Biogeosciences Discuss., author comment AC3
https://doi.org/10.5194/bg-2021-304-AC3, 2022

Reply on CC3

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-AC3, 2022

Thank you Val for your interesting and helpful feedback about our manuscript.

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

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

The authors reconstruct historical air-sea CO2 fluxes from 1957-2020 using a mixed layer scheme constrained by observations of sea surface pCO2 from the SOCAT database.

The importance of SST, interannual variations in SST, and squared wind speed to the internal DIC flux from the mixed layer to depth are investigated using a multi-linear regression.

The multi-linear regression technique is used as a prior for a hybrid approach that estimates historical air-sea CO2 exchange from 1957 to 2020.

The authors' methodology has clearly been exhaustively researched and many sensitivity studies have been explored to consider alternative options. This is work of a high quality that adds an important and new reconstruction of historical air-sea CO2 fluxes to the scientific field.

Thank you for your positive overall rating.

My comments/questions are:

The article would be improved if the sections outside of Method could have greater focus on the scientific findings instead of the method.

Indeed, a large fraction of the manuscript is dealing with methodological issues. However we feel that this material is needed to establish the robustness of the estimates. In a revised version, we will try to make the scientific parts better identifyable, by possible re-ordering of the material and clearer section headings.

For example, much of the discussion I expected to see in terms of comparison to

other recent reconstructions was in the Appendix. This is quite interesting, and could be moved to discussion.

The material in the appendix is limited to the long-term mean and the secular trend of the global flux. The discussion of the long-term mean flux was not placed in the main part because this manuscript is dedicated to variability such that the mean flux is actually off-topic. The discussion of the secular trend would have been a very interesting part of the variability, but unfortunately it turns out that we cannot estimate the secular trend robustly by the presented method.

Concerning comparison to other pCO2 mappings, we feel that further detail would go beyond the scope of a paper like this, presenting a new calculation. We feel that it is better to first introduce the individual products and to only then bring them together in a separate study.

How do we know whether a "spin up" of 6 years (1951-1957) is adequate?

The length of the spin-up period was chosen during the development based on test runs with different lengths. The main purpose is to ensure that the initial condition of the mixed-layer budget equation does not influence the solution of the budget equation after the spin-up period any more. This can be tested by direct comparison of the time series after differently long spin-up periods.

Why does including the final year (2021) cause problems?

We are not sure which statement in the manuscript this question refers to - can you specify?

Don't we expect the sensitivity of the internal DIC fluxes to SST, wind speed, and interannual SST variations to be smooth because internal DIC fluxes are forced to be smooth within the approach?

In the regression run (R), the sensitivity fields γ_i are directly forced to be smooth by the a-priori correlations implemented. The a-priori correlations of the sensitivity fields have been implemented in the same way as the a-priori correlations of the internal DIC fluxes in the explicitly interannual mapping (E) or the hybrid mapping (H).

We note that the internal DIC fluxes in the regression run are not directly subject to any smoothness constraints, that is, they can become as unsmooth as the explanatory variables are.

The method is complex. The additional sensitivity studies are challenging to understand as presented in the main text. Moving these fully to supplementary would allow more emphasis on the main results. In the main text, it should be sufficient to state to that the sensitivity tests have been done to explore X,Y, Z, and A,B,C are the primary things learned.

As mentioned above, we feel that the uncertainty cases are needed to establish the robustness of the estimation. In order to make the Methods section easier anyway, we will move the description of the uncertainty cases to a separate sub-section, in order not to disturb any more the progression from the regression (Sect. 2.1.4) to the hybrid mapping (Sect. 2.1.5).

Some of the figures have many subplots, which takes away from the readers' ability to see the main findings. A good number of the subplots – those not discussed in the text - could be moved to supplementary to make room for

larger subplots of interest.

We will consider how different ocean regions could better be prioritized according to this suggestion. On the other hand, we feel that a complete representation of the ocean is actually desirable in Figs. 3 and 6.

In the comparison in Figure 9 to other estimates, the text acknowledges the uncertainty on the river flux adjustment, but this uncertainty is not presented in the figure. Nor is the uncertainty in Fant from Gruber et al. 2019. If these were included, there would not be far less appearance of discrepancy between estimates. Instead of a dash line and a dotted line, a shaded area would be a better way to present this.

We agree, thank you for this suggestion.

It would be helpful to add discussion of what other reconstruction approaches can learn from the findings here.

We fully understand that colleagues working on pCO2 mapping methods themselves are interested in information helpful to their own developments. However, we feel that explicit statements in this respect are very hard to provide here, because that would necessarily be specific to which particular other technique we would think of. But we are certainly very happy to discuss such questions by direct communication.

Page 22, line 21-26. Replace colons with periods here.

Thank you for pointing this out.

Page 22, last paragraph / Figure 10 and 11. Yes, an estimate of the pre-observed trend is needed, but it seems that this method is over-amplifying that trend. Can the authors propose approaches that might improve this going forward?

We already tested several options, but have not yet found a fully convincing solution. We are working on this issue and hope to improve the trend in a next version.