

Interactive comment on “Interannual sea–air CO₂ 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

Received and published: 13 June 2014

We would like to thank Anonymous Referee 1 for her/his thoughtful comments.

The manuscript by Rödenbeck et al. provides a new estimate of the sea-air exchange of CO₂ and its interannual anomalies using a novel diagnostic model of mixed layer biogeochemistry, driven by observations of the SOCAT v2 dataset. The authors clearly discuss strengths and weaknesses of the approach and focus their analysis (of the main manuscript) on the tropical Pacific, given that the tropical Pacific is one region with the best data constraint and that the authors find the strongest variability of the sea-air flux in this region. One interesting aspect discussed is the time lag in the sea-air flux of CO₂ between East and West in the tropical Pacific in response to ENSO. Furthermore, the authors find that sea surface pCO₂ data constrain the sea-air flux

C2422

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



variability of CO₂ better than atmospheric CO₂ data, hence their recommendation to use pCO₂ data as prior to improve land flux estimates in atmospheric inversions.

While the method as well as seasonal variabilities have been previously published in Rödenbeck et al. (2013) this manuscript focuses on interannual signals, areas where these signals are best constraint by observations and the APO flux. The manuscript is well written, although it lacks clarity in some cases (see specific comments below). Studies focusing on basin-wide and global interannual CO₂ flux anomalies based on observations are rare, hence this study clearly provides an important contribution to our current understanding of the global carbon cycle. I would therefor like to recommend this manuscript for publication, after consideration of the minor points below.

Specific comments: The introduction is very short. One strong point mentioned above is the lack of (surface ocean) observation based estimates regarding interannual variations of the CO₂ flux and the challenges arising from the heterogeneity of observations. Rödenbeck et al. (2013) do provide this information in the introduction, hence I do not recommend to repeat what has been done already. However, I do believe that the current manuscript as a stand-alone-publication needs to include at least one paragraph highlighting current knowledge in terms of interannual variations of the sea-air CO₂ flux.

We enlarged the 1st paragraph as suggested.

There are several occurrences (page 3169 line 2; page 3169 line 23; page 3171 line 15; page 3190 line 2) where the authors refer to the SOCAT pCO₂ observations. To the extend of my knowledge SOCAT reports fCO₂. How has this been dealt with? Has fCO₂ been converted to pCO₂? Please clarify.

We used a fixed 1/0.996 conversion factor as in the companion paper (Tab 1 of Rödenbeck et al., OS, 2013).

Page 3171 line 1: "We further compare the SOCAT-based estimates to ocean process

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

model results.” Here, the reader gets the impression that a comparison to several (plural) model results will follow. Although the authors mention more models in section 3.6, the comparison is only done for one model, hence I would change plural to singular here.

Changed as suggested.

Page 3171 line 10: The authors mention the use of the Wanninkhof 1992 wind speed parametrisation but later on (page 3176 line 1-2) they use the the range of Naegler 2009 in their sensitivity test. Furthermore, Rödenbeck et al. (2013) uses the wind speed dependency of Wanninkhof 1992 with the parametrisation of Naegler 2009, hence I was wondering if the authors forgot to mention the use of Naegler 2009 on page 3171 line 10, or if they decided to use the parametrisation of Wanninkhof 1992 instead?

Naegler (2009) reference added as suggested.

Page 3171 line 12: The authors mention that environmental variables are listed in Table 1. Individual data like e.g DIC and MLD are first mentioned later on (page 3172 line 15 and page 3182 line 6, respectively). It would help the reader if the authors would refer to Table 1 again when new data are mentioned, so the reader can quickly check what has been used.

Addition done as suggested.

Page 3172 line 1: Did you average the observations onto the same 4x5 grid? Please clarify.

Yes we did. The formulation has been changed accordingly.

Page 3174 line 5-6: “pseudo-random realizations of a-priory errors and model-data mismatch errors” Please give a bit more detail on how these errors have been obtained.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

We added more information on the pseudo-random numbers with the required covariance structure.

Page 3175 line 1-5: I can not follow your logic why IAV in the drivers should be “overwritten”? Would you not expect that the pCO₂ IAV reflects the driver IAV? Please clarify.

No, this would only be the case if the pCO₂ field would be regressed against the drivers, which is however not done in the present scheme. "Drivers" here only refers to the fields driving the parameterizations (solubility, gas exchange, chemistry). We added a note to clarify.

Page 3175 line 20 – page 3176 line 7 (section 2.3.4): Which source of uncertainty has large and which one has small effects? There is information on page 3182 line 3-6 regarding the sensitivity of the ocean internal DIC sources/sinks and MLD, but not on the others.

Sect 3.3 names the dominating effects, ie., all other effects are smaller. We chose a slightly different wording to clarify.

Page 3176 line 10 - referenced supplement section 3.1, figure S5 and S6: The Pacific 90S-45S (bottom left in figure S5 and S6) appears to have both bias and scatter increasing in time. Particularly the strong increase in the annual mean scatter is striking. Is this due to data sparsity/heterogeneity and how does this effect trends and IAV results for the global ocean?

As these are regions with few data points only, calculated average biases are strongly affected by individual data points. As mentioned in Sect S3.1, the larger scatter (incl. its rise over time) are exclusively due to points located South of 60S. These areas have little share in the global average. We added the statistics for the open ocean into the figure to illustrate this point.

Page 3180 line 24-25: “...almost reverse the a-priori anomalies ...” - looking at Figure

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



5 it appears that the SFC run and the gray a-priori line are shifted (âLij1-2 years) rather than reversed.

