Biogeosciences
Open Access
Discussions

Interactive
Comment

Interactive comment on "Interannual sea–air CO$_2$ flux variability from an observation-driven ocean mixed-layer scheme" *by* C. Rödenbeck et al.

C. Rödenbeck et al.

christian.roedenbeck@bgc-jena.mpg.de

We would like to thank Anonymous Referee 3 for her/his detailed comments.

The manuscript 'Interannual sea–air CO2 flux variability from an observation-driven ocean mixed-layer scheme' by C. Rödenbeck et al. describes a new inversion-based estimate of the sea-air CO2 exchange from 1980 to 2011. The methodology employed in this study relies on a new mixed-layer model driven by the SOCATv2 fCO2 gridded dataset as well as several other gridded data products (e.g., GLODAP, WOA2001, NCEP. . .). The authors focus on the interannual variability of the sea-air CO2 fluxes, with a particular attention to the tropical Pacific (where data coverage provides the best constraint on the algorithm). The manuscript provides furthermore an in-depth evaluation of the new inversion-based estimates of CO2 fluxes (with comprehensive models

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

(e.g., RECCAP) and atmospheric-driven inversion product) demonstrating the benefits of using pCO2 data to constrain inverse estimates over the ocean. The manuscript is clearly structured and reads well. Nevertheless, I think this paper needs some clarification that have to be addressed first, and which prevent me of accepting this paper in its present form. Therefore, I recommend acceptance of this manuscript after some minor revisions.

While I am convinced by the authors' demonstration on the use of such an innovative methods, my largest concern is in the lack of discussion about the following points: (1) Uncertainties associated to the various data priors used to drive the mixed-layer models. For example, does biases in NCEP wind speed affect the amplitude and the variability of fgCO2 estimates ?

Yes of course it does, as discussed in Sect 3.3. On average this uncertainty (assessed based on the Naegler (2009) range) is reflected in the gray band, but we cannot exclude larger errors in certain areas or time periods from wrong spatial wind patterns. To test that, we performed an additional sensitivity case using CCMP winds (Atlas et al., 2011). Results mostly stay within the range based on Naegler (2009), with occasional exceptions still of the same order of magnitude. Unfortunately we cannot add this CCMP case to the grey bands as a regular sensitivity case because CCMP winds are only available from mid 1987 to mid 2011.

Note that the sensitivity case using cubic wind speed dependence also comprises some of the uncertainty related to spatial wind speed gradients.

Does the use of a seasonal climatology of mixed-layer depth instead of a data-product varying year by year (ocean reanalyses SODA, GLORYS, ECCO2) would impact the fgCO2 estimates ?

Part of that is also contained in the grey band, though it is not clear to which extent IAV in MLD is within the assumed range (0.5 . . . 2). To our knowledge, it is challenging to have realistic MLD in ocean reanalyses, which is why we did not employ it here.

(2) The evaluation with only one comprehensive models (RECCAP initiative accounts for more than 5 comprehen- sive models) and solely one inversion product driven with atmospheric measurements (s90_v3.5) while there are at least 11 inverse estimates in Peylin et al. (2013).

We did not intend a comprehensive comparison to ocean process models or atmospheric inversions, partly for the very reason that such comparisons are already available from the recent RECCAP papers. Rather, Sects. 3.6 and 3.7 are meant to make their specific points.

Specific Comments: P 3169 L9 I agree the amplitude of your fgCO2 estimates is consistent with the one estimated from atmospheric O2 data but the phasing differs substantially. Could you provide quantitative metrics (correlation between these two) to complement this statement?

Linear correlation coefficients are not high, as one immediately expects from the differences in relative peak heights and temporal shifts. However, both estimates have correlation with ENSO (see Fig. 6 and Rödenbeck et al. (2008), respectively). We argue that the level of agreement has to be seen in light of the (many) potential reasons for disagreement, as discussed. A note concerning the ENSO correlation has been added to Sect 3.5.

P 3169 L10 Since the abstract is quite short, could you expand a bit this latter in describing further the "discrepancies in detail"?

