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Interactive comment on "Scaling laws for perturbations in the ocean–atmosphere system following large CO_2 emissions" *by* N. Towles et al.

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1 Reviewer Major Comment 1

The authors use the LOSCAR multi-box model of Zeebe as the generator of carbon cycle response, so the scaling relationships they seek are not data-based but rather meant to present a simplification of what is otherwise a fairly complex model meant to capture C cycle interactions on various timescales, but with a fairly simple representation itself.

I think the paper largely accomplishes its objectives. The authors explore in detail one particular scenario of emissions amount and duration, and conclude that the (appar-

ently) expected relationship between rate (Emission/Duration) and perturbation (e.g., of atmospheric CO2 partial pressure). I think here the authors should be more explicit about why this relationship should have the form they state (where the exponents of the scaling relationship add to zero). They might start with a simple ODE e.g., $dCO2/dt = V - k\ CO2^{(1/n)}$ *and show that n = alpha + beta, etc.*

1.1 Response

The *long-term* steady state balance of atmospheric CO2 is assumed to be set by the balance of CO2 rates of input via background volcanic processes and the rates of removal via weathering of silicates and subsequent burial of marine carbonate sediments - (As discussed pg 97 lines 17-23 of original manuscript). This steady state balance is thought to be achieved on timescales $>$100kyr. Given that our simulations were all for emission durations $\leq$100kyr and the variety of timescales involved in the interactions between the different carbon reservoirs, there is no a-priori assurance that a scaling law should exist at all, much less one that would take form of a power law. We adopted the particular power law form because it lends itself to a simple interpretation of the long-term assumption of rate dependence eg E/D or alpha+beta=0. Our twin goals are to (1) find our if power law scalings exist for large transient perturbations, and if so, (2) quantify how they differ from steady-state predictions. We have modified the introduction to make these objectives clear.

The symmetric-triangular shape of the emission scenario was adopted to facilitate observation and interpretation of peak system responses. As opposed to using a heavy-sided emissions shape with constant emissions rate, the symmetric triangular forcing provides a scenario where the peak rate of input occurs coincident with the time when 1/2 the total magnitude of emissions, at 1/2 the total duration of the event. We have added a statement to this effect.

2 Reviewer Major Comment 2

The paper would have more utility if the authors could then show how this simplification of LOSCAR helps in the interpretation of or prediction of system response to a real-world perturbation. I'm not sure what to do with the scaling relationship, especially since it is derived from a fairly simple box model rather than observation.

2.1 Response

The scaling laws are intended to (hopefully) be used as a way to quickly estimate what particular emission-duration combinations one would need to produce particular peak changes in different parts of the earth-system. These could then be used as a starting point for a more complex, targeted, modeling assessment. In the supplementary material of the revised manuscript see the additional comparisons of scaling predictions to a more detailed assessment of expected peak changes due to those by realistic fossil fuel emission scenarios. Using the simple scaling relationships, one could have estimated the peak perturbations to total atmospheric carbon to (in the worst case) within (17%) of the full model results.

In order to develop scaling laws based on observations, one would first need to have quantitative data on the total magnitude of emissions and their duration as well as observations of peak changes in system variables, all of this for a range of emission sizes. In the case of modern fossil fuel emissions, we have information on our emissions, however we are not yet in a position to predicts what the actual peak system perturbations will be. For the case of past changes, we have some constraints on what the peak perturbations to different system variables were, but we typically rely on models to infer the information on the total magnitude and precise duration of the emission events that caused said perturbations. In other words, the reason to develop and use model-based scaling laws is that the observational record lacks critical pieces

of information. We hope that the revised introduction now makes this point.

3 Reviewer Comment 3

I believe the authors have mischaracterized the Genie model and its application by Ridgwell, Kump and colleagues to events like the PETM. Genie has a fully interactive sediment component, similar to that in LOSCAR but calculated at each benthic grid cell. It should be listed with the Bergen model on line 10 of page 98 as an Earth system model that fully simulates the carbonate part of the global carbon cycle.

The comparison to Genie results is incorrect because it apparently presumes that Genie doesn?t have an interactive sediment module that can dissolve if overlain by undersaturated waters (or even over saturated waters, because CO2 can be produced by aerobic decomposition in the sediments during early diagenesis).

3.1 Response

The revised manuscript has been updated to address these particular concerns.

4 Reviewer Comment 4

The scaling relationships developed for d13C are based on a constant biological pump and carbon burial and thus do not allow for changes in the organic C part of the C cycle. This seriously compromises the ability of the model and the scaling relationships derived from it for fully capturing carbon cycle response to perturbation.

4.1 Response

As discussed on pg 112-113 of the original manuscript we agree that there are certainly additional important biological feedbacks that require further in-depth considerations; however, these considerations are beyond the scope of this present study. Additionally, as noted on pg 112 ln 15-25, a robust connection between changes in the biological pump and climate remains uncertain. However, we do agree that this means one cannot blindly apply the scalings developed from one epoch to the scalings across Earth history. We hope that the revised manuscript now makes this point.

5 Reviewer Comment 5

The comparison to Cui et al. also is a bit of apples and oranges because they (Cui et al.) have found that the isotopic composition of the carbonates that are being dissolved, for example, im- pacts the isotopic response of the ocean to a particular emission rate and composition. Without better knowledge of how this works in both models, a comparison of the two is likely to be misleading and mis-interpreted.

5.1 Response

After further review we agree that this comparison may be potentially misleading and has thus been removed in the revised manuscript. The comparison of the aforementioned fossil fuel scenarios in the added supplementary material will instead serve as one example of the utility of the scaling laws when comparing with detailed modeling results.

6 Reviewer Minor Comments

The authors should refer to "steady state" rather than "equilibrium" to avoid unnecessary confusion with true chemical equilibrium when referring to model states.

Line 19 on page 111 should read deep ocean pH DECREASES, right?

6.1 Response

- These corrections have been implemented in the revised manuscript.