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Climate of the Past Discussions

Interactive comment on "Modeling the consequences on late Triassic environment of intense pulse-like degassing during the Central Atlantic Magmatic Province using the GEOCLIM model" *by* G. Paris et al.

G. Dickens (Referee)

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Dear Editors, Authors and Referees:

At sometime and at someplace in YouTube land, I recall watching a video regarding "Referee #3". Just when evaluations on a work seem straightforward, along comes commentary from "Referee #3". I never fully appreciated the video until now.

First, I always sign my reviews, because it forces me to really think about my commentary. Second, I never read other reviews before giving my evaluation, because this

might bias opinion. Third, when appropriate, I explicitly state bounds on my review, because there may be caveats. Fourth, I return my reviews in a timely fashion, because this is fair to the authors.

I dutifully adhered to my reviewing guidelines on the submitted manuscript, noting especially that I was asked to review the manuscript many months after other referees. Shortly after I sent my review, discussion on the manuscript was closed, which was wholly appropriate given the duration of time since manuscript submission. But then came commentary from "Referee #3", which in both timing and content, appears to be a reaction to my review. This is awkward across the board, especially because portions of the review make little sense, at least to me.

The submitted manuscript presents model simulations pertaining to rapid and massive carbon input across the Triassic/Jurassic boundary (TJB). Now what is particularly interesting about the manuscript is that the authors simulate responses of carbonate d13C, organic carbon d13C, atmosphere pCO2, and carbonate accumulation. This is rare, although not unprecedented, when one considers modeling exercises for other seemingly similar carbon cycle perturbations in the geological record, notably the PETM.

There are two generic (and related) problems with the manuscript. These come forth in all three reviews, now that I have read them. 1/ The actual records being targeted for model simulations are not clearly articulated. 2/ The carbon injection scenarios seem arbitrary.

Other comments across the three reviews seem valid and appropriate, except several from Referee #3, which apparently stemmed from my review. I address these here.

(A) I agree with Referee #3 that it is possible to have rapid and massive carbon inputs to the ocean and atmosphere that do not impact carbonate d13C records. The simple theoretical case occurs when the carbon source is not depleted in 13C. However, such input should impact organic carbon d13C records (because of fractionation) and

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definitely will affect carbonate accumulation. For the latter, if the carbon input is really fast, there should be carbonate dissolution followed by excess carbonate precipitation. These are primary points of the submitted manuscript, as well as of published papers regarding other time intervals (e.g., our future and the PETM).

(B) I challenge Referee #3 to suggest, and with good reason, that we, as a community, can reconstruct pCO2 and carbon cycling better across the TJB Boundary than in the Cenozoic, where we have major problems and debates with far more information. This is particularly true when published pCO2 records across the TJB diverge significantly (as I pointed out in my review), and some do not make sense (as discussed below).

(C) I agree with Referee #3 that rapid and massive carbon inputs might lead to different d13C records in different phases across Earth's surface. This should occur for multiple reasons as highlighted in several papers (e.g., Sluijs and Dickens, GBC, 2012), especially including carbon isotope fractionation, as the submitted manuscript nicely points out. (A good and appropriate paper to read on this matter is Schubert and Jahren, GCA, 2012). However, I think it a really poor suggestion that the authors should therefore ignore d13C records, and take proxy records for pCO2 to constrain their simulations.

And here is where things become muddy if one delves into details. Both the referee and the authors seemingly want to use a recently published TJB pCO2 record (Schaller et al., Science, 2011) as a valid modeling target. However, this record is puzzling at best, especially and ironically in light of commentary by Referee #3 and text in the manuscript.

Referee #3 specifically indicates the authors should ignore d13C records and focus on explaining the "established" pCO2 record. But the pCO2 record is entirely based on differences between the d13C records of soil carbonate and terrestrial organic matter.

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The authors suggest that the d13C of terrestrial organic matter should decrease significantly with a major rise in pCO2. But the data presented by Schaller et al. (2011) show no significant change in the d13C of terrestrial organic matter across the TJB; that is, the purported rises and falls in pCO2 across the TJB almost entirely derive from changes in the d13C of soil carbonate.

One cannot have things both ways. Either a rise in pCO2 changes carbon isotope fractionation during photosynthesis and the composition of organic matter (in which case the pCO2 record presented by Schaller et al., 2011 makes no sense), or it does not (in which case this aspect of modeling in the manuscript makes no sense).

As an aside, I refer to the Supplementary Information provided by Schaller et al. (2011). The pCO2 record was generated using a model regarding diffusion of CO2 from soil to the atmosphere. They have set soil CO2 at 3000 ppmv, but suggest atmospheric pCO2 exceeded this concentration during the TJB. It seems to me that this would necessarily imply diffusion of CO2 from the atmosphere to soil, but there is no discussion in their work regarding this inexplicable concept and how it affects their interpretations.

(4) Referee #3 states “Anyone can model a d13C excursion and propagate it though various reservoirs (e.g., Gerry Dickens and the PETM ad nauseum), but those all require a suite of assumptions and made up stuff that just make the story muddy and don’t apply." The last part of this statement “and made up stuff that just make the story muddy and don’t apply” is absolutely correct, as long as one ignores the above commentary.

I am, however, confused by the first part of the statement. I wonder if the referee has ever seriously – and I mean seriously – read any of my papers? At a cursory level, I think it’s very easy for one to read a few review papers and think some of my work regarding rapid and massive carbon inputs to the ocean and atmosphere all the same: methane release from the seafloor, blah, blah, blah, and all so simple and easy to discount, well unless one really understands and thinks about carbon cycling across

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Earth's surface. On this matter, I only respectfully ask that Referee #3 carefully reads Dickens et al. (Paleoceanography, 1995; Geology, 1997), where we were trying to comprehend and simulate how to reconcile massive injection of carbon into the exogenic carbon cycle with available carbon isotope and carbonate accumulation records, and Dickens (Clim. Past, 2011), where I discuss the overall issue and debate 17 years later.

I entirely agree with Referee #3 that "anyone can model a d13C excursion and propagate it though various reservoirs". However, the fact remains that very few people have actually done this exercise, and really thought about how it all must work and become incorporated into the geological record. I only pointed out some of my papers in my review to offer a framework for the authors; some of us have actually conducted model simulations for how Earth surface carbon reservoirs should respond to rapid, pulsed inputs of carbon (as another example, see Zeebe et al., Nat. Geosci., 2009).

In the end, we collectively have the makings for a really good and new YouTube video concerning "Referee #3". The referee acknowledges there is a time interval (the PETM) where we know the stratigraphic record much better than the TJB, and all evidence suggests that a rapid and massive input of carbon to the ocean and atmosphere drove major perturbations in the carbon cycle; the referee states that, because we do not fully understand carbon cycling during the PETM, we should therefore discard all studies on this time interval and focus on the limited and perplexing records across the TJB; the referee recognizes that model simulations involving direct carbon injection from volcanism cannot reproduce basic geochemical observations across the TJB, essentially for the same reasons articulated in discussions regarding the PETM at least 10+ years ago; the referee therefore concludes that primary observations that led to the modeling in the first place should be tossed away; the referee succinctly summarizes everything by suggesting a root problem: others just assume things and make stuff up. Not sure if it would reach the hit level of "Gangnam Style", but would be

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an instant classic.

As mentioned in my review, I would like to see this manuscript published, but only after deeper thought, focused presentation, and clear writing.

Sincerely

Gerald (Jerry) Dickens

Interactive comment on Clim. Past Discuss., 8, 2075, 2012.