

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)

jerry@rice.edu

Received and published: 13 January 2013

I begin by stating that I received this manuscript on December 28, and that I am not an expert on the Triassic/Jurassic Boundary (TJB). I have now read through it three times, and have very mixed views. I have not read other reviews. The following commentary comes with these bounds.

Paris and colleagues present numerical model simulations for various theoretical carbon injections in an effort to explain carbon cycle perturbations across the TJB. Much

C3106

of the effort and much of the text comes with the notion that a massive input of carbon from the mantle might cause a prominent negative carbon isotope excursion (CIE) and ocean acidification. Although various aspects remain murky, the authors, by the end, realize that mantle CO₂ is an unlikely cause for such carbon cycle perturbations. I agree.

The present manuscript is okay but frustrating to review. At one level, it is not very illuminating; however, some embedded ideas are really interesting. With some additional reading, modeling, thinking and writing, the present effort might become a really nice paper. I elaborate below.

In my opinion, the manuscript might be accepted, but only after major revisions. I trust this review is fair and constructive, and please remember the bounds stated at the outset.

Sincerely,

Gerald Dickens

Background Aside

The Paleocene-Eocene thermal maximum (PETM) is characterized by a prominent negative CIE and widespread dissolution of carbonate in deep-sea sediment. For about 18 years, these features have been attributed to a rapid and massive input of ¹³C-depleted carbon into the conventionally defined exogenic carbon cycle (ocean-atmosphere-biosphere) from some outside reservoir. More to the point, numerous papers have presented and discussed model simulations for such a carbon input. I hesitate to suggest some of my work on the topic; perhaps the most interesting to the current work would be Dickens (Bull. Geological Society France, 2000) and Dickens (Geol. Soc. London Spec. Pub. 183, 2001).

Obviously the current submission pertains to a different negative CIE in the geological record. However, from work on the PETM, the community knows certain basics.

C3107

We know that the shape, magnitude and timing of a CIE relate to characteristics of the carbon input, albeit modified because of transient changes in other carbon inputs and outputs. We know that the observable CIE, as recorded in various carbon-bearing phases, can become modified through changes in fractionation, component mixing, and local reservoir variations. We know that significant carbonate dissolution should not occur in surface reservoirs because the carbon flux, while geologically abrupt, is much slower than ocean turnover, and the weathering cycle also “kicks in”. We can suggest and test expected carbonate dissolution and subsequent overshoot precipitation in deep-water environments. We have as a community, and for a long time and for good reasons, dismissed direct carbon input from volcanism as a cause of the PETM carbon cycle perturbation. The primary reason for the latter comes from mass balance and modeling, as subsequently discussed in papers regarding the TJB, including the present submission. For the PETM, it also comes from recognition and discovery of multiple CIE excursions that seem to relate to one another in time and magnitude and to external forcing. Of course, the PETM and other early Cenozoic CIEs may have no causal relation to the TJB, except that both events (and also for example, the early Toarcian) happen to occur during times of massive volcanism, and the CIEs are really difficult to explain with conventional carbon cycle models.

The paper would improve with consideration that the TJB may have other analogs, at least at the modeling level.

Main Criticisms:

(1) The “carbon cycle targets” are not scoped correctly. As noted above, the shape, magnitude and timing of a CIE are critical to modeling the external source of carbon. Indeed, this is shown through the model results in the present submission. The nature of the carbon recorder (substrate) is also crucial, though, because the parameters of any given $\delta^{13}\text{C}$ record might be different than the true changes in the exogenic carbon cycle. There is some discussion on this matter, particularly regarding CO_2 -driven fractionation of organic carbon, which definitely should be incorporated into simulations

C3108

of a CIE (including in modeling studies of the PETM). However, a good framework for modeling is absent in the present work. Look at Figure 1. There is no age axis or values for the $\delta^{13}\text{C}$ excursion. It is really never clear what observations are being modeled. I also note that other pCO_2 estimates across the TJB have been offered (e.g., Steinhorsdottir et al., P3, 2011).

(2) For a given carbon input, the resulting CIE and carbonate accumulation response (excess dissolution followed by excess precipitation) depend on parameters of the quasi-steady state exogenic carbon cycle. For example, the amplitude of the CIE should increase with a smaller exogenic mass, the recovery of the CIE should slow with reduced external fluxes, carbonate dissolution on the seafloor should depend on initial conditions. The authors tabulate some important initial fluxes (Table 2), but not all relevant fluxes (e.g., “background volcanism”, organic carbon burial, etc.). They also do not provide information on initial masses. The basic model needs better documentation, ideally a figure and table showing all masses and fluxes important to the modeling.

