

# ***Interactive comment on “Scaling laws for perturbations in the ocean–atmosphere system following large CO<sub>2</sub> emissions” by N. Towles et al.***

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## **1 Reviewer Major Comment 1**

*I am confused about what seems to be the underlying precis 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 CO<sub>2</sub> input by volcanism and CO<sub>2</sub> removal by silicate weathering". The clue here is that estimates for the time-constant of "the long-term equilibrium between CO<sub>2</sub> input by volcanism and CO<sub>2</sub> 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*

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*conform to a 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.)*

## 1.1 Response

As the referee correctly points out, given that our simulations were all for emission durations  $\leq 100\text{kyr}$  and in light of the variety of timescales involved in the interactions between the different carbon reservoirs, it seems unlikely that equilibrium balances would apply to transient emission events. Nevertheless, equilibrium assumptions have often been used to interpret climate change signals, even in cases where the signal is clearly a transient. In addition, the classical emission-weathering flux equilibrium is one of the very few balances that is simple enough and objective enough to offer quantitatively testable predictions. It is for these reasons we chose to include it as our straw man. That said, we take the referees's point that it was over-emphasized in our original submission. Accordingly, we have deleted its reference from the abstract and from most places in the text, but retaining it as part of the motivation discussion in the introduction.

## 2 Reviewer Major Comment 2

*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,*

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*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 CO<sub>2</sub> 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.*

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## 2.1 Response

We hope that the supplementary material (now included with the revised manuscript) provides the appropriate comparisons with realistic fossil fuel emission scenarios as suggested above. Additionally, we hope that the inclusion of our scaling analysis for the paleo version of LOSCAR and the corresponding discussion regarding the differences between the two configurations will provide useful insight on how scalings may be applied across different geological time periods. As discussed in the revised manuscript, future studies are required to investigate these important questions further.

### 3 Reviewer Major Comment 3

*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 CO<sub>2</sub> 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 [CO<sub>2</sub>]:[CO<sub>2</sub>?] would almost certainly been much greater for much of the earlier Cenozoic and mid-late Mesozoic (both pCO<sub>2</sub> and [Ca<sup>2+</sup>] 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.*

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### 3.1 Response

In the revised manuscript (beginning on pg 15) we have rederived the equation without making the approximation at the outset. It turns out that the final answer, expressed in terms of the bicarbonate and carbonate ion concentrations, is identical.

## 4 Reviewer Major Comment 4

*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.*

### 4.1 Response

We hope that the earlier discussion and the revisions/additions to the manuscript have clearly addressed these points.

## 5 Reviewer 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 .)*

- Super volcanoes should not be considered large in the same sense as the

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other mentioned examples. This has been removed and proper context and references have been added to the revised manuscript.

*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.*

- We have carefully revised our wording of this. We did not mean to imply that this balance had been applied to shorter-scale transient situations. As mentioned above, our intent, was rather to question, if/how well this particular balance may be applied to these intermediate timescales (100 yrs - 100kyr).

*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.*

-It was not our intent to marginalize the importance of EMICs, but rather directly contrast the limitations of comprehensive models with simple models. We have more carefully explained our targeted message as it aligns with the final statement you made.

*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*

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*different (configuration of climate and ocean circulation). (Unless you envisage having each set of equations being for a specific past time interval.)*

-This has been clarified to show that the study was using the modern configuration, where applicable (considering that the paleo scaling have now been added). We also agree with your point about the possible caveats and have made revisions accordingly.

*page 99 - I feel that the text of Section 2 would be better off as part of 'Methods'?  
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.)*

-We agree with these suggestions and have reordered the text accordingly in the revised manuscript.

*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.*

-We agree with this point and explicitly (pg 112 of original manuscript) alluded to these potentially important feedbacks that would warrant significant further analysis because the robust consideration these feedbacks is still uncertain.

*page 100-101 - In Methods, we are missing any description of the model spin-up used.*

- The model was in a known steady state (provided as the default modern configuration with LOSCAR). This can be seen in the results by looking at the 100yr run

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time prior to the onset of emissions.

*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.)*

- The chosen symmetric shape of our emissions scenarios means that the peak emissions always occurs at 1/2 the total duration. So knowing the total duration and when the peak perturbation occurs relative to emissions onset is all one needs.

*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".*

-Upon further review of our description we agree that our language could potentially be misunderstood regarding what we were referring to. We have updated our manuscript to hopefully mitigate any unnecessary confusion about the connotations of the carbon tail.

*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.)*

- The revised manuscript has been updated to to reflect this important clarification.

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*page 102/lines 14-15 - Define  $E(t)$ .*

- The revised manuscript has been updated to reflect this definition.

*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/lines 5-7 ? Again, I am failing to appreciate what  $G_{\text{sys}}$  is telling me that I need to know.*

- As briefly mentioned in pg102/ln19-21 (of the original manuscript) this tells the time dependent partitioning of carbon between the atmosphere and ocean reservoirs. To elaborate, after emissions onset a positive value  $<1$  indicates that the atmosphere reservoir contains relatively more of the perturbation. The zero crossing indicates the time when the relative system response is equivalent in the atmosphere and ocean reservoirs. For negative values  $<-1$  it means the system has amplified the perturbation and, if  $G_{\text{atm}}$  is always positive, all of the extra carbon in the system is located in the ocean reservoirs.

*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.*

- We have updated the manuscript to reflect approximations instead.

*page 103/lines 8-15 - Would  $\Omega$  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.)*

- We show TA because of its extensive use in the analysis contained in the Dis-

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cussion section. The saturation state of course would be important if we were focusing on the characterization/interpretation of a response to a real-world event; however, the case study is there to give examples of the variety of LOSCAR outputs and an example of our interpretation of that information.

*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).*

- We have updated the manuscript to reflect the appropriate inclusion of the Zachos et al. reference.

*page 103/line 25 - Why not calculate a weighted mean? (There is no justification for taking an unweighed mean.)*

*page 104/lines 20-21 ? Please calculate weighted mean.*

- We have updated the manuscript to reflect the volume-weighted means where appropriate.

*page 103/line 27 - Typo (about the only one I spotted!) ? "temperatures" should not be plural.*

-This typo has been corrected.

*page 104/line 26 - These experiments did not seem to be explicitly detailed anywhere. Could we at least see time-series for emissions and one or two key environmental variables (e.g. pCO<sub>2</sub>)?*

*Table 1 - It would help to add columns detailing the values of D, E, R.*

- The time-series in the case study results are for case 1 in the table. We do not feel that the addition of plots illustrating the higher 20000PgC case would aid in

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the overall interpretation of the results. However, the relevant E and D of each case was added to the table for clarification.

*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).*

- The statement was not meant to indicate an expectation of linearity; however, it was meant to serve as a comparison of results to those that would be expected if the system produced linear results. We have rephrased this in the updated manuscript.

*page 105/line 7 - No, because ALK (TA) also changes ...*

-We believe that the reviewer means p. 106 here? We have largely moved this section to the introduction where we discuss what can be learned from equilibrium scaling laws in order to motivate exploring whether there are transient scaling laws.

*page 107/lines 18-28 - make sure you fully explain why all of this might be of use/interest.*

- We believe the above comments have addressed this, as well as , the updates in the revised manuscript.

*page 111/line 19 - pH 'decreases' surely? Or technically: carbonate saturation decreases.*

-This typo has been corrected.

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Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/11/C650/2015/cpd-11-C650-2015-supplement.zip>

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

**CPD**

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