

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

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Response to Review by Prof Gruber

Summary:

We very much thank Professor Gruber for his review of the manuscript. He is one of the pioneers in data-detection methods that have progressed our understanding of the oceanic CO₂ sink and his comments on the manuscript are valuable and have been taken with utmost thought. Although supportive of publication, the largest criticism was that “it lacks originality” with our method “not really that novel as it has been discussed before (e.g., Keeling, 2005; Levine et al., 2008)”. We respectfully disagree with this

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critique, although probably comes about in part due to a lack of clarity in the manuscript within the relevant section (3.1) which lays out the novel data-detection technique. In our full response below, we have taken the liberty to include the reviewer comments and our response for clarity.

Prof Gruber: Evidence is growing that variability and change in the ocean's carbon cycle has a significant effect on the net uptake of CO₂ from the atmosphere. So far, this contribution tended to be neglected in most approaches that attempt to estimate the accumulation or uptake of anthropogenic CO₂ 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.

Our Response: We agree and this was our motivation for the writing the paper.

Prof Gruber: 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

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accepted for publication in this journal.

Our Response: Structurally, there are two parts to the paper that are essential to the whole manuscript. The first review section introduces both mathematically and empirically the conceptual partitioning of the carbon sink into steady and non-steady state terms (which as far as we know have not been presented before). Included in this section is a combined estimation of the non-steady state carbon sink from combining previous modeling work (Keeling, 2005 and Sarmiento et al., 2010). But the next section of the manuscript is where we present a both a new idea along with an independent data-based estimate of the non-steady-state signal, which Prof Guber seems to have overlooked in his novelty measure. The Keeling, 2005 and Levine et al 2008 manuscript (as noted by Prof Gruber) in no way describes the independent data-based idea that we present to estimate the non-steady-state signal. Those papers, in particular Keeling (2005), which was an important part of initial review, discussed the concept of a non-steady-state before, but not the new idea that we propose in section 3.1 of combining more accurate but diverse data-based methodologies that specifically capture different signals in the decomposition of the equations. As far as we know, this idea of quantifying the signal using different data-based constraints that explicitly constrain both the steady and non-steady-state signals, has not been proposed before; therefore we challenge the critique that the manuscript “lacks novelty”. However, we do accept that the section that outlines this concept (3.1) could be clearer. In particular, coming up with a name and title of the method (and section) while expanding its discussion would help overcome this issue.

Further, we feel re-interpreting existing scientific results is an important part to advancing scientific understanding and for this reason the lack of new modeling results should not be on its own sufficient to reject the paper. To date most effort using different data techniques to quantify anthropogenic carbon concentrations in the ocean have focused on reconciling their differences. However, here we emphasize with a non-steady state ocean carbon cycle, one expects differences in the different methods and equally im-

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portant there is the potential to exploit these differences to advance our understanding of the global carbon cycle. With a growing non-steady state signal and multiple independent estimates of anthropogenic carbon concentrations, the ocean carbon cycle community has the potential to provide important insight into how the global carbon cycle is changing with climate change. Hopefully, this paper will help motivate the carbon science community to go after this question.

Minor Comments:

All of the minor comments made by Prof Gruber are helpful and warranted for the revision, and only two require a more detailed response.

1) “p13163, lines 19-23”. We agree that different methods will have different sensitivities to the non-steady-state signal, which is where the power of our idea of combining different data-based constraints comes in. For example, as noted by Prof Gruber, the non-carbon-based Greens function methods and TTD methods will definitely have less ability to capture the non-steady-state signal than a carbon-based method, implying they are ‘better’ at capturing the steady-state signal from the atmosphere (see Table 1). On the surface, this sounds like a major drawback in using the TTD/Greens function approaches. However, because of this, these techniques will be more powerful when comparing it’s final ocean CO₂ uptake with those data-based techniques which do better constrain the total combined CO₂ signal (ie steady + non-steady) like pCO₂ or O₂/N₂. This comparison between these different data-based techniques is where the non-steady-state signal will emerge. The problem with some techniques however, like the C* approach and MLR approaches, is that they don’t capture the steady-state-signal or the net-CO₂ signal well. Because they capture a mixture of the different signals, they are limited in how they can be used when comparing with other data-based approaches in order to detect the non-steady-state signal.

