

Interactive comment on “Insights from year-long measurements of air-water CH₄ and CO₂ exchange in a coastal environment” by Mingxi Yang et al.

Anonymous Referee #2

Received and published: 28 January 2019

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

Printer-friendly version

Discussion paper



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? Frankignoulle 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

Printer-friendly version

Discussion paper



should shortly describe processes that can affect 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 presents 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

[Printer-friendly version](#)[Discussion paper](#)

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 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 $\Delta p\text{CO}_2$ (see next sections on this calculation) changed from about $-40 \mu\text{atm}$ during the day to about $15 \mu\text{atm}$ 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

BGD

Interactive
comment

Printer-friendly version

Discussion paper



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 ?

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-503>, 2018.

BGD

Interactive
comment

Printer-friendly version

Discussion paper