As ENSO is almost periodic, sign reversal and time shifts have a similar effect on the time series, hence there is indeed some ambiguity between these terms. We feel that "sign reversal" is the more appropriate description because, in terms of origin of the anomalies, both the enhanced prior (following enhanced SST) and the reduced posterior (following the circulation changes) are linked to the (same) positive El Niño phase (as opposed to potential memory effects that would lead to real "time shifts").

Page 3181 line 12: Again, (same as on Page 3174 line 5-6) please provide some more information regarding the uncertainty setting.

The full specification is given in the companion paper (Rödenbeck et al, OS, 2013).

Page 3182 line 7-8: "... this range has been chosen to account for the missing interannual variations in MLD values used, ..." - How did you change the MLD values? E.g. did you change MLD for all years by a factor 2 and 0.5? It seems more plausible to create random variations for each year/month/day in order to test the effect of interannual variations in MLD.

We used constant factors. Though we agree that this is far from comprehensive with respect to variability, random variations would require a whole ensemble of random realisations for proper statistical treatment. This however seems a too high computational effort, given that the random variations would still be ad hoc, and that MLD errors do not dominate the most important variables (pCO₂ and sea-air flux).

Page 3184 line 3-4: "Sea-air CO₂ flux (...) and ocean interior carbon sinks are constrained from SOCAT" - This is a repeat and can be removed.

Has been changed to "are the quantities constrained by SOCAT"

Page 3185 line 7: "Both estimates are of roughly similar amplitudes". I am not convinced that you can say that looking at figure 7. Please provide numbers here to

underline this statement.

The temporal standard deviations 1993–2008 are 33.7 Tmol/yr for run SFC and 28.9Tmol/yr for the APO inversion, respectively, which we feel can still be considered "roughly similar". We added these numbers into the text as suggested.

Page 3185 line 10-11: "Both the SOCAT and the APO constraints suffer from incomplete spatial coverage, where the regions of good coverage do not necessarily coincide." - Figure 7 shows the APO comparison in the tropical Pacific. Since SOCAT coverage is good in this area (reference to Figure 4) does that mean the atmospheric inversion based APO suffers from poor data coverage, or is this a region where "regions of good coverage coincide"?

The APO inversion should be relatively well constrained in the tropical Pacific due to the Samoa (SMO) station. Though the APO inversion paper (Rödenbeck et al., Tellus 2008) itself only shows that 3 latitudinal bands can be resolved by the APO data, the sythetic-data test in Sect S4 suggests that IAV in the tropical Pacific can be constrained in the APO inversion to a useful level (compared to the signal). We added a note on that.

Page 3185 line 28: "the partial agreement in the interannual APO flux variation is remarkable" - Looking at figure 7, I think the term "remarkable" might be a bit overconfident, or does this statement refer to regions that are not shown?

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

Page 3186 line 17-18: "... using the same gas exchange parametrization . . ." - What about the wind product? Was the model output created with the same winds?

The relation between the flux in Fig 8 and the PlankTOM modelled pCO₂ was the same as between flux and pCO₂ in our scheme (both analytical formula and driving fields).

BGD

11, C2422–C2429, 2014

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



The model simulations themselves were done with different forcing fields (published version). As such, using our gas exchange parameterization potentially creates a certain inconsistency. In the revised version, we replaced the PlankTOM5 lines in Figs. 8 and S2 by the sea-air CO₂ fluxes directly computed by the process model. The differences between these two options is very small (both use similar formulations). As an additional advantage of the replacement, we can now show the comparison over the full time period of the figures. We would like to thank Erik Buitenhuis for kindly providing these process model results.

Page 3186 line 20-23: Why is this model used in particular and not one of the others (or several) mentioned in Wanninkhof et al. (2013)? You might give a false impression that there is actually a better model estimate - observation estimate agreement, considering that this model “agrees by far best with the SOCAT-based estimate”.

We did not intend a comprehensive comparison to models here, but actually to identify the model with the best match. As this is the model with the largest amplitude, this also makes a point for the RECCAP ensemble, providing some indication that the models with larger amplitude may be more realistic than those with smaller amplitude. We added this statement.

Page 3187 line 13: “... agree that the 1990-1999 period saw a negligible or even reversed trend . . .” - I do not think that you can say that trend estimates (plotted in figure 8) are in agreement, considering the $\Delta_{Lij} -0.1$ PgC/yr/decade trend of the model and the $\Delta_{Lij} +0.3$ PgC/yr/decade trend of the SFC run, even if these trends are not statistically significant.

We only state agreement in certain qualitative features, but do not claim any quantitative agreement. The word "qualitatively" has been added to clarify.

Page 3202 Figure 4: It would be interesting in terms of % (or absolute area) how many 4x5 pixels actually show an $RoU > 0.2$. It is very difficult to see in Figure 4.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Fig 4 was intended to identify the areas where analysis of the estimates may be worthwhile, but unfortunately the absolute RoU values have no easy interpretation. Therefore, the threshold of $\text{RoU} > 0.2$ is actually quite arbitrary, which would also limit the interpretation of the suggested area diagnostic.

Page 3203 Figure 5: Please expand the y-axis of the bottom figure, as part of the gray uncertainty band is cut off.

We agree that this cutting is not ideal, but otherwise the visible amplitude of the main line would be halved.

Page 3207 Figure 9 Bottom: One conclusion drawn in this study is that the pCO_2 -based estimate can improve atmospheric inversion land flux estimates, hence it would be interesting to compare both standard inversion and inversion using the SFC results to other land flux estimates (e.g. from the Global Carbon Project).

We agree that this is 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.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)