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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.

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We would like to thank Anonymous Referee 2 for her/his interesting comments.

The authors present a very clear exposition of an interestingly novel approach to reconstructing a data-anchored gridded CO2 flux product, a high-priority item for the carbon research community. The presentation is clear, given that the method presented is effectively part 2 of the seasonality paper published in Biogeosciences last year. Although I think that the article is suitable for publication with minor revisions, below I have detailed several points that I think would strengthen the presentation.

First, my attention was drawn to the conceptual and analytic framework for sea parting between OIS (ocean interior sources) and TE (thermally induced exchanges) contribu-

tions to the inter annually varying CO2 fluxes. If I understand correctly, the TE considers changes to air-sea CO2 fluxes associated with the local warming/cooling of water parcels. This information is then used in the calculation of OIS. This is a valuable step towards deconvolving the various contributions to flux variations.

However, the question arises is whether what might be missing here is an accounting of the diapycnal transports of DIC associated with the large-scale overturning circulation. By this I mean the net (near-surface) cross-frontal thermodynamic transports of DIC associated with lightening and densification of water masses. This has been shown by Iudicone et al. (2011, Biogeosciences) to be of first-order importance for determining the pre-formed DIC concentrations over the Southern Ocean for natural carbon, but is expected to be more generally true over the global ocean. This is certainly not expected to be independent of TE/SST contributions, but it is more directly connected to the surface buoyancy fluxes and the buoyancy gain/loss of surface waters that drive diapycnal transports The more focused question here would then be: do the authors think that by ignoring diapycnal transports that this significantly important contribution of the surface ocean DIC budget gets bumped over to the inferred OIS, thereby complicating interpretation?

Indeed, all processes other than those explicitly involved in TE will be included in the unknown OIS. This also includes any diapycnal (or other horizontal and vertical) DIC transport. As the reviewer correctly states, this makes the interpretation of OIS in terms of processes a complex task. This could only be resolved by parameterizing more processes explictly (to the extent that such parameterizations exist), which however would move the scheme away from a data-driven setting. Luckily, the estimated variability of pCO2 (and hence sea-air CO2 flux), which is our main target, does not depend on process attribution. What is potentially affected is the inferred O2 flux, to the extent that participating processes deviate from the assumed Redfield stoichiometry (Sect. 3.5).

It would benefit the manuscript to mention in a Discussion or in the Conclusions section that the contribution of water mass transformations on DIC as considered by Iudicone

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et al. (2011) to air-sea flux variations may be important to the interpretation of the OIS-TE separation, and that further exploratory work may be needed to assess whether this can be incorporated into the framework considered here.

We enhanced the last paragraph of Sect. 3.5 to convey that more clearly.

Second, the analysis of ENSO is intriguing, and a potentially important contribution. However, I feel that the analysis of the phasing of the ENSO response in carbon could be pushed a bit further by including surface pCO2, sea surface height anomalies (SSHAs) from altimetry in addition to the MEI index to identify ENSO phasing. The MEI index can be a bit tricky to interpret in terms of the specifics of thermocline depth variations, but nearly continuously observed SSHA offers nearly direct access to this quantity. In fact I would recommend that as an expansion to Figure 6 that the authors create Hovmoller diagrams for not only SSHA, but also for SSTA and heat flux anomalies from an assimilation ocean product such as ECCO. The interplay between thermocline depth anomalies (via SSHA) and warming and cooling via surface heat fluxes in impacting SST may provide more direct access to the underlying mechanisms.

We thank the reviewer for these interesting suggestions. We had previously looked at an SST Hovmöller plot which reveals that SST does not show the slow apparent propagation as pCO2, as stated. From a first look at SSH, there indeed seems to be interesting information. However we feel that a satisfactory analysis of that would not be possible within the revision time of this paper, and thus be more appropriate to follow up separately. Dear Reviewer, if you would be interested we are certainly open to a collaboration on this interesting subject.

Concerning the complexity of MEI: In the correlation analysis (Fig 6 right panels) the main role of MEI is in setting a "clock" such that phase shifts can be obtained from the lag of the correlation.

But is it possible that thermodynamic processes interacting with circulation over the equatorial pacific are not fully capture by the TE-term, and thereby being folded into

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the "Internal DIC flux" shown as the final time series in Figure 5? And that it would thereby not be clear how they would project onto O2 fluxes?

That is right (unfortunately!), see answers above.

it seems that the very large excursions in the "Internal DIC Flux" in 1997 and 1998 (Figure 5c) pose an exciting an important challenge to interpret, since these drive important deviations from APO inversion results, if I understand correctly.

We agree. Indeed, a substantial change in TE (prior) would also change the inferred APO flux, potentially decreasing the mismatch in Fig 7. On the other hand, it is also well possible that the APO inversion underestimates the size of the 1997/1998 feature.

More detailed comments: In all figures showing fluxes (such as Figure 3), the authors should indicate in the captions the sign of the flux (outgassing as positive or negative etc.)

We added a reference to Fig 3 to all relevant captions.

For the comparison with the ocean model run so Buitenhuis (2010), the authors should highlight in the text and figure caption (Figure 8) the forcing used with the forward ocean model, was it NCEP or ERA-40-derived?

The model is forced by NCEP reanalysis (Buitenhuis et al., 2010).

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

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