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Discussions

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Reply on RC2

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Author comment on "Observation Based Budget and Lifetime of Excess Atmospheric Carbon Dioxide" by Stephen E. Schwartz, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-924-AC4, 2022

I thank Reviewer 2 for his/her Review. In this response, for convenience, I repeat the Reviewer's comments here, identified by paragraph number ¶, with my responses interspersed in Bold.

¶ 1. This study by Stephen E. Schwartz used a five boxes model to trace turnover time of anthropogenic carbon emissions. This manuscript actually simplified the complex global carbon-cycle in real world by a simple box-model, but indeed is a long and "complex" paper. After reading the manuscript, I agree that simple box-model can answer the questions raised in this study.

I am gratified in the Reviewer's statement that "This manuscript actually simplified the complex global carbon-cycle in real world by a simple box-model," and that the Reviewer concurs that the questions raised in this study can be answered by the simple box model introduced and employed here.

As I do not have expertise on ocean carbon cycle, I did not check the equations for the ocean part. For the terrestrial carbon cycle, I would like to remind the progressive nutrient limitations on CO_2 fertilization, which may have impact on the turnover time estimated in this study.

In response to the Reviewer's concern over nutrient limitation of CO_2 fertilization, I would note that the representation employed here (and by others) of the dependence of the increase of gross primary production (GPP) on increased CO_2 over the Anthropocene via the fertilization exponent *b* is a proxy for whatever has actually been responsible for that increase. Current observation- and model-based estimates of this exponent, summarized in Appendix C of the manuscript (Figure C1), range from 0 (no increase over the Anthropocene) to 1.3 (63% increase). Comparison of results from the present model with observations in Figure 9 of the manuscript limits this exponent to the range 0.4 to 0.9 (16% to 40% increase), a considerably narrower range than encompassed by prior estimates. With respect to applicability of that range to the future, as the domain of the calculations presented in this study is not extended beyond the present levels of atmospheric CO_2, the concern of nutrient limitation that might further constrain GPP at higher values of CO_2, would not seem to attach to the present calculations, in which atmospheric CO_2 decreases from its present value in response to cessation of anthropogenic emissions.

Overall, this study could be publishable on scientific views, but the organization and representation could be improved more friendly to readers.

I appreciate the Reviewer's conclusion that the study is publishable based on its scientific views. As for organization, I tried to restrict the body of the paper to the most important material, with details in the Appendices and Supporting Information.

¶ 2. Before showing results of this study, one figure of a five boxes model and fluxes between boxes with their notations most shown in Table 1 would be helpful to understand the text.

Figure 2, showing the five boxes, together with fluxes between them and the notations for these fluxes, is introduced at the same time as Table 1 (lines 317 and 318 of the APCD preprint). I hope that this satisfies the Reviewer's concern.

¶ 3. I believe the principle "Entities should not be multiplied unnecessarily", and support the simple box-model used here to investigate the turnover time of excess carbon on a global scale.

I am enormously gratified that the Reviewer characterized this study by this principle (Occam's razor) and that the Reviewer supports the applicability and use of the simple box model developed in this study. That was indeed the intent of this study and this is exactly the sort of characterization of this study that I had hoped to receive.

But lots of uncertainties should be acknowledged for the simple model or assumptions, such as nutrients limitation on CO_2 fertilization mentioned above, land use and land **[c]** over change, and land-to-ocean carbon flux/exchange (https://www.nature.com/articles/s41586-021-04339-9), tipping points of carbon cycle (e.g., forest dieback, permafrost thaw) etc. These caveats should be uncovered to readers along with the conclusion in this study.

The Reviewer raises several points here. First with respect to uncertainties, the known quantifiable uncertainties and their propagated consequences are explicitly stated in Table 1 and Figure 2 and more broadly throughout the manuscript. The propagated consequences of these uncertainties are shown as well, for example, in the ranges of curves of decay of excess CO_2 and of the adjustment times shown in Figure 1.

With respect to nutrient limitation, see response to ¶ 1 above.

With respect to LUC emissions, the uncertainty associated with present inventories of these emissions is explicitly accounted for in estimates of total uptake by the global ocean and total terrestrial biosphere TB, together and separately.

