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Biogeosciences Discussions

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

Anonymous Referee #2

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This manuscript is a review that provides a summary and comparison of recalculated anthropogenic CO_2 flux estimates between the ocean and atmosphere that are based on a variety of models and measurements, with the flux calculated from the ΔpCO_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 ΔpCO_2 compilation, updated wind speeds and the modified gas exchange relationship, are compared with those derived from OGCM, atmospheric models, O_2/N_2 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 CO_2 by the

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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.

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. 2) Page 10968, lines 15-17: I know that there nothing that can be done about the assumption made by Takahashi et al (2009) that the $\Delta pCO2$ 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 acknowledged that changes due to circulation and biogeochemistry are poorly known... and on page 10972, lines 3-11 document changing $\Delta pCO2$ in several regions. 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? 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. 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... 6) Page 10971, line13: insert "the" between "some.." and "of..." 7) Page 10973, line 22, delete "at" between "data" and "available" 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

BGD

9, C6979–C6983, 2013

and Takahashi et al 2009? 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… 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 pCO2sw-SST is a well-defined function)… and why were one to four linear fits used? How were these derived? 11) Page 10980, line 26: change “differs” to “differ” (plural). 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?) 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. 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). 15) Page 10982, lines 6-7. I would insert “however” after “variability” and “than for OGCMs “ at the end of the sentence. 16) Page 10982, lines 10-11: Why? A brief explanation as to why this is might be helpful. 17) Page 10982, lines 11-13: “greater” than what? OGCM’s? Please specify. 18) Page 10982, lines 13-15: This sentence also needs clarification/explanation. 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). 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! 21) Page 10983, line 7: “detailed in the chapters of individual basins”… I asssume that this refer to chapters within this special issue, please specify (i.e., add “in this volume”) 22) Page 10983, line 15: Is the difference between 11 and 10 m/s in the two oceans statistically significant? 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. 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. 25) Page 10984, lines 17-19: Subject verb disagreement, please correct the grammar. 26) Page 10985, line 26: Figures 10a and 11d show different things, yet a single atmosphere CO2 increase value is provided. Please clarify. 27) Page 10989, line 1: Why is the acronym “IAV” only now defined?

Interactive comment on Biogeosciences Discuss., 9, 10961, 2012.