

Interactive comment on “The non-steady-state oceanic CO₂ signal: its importance, magnitude and a novel way to detect it” by B. I. McNeil and R. J. Matear

Anonymous Referee #2

Received and published: 12 December 2012

* Summary:

McNeil and Matear review estimates of ocean CO₂ uptake and anthropogenic CO₂ in the ocean, highlighting the importance of non-steady-state processes for the historical and present-day estimates. They propose a way to quantify the non-steady-state component using a combination of methods previously applied to estimate oceanic CO₂ uptake.

* General Comments:

This is a very well written and very clearly presented manuscript. It nicely lays out how the assumption of a steady state carbon cycle affects current ocean-observation

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based estimates of anthropogenic CO₂ in the ocean and why it is important to consider non-steady-state effects. It also provides a concise review of the individual methods that have been used so far to estimate ocean CO₂ changes and how those could be used to separate steady-state from non-steady-state effects. The manuscript is rather short on fundamentally new results and conclusions, but I don't see this as an obstacle for publication given the review character of the contribution.

My main criticism of the manuscript relates to how the authors decided to present their analysis and their few new findings. The authors, in my view, tend to overstate the novelty and the significance of their contribution by referring to it as a "paradigm shift in understanding" and mentioning how "new" or "unexpected" the recognition of the importance of the non-steady-state CO₂ signal is. However, as actually nicely presented in their review of the existing literature, much of this has been recognized before, though not presented as succinctly as done here.

In summary, the paper will make a useful contribution and is well suited for publication in Biogeosciences. I thus recommend publication after minor revisions.

Please find below a number of specific comments for the authors to consider in their revisions.

* Specific Comments:

- 1) page 13162, Abstract: suggest to add estimates of uncertainties to all the numbers provided in the abstract, in particular those that result from the analysis presented here.
- 2) page 13162, line 9: suggest to delete "unexpected" before "CO₂ outgassing signal" (same on page 13166, line 3/4)
- 3) page 13162, line 12: suggest to delete "new"
- 4) page 13162, line 19: "has likely increased" – why likely? This only makes sense if uncertainty estimates are provided along with the best estimates. Could those be added?

5) page 13162, line 20: suggest to delete "a level which cannot be ignored" – it seems to imply that it has been ignored so far, which I don't think reflects correctly the state of the discussion on this issue (see the author's discussion of the relevant literature).

6) page 13165: line 12: suggest to add a brief explanation of the term "airborne fraction of CO₂"

7) page 13170, line 3/4: "since we combine both the natural and anthropogenic non-steady-state signals for simplicity" – suggest to elaborate a bit more if and how the two non-steady state signals could be estimated separately. There seems to be a mismatch here with the discussion and quantification of the natural component when compared the anthropogenic component discussed in Section 2.1.

8) page 13170/13171, lines 19-25/1-8: suggest to add estimates of uncertainties to all the numbers provided here in order to put these numbers into perspective when discussing the combination of multiple methods in Section 3.1. One of the main conclusions there is that the are too large to arrive at a statistically significant results, thus providing all the information about uncertainties is crucial.

9) page 13172, lines 1-4: Combination of Keeling, 2005, and Sarmiento et al., 2010, results: I am confused. From the discussions so far my understanding was that Keeling (2005) provided an estimate of the anthropogenic non-steady state component (7+10PgCyr⁻¹) whereas Sarmiento et al. (2010) provided an estimate of the combined non-steady state signal (6.3PgCyr⁻¹). Here the authors now present a combination of these two results and estimate a combined non-steady state signal of (13+10PgCyr⁻¹). But doesn't the combination of the Keeling and Sarmiento estimates as done here result in a double counting of the anthropogenic non-steady state component? It seems that this requires correction. Anyway, please make sure to better explain how exactly you arrive at the combined estimate and clarify the approach in the text (and table).

10) page 13172, lines 13-15, 20-24 and 25-28: two independent estimates for the non-steady-state outgassing are given on this page: (i) from the combination of observation

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based method (0.1-0.5 PgCyr⁻¹) and (ii) from ocean carbon cycle models (0.4 PgCyr⁻¹). However in what follows, i.e., the tentative revision of the oceanic carbon budget for the 1989-2007 period, only (ii) is being used. It's not clear to me why (ii) would be the "better" estimate, resp. why in the tentative revision of the budget the large range resulting from (i) has not been considered, not even in the discussions. I suggest to expand this discussion substantially.

11) page 13174, lines 1-9: the paragraph on the role of the coastal carbon cycle comes like an afterthought and seems a bit disconnected from the rest. If relevant, I suggest to mention the open ocean - coastal ocean issue much earlier in the manuscript, e.g., in the introduction. From this short paragraph, it does not become clear why the highlighted variability of CO₂ in the coastal zones would impact the estimates of net ocean CO₂ changes as discussed in section 3.

12) page 13175, line 8: Suggest to delete "Given this paradigm shift" (see general comments) and to just start the sentence with "The challenge for the..."

* Recommendation:

I recommend publication after minor revisions.

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