2) “p13170, whole section” We initially did a model analysis which looked at the different oceanic CO₂ signals in a few different sections in the ocean. However, as eluded to

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above, in subsequent revisions to the manuscript we omitted this analysis on the basis of two things. Firstly, the additional analysis added greatly to the length and complexity of the manuscript. In doing so, it took away from the core idea and message of the manuscript, which is that the non-steady-state signal is globally large and growing since 1990, and by using two different data-based techniques that capture different CO₂ signals, it can provide an independent data-based constraint on the non-steady-state CO₂ signal. Therefore, due to these issues, the model analysis didn't add any value to either the presentation of the message or the novelty of the idea presented.

Response to Review by Anonymous Colleague

Summary:

We very much thank the anonymous reviewer for their review of the manuscript. Although supportive of publication, their comments will help us clarify some of the elements and language of a potential revised manuscript. The following is a detailed response to the reviewers comments. We have taken the liberty to include the reviewer comments and our response for clarity.

Anon Comment: 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.

Our Response: If warranted by the editors, we will rewrite some of the language to make it clearer in order to not overstate the issue. However it's important to emphasise that the comments like "paradigm shift in understanding" and "unexpected results" are not referring to our study but to other studies (Le Quere et al (2007) and Sarmiento et al (2010)), who reported large deviations in the steady-state ocean carbon cycle.

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Although the phrase ‘paradigm-shift’ is somewhat subjective, we believe the scale and magnitude of the non-steady-state signal warrants the use of this term. We feel this language is important in describing the emerging trends in carbon cycle understanding, however given the comments from both reviewers; we are open to mediating the language.

We agree that the new idea presented here was only briefly touched on (section 3.1) due to the large uncertainties introduced when estimating the non-steady-state signal from data-based constraints. We agree with the reviewer here that this section (which is the ‘novel’ part of the manuscript) could be reworked, as noted by our response to Prof Gruber’s review. By adding uncertainties to these numbers would help in doing this. On balance we probably also agree that our article is more a review article than a research article. However the article introduces an important novel scientific idea after the review analysis, which is a better fit within a research category of manuscript. We are happy for the editors to decipher those editorial issues in evaluating the type of manuscript.

Specific Comments: All of the minor comments made are helpful. Only two comments require a more detailed response in relation to a potential revised manuscript.

1) There are systematic and random uncertainties in the estimate of the non-steady-state signal estimated from the multi-methodological approach here. Although these uncertainties are estimated differently when applying each different technique, we could make a crude estimate of the uncertainty estimated here if assuming independence between the different approaches. In a revision, we could include uncertainties in the estimate of the non-steady and steady state estimates.

9) The reviewer queried the application of the Keeling (2005) estimate and the Sarmiento et al (2010) estimate and asked if their was ‘double accounting’. We thank the reviewer for this note; since we need to better clarify this. The Keeling (2005) estimate was made using the Sabine et al (2004) numbers for the anthropogenic CO₂ sink

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up until 1994. The Sarmiento et al model results were made from 1990 onwards. Yes, there is a four year overlap in the combining both approaches. However, if we take the models from Sarmiento et al (2010) as a gauge, the largest signal in the non-steady-state comes after the mid-1990s, so this overlap would probably result in a very small bias ($\sim 10\%$). Despite this, we would be more explicit in a revised manuscript of this issue and the potential biases and how we apply both estimates.

10) We saw ii) as the 'better' estimate simply due to the large uncertainty using the data-base approach (i) at this stage. We agree however that i) is the novel, new result and requires reworking in the discussion, as we noted above.

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