation 3 states RHsample is "the RH of air sample (%)", with line 26 on page 394 stating " PLicor is the pressure of the air and seawater samples" implying "air sample" means atmospheric air (and possibly span gas?). Does air mean atmospheric air or the gas in the Licor820 (ie atmospheric air, equilibrator headspace gas/air and span and zero gases)? This could be made clearer if "RH of air sample (%)" is changes to mean the RH of the gas stream measured by the Licor 820, or something like this. Also, is RHspan in equation 3 taken to be a background residual value for the system? These parameters and equations should be made clearer.

Page 397 "Further work to smooth and deseasonalize the data, as in most growth rate analyses, could provide further insight into the source of the larger differences between the MAPCO2 and MBL observed at the KEO and MOSEAN/WHOTS sites." This sentence seems to be stating this approach should give a better data product and that the authors will not be doing this. More detailed trend analyses may also have the opposite effect of pointing to larger discrepancies between the MapCO2 atmospheric data and MBL data. Either a more complete trend analysis should be performed, or the sentence removed.

Page 397 & 398: Line 19 on page 397 says the high-frequency MAPCO2 varies from

smoothed MBL data by <5 umol/mol. Page 398, lines 14-17 state, "Since atmospheric CO2 is well mixed in the open ocean MBL, environmental variability introduces little error to the MAPCO2 and MBL air comparison. The resulting mean differences in the atmospheric data are likely due primarily to uncertainty in the measurements, which in this case, we associate with the MAPCO2 system". While this is a good check on the MAPCO2 data, it only applies to a check near the atmospheric CO2 value (about 395 umol/mol), and it is necessary to assume the zero is good throughout the whole deployment, or there could be offsets for values different from the atmospheric measurement. As far as I know, the MBL product is smoothed and is not particularly well constrained by data in all areas that measurement are likely to be made with the MAPCO2 systems. My understanding is the MBL values are estimated from measurements made weeks apart in most cases and using data that can be large distances from the MAPCO2 system locations. Given the seasonality and spatial changes in atmospheric CO2, particularly in the Northern Hemisphere, I am concerned how reliable the MBL estimates are at the mooring sites and the use of this comparison to determine offsets for the MAPCO2 data could bias the results, although probably at a level of less than 1-2 umol/mol. A brief discussion of the reliability of using the MBL estimates for atmospheric CO2 would be useful. It may also be useful to show a frequency distribution plot of the offsets for one of the moorings and show if it is a normal distribution.

Page 399, why were the Leuker constants used for the CO2SYS calculation? A 5% error is large (about 15-20 ppm).

Page 401: The deployments over a year or longer are described. It would be useful to include a description of what happens to the RH values over this time and if the drying agent continues to work well over the period. This might not be relevant if the seasonal temperature change masks any drift in the RH values.

Figures: Figure 3. The interannual and seasonal rings in the plot are too difficult to see. A second panel with a pie chart for seasonal and interannual variability may provide better information or the size of the outer rings could be increased. The use of the

circle diameters and the color to indicate the variability and mean deltapCO2 is clear.

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