(3) The details of the model simulations are neither clearly stated nor explained. For example, take Figure 5: why do the differences in $\delta^{13}\text{C}$ response occur? I am assuming that, after each pulsed carbon injection, pCO_2 rises fast and partly recovers to higher baseline because of carbon sequestration into the ocean and biosphere, as well as weathering, and $\delta^{13}\text{C}$ drops fast and partly recovers because of quasi steady state “flushing” by carbon inputs and outputs to the rock cycle. Yes, one can figure this out and suggest, if one has run carbon cycle simulations. However, the basic results should be explained in clear terms to a broad audience.

(4) The primary conclusion is: “This CAMP degassing scenario [and associated present modeling] fails to reproduce the observed negative $\delta^{13}\text{C}$ excursion . . .” This is not very enlightening, because the reasoning is not succinctly explained, and because this should have been the assumption from the start, given previous work on the PETM, the TJB and other such intervals.

C3109

(5) There remains an interesting issue introduced in the text but omitted in the modeling. From a mass balance perspective, the simplest conceptual explanation for geologically short negative CIEs is to invoke an organic capacitor (e.g., gas hydrates, as mentioned in the text, but possibly also peat or permafrost). This would be an extra box connected to the exogenic carbon cycle with slow inputs and outputs to and from the ocean, but also a mechanism for rapid output; it would be separate and distinct from the rock cycle (e.g., a magenta box with arrows to and from reservoirs 5 and 9 in Figure 2). The importance of adding a capacitor, at least from a modeling perspective, is twofold. (a) It changes perspectives and math equations on the 10^3 to 10^6 year time scale (the scale of interest to the current work). One can now have large changes in the isotope composition of the exogenic carbon cycle, but with moderately large (but not absurdly large) changes the mass of the exogenic carbon cycle. (b) It changes the interpretation and expression of the $\delta^{13}\text{C}$ signal, because of capacitor recharge. For example, following a carbon input, the capacitor grows, which drops outputs to the exogenic carbon cycle. I point this out because the authors discuss the possibility of gas hydrate dissociation several times, but use a model in the manuscript where this possibility cannot be assessed. I have offered an example of such a capacitor (Dickens, *Clim. Past*, 2011, Figure 1). Admittedly, it is pretty speculative, but it shows conceptually how such a capacitor might work.

(6) The writing is awkward in many places.

Several of the above problems appear linked to a root issue: the authors seem attached to a pre-conceived notion that CAMP directly caused observed carbon cycle changes across the TJB, including the $\delta^{13}\text{C}$ excursion(s) and apparent carbonate dissolution. Because the conclusion diverges from this idea, the manuscript then becomes a difficult construction.

The manuscript would almost necessarily improve in terms of interest and impact if they “stepped back” with the following framework:

C3110

- The TJB appears associated with a rapid and massive input of carbon to the exogenic carbon cycle. - This is evidenced by a prominent negative CIE and XXX (and show the records in terms of magnitude and time). - Various explanations for these observations have been given. - Here we address these explanations from a modeling perspective (and use a model that can explain the observations and conduct simulations that conform to observations).

I would then rearrange Sections 2 and 3, so the ideas for carbon cycle perturbations come before the modeling.

Some Specific Comments: – Figures – Figure 1 is important but difficult to follow. First, the $\delta^{13}\text{C}$ curve is a crude sketch. It needs to show actual data. (As a aside, what does «290 ky mean? The timing should relate very much to the carbon input and the parameters of the carbon cycle, and «290 ky diverges from the model simulations). Second, the stratigraphy does not make sense, at least to me. Third, where does pCO_2 data come from? Ideally, records of $\delta^{13}\text{C}$ and pCO_2 would come from the same location.

Figure 3: I do not fully appreciate panel “b”. First, there is a switch from Gt (caption) to mol (figure). More crucially, why is such a model offered when it appears to directly conflict with documentation? That is, a 10-pulse carbon injection should manifest as 10 “sawtooths” in the $\delta^{13}\text{C}$ record (Figure 5; for fun also see simulations of theoretical pulsed carbon inputs in Dickens, 2001) but this is not observed in the geological record (Figure 1).

Figure 4: Fix the caption, so that it reads “. . . evolution of (a) epicontinental . . . “ (i.e., put the letters first not last in this caption and the following ones). More importantly, I do not understand how the responses in panels “a” and “c” can be so different, given the time scale. This would seem to imply no surface ocean circulation or different transient temperature responses. This needs explanation. (A brief comment on mixing time appears on page 2092, but even this is not clear).

C3111

Figure 5: The caption should state the $\delta^{13}\text{C}$ composition of the carbon input(s). Again, more importantly, explanations of the simulations remain incomplete. For example, it is not obvious, at least in the text, why the $\delta^{13}\text{C}$ is rising while the carbon input is still occurring in the “Gaussian input” scenario. I think this may be a model effect of having a slowly decreasing input (the Gaussian tail).

