

Response to reviews of **Global Ocean Carbon Uptake: Magnitude, Variability and Trends**
(bgd-9-10961-2012)

By Rik Wanninkhof and 14 co-authors

We appreciate the thoughtful and thorough comments of the manuscript by the two reviewers. The reviews show a commonality in comments and we feel that the issues raised are appropriate. The following changes are made to the revised version:

The text has been reorganized and streamlined. We describe the overall objective in the abstract and emphasize that this work as an integral part of overall RECCAP effort. The revised abstract is at the end of this response. We provide some more background of RECCAP and choice of models. We have decreased the amount of numbers in the text but rather provide more description and refer to the tables that have the quantitative information. We have eliminated table 1 and figure 3 which are summaries of previous work. Rather we have now included the output of the 50-year run of the models used in RECCAP that confirm the overall results, albeit at lower fidelity. (New figure 3, attached).

Both reviewers requested more detail on the revised data based sea-air CO₂ flux estimates. We provided more information, in particular where our approaches differ from published results.

The reviews have helped focus the overall take-home message that:

- There is good agreement in global anthropogenic uptake between the different and largely independent approaches. We show that apparent differences pointed out previously are in part because of differences in assumption and treatment, such as riverine carbon input, and ocean areas. In some cases inconsistent treatment of contemporary and anthropogenic fluxes contribute to differences. Consistent model-based and data-based results agree within their uncertainty.
- There appear to be systematic differences in trends between different approaches with those based on ocean inventory carbon change yielding larger trends than the methods relying on the sea-air flux. While two decades is short to do a trend analysis, these conclusions are in agreement with the 50-year runs (see new figure 3 appended to this response).
- Different approaches show appreciable differences in sub-annual to interannual variability.

The specific comments of the reviewer are posted below in italics. Responses are provided in regular font.

Review #1

Review of Wanninkhof et al. "Global Ocean Carbon Uptake: Magnitude, Variability and Trends This paper presents a recalculation of the flux of CO₂ into the ocean based on the ΔpCO_2 compilation of Takahashi et al. 2009 and a reassessment of the wind speeds and gas exchange-wind speed relationship. The value for the year 2000 is compared with results from models and ocean inventory changes, and then the interannual variability from all models is discussed. All methods indicate an increase in ocean uptake rate between 1990 and 2010, but the efficiency of the ocean as a CO₂ sink is decreasing. Changes in the absolute values of CO₂ uptake have not changed much even with the recalculation. The more relevant result is the magnitude of the increase in flux to the ocean with time and how this varies among the different methods.

I think this paper represents an important review of this subject. Each time improvements are made to the methods of determining anthropogenic CO₂ uptake without revealing a very different result, it illustrates how close the community is to a consensus on the true flux. This result plus the assessment of the interannual change in the flux to the ocean are the highlights of the manuscript. I believe the paper has real scientific value and should be published, but hopefully not without rewriting and editing. The manuscript is sloppy, poorly organized and too glib in places. It reads like it was put together very fast by sewing together bits from different co-authors. An example of this is the Discussion which consists of three subheadings and eight sub-subheadings. This section would be clearer and more concise if most comparisons were moved to tables and only the main points were described in the text. The present form is hard to read, which I fear and will diminishes its long-term value. I recommend publishing the paper after it has been substantially rewritten.

The layout of the paper is along the lines of the other RECCAP papers following a common template. We inserted the subheadings for clarity but recognize that this impedes the readability and flow. We have rewritten the text with more explanation in the introduction. We have deleted the secondary [“ sub-sub”] headings and improved the flow between paragraphs.

I have the following specific comments.

Pg. 7. Table 1 is from IPCC4 and comes out of the blue with very little explanation. We do not even know what of the many methods were used to derive these values. A much better explanation of what is there would be helpful.

Several of the co-authors questioned this background material as well. We have deleted this table and short explanation. This table is deleted and figure 3 is replaced with 50-year model runs of the OBGCM models used.

Pg. 10. Equation 4. The mechanism of determining the mass transfer coefficient wind- speed dependence is essentially that of Wanninkhof, 1992 with a better estimate of the inventory of bomb-14C. This is simple and totally independent of the massive efforts from the tracer release experiments. Is this really an improvement? The reader deserves to know how this result compares with gas exchange-wind speed correlations that preceded it (e.g., Wanninkhof, 1992; Sweeney et al., 2007, Nightingale et al., 2000; Ho et al., 2011).

The good correspondence between the global estimate based on bomb-14C and tracer release studies is articulated.

