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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.

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First of all, we thank the three referees for their comments on the paper that will bring substantial improvements of the present article. We also apologize for the delay in this response to their comments. We are excited by the positivity of their reviews and intend to reply to as many comments as possible. Some suggestions turn out to be partially contradictory however and might need further thinking from our side. The first point made by two of the referees concerns the writing and the style of the article, which is a problem that can be solved and on which I've been working. Being currently in the US, I will easily get help writing better manuscript from a purely grammatical point of view. We will not address here all of the useful writing corrections suggested by G. Dickens but they will be used in the revisions of the manuscript, together with his comments about figures and units as well as the suggestions from referees 1 and 3. Geological and geochemical background of the study and degassing scenario. The geological background of the Triassic-Jurassic Boundary (TJB) is a bit tricky. The record and shapes of the CIEs do vary from one place to the other, and deposition of each geological section occurred in significantly different contexts. Here is a compilation of all of these sections (Fig. 1 of this document).

To overcome this difficulty, we chose to present a synthetic and simplified version of the geological record. Two of the referees pointed out that by doing so, we were actually missing key pieces of information for the context of the present study. The first referee points out the unclear definition of the second CIE (or "main" CIE). The definition and even existence of this second CIE (it cannot be a "main" one if it does not exist, but is unfortunately described this way in the literature) has been challenged by the work of Ruhl et al. (2010). They show that the second CIE is actually, at least in the section of St. Audrie's bay, a switch to a different background δ13C value. The interpretation there is more complicated and we do not attempt to provide any explanation for it. However, we will be clearer about this point in the article. We agree with these points and figure 1 will be changed to include more accurate information to the reader. We might also include the overview of the different sections as well (as presented above), even if such a compilation is beyond the scope of the present paper. We are confident that by doing so, the feeling of blurriness pointed out by the reviewers will be greatly reduced.

The timing of the excursions does remain an issue. Significant improvements have been made by Ruhl et al. on the one hand, using astronomical constraints, and Schoene et al. on the other hand using U/Pb dates (Ruhl et al., 2010; Schoene et al., 2010), showing that the negative excursion itself probably lasted 20-40 ka and that the time span between the onset of the CIE and the TJB itself is shorter than 240 ka. These two studies provide valuable pieces of information and we included them in our figure 1 to provide a better constraint. The understanding of the total duration and timing of the CAMP volcanism itself improved over the time this paper was written and reviewed and we agree that including a new model run following the Schaller et al. scenario would generate even more intense degassing than the one modelled here (Schaller et al., 2011). Referee 3 points out the arbitrary choice of ten pulses. This was based on the number of 7 provided by a former study (Knight et al., 2004) and the assumption that some could have been missed. Referee number 3 makes the point that significant uncertainty exists in the Knight et al. data and more importantly, that they might not be relevant to establish a degassing scenario. Referees point out the stomata density-based reconstruction from Steinthorsdottir et al. (2011), which makes the same points as the reconstruction from McElwain et al. (McElwain et al., 1999; McElwain et al., 2009) cited in our article. We will thus include both reconstructions in our discussion. We agree that the paper could gain in clarity with a run following Schaller et al. scenario and we will run such a scenario. We want to mention however that our scenario is not that different and that the results would not be significantly changed. Carbon cycle perturbation and CIEs G. Dickens points that a lot has been learned from the PETM. We agree with this fact; the PETM is the most documented CIE. We will point this out in the present article and reference to the Toarcian is relevant indeed. There are significant differences between these two events and the TJB however. First, the level of knowledge achieved for the TJB will most likely never equal the one for the PETM, because there are no deep-sea cores of the same quality and we need to rely on less complete and less-well preserved sections. The limitation of the TJB dataset for comparison to the PETM and even the Toarcian also explains some of the limitations pointed out by G. Dickens in his comments (for example, to our knowledge, there is no record able to provide reconstruction of pCO2 and high resolution d13C record). Second, while volcanic input might not have been the cause for the PETM CIE and carbon cycle perturbation, it could be the case in this study for different reasons. (i) The TJB is markedly characterized by a significantly different carbon cycle. One of the main differences is the much less abundant carbonate pelagic production and open ocean carbonate deposition, thus limiting the CCD effect. These organisms become massively abundant during the Jurassic (Hart et al., 2003; Martin, 1995; Ridgwell, 2005), which is the reason why pelagic carbonates were restricted to epicontinental oceans in the model, making the carbon cycle possibly more sensitive to perturbations (Donnadieu et al., 2011). (ii) We are here modeling peaks of 400 years, which implies that the carbon input is actually faster than oceanic turnover. (iii) There are plenty of negative (and also positive) CIEs over the course of the Phanerozoic or earlier in Earth history. Most of them remain unexplained up to now. Within the specific framework of the PETM, and given the numerous clues and works on this event, it can be reasonably supported that the CIE is due to a biogenic methane release into the atmosphere. This is one thing. It is however possible to argue against the association of each negative CIE to a methane release, especially for geological times when data don't support such a statement. The TJB CIE is simultaneous to the CAMP eruption, at least to the emplacement of the first CAMP unit. We certainly agree that temporal correlation is not causality. This correlation is nonetheless an existing geological fact, and we try here to show that a causal link can be established, relying only on the carbon emitted from the CAMP. Accordingly, we only test the possible role of the CAMP and the isotopic signature of the associated degassing on the carbon cycle, which seems like a reasonable way of working.

