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Biogeosciences Discussions

Interactive comment on "Insights from year-long measurements of air-water CH_4 and CO_2 exchange in a coastal environment" *by* Mingxi Yang et al.

Anonymous Referee #1

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- General comments: this manuscript by Yang et al deals with CH4 and CO2 fluxes in a coastal environment. Assessing CO2 and CH4 air-water exchanges is an important exercise to determine the impact of given ecosystems on the atmospheric CO2 and CH4 burden. It is particularly the case for aquatic ecosystems such as estuarine and coastal ones which are of relative influence compared to the area they are covering at the global scale. Most of the previous studies dealing with the subject have been based on indirect estimate through air-sea concentration difference and gas transfer velocity, the so-called Boundary-Layer method. The work by Yang et al presents an interesting and rather rare time series of EC measurement performed over one year. The authors have done a good job in data collecting and study design at the Penlee Point Atmospheric Observatory (PPAO), on a nearby buoy (L4), and from different research

Vessels. Data base includes CO2 and CH4 exchange fluxes as well as a description of meteorological data and some of the water quality parameters (Chla for example). This MS is generally well written, is timely and interesting to understand the parameters of influence on CO2 and CH4 exchanges in coastal environments. Several parameters of influence on transfer velocities have been checked, all of them are relevant. Though, curiously, the effect of precipitation rate on fluxes have not been investigated. Impact of drops on the water surface can enhance significantly (several tens of percent) the gas transfer velocity. Were the precipitation periods withdrawn from the date as part of the EC quality control process? In all cases, the influence of precipitation of the data set (whether on the EC data quality or on the transfer velocity) should be discussed.

As pointed out by Nilsson and colleagues, statement by Yang and colleagues on the performance of open-path sensor should be revised. Sentences should be reworded to include a more tempered statement on potential interferences of open-path analyzer over water bodies. Effect of salinity on these spectral interferences should be discussed as suggested by Nilsson.

- Specific comments: here are some specific comments that should strengthen the MS.

P5, l 133: Can you quantify more precisely the effect of stability on the Xmax and X90 distances? This would help for the discussion on CO2 fluxes on p8

P7, l 185-195: Not clear, mean flux should be the same whatever the way it is calculated.

P7, l 192: not clear, but 6h fluxes should be the reference fluxes when compared to annual fluxes, how could they be skewed?

P8, l 212: give details on how the total CH4 flux was calculated

P8, l 216: give details to the reader on how the random instrument noise is calculated. Is the instrumental noise mentioned on line 219 the same noise?

P8, l 229: only daytime measurements of pCO2 are mentioned, no night time mea-

surements performed, right?

P8, l 231: Not clear which data were interpolated, and how they were interpolated

P8, l 235: see comment on page 5. How far further upwind?

P9, l 248-29: again, not clear why mean calculated from monthly mean and from 6h mean are (so) different

P9, l 259: there are many speed-dependant transfer velocity relationships. Choice of the only one from Nightingale et al 2000 paper should be justified. On which basis this choice was made.

P9, l 261: wind speed threshold above 5 ms-1 seems quite high. Any justification of that wind speed value?

P9 l 264-268: saturation level relative to atmospheric saturation are defined but not used on figure 6. This could be done for the reader to better follow the discussion

P9, l 268: Is the effect of salinity and temperature accounted for in the 14% variation of CH4 solubility? Not evident on figure 6.

P10, l 277: which time series is commented here, 6h or 1h mean data?

P10, l278: same pattern that what? Semi-diurnal variability? That is not possible, this must be something else. . .

P10, l 285-287: comparison is made on two set of data without the same number of monthly data. Not sure it is meaningful.

P11, l 299-300: seems that the sentence should be reworded

P11, 309: syntax? Missing word?

P12, l 350: not the highest saturation, but highest absolute concentration.

P12, l 353-354: data at L4 not on figure, could be added for comparison.

P12, l 357: four "times" higher. Times is missing

P13, l 359: mentioned that it is the flux footprints for the open water sector

P13, l 383: how did you choose to discard interpolation more than four days away?

P14 l 393: how this threshold of 20 μatm was chosen/determined? Any justification?

P14, l 397: be more explicit on what you call "measurements uncertainties in flux, variability in pCO2, as well as processes other than wind speed . . ."

P14, l 413-414: why did not you check the effect of planar fit vs double rotation to confirm your assumption?

P15, l 441: what is the effect of the 40μatm threshold on that gap?

P15 : the highest Chla measurements in Plymouth Sound is well above the regression line. Any clue for that?

P14, l 467: Be more clear about "to reduce the temporal mismatch between the flux and pCO2 measurements".

P15, l 479: ". . .could result in biased annual mean flux estimates". Could this be more precisely quantified for example in the case of only daytime measurements?

Figures P23: use consistent legend throughout the paper with: SW (open water) and NE (Plymouth Sound) all along.

P25: figure 5A: you should not display negative value for K which have no physical meaning. You could shade the night time period on the figure.

P27: figure 9: add units to the X and Y axis

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