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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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Received and published: 29 October 2012

Response to Referee comments by R. Wanninkhof

We thank Rik Wanninkhof for his interesting and detailed comments. In the following reply, his original comments are included in *italics*.

Review of: Sea-air CO₂ flux estimated from SOCAT surface-ocean CO₂ partial pressure data and atmospheric CO₂ mixing ratio data C. Rödenbeck, R. F. Keeling, D. C. E. Bakker, N. Metzl, A. Olsen, C. Sabine, and M. Heimann

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Rödenbeck and co-authors assimilate the recently released SOCAT surface water $p\text{CO}_2$ dataset into a global ocean surface mixed layer assimilation scheme as part of a global carbon inverse. They compare the results with a climatology developed by Takahashi et al. Their overall conclusions are that constraining the atmospheric inversion with surface ocean $p\text{CO}_2$ data improves the land CO_2 estimates, and that the ocean assimilation scheme provides a $p\text{CO}_2$ field similar to that of the Takahashi climatology with some important exceptions. The strength of the paper is that it exhaustively describes the procedures and assumptions in this research effort. The text is clearly structured which is important for a subject matter that is inherently confusing to those not familiar with inverse and assimilation procedures. The results are well presented with illustrative figures. It however does not provide a clear rational of some of the manipulations that at times seem a bit convoluted. The conclusions that an better surface ocean constraint will improve land CO_2 fluxes, and that the assimilation of the SOCAT data into a "diagnostic data-driven model of mixed-layer biogeochemistry" yields results similar to the Takahashi climatology are not surprising.

The primary goal of the work was to provide a data-based estimate of the sea-air CO_2 flux, that also includes short-term and interannual variations. In the paper we tried to present this product and to evaluate it. The comparison to the Takahashi et al climatology was meant to ensure that those modes of variability common to both products –the seasonal cycle and its spatial pattern– do agree to the expected extent, confirming that the results of both methods are informed by the data and not primarily method-dependent.

Beyond the before-mentioned goal, an additional reason to choose the method was to be able to use further data constraints in a consistent framework in planned future extensions.

We realize that we had not phrased our aims clearly enough, and re-wrote the respective parts.

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For areas/seasons where there are differences the discussion is weak on attribution and which method is "right".

We reformulated this section to make it more clear, including the link to the differences in method. Again, however, the main purpose of the comparison was mutual confirmation.

General comments:

-Title: while it is nice to give the community based SOCAT effort some airplay, it is a bit misleading. Readers (including myself) are expecting a flux estimate like provided in Takahashi et al. (2009).

We indeed considered sea-air fluxes the main target quantity, but had decided to rather discuss pCO₂ for practical reasons. We now added a figure showing the seasonality of sea-air fluxes.

The title has been revised as well.

The SOCAT dataset has little unique contribution in constraining the mixed layer model. Indeed, it is the mixed layer model that constrains the fluxes.

No, it is the pCO₂ data that are the primary source of information, especially for the seasonal cycle. This can be clearly seen from the large difference between the prior (mixed layer model without the data) and posterior (with data), see Figs. 7 and 8 (the prior for all regions is given in Fig. 10). We added a new section that not only illustrates the mechanics of the method but also tries to make the data contribution more clear.

The title must include mention of inversion and surface mixed layer model.

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The title has been substantially revised.

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- Abbreviations are a bit cumbersome. For example, pCO_2 with CO_2 as a superscript is unconventional. The authors should use CO_2 as subscript.

We are aware that some symbols are slightly unconventional. The notation was chosen (1) to allow further subscripts to "pCO₂" (m, a, obs, mod) and (2) to allow a consistent notation between all model quantities (p, C, f). Table 2 defines all symbols.

- If the authors are focusing on sea-air CO_2 fluxes as title suggest they should include comparisons with other estimates. [e.g. as provided in the RECCAP effort]

We already compare to Takahashi et al (2009) as a well-accepted and widely used reference. Including more comparisons would make the paper even longer. We feel that a more comprehensive comparison would be more appropriate for a dedicated further study.

- Using both Appendices and supplemental information is peculiar

This is chosen to reflect the different levels of importance. We feel that the model documentation in the appendix is essential and cannot go into a supplement. On the other hand, the supplement would be much too long to be moved into another appendix.

