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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 #2

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General Comments

Yang et al. present an annual monitoring of CH4 and CO2 fluxes from a coastal location in the southwest UK, using the eddy covariance technique. The paper is generally well-written and easy to follow. The manuscript shows important findings related to the investigation of carbon dynamics in coastal zones, which remains largely unknown due to its intrinsic high spatio-temporal variability. The authors showed high differences between fluxes measured at daytime and nighttime, implying that results covering only daytime may be biased due to the influence of biological activities (called diurnal) and tidal processes (called semi-diurnal). The methodology of in situ measurements and data processing are consistent. However, the authors must to address the important appointments from Erik Nilsson and the other anonymous referee. The authors also

must improve the description of state of art of CO2 and CH4 dynamics in coastal zones and estuaries (introduction). This section is poorly described. The same appointment is also true for the discussion section related to the dynamics of these GHGs. This part of the manuscript is also unsatisfactorily constructed. The manuscript is well described/structured in terms of technical aspects, but the data interpretation is not sufficient discussed in terms of ecological/biogeochemical processes. A more detailed bibliographical survey is strongly recommended to support your findings and to better contextualize this study.

Specific Comments

Abstract What it means the semi-diurnal timescales (tidal processes)? Please, describe what is the pCO2 (partial pressure of CO2 in water/air).

Introduction

See the general comments.

Lines 31-32: "...have been increasing over the last few hundred years primarily due to human activities (Hartmann et al. 2013)." I agree. However, the fastest increase is related to the last 50 years... Lines 38-43: This paragraph is a simplistic exposition of the CH4 dynamics/cycling. You should go deep in this part, especially in the studies covering the coastal oceans/estuaries. I recommend a better literature research.

Lines 45-47: "Globally averaged, the open ocean is modelled to absorb about a quarter of the anthropogenic CO2 emission (Le Quéré et al. 2015)." You can give numbers, and update this reference (from the global carbon budget 2018; Le Quéré et al. 2018). https://www.earth-syst-sci-data.net/10/2141/2018/

Lines 50-56: "Estuaries, on the other hand, are generally net sources of CO2 for the atmosphere (e.g. Frankignoulle et al. 1998)..." How much? Frankinoulle et al. 1998 is a classical reference. However, there are more recent references that you should include when is describing the global emissions of CO2 by estuaries. In addition, you

should shortly describe processes that can affects the dynamics and fluxes at the air-water interface.

Lines 55-56: “The shallow seas are predicted to become a greater net sink of CO2 in the future due to rising atmospheric CO2 and increasing inorganic nutrients (e.g. Andersson and Mackenzie, 2004).” However, other studies showed that the estuaries could emit more CO2 due to the enhancing of organic matter respiration.

Lines 73-74: “Thus a wind speed-only dependent representation of K, incomplete for the open ocean (Wanninkhof et al. 2009).” Please rephrase.

Experimental Lines 105-107: “For this paper, wind data from the Windmaster Pro sonic anemometer were used between September 2015 and March 2016. Since March 2016, wind data from the R3 sonic anemometer (not operational for the first 6 months of this annual study) were preferred because of its higher precision and better performance during heavy rain events. Did you compare the wind velocity from the 2 different anemometers used in this study?

Results Fig. 1. Could you better explain the causes of the negative fluxes of CH4? This means that the water was sub-saturated with respect to atmospheric CH4 concentrations? What are the main causes of this under saturation?

Could you add a plot combining the results of chl a concentrations and the pCO2 fluxes/values (scatter plot)?

Lines 210-213: “If the PPAO open water footprint is representative of the nearest 1.4 km (i.e. X90 of our fluxes, see Section 2.2) of the UK coast, our measurements extrapolate to a total CH4 flux of 4.8 Gg yr-1 in UK coastal seas.” The extrapolation of the results to other areas is a good exercise. However, CH4 is a gas that present very special conditions of production and consumption. I am not convinced about this calculation.

Lines 219-221: “Ambient variability (in e.g. dissolved concentrations) largely drives the rapid temporal fluctuations in CO2 flux, which is unlikely to be fully captured by weekly

or monthly seawater sampling.” This passage is confuse. Please rewrite.

Fig. 4. CO2 flux from the Plymouth Sound sector appeared to be more positive than from the open water sector in some months. I would expect larger differences, but this is not the case. Could you explain this?

Lines 239-242: “Wind speed was generally higher at night during these few days and the measured fluxes imply that ΔpCO2 (see next sections on this calculation) changed from about -40 μatm during the day to about 15 μatm at night.” Then, during this period, the system was a net sink of CO2?

Lines 283-285: “The greatest undersaturation in CO2 is observed in late spring and early summer, coinciding with an increase in chlorophyll a concentration at the nearby L4 station (Figure S6).” As I said before, I would like to see graph showing the chl a and the CO2 concentrations.

Lines 295-298: “The diurnal variability we observed is important in the context of estuarine CO2 (and carbonate system) observations that are predominantly carried out during daytime when sampling and navigation are easier.” Many studies were published recently covering the diurnal (biological effect) and the semi-diurnal (tidal effect) variability on pCO2 changes, which are poorly described and constrained in the present manuscript.

Line 304: Spatial variability in seawater concentrations. Please see the general comments. This could be because the influence of Tamar estuary on the PPAO flux footprint is less in terms of pCO2 (e.g. due to the already large burden of carbonate and bicarbonate in seawater), and more on physical and biogeochemical processes. I am not sure about this statement.

Line 306: “dissolved pCO2.” This is unusual. You should refer “pCO2” or “dissolved CO2”.

Lines 329-331: “It could be that the large burden of carbonate and bicarbonate in

seawater partially buffered the impact of the freshwater input on pCO2 within the flux footprints." This is poorly discussed.

Lines 373- 377 "The implied pCO2 from EC fluxes in monthly bins and in situ measured pCO2 agree quite well over the annual cycle for the open water sector (Figure 7), suggesting that the use of the wind speed dependent transfer velocity parameterization of Nightingale et al. (2000) is largely reasonable." I am not sure that this type o graph is the best to show a comparison between estimated and measured pCO2.

Figure 7. Y-axis : seawater pCO2 ?

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