

Interactive comment on “Feasibility of reconstructing the basin–scale sea surface partial pressure of carbon dioxide from sparse in situ observations over the South China Sea” by Guizhi Wang et al.

Anonymous Referee #2

Received and published: 7 November 2020

The authors have used a remote-sensing-based pCO₂ field to derive EOFs, fit those EOFs to in situ pCO₂ observations collected over almost 2 decades, and then used the scaled EOFs to estimate the full surface pCO₂ record in the South China Sea.

It is a very interesting paper and many parts of it are clearly communicated, but it is also incomplete. The method validation and uncertainty quantification are missing. These should be an entire section of the paper and not just an added sentence or two, so the paper should be returned to the authors for major revisions.

Printer-friendly version

Discussion paper



It is a bit unclear whether this paper is presenting new data along with a new method or just a new method. There are two cruises in 2005 and 2006 with a reference given as "this paper" and, if these data sets are truly being published for the first time in this paper, then the manuscript should highlight that there are new data in the abstract. This would raise the value of this paper if there are indeed new data being made available along with the analysis. I might have missed the text that explained this.

The part of the paper that deals with the pCO₂ mapping approach is not yet complete because the authors have not assessed the uncertainties of their approach. I recommend one or two exercises. First, the approach should be repeated after removing some of the in situ pCO₂ measurements. Each cruise should be removed, one at a time. After removing a cruise, the analysis should be conducted using only the remaining data. Then the withheld cruise can be used to quantify how good of a job the mapping procedure does at reconstructing the withheld cruise. This should be repeated for every cruise in the dataset to get bulk statistics. If there is only one cruise worth of data in each year, then (I believe this reconstruction wouldn't work and instead) large swaths of latitude/longitude should be removed from the cruises and the remaining data should be used to reconstruct the missing data. This will allow the errors in the approach to be quantified. Second, if a model is available for the South China Sea that has pCO₂, then the model can also have the Bai et al. 2015 approach applied, be subsampled where the cruise measurements are, and be analyzed in the same way proposed here. This will reveal both the point-by-point reconstruction errors and allow the uncertainties for the overall pCO₂ average estimate, for example, to be quantified. Currently, the validation is left as an unsupported statement that the results look about right, which is insufficient for publication of a paper describing a quantitative method.

There are other smaller problems that should also be addressed if a revised version of the paper is submitted:

1. The model should not be used in any region where there is no fitting data. This

[Printer-friendly version](#)[Discussion paper](#)

includes most of the South China Sea south of ~ 12.5 N.

2. There should be an assessment of how good of a job the Bai et al. approach does at reproducing the in situ observations in a RMSE and bias sense. The estimates from this approach should be compared to the measurements from the data sets that are used here (and that Bai et al. did not use to design their routine). If the Bai et al. approach gives a different average pCO₂ than the in situ measurements, then the climatology created from the remote sensing product should not be used to generate the Standardized Anomalies of Obs. Data (as indicated in Figure 1). I believe an independent climatology would then be needed. Otherwise, a significant average bias would have to be compensated by a large average value for one or more EOFs. In a best-case scenario, that would be EOF 1, but if, for example, the observations were mostly found in the dark blue patch of Figure 6c then the resulting reconstruction would be problematic. It seems likely that a large average value of EOF 3, which is highly variable spatially, would then be fit to the measurements to fix a homogenous bias between the in situ and remote-sensing records. This is just one example of the kinds of problems that could occur if the Bai reconstruction doesn't adequately resolve the mean or the variability. If nothing else, the Bai et al. validation should be discussed in this paper.

It would also be interesting to see how this approach compares to competing approaches, for example a neural network that relates the in situ pCO₂ measurements to seawater property values that can be measured using remote sensing. This approach is more commonly used in global reconstructions. The Bai et al. approach is another clear competing approach.

Specific comments:

15: consider deleting “capacity”

23: “The reconstructions always agree with observations.” Delete or quantify this statement. The agreement is not absolute.

28: The ocean

36: The sea-air CO₂ flux is the negative of the ocean carbon uptake, so this sentence is partially tautological.

37: This sentence has several language errors. It also needs to be better-quantified or referenced. What is the decorrelation length scale for pCO₂ generally? How much of the ocean is constrained by those measurements alone without the newly proposed spatio-temporal mapping techniques? Mostly, I think a reference should be added to this sentence that supports this statement.

57: References needed for RS pCO₂ here.

Figure 1. What is meant by “standard deviations”? Standard deviations of grid values, or deviations of values within each grid cell?

Figure 2. Consider changing this map to a 2 dimensional histogram showing number summers with measurements (probably with colored bins).

109: These estimates were. . . change “data” to “estimates” in this section since pCO₂ is not measured. In figure 1 as well.

138: Where is this symbol used?

141: how many EOFs were used? Say here.

182: This only shows the fields. One must compare this field to other figures to get an indication of how well the reconstruction performs. A plot showing differences between observed and reconstructed values is required.

187: It is not enough to say “we fit the model to the data, so it fits the data.” Statistics of goodness-of-fit should be presented. Furthermore, demonstrating that the method works requires withholding several cruises worth of data from the training data set and then using those cruises to verify that the method reconstructs the withheld data. Statistics and plots are required to quantify how well the reconstruction does.

[Printer-friendly version](#)[Discussion paper](#)

195: What is meant by reasonable?

205: it is unclear what is meant by “the large spatial gradient of in situ data.”

214: 2.383 is given to excessive precision. An attempt should be made to quantify the uncertainty and the data should be reported to the appropriate precision.

229: why would a higher rate be expected in a marginal sea? I would argue that 2.4 uatm/year is completely within expectations of the atmospheric pCO₂ trend over this time period given the large uncertainties in this approach and the likely natural variability in surface ocean pCO₂.

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2020-167>, 2020.

Printer-friendly version

Discussion paper