- The calculations in the appendices on carbonate chemistry (1.2) and mixed layer DIC budget (1.3) are convoluted.

The carbonate chemistry (1.2) enters the DIC budget equation (1.3) only via kDIC. The referee probably refers to Eq (A20), which we agree does interrupt the flow of thought

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(it had been placed here because it is only needed for the calculation of the history flux). We moved Eq (A20) into a footnote.

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It expect that the approximations and linearizations save computing time but it would make a lot more sense to calculate the state variables (DIC and TA) and propagate these parameters through the model. For gas transfer the TA and DIC can be used in to determine pCO₂ rather than invoking DIC gas exchange.

"DIC gas exchange" is only used as a short-hand to simplify the budget equation (see p2295 line 8). Notwithstanding, DIC and alkalinity is indeed propagated through Eq (A7) to give pCO₂, and pCO₂ to give the sea-air flux, just as the referee suggests (cmp. Fig 1).

- Figures should be presented using absolute values rather than with the mean subtracted. Subtracting the mean causes a loss of information content in comparisons.

We had subtracted the mean on purpose to remove features that are not focused on in the paper, but also due to the issue on how to comparably define a mean seasonal cycle including the mean given the strong rising trend and interannual variations of pCO₂. However, we now follow the referee's suggestion, using atmospheric p_a CO₂ to make the mean comparable.

- The conclusions state: "and - to some extent - interannual variations." I missed the discussion of interannual variation.

This item refers to the discussion in Sect. 4.3. This material has now been moved to the companion paper discussing interannual variations.

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- When possible comparisons should be quantitative rather than qualitative.

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We added some more quantitative measures of comparison, such as the seasonal bias in the pCO₂ residuals.

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- The paper is very long with too much subject matter. I have listed some sections below that could be omitted for sake of clarity and focus.

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Please see our responses below.

Specific comments: Page 2274, line 19: change "global warming" to "anthropogenic climate change"

Has been done.

Page 2275, line 24: I do not understand what is meant with "delayed sea-air CO₂ fluxes"

We reformulated the sentence into "Even if ocean-internal processes cause carbon sources and sinks of comparable variability as the land biosphere, the resulting sea-air CO₂ exchange is much less variable and smoothed out in time because the carbonate chemistry of seawater slows the equilibration rate of dissolved CO₂ with the atmosphere."

Page 2276, line 13: "this study proposes an extension of the atmospheric inversion method by a diagnostic data-driven model of mixed-layer bio- geochemistry" This is really the subject of this paper and should be articulated earlier

We included this now also into the abstract.

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Page 2278, line 9: "The dependence $pCO2m = pCO2m (CDICm)$ is" this annotation is confusing. Are the authors stating that $pCO2$ is a function of DIC ?

Yes, this had been meant. We now state that verbally.

In equation A4 the authors expand the functionality. Also, as mentioned above the linearization routines are confusing and exact determination would be much preferred.

We agree that full non-linear chemistry parameterization would be preferable, but see p2293 lines 10-13: "The remaining dependences of $pCO2$ are non-linear, but monotonic and can be linearized to good approximation (Sarmiento and Gruber, 2006). Linearization of the dependence on C^{DIC} is important here in order to be able to use the fast minimization algorithm of Rödenbeck (2005)."

Page 2279, line 6: "Similar to the unknown sea-air flux in the pure atmospheric transport inversion, Bayesian a-priori spatial and temporal correlations have been implemented to enforce the flux field to be smooth on scales smaller than around 1910 km (longitude), 960km (latitude), and about 2 weeks (time). " Explain how this large scale smoothing effects the results. That is, how important is the data constraint?

We added more explanation on how the method works, and a figure illustrating the effect of the correlations. We also show and discuss more explicitly that the estimated seasonality is constrained from the data, not from the model.

Page 2283, line 8: "There is relatively good agreement in phase and amplitude of the seasonal cycle" Quantify this.

The section as been moved to a companion paper anyway to shorten this paper.

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Page 2283, line 12: typo- missing "e": "parameterization"

Done.

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Page 2285, line 8: "4.3 Prospects: interannual variability", this is an advertisement for future work; consider deleting

We moved it to a companion paper.