The Reviewer calls attention to the recently published study of Regnier et al. (2022), which extends the conventional picture of apportionment of net anthropogenic uptake of emitted CO_2 between the global ocean and the global terrestrial biosphere to account for lateral transfer from what those investigators denote as terra firme to estuarine systems, thence to coastal ocean waters, and ultimately to the open ocean. This lateral flux, which those investigators estimate globally as 0.15 ± 0.15 Pg yr^{-1}, augments net ocean uptake by that amount and correspondingly diminishes the net uptake by the TB by that amount. Regnier et al. estimate the net anthropogenic flux from the atmospheric

compartment AC to the mixed layer ocean ML as 2.50 ± 1.00 Pg yr^{-1} based on present-day total global net CO_2 transfer to the world ocean (1.85 ± 0.95 Pg yr^{-1} that was in turn based on Landschützer et al, 2014, cited in the present manuscript; see Figure 15*a* of the present manuscript) minus the pre-industrial outgassing derived from their assessment (0.65 ± 0.30 Pg yr$^{\square 1}$, essentially the same as the PI land-ocean flux employed in the present study, 0.6 Pg yr^{-1}), yielding for the rate of uptake of anthropogenic carbon by the open ocean, 2.50 ± 1.00 Pg yr^{-1}. Regnier et al. concluded that this uptake rate must be augmented by 0.15 ± 0.15 Pg yr^{-1} to obtain the total ocean storage rate because of the lateral transport, yielding 2.65 ± 1.00 Pg yr^{-1}. Regnier et al. likewise concluded that the net rate of storage of anthropogenic carbon by terrestrial ecosystems must be diminished from the net flux from the atmosphere to the terrestrial biosphere by the same amount, to 1.70 ± 1.55 Pg yr^{-1}, again because of the lateral transfer.

Although the picture presented by Regnier et al. is substantially more differentiated than that given in the present 5-box model (and requiring many more parameters), the present-day anthropogenic uptake rates given in that study for the global ocean and the terrestrial biosphere are essentially the same, within uncertainties, as those adduced in the present study ($dS_m/dt + dS_d/dt$, 2.1 ± 0.6 Pg yr^{-1}; $dS_l/dt + dS_o/dt - Q_{lu}$, 2.6 ± 0.85 Pg yr^{-1}; where S_m, S_d, S_l, and S_o denote stocks in the ML, deep ocean DO, labile biosphere LB, and obdurate biosphere OB, respectively, and where Q_{lu} denotes the net source of atmospheric CO_2 from land-use change). Comparison of these values with those of Regnier et al. thus indicates no need for any adjustment of the values of the uptake rates determined in the present study and actually lends support to these values.

Regnier, P., Resplandy, L., Najjar, R.G. and Ciais, P. (2022). The land-to-ocean loops of the global carbon cycle. *Nature*, 603 (March 17, 2022) 1-10.

In response to the concern raised by the Reviewer over tipping points, it is re-emphasized that the transfer coefficients determined in this study were based on observations for CO_2 (and global temperature) for conditions ranging from preindustrial to the present. Subsequent to cessation of emissions, atmospheric CO_2 would immediately begin to decrease and global temperature would closely follow; as a consequence these governing properties would remain within the domain for which the transfer coefficients were determined. To be sure these considerations cannot preclude the occurrence of tipping points such as those noted by the Reviewer. The assumption that transfer coefficients would remain constant and not be affected by tipping points is *explicitly noted in the revised manuscript*. I thank the Reviewer for suggesting this caveat.

Finally it is stressed that the uncertainties associated with uptake of excess CO_2 by the DO and the OB are reflected in and responsible for the uncertainty range in the turnover time and adjustment time reported in the present manuscript.

¶ 4. I suggest that the text would be more concentrated on turnover time estimates, now validation or/and calibration of the box-model mixed with the scientific questions. **[Does the reviewer mean "rather than validation or/and calibration . . ."?]** Mixing all together is difficult to find the threads.

Much attention is paid in the manuscript to identification of tightly coupled compartments (found to be the AC, ML, and LB), to determination of the stock in this combined compartment, and to determination of the net leaving flux from this combined compartment into the receiving compartments DO and OB,

culminating in evaluation of the turnover time of CO_2 in the combined AC–ML–LB compartment (Section 5.9). All this determination precedes development of the numerical model and the use of that model to determine the adjustment time of excess CO_2 subsequent to hypothetical cessation of emissions. I would stress that the determination of turnover times, and of the development and application of the resultant numerical box-model, is motivated explicitly for the purpose of asking and answering the pertinent scientific questions, most importantly, the rate at which, in the absence of anthropogenic emissions, CO_2 would decrease substantially towards its preindustrial value.

¶ 5. In the abstract, line 19, what are the current carbon-cycle models? Not clear here.

With respect, in an abstract one cannot go into sufficient detail to identify these models; however the models are identified early in the manuscript, third paragraph of the Introduction and Figure 1.

In summary, I am most pleased with the Review and thank the Reviewer for his/her attention to the manuscript and concurrence in the approach of the study and the underlying intent.