

Interactive comment on “Air-sea carbon flux from high-temporal-resolution data of in situ CO₂ measurements in the southern North Sea” by Steven Pint et al.

Anonymous Referee #2

Received and published: 4 January 2021

The manuscript by Pint et al. describes a 10 month long time-series of CO₂ observations from a coastal mooring in the southern North Sea. From the 10 month time-series the authors infer an annual cycle of CO₂ fluxes.

The coastal ocean is highly variable over space and time, and shortage of data with sufficient temporal and spatial resolution is needed to understand the processes that are at play. In that sense this data set is potentially important contribution complementing a number of earlier studies that are cited in this manuscript. Simply the fact that this is a temporally well-resolved data set would justify its publication.

However, the study fails on clarity for a number of issues. Most important is probably

the poorly explanation of the large (huge) corrections of the data from roughly July – December time-period. It seems to be a correction in the order of 100 uatm. Without that correction the flux would be more consistent with the large positive flux reported by Kitidis et al. (2019), for instance. I am not saying it is wrong, but it is suspicious, in particular since the correction seems to converge back to a very small correction for the later part of the time-series.

I don't think it is correct to infer an annual flux estimate and an annual cycle from a 10 month time-series (let alone from one with doubtful data for 4-5 months). What is the reason for only using these 10 months of data? Is the time-series aborted? If more data is available, I would strongly suggest to include at least one more year of data, if not also the 2020 data (if available). That would make the conclusions more convincing. Now I am not that convinced at all.

The oceanographic setting of the stations is poorly described in the text. Noting that the station is close to the Rhine estuary and the large difference in terms of CO₂ fluxes one can expect from an estuary- relative to a shelf-station. What is the influence of river run-off on the observations, and how much of that driving the variability that is observed at the station, and how much can that drive inter-annual variability (different flow and pathways of the river water)?

Minor issues: Line 53: On the sensors. I would recommend more information on the sensors, calibration etc. Simply stating "commercial sensors" is not convincing although more information is available in Table 1. However, how can the authors claim an accuracy of +/- 10 uatm, and then do a correction of almost 100 uatm? I am sure this is the stated precision from the manufacturer, and evidently there are some issues with the instrument, at least for a part of the time-series.

Line 169: It is not surprising you found higher fluxes for higher wind-speed. ... That is so obvious from the flux calculation the authors used.

Line 223: How did the author determine the flux of anthropogenic carbon? The mea-

[Printer-friendly version](#)

[Discussion paper](#)



sure pCO₂ and have no way of teasing out the anthropogenic carbon flux from the natural.

Line 235: The authors refer to high inter-annual variability, so that makes even more of an argument to include more data in this study.

The data will have to be available at ICOS before this article can be considered for publication.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-442>, 2020.

BGD

Interactive
comment

[Printer-friendly version](#)

[Discussion paper](#)

