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Biogeosciences
Discussions

Interactive comment on "The non-steady-state oceanic CO_2 signal: its importance, magnitude and a novel way to detect it" *by* B. I. McNeil and R. J. Matear

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1 Summary

McNeil and Matear discuss the growing importance of the non-steady-state contribution to the net balance of CO_2 between the ocean and atmosphere. They first introduce a categorization of the different components that can lead to changes in the oceanic uptake and storage of carbon, then review which components the different approaches cover, and then make a first, rough estimate of the magnitude of the non-steady-sate contribution to the oceanic CO_2 uptake over the last two decades.

2 Evaluation

Evidence is growing that variability and change in the ocean's carbon cycle has a significant effect on the net uptake of CO_2 from the atmosphere. So far, this contribution tended to be neglected in most approaches that attempt to estimate the accumulation or uptake of anthropogenic CO_2 from the atmosphere, as these approaches assume a steady-state ocean. This assumption was probably justified when considering the total uptake over the industrial period up to the mid 1990s, but with the human impact on climate becoming a major driver for climate change, this may no longer be the case.

Thus, by systematizing and discussing this issue in a thorough manner, this manuscript makes an important point and therefore contributes substantially to the debate. The manuscript is well written and leaves little room for critique in the details (see minor comment section below). Based on these criteria, this manuscript is clearly acceptable for publication.

But there is one large concern, that in the end is largely an editorial one: This contribution lacks originality. It is essentially an opinion piece that includes a review of the different methods. I find this a very useful and substantive contribution, but there is hardly any new material in it. And the "novel" method that is presented is not really that novel as it has been discussed before (e.g., Keeling, 2005; Levine et al., 2008). I have to admit that have not been presented as clearly and succinctly as done here, but still the ideas have been floating around for a while. Furthermore, the actual estimates of the non-steady state contributions stem largely from already published material. So in the end, the editorial question is whether a review/opinion piece can be accepted for publication in this journal.

3 Recommendation

I recommend acceptance of this manuscript after minor revision. This recommendation hinges critically, however, on whether Biogeosciences accepts also opinion/review contributions.

4 Minor comments

p13163, lines 19-23, "fundamental to these estimates is the assumption..." I think that somewhere in the text it would be important to point out that the sensitivity of the different approaches toward the non-steady-state situation might actually be quite different. For example, since the back-calculation methods are based on the actual carbon measurements, they contain in part the non-steady-state signal and some of this signal will be projected back into the estimates. This is particularly the case with the ΔC* method, which employs over a substantial part of the thermocline a combination of two methods to estimate the air-sea disequilibrium. I am far from saying that the non-steady state signal is properly captured - it is quite likely not, but neither will be completely ignored. In that sense, the purely tracer-based methods, such as the TTD or the empirical Greens Function methods, tend to be more sensitive, although also those methods will capture part of the non-steady-state signals.

p13164, lines 16-18: In this context, it is not only declining O_2 concentrations, but changing O_2 concentrations in general that indicate the absence of a true steady-state. For example, Stendardo and Gruber (2012) showed recently that large O_2 changes occurred in the North Atlantic over the past 50 years, but when they integrated them, little net change over the entire North Atlantic remain. This is because the regions of losses were compensated by regions of gain. For CO_2 that might not be the case. I therefore recommend to cast the net a bit wider here.

p13164, line 26: "groundbreaking" Here and elsewhere. Of course, the use of adjectives with a strong value judgment is an issue the author's personal style, but in my opinion, they are used too often here, underscoring the impression that this is largely an "opinion" piece.

p13166, whole section 2: This decomposition is only done here only in terms of changes in dissolved inorganic carbon, i.e., in terms of carbon storage. Later on, this concept is directly transferred to the air-sea CO_2 flux. Fundamentally, this is ok, but I recommend that the authors introduce this separation also in terms of fluxes. One important reason for why this is highly relevant is because the largest contribution to variability in the ocean's interior, namely Δ DIC'$_{nat}$, generally leads to much smaller relative changes in the surface fluxes. This is because most of the changes in Δ DIC'$_{na}t$ are compensated within the ocean, as they emerge, for example, from a lateral change in the position of a gyre.

p13166, line 17: "Sarmiento et al., 1992". This is clearly a very fundamental study, but it does not fit here, since it did not show the importance of the non-steady-state. But, e.g., Joos et al. (1999) did.

p13167, lines 8-9: as above

p13167, lines 20-23: "What is important to remember here is that correcting for the natural DIC signal in the ocean from back-calculation techniques like ΔC* does not account for either the natural non-steady state signal (Δ DIC'$_{nat}$) or the anthropogenic non-steady state signal (ΔACO$_2$')". See comment above. Although this statement is correct with regard to the actual concept, in reality, this is not quite so clear.

p13170, whole section: The authors directly switch from changes in DIC to comparing air-sea CO_2 fluxes. As commented above, this needs to be done more cautiously and also more explicitly.

p13170, whole section: In my opinion, the authors have missed an opportunity here to get out of the novelty trap criterion. They could have analyzed their model result in much more detail and to have begun to extract the different components (both in the interior and in terms of surface fluxes) and discuss them in detail.

p13174, lines 16-19: "This global non-steady-state..." I suggest to repeat here the basis for this estimate. In my opinion, it is anything but an indication of the size of this signal as it is based on very weak constraints.

Figure 2: I consider it to be important to include here the "steady-state" outgassing of CO_2 due to the riverine input of carbon. Incidentally, we recently submitted a paper (first author is Pierre Regnier), where we attempted to quantify the non-steady-state part of that particular flux.

Nicolas Gruber December 2, 2012

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