The other reviewer brought up this issue as well. We have provided appropriate detail and background on the gas transfer parameterizations used based on the studies mentioned above.

Pg. 10. Paragraph 3.2. This is the essence of the new calculation presented in a few sentences. I find this section to be much too short and glib. Are these the results in Table 2? What is the undersampling correction and how is it made? What is the continental shelf correction and how is it made?

We have rewritten this section and provide significantly more detail and clearer reference to the supplementary material that provides a quantitative description of the continental shelf correction based on the regions used for the ocean inverse model.

The adjustments to the canonical “Takahashi CO₂ flux climatology” has been better described

Pg. 10. Paragraph 3.3 midway through the first paragraph. “The sub-annual.... “ I find this sentence difficult to understand because the method description is too brief. paragraph “is”

We appreciate the need to better explain adjustments to the canonical climatology and have done

so.

Grammatical problems: Pg. 11. The first sentence in the 3rd paragraph makes no sense. Later in this

Pg. 14. Paragraph 4.1.2. The second sentence is very rough. Pg. 16. First full paragraph, second to last sentence makes no sense

Pg. 20. Conclusions. The second sentence makes little sense.

Third to last line has some problems

We had some problems finding the exact sentences referenced but we have corrected the grammar and syntax issues. In particular, we eliminated several run-on sentences.

Interactive comment on “Global ocean carbon uptake: magnitude, variability and trends” by R. Wanninkhof et al.

Anonymous Referee #2

We appreciate the comprehensive and valuable review of this referee. We have accommodated most of the comments. Several of the comments focus on providing a better understanding of why the models and observations show the particular results. While these are critical questions they are not the focus of the RECCAP effort. Addressing them comprehensively would require a lot more text and in many cases new diagnostic model runs. As indicated below we have provided some terse explanations and pertinent references but cannot address the causality of differences comprehensively in all cases.

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This manuscript is a review that provides a summary and comparison of recalculated anthropogenic CO₂ flux estimates between the ocean and atmosphere that are based on a variety of models and measurements, with the flux calculated from the $\Delta p\text{CO}_2$ compilation of Takahashi et al. 2009, revised wind speeds over the globe, and a re-evaluation of the relationships between wind speed and gas exchange. The year 2000 (halfway through the period of study 1990-2009) results, calculated from the $\Delta p\text{CO}_2$ compilation, updated wind speeds and the modified gas exchange relationship, are compared with those derived from OGCM, atmospheric models, O₂/N₂ ratios (only 1990-2000), and oceanic inverse models. There is remarkable coherence in the flux estimates obtained by the various methods (including a generally slight refinement over prior estimates), considering the not insignificant differences in approaches and their respective uncertainties. Overall, there is a consensus that the uptake of CO₂ by the world ocean is increasing, although the fraction of anthropogenic/atmospheric CO₂ taken up by the global ocean is decreasing over time. It can be said that, because the new estimates presented here and those from prior work are rather similar, there must be a sense within the community that we are close to getting this correctly! There remain, however, some important differences in the various approaches, especially in terms of their respective uncertainties, and I it appears (I am not a modeler) that what goes into the models and how sensitive the different parameterizations are, lead to much of the observed differences between approaches.

The value of this manuscript is in the confirmation of a relatively tightly constrained range of estimates of CO₂ fluxes, and suggestions as to what some of the main drivers are for inter-

annual variability, as well as the fact that our oceans clearly do not have a capacity to continue to take up a large fraction of what we are releasing into the atmosphere (e.g., decreased fraction taken up each year). The latter aspect is an important point to get out to the public, many of whom may feel that because our oceans are taking up more and more CO₂, the atmospheric increase and problems associated therewith may not be that important. . . Obviously this erroneous perception neglects any aspect of how OA might impact our planet.

As an observational and experimental scientist, however, I do have some issues with the paper. The paper may not have been put together in a manner that makes it appealing to the non-modelers because, for example, it does not provide sufficient detail (either in the original description or in the appropriate discussion sub-section) of what a particular model/approach brings, its advantages and shortcomings. There is also a need for a better discussion of what caused the differences between models, especially their range of uncertainties. Some sections do better than others addressing the above; thus, I am left with the thought that we have a patchwork of contributions from many authors that were probably not sufficiently well integrated by the senior author. Of course modifying the paper to address some of my concerns will lengthen it, but I think that it would improve the reach and interest of the paper to the broader community. This paper should be published, but it needs minor to moderate revisions.

