

REVIEW OF: *Towles et al.* – Scaling laws for perturbations in the ocean-atmosphere system following large CO₂ emissions

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Summary

This is a neat analysis of how key global carbon cycle and proxy-relevant environmental parameters respond to a carbon perturbation (CO₂ release to the atmosphere) and how these scale with the size of emissions as well as the time-scale of the carbon release. As it stands, the paper is in relatively good shape, but needs to be significantly improved in a number of aspects.

Overarching comments

I am confused about what seems to be the underlying *précis* or null hypothesis, of the paper – that of “*simplified equilibrium considerations*”. Why, in a <100 kyr time-dependent response, would anyone assume a behavior completely consistent with “*the long-term equilibrium between CO₂ input by volcanism and CO₂ removal by silicate weathering*”? The clue here is that estimates for the time-constant of “*the long-term equilibrium between CO₂ input by volcanism and CO₂ removal by silicate weathering*” start at about 200 kyr, and some even longer than this (in LOSCAR, it is not complete even by 1 Myr). Why would something occurring transiently on e.g. order 10 kyr conform to the end result of a process that requires the best part of 1 million years to complete? Hence I just completely don’t get this argument – it makes absolutely no sense but pervades the entire manuscript (starting with the Abstract text) and sets the agenda (i.e. null hypothesis). Maybe it just needs to be explained *much* better, but more likely, I don’t see such thinking as having a logical part to play in the paper. (The study and analysis is perfectly justifiable without what seems like the creation of a false controversy.)

I appreciate the reasoning for adopting an abstracted and conceptual shape for the carbon emissions (one could chose a whole variety of alternative shapes such as pulses, but the primary findings of the study would likely be largely unchanged). What is missing however is a better connection to reality and essentially, a test of the predictions of the authors’ ‘empirical’ (in the sense that the model is based on a fit, but not on experimental or observational data) model for global environmental change in response to carbon emissions. What I am thinking of specifically, and strongly feel that is needed, is a test of the empirical description against an anthropogenic fossil fuel emissions scenario, or scenarios (run using LOSCAR and potentially also contrasted to other model projections). I’ll leave it up to the authors quite what emissions scenario to take. Obviously, emissions should follow historical reconstructions up until 2010 or 2014 or thereabouts. For terminating the emissions scenario, commonly people create a linear decline with the rate of decline chosen to create a specific total of emissions (e.g. 3000 PgC). One could also apply a logistic curve to represent historical and future emissions (e.g. see *Caldeira and Wickett* [2005]). I expect (hope for!) a relatively good correspondence between the authors’ empirical predictions and the explicitly run scenarios given that typical fossil fuel CO₂ emission scenarios have a shape not entirely unlike the authors’ assumed form (Figure 1a).

However, other emissions scenarios and particularly geological carbon release episodes might not be as easily representable in a simple symmetrical linear up/down form. Knowing then on what time-scale and for what environmental parameters, the empirical model deviates most from explicit projection, is important. The easiest scenario for the authors to pick in this context would be the PETM LOSCAR scenario of *Zeebe et al.* [2009] (*Nature Geoscience*). Comparison of empirical model with the actual results of a more complex shape of emissions will help outline a possible ‘worst case’ scenario for the applicability of the authors analysis. Note the additional (but scientifically rather healthy) challenge posed by the change with time of assumed carbon sources and hence $\delta^{13}\text{C}$ values in *Zeebe et al.* [2009].

For both these tests of the empirical model, misfits (anomalies) for key environmental variables should be calculated and appropriate discussion added. One might attempt to place some sort of confidence limits (though they will not be formal, statistical, numbers) on the model.

