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Climate of the Past
Discussions

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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This manuscript strives to develop scaling relationships between perturbations to the carbon cycle and the total amount of and duration of emissions of carbon to the atmosphere (reflecting such processes as volcanism, methane clathrate destabilization, or fossil fuel emissions. 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 (apparently) 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 - kCO2ËĘ(1/n) and show that n = alpha + beta, etc.

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.

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

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 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.

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

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). 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, impacts 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.

Interactive comment on Clim. Past Discuss., 11, 95, 2015.

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