Biogeosciences Discussions

Biogeosciences Discuss., author comment AC5
https://doi.org/10.5194/bg-2021-304-AC5, 2022

Reply on RC2

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-AC5, 2022

We thank the anonymous reviewer for her/his interesting comments.

In the following, original comments are quoted in bold italics.

The authors reconstructed the sea-air CO2 flux during the period 1957-2020 by applying the mixed-layer scheme method (Rödenbeck et al 2013) in combination with multiple linear regressions between DIC fluxes in the mixed-layer and SST/wind speed. Surprisingly, the reconstruction starts at 1957, which is the dawn of history of surface ocean CO2 observation and seems to have the longest term among similar studies ever done constrained by surface pCO2 observations. Such a study can contribute to performing more accurate atmospheric inversion systems and validating ocean biogeochemical model outputs, as well as evaluating the oceanic CO2 sink evolution during the industrial era. The authors also struggled to analyze ocean biogeochemical processes which dominate the carbon flux in the mixed layer by using their scheme used here. This study provides a long-term reconstruction useful for several communities interested in the global carbon cycles and novel knowledge on ocean biogeochemical process relating to surface ocean CO2. To this end, I think that the manuscript has sufficient values to be published in this journal, after some minor concerns listed below are properly addressed. In addition, major viewpoints of the study had already discussed actively before the time I received the review offer. So, there are little comments I can show here, and I would apologize if some comments were duplicated.

Thank you for your positive rating.

I'd like to encourage the authors to improve the study and to revise the manuscript for better understanding

We will try to revise our formulations to improve the accessibility of the manuscript.

General comment

The authors adopted an approach using the multiple linear regression (MLR) analysis. As they described in the manuscript, MLR has a potential to express oceanographical processes which could alter the carbonate chemistry, though the method is computationally primitive. They used a hybrid method to cover up

some demerits of MLR. This succeeded in diminishing the uncertainty of reconstruction, but lacked consistency in the method.

We are not fully sure what this comment exactly refers to. Indeed, unfortunately, the hybrid method involves some unavoidable temporal inconsistency, as the more data-rich recent decades can be corrected while the data-poor early decades remain identical to the MLR result. Due to this, the variability in the early decades is likely underestimated, as stated in the manuscript. The representation of the carbonate chemistry, however, is consistent for both MLR and hybrid mapping, as both of them just differ in the ocean-internal DIC flux field.

This study well reconstructed a recent trend of increase in oceanic CO2 sink but failed to give information on the sink evolution in earlier decades because of the use of model output for the period. This might not be a major problem, because the authors mainly focused on the interannual variability of the flux and the mean state of ocean biogeochemical processes, as they said in the manuscript.

We agree with the reviewer. We would have liked to constrain the secular trend better, but it seems to us that other data than pCO2 needs to be used for that.

Specific comment

P14 3.5.1 to 3.5.3: What is the reason that u2, not dSST/dt and/or SST, has the largest effect on DIC flux in the marginal subtropics and subpolar region, where mixed-layer deepening affects the DIC concentration in the winter? All three are good indicators for mixed-layer development. Is it because mixed-layer deepening is not an interannual but a seasonal phenomenon?

Indeed, the sensitivities calculated by our MLR represent interannual responses, thus they do not reflect correlations along the seasonal development of the mixed layer.

P16 L1-4: In general, the wind speed often correlates to SST during the deepening of mixed-layer. It is needed to mention a potential reason why the two are mutually independent.

We agree that the relative independence between the sensitivities against SST and u2 in the MLR is somewhat surprising. Potentially, there are sufficient differences between the SST and u2 fields in terms of their detailed spatial patterns or sub-monthly temporal changes, which would make their "fingerprints" sufficiently different.

P17 L7-15: I was surprised that chlorophyll-a concentrations don't add any information on interannual variability of DIC flux (and I guess that dCHL/dt also cannot add any information too). Once more, is it because blooming is not an interannual but a seasonal phenomenon?

Maybe chlorophyll-a concentrations would indeed have a stronger impact on seasonal variations, even though the timing and intensity of blooming may also be expected to vary from year to year. Possibly, chlorophyll-a data sets contain too much variability from processes not related to the DIC fluxes, precluding a closer correlation. A weak relationship between chlorophyll-a and pCO2 variability has also been found by other authors.

P18 L24-26: SST has an obvious secular trend like CO2 concentrations in most of the surface ocean. High-SSTs caused both by long-term global warming and by interannual variability have similar effects on stratification, and the same can be said for the case of wind speed, if any secular trends exist. So, the discussion in

4.3 is reasonable. However, that is not sure in the case that some biological and/or biogeochemical processes influence DIC flux, including changes in biological species, which should be considered when long-term analyses be done. Please consider adding more explanations about that, if needed.

Thank you for pointing out these processes involving trends. As the method does not offer much constraint on the secular trend, unfortunately, it cannot conclude anything about these processes.

P19 4.5: There are little processes which can alter the alkalinity especially in seasonal to interannual timescales, but it can change due to biological/biogeochemical transition during long-term analyses. Please consider adding more explanations about that, if needed.

Thank you, we will add a note on the biological influence on alkalinity.