We appreciate the thoughtful comments of the reviewer and his general assessment of the paper as well as pointing out some of the grammar and syntax errors. We have rewritten key parts of the paper with these comments in mind. However, we did not expand on the model description significantly. As now detailed in the text the basic model parameters are provided at the RECCAP site and in the papers referenced in Table 3. The reasons for the different model behavior are not always apparent without thorough diagnostics. We have clarified in the introduction why these models were used and that a thorough comparison with skill metrics such as was performed by Matsumoto et al. (2005) in the OCMIP project is recommended.

Below I provide some specific comments, keyed to the pages/lines as shown in the BGD interactive text.

1) The abstract is number heavy. . . Although I appreciate being immediately given the numerical results, I think a few more descriptive lines of the key findings (all in text) may be needed right up front. I think the “for” after -1.9 on line 5, p10963 is un-necessary.

We have changed the abstract and introduction to be more descriptive and better reference the tables containing the quantitative information.

2) Page 10968, lines 15-17: I know that there nothing that can be done about the as- sumption made by Takahashi et al (2009) that the $\Delta p\text{CO}_2$ does not vary on multi-year time scales, but I think a short statement should be provided (e.g., taking data over many years to generate a large global compilation requires time. . . so then you have to assume things did not change if the compilation is to be of any use). Obviously, this assumption is not likely to be universally valid! Later on page 10970, lines 7-8, it is ac- knowledged that changes due to circulation and biogeochemistry are poorly known. . . and on page 10972, lines 3-11 document changing $\Delta p\text{CO}_2$ in several regions.

This section has been rewritten focusing on our key assumptions and differences from the canonical Takahashi flux climatology.

3) Page 10969, line 5: reference to Table 1: Said table is rather sparse. The text says the numbers for each period are based on many data/methods... which are then listed, but were all used for all periods; maybe a slightly more detailed explanation is in order?

We have deleted Table 1 and rather focus on the 50-year model output from the models used in RECCAP shown in the new figure 3

4) Page 10969, lines 18-21. I think this is probably the most notable result of this work, and this should be highlighted in the first two sentences of the abstract.

Yes, we agree. We have highlighted this in the abstract and introduction. The inclusion of a new figure showing the 50-year model outputs highlights this point.

5) Page 10969, lines 26-27: the assumption that biological activity has remained roughly constant over the past 250 years is questionable, especially given that major climatic reorganizations have taken place on rather short time scales. This is obviously an assumption, and it is indicated that it may be inaccurate on the next page although it is also assumed that these cancel out on decadal scales...

We have clarified that this is an implicit assumption in the models

6) Page 10971, line 13: insert "the" between "some.." and "of. . ."

Done

7) Page 10973, line 22, delete "at" between "data" and "available"

Done

8) Page 10974, lines 6-8, equation (4): It might be helpful if the authors provided a short explanation of how/why this equation was chosen, how much improved value is there in using this parameterization over those recommended by Ho et al., 2006; 2011 and Takahashi et al 2009?

We explain that this parameterization is very similar to Ho et al., and a small improvement to Takahashi et al. in that it accounts for regional variability in wind rather than assuming a constant wind speed variability spectrum

9) Page 10974, lines 15-22. Although this paragraph is meant to illustrate how the updated gas transfer parameterization was derived, it really does not provide detail as to how new corrections were applied. . .

See above, we provide some more detail

10) Page 10975, lines 11-14: I am not clear as to what sub annual segments means (but I assume this refers to short periods of time when the $p\text{CO}_2\text{sw-SST}$ is a well-defined function)... and why were one to four linear fits used? How were these derived?

A very brief explanation is provided but the reader is referred to the paper by Park et al. 2010a where the approach is detailed and justified.

11) Page 10980, line 26: change "differs" to "differ" (plural).

Done

12) Page 19081, lines 6-7. . . maybe an explanation is in order as to why the models with biogeochemistry show less uptake (is the global system net heterotrophic vs net autotrophic?)

This was a revelation to us, and the short answer is that we don't know. The suggestion offered by the reviewers is a very interesting one but not verifiable without a lot of diagnostic runs that is beyond the scope of the work. I anticipate future papers delving into this issue

13) Page 10981, lines 13-16, Why there is such a big difference between the peak to peak change between NCAR and UEA models probably should be explained better.

The paper is focused on the output of the models, understanding why models differ is a critical but daunting effort that is beyond the scope. In the introduction we better describe the scope and limitations of our effort.

14) Page 10981, line 18: Insert “to” between “compared” and “the mean”, the last sentence in this paragraph also has grammar errors (subject verb agreement).