I have some doubts about much of the analytical analysis presented in the Discussion, which at the outset, states the key assumption: "This approximation is only valid when the aqueous CO₂ is small in comparison with the carbonate ion concentration, as it is in the modern ocean". Surely, the entire point of the overall study is assess the impacts of "large" (first line of Introduction) and "up to 50,000 PgC" (Abstract) carbon emissions, where the assumption will be quickly invalidated. Secondly, a key focus of the paper is past events, when the ratio of [CO₂]:[CO₃²⁻] would almost certainly been much greater for much of the earlier Cenozoic and mid-late Mesozoic (both pCO₂ and [Ca²⁺] higher). I don't feel that this analysis is essential to the paper, which could easily live without it, but if it is going to be done, it needs to be done properly. If the approximation turns out to be acceptable, then this needs to be demonstrated.

Lastly ... it needs to be clearer what the point of the paper is and, which might be the potential utility of the analytical expressions(?) I see this (provision of simple relationships to make forward projections of the maximum occurring global environmental change in response to massive carbon release, particularly in a paleo context) as a big plus of the paper.

Minor comments

- page 96 / line 23 – Are "super volcanoes" know sources of "large" carbon emissions? (Maybe define or give some context to 'large', and appropriately reference throughout this sentence .)
- page 97 / line 27 – Give examples of these "paleoclimate studies".
- page 97/98 – This is where the confusion is sown and a straw man created. I am not aware of long-term volcanic CO₂ emissions/weathering equilibrium/balance assumptions, being applied to much shorter-scale transient situations.
- page 98/lines 3-5 – Why? i.e. why are you interested in these carbon release amounts and time-scales, i.e. give explicit examples/references.
- page 98/lines 5-17 – There are Earth system models of 'intermediate complexity' too ... many if not most, include sediment interaction. You have sort of air-brushed out 10-15 years of modelling innovation at this point in the Introduction ;) However, you can still make the argument that for rapid assessment of events, particularly in order to explore a wide variety of emissions totals and time-scales, and potentially assess events analytically (using the equation), there is a need/role for simple (empirical) analysis.
Also at this point in the Introduction, relevant and helpful would be some discussion of existing empirical (fitted) descriptions of the impact of carbon release, e.g. some of the late 90s work by David Archer and others (e.g. 1997 and 1998 papers in GRL and GBC, respectively).
- page 98/lines 25-28 – Again, the straw man – who exactly expected an equilibrium silicate weathering response ... ?
- page 99 – I feel that the text of Section #2 would be better off as part of 'Methods'?
- page 100/line 5 – Clarify what the "background emission rate" reflects or represents (in the real World).
- page 100/lines 14-18+ – Clarify whether modern or paleo configuration of LOSCAR. Regardless, in the Discussion, mention possible caveats to basing an empirical function on one particular configuration of climate and ocean circulation, whilst applying it to a different (configuration of climate and ocean circulation). (Unless you envisage having each set of equations being for a specific past time interval.)
- page 100/lines 14-28+ – There is some overlap with Section #2, and moreover, Section #2 would arguably make more sense in the context of having a summary description of LOSCAR precede it. (Hence I think merge the Sections.)
- page 100-101 – An important caveat and need for extended Discussion, concerns the lack of explicit climate feedback in LOSCAR and what implications this might have for the subsequent analysis and empirical equation.
- page 100-101 – In Methods, we are missing any description of the model spin-up used.
- page 101/lines 15-18 – Perhaps clarify when the peak perturbation occurs relative to emissions peak and emissions end. (Given that you later vary the duration of emissions, it is not enough to know just when the emissions start.)
- page 101/line 19 – Expand on what the "carbon tail phenomenon" is.
- page 101/line 19-21 – I am pretty sure this is completely incorrect, but you do not describe exactly what in Archer *et al.* [2009] you are looking at. In general – given the short time-scale of the Archer *et al.* [2009] experiments (ca. 10 kyr) relative to silicate weathering (>100 kyr), the tail of the trajectories presented by Archer *et al.* [2009] will in general likely be dominated by ocean-sediment interactions, and contrary to your statement about being "controlled primarily by silicate weathering fluxes".
- page 102/line3 – You need to be much more careful with your wording here – total carbon, yes will be amplified, but not the effect on atmospheric pCO₂ because of the linked increase in ocean ALK. (Buckets of potential for misreading of this and confusion.)
- page 102/lines 14+15 – Define $E_{(t)}$.
- page 102/line18 - I don't find Equation (4) as having any particularly useful meaning. Perhaps try expanding on its meaning and utility.