The formulation "discrepancies in detail" was intended to set the term "consistent" into perspective. We do this now by "roughly consistent". (The actual consistency test as of Sect 3.5 seemed to be too complex to be summarized in more detail in the abstract.)

P 3170 L7 Figure 4 presents a nice overview of the fgCO2 variability at interannual time scale. On this Figure, we show that oceanic domain contributing to the interannual variability are the North Atlantic, the North Pacific, the Southern Ocean and the tropical

Pacific. However, little attention has been paid in the main text to other modes of variability except ENSO. It would nice to either emphasize the focus on ENSO here or to complement the section 3.1 with other observed climate indexes.

We have looked at other regions as well, including correlation to other climate indices (such as PDO, NAO, SAM). Results are less clear than in the tropical Pacific. We felt that more analysis was needed to convey an interesting enough message. Given the length the present manuscript already has, we therefore decided to retain this for a follow-up study, for now leaving it at the remark in Footnote 6.

P3171 L1 Considering the use of one comprehensive model and one atmospheric inversion product, I would recommend to use "result" instead of "results".

Changed as suggested.

P3171 L12 Consider mentioning that these "observation-based" data are either gridded-data, climatology or reanalyses.

We tried to only give the most important points here. The full information is available in the companion paper (Rödenbeck et al., Ocean Science, 2013).

Could you also mention the temporal coverage of these data priors? Regarding the scope of the paper focusing on the interannual variability, I wonder if an interannual estimates of the mixed-layer depth (e.g., SODA, GLORYS or ECCO2 reanalysis) would have more adequate than an annual climatology? Nonetheless, I understand the motivation of the authors to stick to the methodology employed in the previous paper (i.e., Rödenbeck et al., 2013) but some discussion on this point is needed.

We agree that interannual MLD would be desirable, but see response to comment on MLD above. It is important to realize that errors in MLD do not have a large effect on the estimated IAV of pCO_2 and the sea-air CO_2 flux (even though they do have on the internal DIC flux).

P3175 L9-19 Since the use of "synthetic data" referred to section S4 in the suppleme-

nary materials. I recommend the authors to move this section in the supplements.

Sect 2.3.3 refers to "synthetic data" tests for the pCO2-driven diagnostic scheme (used to back up RoU), while Sect S4 refers to "synthetic data" tests for the APO inversion.

P3176 L9 Explain what is your consistency check. Consider mentioning p-value of your test on the Figure S6 and in the man text.

The term "consistenct check" was used for the assessment against the data used in the fit (as opposed to the independent data considered afterwards). The details are given in the Supplement. Fig S6 does not involve a formal statistical test, but rather argues that the size of the residuals is negligible compared to the interannual signals (well less than 2.5 per cent in the Tropical Pacific focus region).

Regarding Figure S6, Pacific 90S-45S residuals seem to have a positive trend. How do you explain this?

This apparent rise is only due to few data points South of 60S, but is not representative for the 90S-45S regions. We added the residuals North of 60S into the figure (small crosses) to illustrate that.

P3178 L1 Consider evaluating the degree of freedom used for your statistical test (correlation significance level) because the time series you employed have been filtered. I think it won't change a lot the significance of your results.

The assumption of 1 degree of freedom per year is actually based on the use of the interannual filter (which is roughly equivalent to yearly averages). We believe this to be fairly realistic, assuming a-posteriori correlations in the flux errors to be shorter than a year. We added a note on that.

P3179 Section 3.1 It would be nice to have a table of values for each region you menioned with its contribution to the global interannual variability of sea-air carbon fluxes. Temporal correlations between your estimates and MEI, NAO, PNA, NAM and SAM indexes would also complement this section.

We considered this suggestion, but as mentioned above, without substantial further analysis there would not be a clear scientific message to be conveyed from such as table. We therefore feel it should better go into a next paper, and leave it for now at the remark made in footnote 6.

P3181 L3 While I agree that the supplementary is already consequent. It would nice to complement your rebuttal with the additional figures corresponding to these "not shown" materials, especially here since your findings tend to demonstrate that pCO2 data provides a largest constrain on fgCO2 estimates than the other data prior driving your scheme.

