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Interactive comment on "Data-based estimates of the ocean carbon sink variability – first results of the Surface Ocean pCO_2 Mapping intercomparison (SOCOM)" *by* C. Rödenbeck et al.

Anonymous Referee #2

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The manuscript presents a comparison of a number of spatial-temporal interpolation methods used to map surface pCO2 data of the newly established large data sets. This is a timely and very useful effort in itself. The authors then proceed to infer seasonality and interannual variability in regional and global air-sea CO2 fluxes from the mapped pCO2 interpolations.

The manuscript is generally well written, though sometimes the heavy use of footnotes and comments in brackets disrupts the flow and makes some details difficult to understand. Why not give up on footnotes and simply include them in the text (perhaps in brackets like this?). I don't think that there is any reader who will not read the footnote - thus jumping to the bottom of the page and than not finding back into the main text

can be avoided when including it in the main text.

Overall I found the manuscript a very interesting and useful scientific contribution, but have a few general issues and a number of more specific comments listed below.

General comments:

(i) Given that intercomparisons tend to be tedious efforts with difficult choices to be made as to how present exciting science without falling into a ranking trap, the authors have done a great job here. However, I still think that the readers would like, and should receive, some more information about what methods are 'best' for regional or global purposes, what aspects of different methods are most/least problematic, and where future research should focus on. It would also be good to get back to the very good scheme of Fig.1 and say in the concluding section of the paper to what extent this scheme has been confirmed by your study.

(ii) the maps are compared against the SOCATv2 gridded product. It is not clear to what extent the SOCAT data are independent and to what extent these have been used by all/some interpolation methods. Are all interpolated maps at the same level when it comes to a fair comparison against SOCAR data?

(iii) Wanninkhof 1992 is used to compute air-sea CO2 fluxes from pCO2 differences and uncertainties in air-sea gas exchange are not considered. It would still be useful to compare uncertainties in CO2 fluxes due to pCO2 interpolation to uncertainties in the air-sea gas exchange. Which one is larger?

(iv) Seasonality is shown for the relatively well-behaved North Atlantic subtropical permanently stratified biome (sec.4.1.1). Results for the East Pacific Equatorial Biome look much worse (typical deviations of 20-30uatm compared to 10uatm in the Atlantic) and are shown only in the Appendix (Fig.A1). This is relevant as the East Pacific is the region used in the analysis of interannual variability (sec.4.1.2). Apparently the 12month running mean helps to remove some(?)/most(?) errors in the description of

the seasonal cycle? This has to be discussed in more detail. the first point of the conclusions, that seasonality is constrained mostly within 10uatm may have to be revised.

(v) Having read all the positive comments about the interpolation methods and interannual CO2 flux variability in the equatorial Pacific, I was quite surprised to see so little agreement in the interannual global sea-air CO2 fluxes (Fig.5c). It would be very useful to understand this better: What are the regions responsible for the very different behavior of different interpolation schemes? Where would more data be most useful? Or do some interpolation routines particularly well/poorly in some regions? This is potentially a very important figure that may be copied and used a lot. Thus it would be reasonable to provide a robust explanation to avoid giving the impression that "data-based estimates of interannual CO2 flux variability are all over the place".

Specific comments:

p14051,l9 spread p14051,l9 "mapping methods with closer match to the data also tend to be more consistent with each other." Not clear what is meant here by "consistent". It would be redundant information that points closer together (because closer to the data) are somehow more 'consistent' with each other.

p14052, l14: For model tuned against all WOCE and pre-WOCE data (e.g. ECCO), I would expect an enhanced model skill in predicting trends and variability during the tuning period. Perhaps adding 'beyond the tuning period' expresses better what was meant here? Still, I'm not sure whether this is correct. I think I would always prefer a tuned model over an untuned one, and I would also think that a tuned model could be better outside the tuning period. So I think "cannot be expected" is not always right.

p14054,l24. How can process model simulations provide information on correlation scales? How do you ensure that the process models resolve the right scales and feedbacks/correlations? Perhaps I didn't understand what was meant by 'process' model?

p.14055 "for a quantity determining pCO2" sounds very cryptic and the reader doesn't

have much chance to understand it. Can you explain this better?

p14056,l21 I don't understand why SOM have this advantage and FFNs don't have it. Why should FFNs need a-priori knowledge? Is this really the case? Why can't you feed the same information into a SOM and FFN?

p14058, l1 In high latitudes (where, presumably, most of the data gaps occur) this procedure is different from that used by Takahashi. Why do you assume that pCO2 of high-latitude surface waters increases at the same rate as atmospheric pCO2? Isn't the upwelling of deep & old waters keeping a tendency towards pre-industrial surface-water pCO2 despite increasing atmospheric pCO2?

p14059,l10ff: "we use the amplitude (temporal standard deviation) of the average difference between map and data". This is not completely clear: what average difference do you mean? Spatial and annual difference for each biome?

p14062,l11 "where data exist" Does this refer to locations in the maps where data used by the interpolation exist, or does this refer to locations of the SOCAT gridded product with data in this particular month? Please clarify.

p14064,l20ff and Fig. 4c. Not clear how you compare biome/yearly pCO2 values with monthly SOCAT data. Shouldn't one see the seasonal cycle?

p14066,l26. Not clear what is meant by "the higher IAV amplitudes are likely. Higher compared to what? Likely = plausible? ?

Fig.1 Why does a linear regression require more model assumptions than a nonlinear one?

Fig. A6: What is the meriodional empty stripe in the Pacific Ocean UEx-MLR? What are "other reasons" in the last sentence of the figure caption?

Interactive comment on Biogeosciences Discuss., 12, 14049, 2015.