

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 $\Delta p\text{CO}_2$ 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 will diminishes its long-term value. I recommend publishing the paper after it has been substantially rewritten.

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.

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).

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?

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.

Grammatical problems:

Pg. 11. The first sentence in the 3rd paragraph makes no sense. Later in this paragraph “_{is}”

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