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Page 2286, the section on linking nutrients is weak. The arguments appear somewhat circular, and the Redfield ratio of over 100 between C:P mean the errors/data limitations in P will overshadow any meaningful interpretation. (see footnotes 3 and 4). Consider deleting section

The main purpose of this section was to show that f_{int} are meaningful beyond being a mathematical device to fit the data. We rewrote the section to make this more clear. Some technical details have been moved to the appendix to shorten it. Also, NO₃ has been removed for shortness.

Page 2287, line 8: typo- missing "m": "issing"

Done.

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Page 2291, line 20: provide the global scaling factor here in addition to putting it in the table

We decided to refrain from putting numerical values twice.

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Page 2292, line 4: $pCO_2(a)$ is not a straight proportionality to XCO_2 but rather a function of P

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We moved the equation of $pCO_2(a)$ here to make that clear.

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Page 2292, section A1.2 This section is unduly confusing with unquantified uncertainties in linearization and approximations.

All of the equations are needed in a complete description, but we tried to motivate the steps better as suggested by the other referee. We also moved part of App A1.3 to A2.2 as it is inversion-related material.

Concerning the uncertainties, we added more sensitivity cases into the uncertainty range in the figure. Also, it is important to note that any uncertainties in the chemistry do not affect the estimated pCO_2 field or sea-air flux much, but only DIC and f_{int} ; we made this more clear now.

Page 2292, line 25: pCO_2m appears both on left and right side of equal sign. Please check all equations carefully.

The left-hand side of Eq (A4) has the actual pCO_2 , while the right-hand side has pCO_2 at reference temperature (note the change in the functional arguments).

Page 2294, line 9: My impression is that Egleston et al. (2010) determined the gamma response factor from R . It seems convoluted to get R from gamma. Again, the modeling approach of carbon chemistry is very convoluted and unnecessarily complicated.

The data set originally provided from the Egleston et al study is γ_{DIC} (the Revelle factor was just given as the supposedly more commonly known quantity). We merged Eqs (A8) and (A9).

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All relations used to approximate carbonate chemistry are taken from well-accepted textbooks or papers. Some reformulation has been done to clarify.

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Page 2297, line 2: "and that all salinity variations are related to freshwater fluxes" This probably is OK globally but not regionally. (riverine input, ice melting/freezing, (small) DIC input by rain (see e.g. Turk et al. 2011) .

We agree that this is a source of uncertainty, but we are not aware of global data sets currently available to parameterize that better. While this issue is on the agenda for further improvements, note again that these uncertainties do not affect much our main target quantities $p\text{CO}_2$ and sea-air flux.

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Page 2298: "Nevertheless, we simplify the numerical implementation by not calculating f DIC hist for the actual concentration field CDICm , but rather always from seasonal CDICm variations inferred from the $p\text{CO}_2$ climatology (Takahashi et al., 2009)", I do not fully understand this. Also, discuss the uncertainty/error in these assumptions.

Any approximation of f_{hist} will just lead to compensating adjustments in the internal flux, thus effectively only change the a-priori flux slightly. It will therefore not affect the $p\text{CO}_2$ or sea-air flux field much.

Even in the a-posteriori internal flux, the approximation of f_{hist} can be corrected after the fact by recomputing f_{hist} from the actual DIC field. We had not done this before but are doing so now (relevant only for calculating the PO4 concentration). We describe this in the revised paper.

Page 2305: The Takahashi et al. (2009) climatology using a surface advection scheme with daily intervals. I expect that this data would be available rather than interpolating monthly data.

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In the context of the synthetic data, the exact time course of the “known truth” is not relevant (as long as it is similar to reality). The proposed use of original daily interpolation would therefore not change the outcome of the test. Moreover, tests based on model simulations (that even include more daily pCO₂ variability) give the same qualitative answer (see response to the other referee for more information).

Page 2306, line 23: Provide the amount of data available. The N Pacific is well covered with ships of opportunity.

Despite the frequent ship tracks at 65N, the northernmost pixels in the North Pacific remain only sampled occasionally. We added a reference to Fig S7.4 which gives this information.

Figures: labels are quite small (e.g. fig 8) and colored lines in figures are difficult to distinguish [for those who are color blind]. Perhaps include the data points when showing data in the model-data comparisons

We redid Figs 5, 7, and 8, including data points. The regional plots will be larger in the final OS layout. The colors have been revised according to the judgement by color blind colleagues.

Interactive comment on Ocean Sci. Discuss., 9, 2273, 2012.

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