Done

15) Page 10982, lines 6-7. I would insert “however” after “variability” and “than for OGCMs” at the end of the sentence.

Done

16) Page 10982, lines 10-11: Why? A brief explanation as to why this is might be help-ful.

We have added a sentence that the atmospheric inverse models do not separate ocean and land carbon sources and sinks well and that this is likely caused by “bleed over” from the terrestrial systems.

17) Page 10982, lines 11-13: “greater” than what? OGCM’s? Please specify.

We added “compared to other modeling approaches”

18) Page 10982, lines 13-15: This sentence also needs clarification/explanation.

We clarified this (see comment #16)

19) Page 10982, line 16 “used as a prior within the inversion”... please clarify what is meant (this may be clear to modelers, but not to me).

We left this unchanged “prior” is a basic and frequently used term in inverse modeling.

20) Page 10983, lines 4-6: This is an important point, short term changes occur as a result of a variety of forcings and, because these can often be stochastic in nature, the need for continued long-term (and relatively high-frequency) observations is quite clear!

Thanks, all co-authors agree

21) Page 10983, line 7: “detailed in the chapters of individual basins”. . . I assume that this refer to chapters within this special issue, please specify (i.e., add “in this volume”)

We have added the references to the chapters and mention the basin chapters in the introduction as well.

22) Page 10983, line 15: Is the difference between 11 and 10 m/s in the two oceans statistically significant?

Yes, because a the huge number of data that go into these averages these are meaningful differences and cause a 20 % difference in fluxes (that scale to $\langle u^2 \rangle$)

23) Page 10983, last paragraph: Here again, no explanation is provided, only a statement of numbers. . . maybe this is common knowledge for many but I think the average reader deserves a few lines of explanation of these various values.

We’ve added references but not expanded the explanation.

24) Page 10984, line 4: I would use the word “parallels” rather than “mirrors”. This is a small point but a mirror is

an opposite trend, in my opinion.

Changed [note, the definition of mirror (verb) is to reflect or to resemble]

25) Page 10984, lines 17-19: Subject verb disagreement, please correct the grammar.

Done

26) Page 10985, line 26: Figures 10a and 11d show different things, yet a single atmosphere CO2 increase value is provided. Please clarify.

We have deleted figure 10. Figure 11 d provides much of the information of Figure 10. That is, the gray line in Figure 11d is that of figure 10a and the black line in figure 11d is what was presented in 10c.

27) Page 10989, line 1: *Why is the acronym “IAV” only now defined?*

We introduced and define IAV earlier in the document.

Other changes for consistency with the RECCAP paper on the Pacific Ocean submitted by Ishii et al. we changed the abbreviation OGCM to OBGCM (ocean biogeochemical general circulation models)

Revised Abstract

As part of the Regional Carbon Cycle Assessment Project (RECCAP) estimates of the anthropogenic global-integrated sea-air carbon dioxide (CO₂) flux from 1990 to 2009 are determined from models and data-based estimated. Numerical approaches include ocean inverse models, atmospheric inverse models, and ocean general circulation models with parameterized biogeochemistry (OBGCM). The median value of different approaches shows good agreement in average uptake. The best estimate of anthropogenic CO₂ uptake for the time period based on a compilation of approaches is -2.0 Pg C yr⁻¹. The interannual variability in the sea-air flux is largely driven by large-scale climate re-organizations and is estimated at 0.2 Pg C yr⁻¹ for the two decades with some systematic differences between approaches. The largest differences between approaches are seen in the trends. The trends range from -0.13 (Pg C yr⁻¹) decade⁻¹ to -0.50 (Pg C yr⁻¹) decade⁻¹ for the two decades. The OBGCMs and the data based sea-air CO₂ flux estimates show appreciably smaller decadal trends than estimates based on changes in carbon inventory suggesting that methods capable of resolving shorter timescales are showing a slowing of the rate of ocean CO₂ uptake. Inspection of RECCAP model output for five decades shows similar differences in trends between approaches.

Fig. 3. (a) The 50-year trends in global-integrated ocean CO₂ uptake from OBGCMs used in RECCAP. The thin solid and dashed lines show the increasing annual uptake of the different models and their interannual variability. The thick solid blue line is the median of the OBGCMs; the thick solid red line is the output of the Green function method (Khatiwala et al., 2012); and the thick black line is the result from the GCP ocean model ensemble (<http://www.globalcarbonproject.org/carbonbudget/index.htm>). (b) The fraction of fossil fuel taken up by the ocean over the last 50-years. The line colors refer to the model output as in (a).