We added this case as a further supplementary figure (now Fig S4) as suggested.

P3181 L19 The envelope you mention mirrors the sensitivity to uncertain parameters at 66

"Envelope" means minimum-maximum range here (Sect 2.3.4), not a sigma range. We chose minimum-maximum range because the set of sensitivity cases cannot be considered a statistical sample, hence we do not think that a sigma across the set would make sense.

P3182 L5-9 Factors used here were applied on interannual anomalies without changing the climatological mean-state or on nominal values (meaning that the mean-state of the MLD can be two/half-fold its values). Could further explain this?

The factors were applied as a plain scaling of the MLD values, thus affecting both the mean and the seasonal amplitude. We added "constant factors" for clarification.

P 3185 L6-8 Please provide quantitative information ratio of standard deviation, correlation between these two independent estimates.

We now provide the amplitudes of the interannual variations. Concerning correlations, see reply above.

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

According to your synthetic sensitivity test, you attribute difference in phasing to difference data spatial coverage. Could you explicit a bit further what is the actual spatial coverage of APO data?

We used 5 APO stations covering a long enough time period (we forgot to mention this information but added it now). These APO data coarsely cover the range of latitudes (mainly across the Pacific), but do not provide longitudinal information. Some information on the station distribution has been added to Sect S4.

P 3185 L26- "remarkable" seems to bit overstate considering the strong difference is phasing.

The term "remarkable" was not intended as an absolute statement (where it would certainly be inapproprate) but only relative to the expectation one may have in light of all the error contributions listed. We replaced by "noteworthy" to avoid misunderstanding.

P3186 Section 3.6 While I understand the arguments on the fact that NEMO-PlankTOM5 uses the same gas exchange parametrization as in your mixed-layer model, I would suggest to (1) include other RECCAP comprehensive models and (2) to provide quantitative metrics (correlation, ratio in standard deviation), at least for this only model but with simulations performed with other wind products (ECMWF and CCMP) as indicated in Wanninkhof et al. (2013).

As the SOCAT-based variability is larger than the model with the largest amplitude within RECCAP, the point of the section can already be made from the comparion to that one model.

For the sensitivity to wind product, we considered a sensitivity run (see reply above).

Besides, how compares your estimates with statistical estimations (1) at regional scale with neural network/MPR (e.g., Schuster et al., 2013) and (2) at global scale with MCMC methods (e.g., Majkut et al., 2014) ?

An intercomparison initiative of pCO2-driven CO2 flux estimates is underway, which

is intended to provide this information more comprehensively than would be possible within this paper.

P3187 Section 3.7 Here also it would nice to complement the evaluation of your fgCO2 estimates with the other 10 atmospheric inversions as described in Peylin et al. (2013) and with the inclusion of quantitative information: correlation, ratio in standard deviation.

The purpose of the section was to discuss the effect of the oceanic constraint in the context of atmospheric CO2 constraints. We did not intend a comprehensive comparison to inversions here, which is available in Peylin et al. (2013).

P3186, P3187 I suggest reorder sections 3.6 and 3.7 in one dealing with the evaluation and other dealing with the implications for the IAV/decadal trends and for the land carbon sink. These new sections would allow comparing IAV and decadal trends for both comprehensive model results and atmospheric inversion estimates. For the IAV/trends, I wonder if decadal trends you have identified in your product can also be found in atmospheric inversion? Can it be identified in other simulations performed with NEMO-PlankTOM5 forced by the ECMWF and CCMP winds? For the land carbon sink, it would be nice to see how behaves process land models that have contributed to RECCAP/GCP versus the other estimates.

This is an interesting suggestion which we considered. However, as we argue that the pCO2 constraint on the ocean CO2 flux is much stronger than the atmospheric constraint, we think that analyzing the ocean flux from the atmospheric inversion would not provide further insight here. The further analysis of the land fluxes would indeed be very interesting, but given the length and the ocean focus of the paper, we prefer to leave that for a follow-up study.

Interactive comment on Biogeosciences Discuss., 11, 3167, 2014.