Figure 6: Shift axis in panel “b” down. Give the value for the reference run. Why are the carbonate and organic carbon responses so different? (Has fractionation been included in this simulation?)

Figure 7: I would avoid using red and green together. Other than high contrast, a fraction of readers cannot discriminate because of red-green color blindness.

– p. 2077 – Line 5: Rewrite. CAMP was not emplaced AT the boundary.

– p. 2078 – Line 5: Awkward. Extended in terms of time or space? Is this supposed to be “areally extensive”?

Lines 7-10: This reads as if it is already clear that CAMP volcanism significantly impacted atmospheric chemistry. I challenge the authors to provide good references where this has been demonstrated rather than suggested. I can accept that changes in the atmosphere coincided with CAMP volcanism, especially if additional references are included (e.g., Steinthorsdottir et al., P3, 2011), but this is not causation, especially when this might be perceived in terms of carbon isotopes.

Line 14: I do not follow this as written. What then is the second excursion?

Lines 18-19: Again, this is stated as fact. I suspect that it is suggestion. (I state this because, across the PETM and other brief times with major carbon cycle perturbations in the early Cenozoic, there are far more records with much better time constraint and much greater coverage, including the deep-sea – and it is very difficult to evaluate whether the biological pump changed).

– p. 2083 – Line 5: Should be “consists of” or “comprises” not “consists in”.

C3112

Line 12-14: Does the stated duration mean from onset through recovery? Also see Comment 1. More specifically, how do these times relate to the «290 kyr in the figure?

Line 18: What are “Observation and geochemical evidences”?

Line 20: Change “be” to “have”.

Line 22: Excursion of what?

– p. 2084 – Line 2: Close-up of what?

Lines 6-15: I do not follow this logic as written. Why would successive lava flows with the same paleomagnetic direction indicate a time less than 400 yrs? How does this relate to the Deccan traps? And how then does this timing relate to that discussed for the CIE?

Line 19: Spelling.

Lines 23-25: It should be stressed that this is a theoretical end-member possibility for carbon injection. Then somewhere, it should be explained that intermediate and more complicated possibilities do not affect the basic conclusions. Ideally, the manuscript would show this.

– p. 2085 – Line 11: Rewrite as 21,200 Gt C (or 77,700 Gt CO₂). (I offer a series of asides here. Following Comment 5, I do not think Berner and Beerling (2007) put gas hydrates into their modeling exercises appropriately; however, I do think their basic interpretation is correct – it is really difficult to make the surface ocean undersaturated with respect to carbonate solubility on modest (>10e4 yr) time scales. My gut feeling is that the sedimentary records across the TJB are being misinterpreted. However, without presentation of the records, Comment 1, and without a great knowledge of the TJB, this is only speculation. I will note, though, that some authors seem to have completely misinterpreted carbonate-poor intervals on multiple continental margins spanning the PETM. With available information, these are intervals of siliclastic dilution related to enhanced physical weathering and not carbonate dissolution. See for example, Slotnick

C3113

et al., J. Geology, 2012 and references therein).

– p. 2086 – Line 9: This is interesting and important but I do not fully understand. I am not sure if “contrasts” refers to different reservoirs or the same reservoir in the time domain. A general question, as previously noted: “what causes the difference between the epicontinental seas and the open ocean?”

Line 21: Spelling.

– p. 2087 – Line 3. Needs an explanation. I assume because of increased P flux.

Line 15: Needs an explanation. I assume that this is related to carbon isotope flushing and that the carbon input flux is decaying. I note though that this is not an easy concept to convey.

Line 22: I think the key here is “minor pool”.

– p. 2088 – This is really long paragraph. I would split, probably at Line 27.

– p. 2089 – Lines 6-8: This is really interesting, but I do not completely follow as presently written. I would expand.

– P. 2092 – Lines 3-5: I do not follow this sentence as written. Clearly, the carbon cycle is perturbed! Is this supposed to mean the carbon isotope signature and carbonate saturation?

I generally agree with commentary on this page. However, I think important to ask whether there really was a significant change in the saturation state on continental margins during the TJB. One might also note that others have made such commentary (papers by Zeebe and colleagues for the PETM). (I give another aside. What happens, both theoretically and observationally, across the PETM is a rapid rise in the lysocline and CCD, followed by an overshoot, when excess carbon precipitates as carbonate.)

– P. 2093 – Lines 17-18: This is certainly correct, but not for any reason given in this submission.

C3114

Lines 25-26. I do not think this is correct, as written. It is not “break-down” and I think this pertains to shallow water.

– P. 2094 – Lines 1-2: Example of awkward writing. “It” and “expected level” are not defined. Do these words refer to productivity or pCO₂?

Lines 7-9: Example of awkward writing. Right now it reads with “not” followed by “agreement”.

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

C3115