- page 103 (and elsewhere) – When stating model years, think about whether these always need to be stated to the nearest single year, or an approximation would suffice.
- page 103/lines 5-7 – Again, I am failing to appreciate what G_{sys}^- is telling me that I need to know.
- page 103/lines 8-15 – Would Ω not be more useful to show and discuss than TA as it has obvious and rather more direct paleo-environmental and ecological relevance? (Or show both.)
- page 103/lines 17-20 – link to and reference the classic *Zachos et al.* Walvis Ridge / PETM paper. Also see *Kump et al.* [2009] (Ocean Acidification in Deep Time, *Oceanography* **22**, 94-107).
- page 103/line 25 – Why not calculate a weighted mean? (There is no justification for taking an unweighed mean.)
- page 103/line 27 - Typo (about the only one I spotted!) – “temperatures” should not be plural.
- page 103/lines 28-29 – “atmospheric temperature mostly recovers after a couple of thousands of years” – I would actually have said the opposite. Maybe clarify what you mean my “mostly”. Perhaps add an additional figure that shows the % temperature anomaly as a function of time compared to the peak anomaly.
- page 104/lines 4-17 – I like this section, if you can expand on this any more, please do.
- page 104/lines 20-21 – Please calculate weighted mean.
- page 104/line 26 – These experiments did not seem to be explicitly detailed anywhere. Could we at least seen time-series for emissions and one or two key environmental variables (e.g. $p\text{CO}_2$)?
- page 105-106 – Hints of another straw man. Why would you expect the responses to be “linear” and in “proportion to E ”? e.g. see *Goodwin and Ridgwell* [2010] (Ocean-atmosphere partitioning of anthropogenic carbon dioxide on multimillennial timescales, *Global Biogeochem. Cycles* **24**).
- page 105/line 7 – No, because ALK (TA) also changes ...
- page 105/lines 14-16 – Again, a false premise (that anyone would assume this equilibrium held always true).
- page 106 – Much of this again concerns the premise of long-term silicate weathering steady state always holding true. Also, I think there is a missing step in the equations, or rather: this could be explained better.
- page 107/line 2– Clarify how (what assumptions/relationships link with e.g. $p\text{CO}_2$) temperatures change in the LOSCAR model.
- page 107/line 11 – Give fit statistics.
- page 107/line 14 – Good (expand on if possible), but please link with existing literature.
- page 107/lines 15-17 – This is important and I know what you are talking about, but no-one else will most likely. Please clarify.
- page 107/lines 18-28 – make sure you fully explain why all of this might be of use/interest.
- page 108/lines 8-9 – Also see *Colbourn et al.* (The Rock Geochemical Model (RokGeM) v0.9, *Geosci. Model Dev.* **6**, 1543-1573), and also *Meissner et al.* [2012] (*GBC* **26**).
- page 111/line 19 – pH ‘decreases’ surely? Or technically: carbonate saturation decreases.
- page 111/112 – The paragraph spanning these pages did not make much sense to me. This stuff might be important, but needs to be explained much better.
- Table 1 – It would help to add columns detailing the values of D , E , R .
- Figure 2 – Even after 1 Myr, the system still seems far from being recovered. This is sort of counter to the assumed ca. 200 kyr time-scale of silicate weathering feedback and e.g. the ESM analysis of *Colbourn et al.* [2013]. It would be helpful to have some discussion of this, e.g. in the context of discussing possible model (and model!) caveats.
- Figure 7 – That atmospheric $\delta^{13}\text{C}$ appears to recover much faster than $p\text{CO}_2$ (e.g. Figure 2) is interesting and deserves some discussion.