

## ***Interactive comment on “Uncertainty of the global oceanic CO<sub>2</sub> uptake induced by wind forcing: quantification and spatial analysis” by Alizée Roobaert et al.***

### **Anonymous Referee #1**

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The authors have conducted a very nice study that works through uncertainties inherent in the global uptake of CO<sub>2</sub> associated with wind forcing uncertainties. The presentation is clear, the methods are transparent, and the results should be of broad interest to the ocean carbon cycle community. The only shortcoming of the study is that it is somewhat technical, and in order to satisfy the scientific priorities of Biogeosciences my recommendation would be that the manuscript would benefit from minor revisions before being accepted for publication. A number of more general and more specific questions/comments are raised in the text that follows.

Broad Comments:

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First and foremost, in the discuss pertaining to Fig. 2 and Fig. 3 and Fig. 4, it would be useful if the authors could identify whether they authors see pertinent dynamical or circulation structures as dominating the uncertainties. Given the linear color scale in Fig. 3 and Fig. 4, it appears that the dominant uncertainties might be mostly over western boundary current regions? This would appear to be important, as there is currently a question being discussed in the carbon cycle community of whether western boundary currents serve as hot spots for carbon uptake. It would be of wide interest if the authors could attempt to quantify a response to the following question: Even with a perfect observing system for perfect pCO<sub>2</sub> measurements that is seasonally-resolving, what inherent uncertainties from gas exchange parameterizations and winds apply over these particular structures? This is already implicitly there in the text, but it would be helpful to emphasize this a bit to make clear what challenges lie ahead for more regionals-focused mechanistic interpretations.

It would also be beneficial for anchoring the present study in the published literature if the authors could relate their results to the study of Sarmiento, Orr, and Siegenthaler (1992; JGR), where it was reported within a modeling context that 100% increases in gas exchange only impact 9.2% changes in air-sea CO<sub>2</sub> fluxes. Although that study from 25 years ago used a simplified representation of anthropogenic carbon, it has long been cited for the argument that gas exchange representation isn't of critical importance for global uptake.

In a related point, it would also be beneficial if the authors could likewise relate the uncertainties here to those reported in the Rödenbeck et al. (2015) comparison of fluxes found for different gridded pCO<sub>2</sub> products from different global research groups.

The authors point out that there are important discrepancies between global and empirical formulations of gas exchange. It could be constructive in this regard to point out that there are expected to be important limitations with the construct of a piston velocity in representing the relationship between surface carbon fluxes and winds. Winds don't only impact air-sea fluxes through microturbulence at the air-sea interface, but

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in the sense of climate dynamics the very same winds can in some regions sustain entrainment of waters through deepening of the mixed layer (through shear-induced turbulence). As the relative amplitude of these processes is not known, one should expect that parameterizations based on the concept of piston velocity (the first of these) will have limitations (Rodgers et al., 2014, BG), and that parameterizations based on local properties at the air-sea interface may thereby have inherent uncertainties that are irreducible. I think that this point in fact strengthens the main arguments in the manuscript, in that systematic and rigorous analysis of uncertainties will continue to be a critical component of carbon cycle research moving forward. Regarding the first of the points above (western boundary currents), this could be pertinent.

Minor Comments:

pg. 2, line 5 “open” should be “opened”

pg. 2, line 12: “observation-based” should probably be “observationally-based”

pg 2 line 19 should be “10 meters”

pg 2 line 28: should be “uncertainty associated with”

pg 3 lines 3-4 should be “the latitudinal distribution of FCO<sub>2</sub>”

pg 3 line 10 I recommend removing “in a nutshell” (rather informal)

pg 3 line 14 change “measure because z” to “measure as z”

pg 4 line 12 again, I think this should say “observationally-based”

pg 4 line 18 I think this should say “using a two-step”

pg 4 line 19 should say “maps for the global ocean”

pg 7 line 17 should say “Within the tropics”

pg 9 line 21 should say “This corresponds”

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pg 9 line 22 should say “despite the fact that these empirical”

pg 11 line 16 replace “the literature also reports” with “one also finds in the published literature”

pg 11 line 24 should say “methods have their relative advantages”

pg 11 line 24 should say “has been shown that”

Summary Statement:

To restate, I believe that with some relatively minor text changes that connect the present study to broader community efforts and scientific interests, the manuscript should meet the standard for publication in Biogeosciences. The authors are to be commended for a very nice and thorough analysis and presentation that will be of brand interest.

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