

Biogeosciences Discuss., author comment AC4  
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## Reply on CC2

Christian Rödenbeck et al.

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Author comment on "Data-based estimates of interannual sea-air CO<sub>2</sub> flux variations 1957–2020 and their relation to environmental drivers" by Christian Rödenbeck et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-304-AC4>, 2022

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Thank you Jamie for your discussion contribution about our study.  
In the following, original comments are quoted in bold italics.

### **This is an interesting paper and an enjoyable read.**

Thank you for your positive rating of our study.

***on page 21, lines 24 to 29, there is some incorrect information and understanding that has led you to some incorrect conclusions, and these are likely to have had a large impact on the results of the analysis.***

***The authors write:***

***Watson et al. (2020) estimated that the sum of these two effects would shift pCO<sub>2</sub>-based estimates of the mean global CO<sub>2</sub> flux by 0.8 to 0.9PgC yr<sup>-1</sup>***

***(stronger sink).***

***which is correct.***

***However the next sentence then says:***

***>So far, however, it is unclear how well the water temperature at the relevant vertical positions can actually be determined (an important source of uncertainty not included in Watson et al. (2020)'s range) and how it varies in space and time.***

***This sentence is partially correct but also a bit misleading and I the authors may be confusing two different issues. The depth that satellite temperature data are relevant for is well understood and well studied (eg see the information from the international Global High Resolution SST (GHRSSST) team and publications eg <https://www.ghrsst.org/>). What is less clear is how the satellite temperature data align with the top and bottom of the mass boundary layer which is where air-sea gas exchange occurs.***

It seems to us that your explanation does confirm that there are uncertainties about the water temperature as used in your algorithm - which is exactly what we say in the manuscript. Independent of the details, it is thus true that there are larger uncertainties from applying your pCO<sub>2</sub> adjustment than suggested by Watson et al. (2020).

While it is clear that sea-air fluxes calculated from pCO<sub>2</sub> measurements are affected in

some way by the 2 effects under consideration here, there is no consensus in the community so far whether the particular adjustments used in Watson et al. (2020) indeed act to cancel these effects, and what further uncertainty they may add (personal communication with various colleagues).

***The authors then continue to say:***

***In any case, we note that our study mainly considers the variability of the flux, for which the effect of a time-constant correction as in Watson et al. (2020) would cancel out.***

***The authors are confused here as well. The Watson et al work presents two corrections. the first corection focusses on the issue that surface pCO<sub>2</sub> data (and their paried temperature data) are all collected at different depths, as sampling depth varies between ships and even within a ship track (eg as the ship changes its ballasting). Whereas the second correction that Watson discuses is the one that the authors can ignore as the authors are interested in variability rather than absolute CO<sub>2</sub> sink value.***

***So first correction in Watson et al focusses on re-analysing the SOCAT pCO<sub>2</sub> data to common and consistent depth. These methods are published and the data are published each year and the re-analysed version of SOCATv2020 are available (Shutler et al 2021) (equivalent data for SOCATv2020 and SOCATv2019 are listed on the SOCAT website). This aspect will be important the work the authors present, as I suspect that some of the variability that the authors characterise in their observation-based data is likely due to the inconsitent and varying depth over which the orignal SOCAT pCO<sub>2</sub> data are collected. They authors can easily check for this by repeating their analysis using the re-analysed and depth consistent SOCAT dataset using the data from the Shutler et al link below. Using the re-analysed SOCAT data may actually strengthen the conclusions in the paper.***

Even if there may be some spatial and temporal variability in the pCO<sub>2</sub> adjustments, the results shown in Fig 1 of Watson et al. (2020) testify that the effect on the estimated fluxes on larger scales is not varying much at all when compared to the signals.

***to help, the issue of how pCO<sub>2</sub> data collected at depth is not always representative of the surface water has been recently identified for Arctic regions by Dong et al 2021. Dong et al show that these issues can result in biased fluxes due to salinity issues. Whereas the Watson work shows that this bias due to temperauter can be more widespread. the theory is well discussed in Woolf et al 2016.***

We note that pCO<sub>2</sub> is used in our algorithm not only to calculate gas exchange, but it is also linked to mixed-layer DIC concentration via carbonate chemistry calculation. In this part of the algorithm, the suggested adjustments to the data points would clearly introduce errors.

***Shutler et al (2021) Reanalysed (depth and temperature consistent) surface ocean CO<sub>2</sub> atlas (SOCAT) version 2021,  
<https://doi.pangaea.de/10.1594/PANGAEA.939233>***

***Dong et al, (2021) Near-Surface Stratification Due to Ice Melt Biases Arctic Air-Sea CO<sub>2</sub> Flux Estimates,  
<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2021GL095266>***

***Woofl et al 2016 On the calculation of air-sea fluxes of CO<sub>2</sub> in the presence of temperature and salinity gradients,  
<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015JC011427>***

