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Comment on bg-2021-304

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Community comment on "Data-based estimates of interannual sea–air CO_2 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-CC1, 2021

Reviewer: Rik Wanninkhof, NOAA/AOML

Review of: Data-based estimates of interannual sea–air CO2 flux variations 1957–2020 and their relation to environmental drivers by Christian Rödenbeck, Tim DeVries, Judith Hauck, Corinne Le Quéré, and Ralph F. Keeling

As concisely described in the conclusion, the authors determine " the interannual variability of the sea–air CO2 flux over the 1957–2020 period, constrained by the pCO2 measurements from the SOCATv2021 data base. Extending the pCO2 mapping scheme of Rödenbeck et al 2013, they employed:

(1) a multi-linear regression against interannual anomalies of sea surface temperature (SST), the temporal changes of SST (dSST/dt), and squared wind speed (u2), and

(2) a subsequent explicitly interannual additive correction, yielding a "hybrid" estimate of spatio-temporal variations in the contemporary sea–air CO2 flux."

The authors provide an exhaustive description of the approach and results with a focus on interannual variability but include variability on longer time scales, and compare them with other global air-sea CO2 estimates from other investigations. The document is well referenced and addresses procedural uncertainties very well. The work is of high quality and procedures are meticulously outlined including the assumptions and caveats in the analysis. I see no major shortcomings in the work and my comments are largely based on personal opinion/biases of the various estimates to determine fluxes.

- Despite the exhaustive description and illustrative diagrams and figures, the approach to create the flux product is convoluted and remains difficult to understand. The step from linear regression to hybrid mapping and impact of f_{int} on the results are not completely clear to me.

- What is the choice of explanatory variables based on? While it is long recognized that SST and MLD are key determinants of pCO2, others are less so

- Why is pCO2 used while the primary variable in SOCAT is fCO2? While the conversion between the two is a simple one, the authors chose a constant of 0.996 (Table 1, "Values have been transferred from fugacity to partial pressure by dividing by 0.996."), while the coefficient will differ by about 0.001 (or 0.5 uatm) between 0 and 30 °C causing small biases that could be avoided.

- It is not completely clear what fCO2 data is used. Is it the actual observations, or the gridded product (on monthly basis)? Also it is data sets flagged A and B or all data holding?

- The authors indicate that the gas transfer velocity (the preferred nomenclature over piston velocity used here) has a major impact on results and show this, in part, by changing the coefficient of a quadratic dependence and including a cubic dependency. It would be of interest to include a linear dependence as well. See e.g.: Krakauer, N. Y., Randerson, J. T., Primau, F. W., Gruber, N., & Menemenlis, D. (2006). Carbon isotope evidence for the latitudinal distribution and wind speed dependence of the air-sea gas transfer velocity. *Tellus B, 58*, 390-417, doi: 310.1111/j.1600-0889.2006.00223.x.

-While different sets of exploratory variables are used, there is insufficient emphasis that there the quality of exploratory variables before circa 1990 is unknown. This is an added consideration why the results prior to this are not well constrained.

Specific comments:

Page 2, lines 7,15, 32 and elsewhere. Reference to " and many others; and many more; and several others " is a bit odd.

Page 4, line 15. While not including alkalinity is mentioned later on, it should be stressed the you need Alk and DIC to determine fCO2

Page 4. Line 26. Mention size pixels (1 degree by 1 degree by mo???)

Page 6, line 25. See general comment regarding " (2) Observation-based data sets for SST and u are available over all our calculation period 1951–2021 " Prior to 1980- 1990 they are of dubious quality.

Page 9. Line 13. I do not fully understand this sentence "Note that the hybrid run is mathematical equivalent to estimating an additive correction to the multi-linear regression from the pCO2 residuals of the multi-linear regression. "

Page 11 line 18. I don't understand this: "even though these decadal trends seem to be the consequence of pronounced anomalies on the faster year-to-year time scale rather than representing actual slowlier decadal variations" Also, "slowlier" is a uncommon word

Page 12, line 33 This is an important point: "it indicates that the variability extrapolated into the earlier decades without data will likely be underestimated, too."

Page 13 line 24 The following is a bit convoluted, rewrite: "a reduction by 17 to 42% of the increase in the Southern Ocean sink strength (relative to the trend of −0.012(PgC yr−1) yr−1". Also, is this for the full time period?

Page 14, line 29. "Upwelling both decreases" I'd include "mixing"

Page 15, line 3. "Yet, this is still controversially discussed" This is a strange way to end a paragraph.

Page 15, line 8. “DIC fluxes” change to “DIC and Alkalinity fluxes”

Page 16, line 15. This is unclear to me: “For example, in test runs with seasonally resolved (rather than temporally constant) sensitivity coefficients the data could be fitted more closely, but the predictive skill deteriorated (not shown)”

Page 17 line 14: This is a bit unclear. “such that the chlorophyll data may contain substantial variability unrelated to the carbonate system.” Also, dChl/dt would be an interesting parameter to investigate. However, many have shown the chl is a poor predictor variable for fCO2 so its time derivative probably has little skill as well.

Page 18 line 20: is the annual average global atm CO2 used? “to atmospheric CO2 (paCO2).”

Page 19 line 5: typo “internal”: “The estimated 5 ocean-interanl DIC”

Page 19 line 24: These “afterthoughts” that appear throughout at the end of paragraphs are a little distracting, “of this problem remains for further work.”

Page 21 line 23; minor point Robertson and Watson corrected the pCO2 in water to the lower temperature of the cool skin not the air value, as was done in Woolf et al. “Further, the cooler ocean skin temperature translates the atmospheric pCO2 to a different concentration than that implicitly calculated based on bulk temperature (Robertson and Watson, 1992).”

Table 1. For completeness add the equation for Fnet: Fnet = Fant,ss +Fant,ns etc, etc.

Figures: Nice informative figures but they are very “dense” and somewhat difficult to read for the color impaired.