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Ocean Science Discussions

Interactive comment on "Sea-air CO_2 flux estimated from SOCAT surface-ocean CO_2 partial pressure data and atmospheric CO_2 mixing ratio data" *by* C. Rödenbeck et al.

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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

Rödenbeck and co-authors assimilate the recently released SOCAT surface water pCO2 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 pCO2 data improves the land CO2 estimates, and that

the ocean assimilation scheme provides a pCO2 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 confusion 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 CO2 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. For areas/seasons where there are differences the discussion is weak on attribution and which method is "right".

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). The SOCAT dataset has little unique contribution in constraining the mixed layer model. Indeed, it is the mixed layer model that constrains the fluxes. The title must include mention of inversion and surface mixed layer model. - Abbreviations are a bit cumbersome. For example, pCO2 with CO2 as a superscript is unconventional. The authors should use CO2 as subscript. - If the authors are focusing on sea-air CO2 fluxes as title suggest they should include comparisons with other estimates. [e.g. as provided in the RECCAP effort] - Using both Appendices and supplemental information is peculiar - The calculations in the appendices on carbonate chemistry (1.2) and mixed layer DIC budget (1.3) are convoluted. 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. - Figures should be presented using absolute values rather than with the mean subtracted. Subtracting the mean causes a loss of information content in comparisons. - The conclusions state: “and – to some extent – interannual variations.” I missed the discussion of interannual variation. - When possible comparisons should be quantitative rather than qualitative. - 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.

Specific comments: Page 2274, line 19: change “global warming” to “anthropogenic climate change” Page 2275, line 24: I do not understand what is meant with “delayed sea-air CO2 fluxes” Page 2276, line 13: “this study proposes an extension of the atmospheric inversion method by a diagnostic data-driven model of mixed-layer biogeochemistry” This is really the subject of this paper and should be articulated earlier 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? In equation A4 the authors expand the functionality. Also, as mentioned above the linearization routines are confusing and exact determination would be much preferred. 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? Page 2283, line 8:” There is relatively good agreement in phase and amplitude of the seasonal cycle” Quantify this. Page 2283, line 12: typo- missing “e”: “parameterization” Page 2285, line 8: “4.3 Prospects: interannual variability”, this is an advertisement for future work; consider deleting 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 Page 2287, line 8: typo- missing “m”: “issing” Page 2291, line 20: provide the global scaling factor here in addition to putting it in the table Page 2292, line 4: pCO2(a) is not a straight proportionality to

XCO2 but rather a function of P and pH2O Page 2292, section A1.2 This section is unduly confusing with un-quantified uncertainties in linearization and approximations. Page 2292, line 25: pCO2m appears both on left and right side of equal sign. Please check all equations carefully. 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. 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) . 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 pCO2 climatology (Takahashi et al., 2009)”, I do not fully understand this. Also, discuss the uncertainty/error in these assumptions. 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. Page 2306, line 23: Provide the amount of data available. The N Pacific is well covered with ships of opportunity.

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

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