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Reply on CC1

Christian Rödenbeck et al.

Author 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-AC1, 2021

Thank you Rik for reviewing our manuscript and for your interesting comments.
In the following, original comments are quoted in bold italics.

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.

Thank you for your positive rating.

- Despite the exhaustive description and illustrative diagrams and figures, the approach to create the flux product is convoluted and remains difficult to understand.

We will check again the methods section for difficult sentences and try to revise them.

The step from linear regression to hybrid mapping and

This step is essentially the addition of an additive correction based on the pCO2 data.

impact of fint on the results are not completely clear to me.

On multi-year time scale, the sea-air CO2 flux is essentially identical to the ocean-internal flux f_int. On shorter time scale, the sea-air CO2 flux responds to anomalies in f_int with some time delay, with dampened amplitude, and with modifications on various time scales from variability in sea-air gas exchange.

- 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

We indicated the reasons of the choice in Sect 2.1.4.
We agree that MLD would be an attractive variable to be added, but unfortunately it would only be available indirectly from re-analysis of the ocean circulation (i.e., not from

observation), which we deem too uncertain (we are aware that the wind speed from atmospheric re-analysis is indirect information as well, which however has been validated against real observations by the meteorological groups).

- 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.

Indeed, the algorithm could also be formulated in terms of fCO2. However, pCO2 is more commonly used in the modelling community, which had influenced the choice during the original implementation in 2013. The bias from using the constant factor 0.996 compared to a more sophisticated SST and SSS dependent factor is very small compared to the many other sources of uncertainty and thus deemed negligible.

- 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?

We used the actual observations from file "https://www.ncei.noaa.gov/data/oceans/ncei/ocads/data/0235360/SOCATv2021.tsv" including all obsverations flagged A-D, but not the additional file flagged E.
Thank you for pointing out this omission, we will add this information in the revised manuscript.

- The authors indicate that the gas transfer velocity (the preferred nomenclature over piston velocity used here)

We will change the nomenclature in the revised manuscript.

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.

We now ran the suggested test with a linear dependence. It's result differs from the base case (quadratic dependence) approximately in the same absolute way as the cubic dependence but into the opposite direction. We will add this case to the revised manuscript (Figs 8-10).

-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.

We agree and will emphasise this point more in the revised manuscript.

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.

We will reconsider.

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

The particular paragraph around Page 4, line 15 is giving an overview of the chain of information implemented, omitting all the details. It seems difficult to mention the Alk influence here without sidetracking from the main thought. We will check other ways to make the point clear.

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

The pixel size is 2.5 degrees by 2 degrees by day (Sect 2.1.7). We will consider how to mention this earlier if it can be done without breaking the flow of thought.

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.

Yes, we will try to consider this important issue more specifically.

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. "

This paragraph just responds to past discussions within the more specialized inverse estimation community. It is less relevant to readers mainly interested in the actual results and implications (it only appears in the main text because footnotes are depreciated). We will try to make the role of this paragraph more clear.

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

The half-sentence tries to express that the primary time scale of variations seems to be about 2-4 years (such as the recurrence time of the ENSO phases). When considering decadal variations (e.g. by decadal smoothing or by calculating trends within 10-year periods), these anomalies on the 2-4 year time scale imply anomalies on decadal time scale, even if they do not actually represent decadal processes. We will try to reformulate.

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."

Indeed.

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".

The quoted formulation actually looks identical to the original wording - which re-wording did you intend to suggest?

Also, is this for the full time period?

Yes, by "secular trend" we refer to trends over the full period throughout (note that the actual time variation of the wind-speed contribution is not strictly linear).

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

We will add "and mixing in from below".

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

The controversy refers to whether the nutrient effect or the carbon effect dominates the upwelling signal (the 2 effects having opposite sign). We will reformulate to make that more clear.

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

Even though changes in pCO2 could indeed be caused by either DIC or Alk fluxes, the suggested formulation would not be appropriate at this particular place ("is estimated to be...") because Alk fluxes are not actually implemented in the estimation.

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)"

The sentence refers to the general trade-off in a regression between the ability to fit the data closely and how generally valid the regression coefficients are, in dependence on the number of degrees of freedom. We will try to reformulate to make this more clear.

Page 17 line 14: This is a bit unclear. "such that the chlorophyll data may contain substantial variability unrelated to the carbonate system."

We mean that if variations in the Chl data sets are caused by color variations from processes other than those affecting the carbonate system, then Chl data are not helpful as a predictor in the regression considered here.

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.

Indeed, we do not expect more skill from dChl/dt either.

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

We use the decadal average (i.e., decadally smoothed paCO2). We will add this missing information.

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

Will be corrected.

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."

We intended to clearly name loose ends.

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)."

Thanks for the hint, we will try to generalize the formulation accordingly.

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

We will add that.

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

We were hoping that the most important color contrasts (blue-orange-green in Fig 3 and blue-red in Fig 7) were sufficiently visible to everyone. Other use of color (e.g. in Figs. 2 and 8-10) is more of auxilliary nature but not actually bearing essential information. Please let us know if there are elements that would urgently require revision.