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Interactive comment on “Interannual sea–air CO₂ flux variability from an observation-driven ocean mixed-layer scheme” by C. Rödenbeck et al.

Anonymous Referee #2

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Review of "Interannual sea-air CO₂ flux variability from an observation-driven ocean mixed layer scheme", by Rödenbeck et al.

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) contributions to the inter annually varying CO₂ fluxes. If I understand correctly, the TE consid-

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ers 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 Ludicne 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?

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 Ludicne 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.

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

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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. 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 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? 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.

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.)

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?

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

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