

Interactive
Comment

Interactive comment on “A simple model of the anthropogenically forced CO₂ cycle” by W. Weber et al.

Anonymous Referee #2

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Weber et al presents new simple model for global carbon cycle. The model has two tunable parameters b and τ , one relates to carbon exchange between atmosphere and terrestrial biosphere and other one between ocean and atmosphere. These tunable parameters are calibrated with observed (as they say) net ocean $((n_s) \dot{I})$ and biosphere $((n_b) \dot{I}(t))$ fluxes and observed total atmospheric CO₂ burden $((N_a) \dot{I}(t))$ (Equations 8 and 9). Then the model estimated fluxes and atmospheric burden over time are validated using $(n_s) \dot{I}$, $(n_b) \dot{I}$ and $(N_a) \dot{I}$.

I do not see where this study has broken any new grounds in terms of innovative modeling approaches. I do not believe the model results from this study merit publication in ESD. The following comments are a few examples that explain why I am making this comment.

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Major Concerns: First, there is a major fundament flaw with their modeling approach. The authors are using the same set of data to calibrate the model parameters and validate the model results. The authors should know that they cannot use the same set to data for both calibrating the model and validating it, because the results of validation will then become meaningless. Since there are no physical constraints that define the magnitude of the two main parameters involved, a model implicitly requires a calibration with the tracers. This is a fundament approach used to develop most of the simple carbon cycle models. I wish they had calibrated their ocean and terrestrial exchange parameters using observed tracers and carbon isotopic data for ocean and terrestrial biosphere. Even much more serious problem with their modeling approach is that the modeled exchange fluxes between atmosphere and carbon cycle reservoirs, particularly for the terrestrial and ocean, are not defined in terms of reservoirs (e.g., surface and deep ocean vegetation and soils etc.). Without this we cannot establish the extent to which these fluxes depend on the content of each of the reservoir and to what extend they are influenced by other external factors (e.g. climate change etc.). Finally, in the conclusion section, it is stated that “Our simple linear model gives excellent agreement with all relevant measurements, i.e. $(N_a) \dot{I}(t)$, $(N_s) \dot{I}(t)$, and $(N_b) \dot{I}(t)$. Also the agreement with the rates $(n_a) \dot{I}(t)$, $(n_s) \dot{I}(t)$, and $(n_b) \dot{I}(t)$ is on average excellent.” However, most of the variables (with bar) they claim are measured variables. In fact, they are not measured values. These values are calculated based on the models participated in GLP project. Also, their estimated reservoir sizes for ocean ($(N_s) \dot{I}(t)$) and biosphere ($(N_b) \dot{I}(t)$) are way too low as compared to IPCC estimates.

Please also note the supplement to this comment:

<http://www.earth-syst-dynam-discuss.net/6/C931/2015/esdd-6-C931-2015-supplement.pdf>

Interactive comment on Earth Syst. Dynam. Discuss., 6, 2043, 2015.

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