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Interactive comment on "Interannual sea–air CO_2 flux variability from an observation-driven ocean mixed-layer scheme" *by* C. Rödenbeck et al.

Anonymous Referee #3

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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 (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 clarifi-

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cation 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 ? 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 ? (2) The evaluation with only one comprehensive models (RECCAP initiative accounts for more than 5 comprehensive 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).

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?

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

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.

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

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

P3175 L9-19 Since the use of "synthetic data" referred to section S4 in the supplementary materials. I recommend the authors to move this section in the supplements.

P3176 L9 Explain what is your consistency check. Consider mentioning p-value of your test on the Figure S6 and in the man text. Regarding Figure S6, Pacific 90S-45S residuals seem to have a positive trend. How do you explain this?

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.

P3179 Section 3.1 It would be nice to have a table of values for each region you mentioned 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.

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.

P3181 L19 The envelope you mention mirrors the sensitivity to uncertain parameters at 66% significance (quite low). I recommend to superimposed on the Figure the 90% significance level, which would give a different view of the uncertainties associated to the data priors (especially for the DIC flux).

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?

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

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

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). 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) ?

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 devia-

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tion.

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.

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