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Interactive comment on “Sea-air CO₂ flux estimated from SOCAT surface-ocean CO₂ partial pressure data and atmospheric CO₂ mixing ratio data” by C. Rödenbeck et al.

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This paper describes a new approach to incorporate observations of the difference between atmospheric and oceanic pCO₂ into atmospheric inversions to estimate air-sea and air-land fluxes of CO₂. The authors develop a novel inverse approach that involves adjusting a simple model of carbon in the mixed layer using atmospheric and oceanic carbon data, which could potentially be quite valuable. To the best of my knowledge, this paper also represents the first attempt to incorporate the SOCAT data into atmospheric inversions, and this dataset is likely to add substantial new information about both ocean and land fluxes of CO₂. However, I found it difficult to understand the

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details of the approach from the manuscript as written, and therefore I do not feel able to undertake a complete evaluation the inverse methodology at this time. I recommend that the authors substantially edit the manuscript to improve the clarity. If they choose to do so, I would look forward to reading the next version very much.

General Comments:

I found the discussion of the description of the methodology difficult to fully understand, even though I have a background in atmospheric inversions. Each of the components of the model was well described in the appendices, but it was very hard for me to understand how all of these pieces fit together or develop a sense for how the ocean carbon data interact with the atmospheric inversion. The authors attempted to show this with a schematic (Figure 1), but I didn't find this figure helpful or informative. Again, it is very good for showing the pieces, but not for really understanding how they fit together. The authors could improve this by expanding section 2.1 to provide a better overview for the inverse framework and developing a better concept for how to present this framework in Figure 1.

The authors made good use of appendices to include all of the detail a reader would need to replicate the work without making the manuscript too dense for a more casual reader. However, it would make the paper much more readable if a bit more detail appeared in the main text. To my mind, all material that most readers would need for a general understanding should appear in the main text, and extra details that most people won't need should go in the appendix. Some specific examples are described in the detailed comments section below.

This manuscript goes into great detail explaining the methods and the comparison with independent data, but I found the analysis of the results to be a bit thin. Section 4.1 indicates that the ocean pCO₂ data provide a powerful constraint on the land regions, but the details of the differences between the land fluxes from this approach and traditional inversions and what they mean for the carbon cycle are never really discussed.

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Section 4.4 touches on the idea that the ocean internal carbon sources and sinks could tell us something fresh about ocean biogeochemistry, but never go into any depth. The authors might prefer to make such an analysis the basis of a follow-up paper in order to keep the manuscript length manageable, but at least some of this analysis should be included.

The manuscript includes a great deal of discussion of the pCO₂ data, but the GLODAP DIC data are also included indirectly through the mixed layer depth model. It would be interesting to also discuss this and how the DIC data influence the inversion. There can also be biases induced by using a gridded dataset based on somewhat sparse data, and some discussion of this might be appropriate.

The authors indicate that the SOCAT data was only used to improve the seasonal cycle (p 2279, line 15). I'm curious about how any underlying trends in the pCO₂ data have been handled over the study period or whether any thought has been given to how much the smooth seasonal cycles predicted by the inversion could be biased by inter-annual variability. I am also interested in seeing what the pCO₂ residuals look like (values predicted by the inversely estimated fluxes minus observations used to constrain the inversion) and if there is any spatial or temporal coherence in these residuals.

In section 4.5, the authors suggest that this could also be considered as a very sophisticated mapping tool for the pCO₂ data. It is my understanding that pCO₂ can change quite rapidly across fronts and other ocean features, whereas the method employed in this approach enforces a substantial degree of smoothing in space and in time through the a priori correlations described in section 2.4. Would the authors please comment on whether they see this as a limitation of this application.

Appendix B: This synthetic inversion represents a good first step, but using a heavily smoothed synthetic dataset with only smooth interannual variability circumvents a lot of the issues that could cause biases in a real inversion. This should at least be discussed.

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The quality of the English and grammar was generally very good, but the manuscript (particularly the appendices) could benefit from a more thorough proof read prior to resubmission. Since I have requested substantial revisions, I have not noted specific cases in this review.

Detailed comments:

P. 2278, top: It could be quite useful to include a concise discussion of the atmospheric transport model. How did it do in intercomparison experiments (e.g. Transcom)? What about evaluation against aircraft data (e.g. Stephens et al., 2007)?

P. 2278, line 4: Including the references to Wanninkhof et al., 1992 and Naegler et al., 2009 (as well as in the appendix) would convey a lot of information to people who are familiar with these parameterisations without adding a lot of bulk to the paper.

P. 2278, line 12: It would be useful to include a short discussion here about how the authors expect the assumptions and approximations described here might affect the results. Are there good arguments to be made that effects would be small? Could they lead to biases? Is anything understood about whether the results would be biased high or low?

P. 2278, line 24: One of the principal advances of this inversion over previous methods is the inclusion of the SOCAT data. I would find it helpful to have here (or in the introduction) a little more information about the SOCAT dataset. How good is the spatial coverage of SOCAT? Where is it weak/strong? How do these data compare with the classical Takahashi climatology, which has been used to constrain inversions through priors for a long time? How much overlap is there in terms of the raw data included in each? What are methodological differences in how the data have been compiled?

Sections 2.5.1 and 2.5.2: In both of these cases, it would be nice to have a concise summary of what was done and what the results were, even if the full details are in the appendix.

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P. 2281, line15 on: This is where it would be helpful to have a deeper understanding of how independent the SOCAT and Takahashi datasets are. Are all the Takahashi data also included in SOCAT? If so, should we really be impressed about agreement between the two?

P. 2281, last two lines should be rewritten to improve grammar.

P. 2283, lines 23-24. This is similar to the finding of previous studies that included both oceanic and atmospheric data (e.g. Jacobson et al., 2007). This should be noted.

A1.2: It would be helpful if the authors would start with a couple of sentences laying out the derivations that they are about to undertake, so the reader can see where they are going more easily.

Equation A6: I think a beta must have been accidentally left out here (it reappears in A7).

A1.3: It would help guide the reader if the authors pointed out that fint is the quantity that is estimated by the inversion in the first paragraph.

P. 2297, lines 5-9: Confusing sentence; please reword.

P. 2298, lines 4-8: I was wondering if this history flux leads to dipoles in the time domain (for example, an over estimate at t=1 leads to a compensating under-estimate in t=2), or if the regularisation scheme corrects for this.

P. 2302, lines 22-23 and P. 2303, line 12: I'm curious about why the spatial correlation length/time scales were changed and how the new values were chosen.

P. 2303, Line 17: It's not clear to me that assuming an a priori fint=0 is the same as assuming that you "have no information on the internal fluxes". In the absence of data, the a posteriori tends to damp to the a priori value. So, aren't you really assuming that the fint is only a small modulation of equation A.16 with this prior? Or, perhaps I'm not fully understanding the method.

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