The model itself Referees 2 and 3 stress some elements that are missing in the description of the model, some points requiring explanation and some additions that they believe would benefit our interpretations. In his point (2), G. Dickens asks us for more details of the model, which can be added to the paper. Point (3) is very close and indicates a lack of clarity on our side, which joins to the general remarks about the writing that we are currently improving. We have a different opinion on point (4), however, for different reasons. As we already showed, results for the PETM cannot be readily applied for the TJB because we assume a significant difference in the carbon cycle, which is not taken into account in previous models of the TJB. It is thus reasonable to start with no assumptions on its consequences, even if the results come to a similar conclusion. We might not have been clear on this point; we try to explore on the one hand the consequences of the observed CO2 emissions on the known carbon cycle, and try to vary some parameters of these known elements to generate CIEs. We show however that by simply assuming a light carbon emitted directly from the mantle, which is a likely assumption, it is possible to couple both carbon cycle perturbation and CIE with no need for an extra reservoir. Our results are consistent with the geological records and micro- and nanofossil studies. Our view on this question is the following: constraints do exist for these hydrates at the PETM, but they don't at the TJB. Furthermore, methane release is initiated by environmental perturbations (temperature, sealevel changes), and then modifies the Earth climate. Methane release is modifying the climate, but climate might be the cause of the methane release. Such problem is avoided when invoking volcanic events (here, the CAMP): the ultimate cause of the environmental perturbation is Earth internal dynamics. We admittedly do not try to focus on the negative CIE but more on the consequences of the CAMP on the carbon cycle. We decided to follow a different path to explain the CIE, namely a modification of the isotopic composition of the emitted CO2; referee 1 mentioned that this hypothesis is also being explored by other authors. Similarly, it is reasonable to assume that there were changes in the location of the source of CO2 in the course of the CAMP active phase, switching the carbon isotope values from -20 to -5 ‰Ȧs suggested by referee 3, this switch would explain why only one of the pulses would be associated with a negative excursion. We could test this scenario to, however. These are the reasons why we do also not focus the entire paper on the CIE but try to investigate also independently the global carbon cycle disturbances and the CIE. The possibility of a capacitor (G. Dickens' point (5)) is interesting and has been successfully used for the PETM but does not seem relevant here given that we do not have enough precision on the shape and intensity on the first TJB CIE, unlike the PETM. We have been able to show that realistic amount of CO2 emitted at a high rate by the CAMP would have generated significant perturbation of the carbon cycle, even without requiring an unrealistically low saturation state of the ocean at the start of our run. As stated by the referee, our model does not account for a clathrate-like capacitor, but we do not attempt to test this hypothesis in the present work. Even if such a capacitor existed at the TJB, it has not been demonstrated. Contrastingly, the emplacement and degassing from the CAMP has been demonstrated and evaluated. Referee 3 asks about the consequences of basalt emplacement in the tropics. This could be explored by changing the lithologies used in the model for the area encompassed by the CAMP as the CO2 pulse start. This point is actually interesting because this is one of the strength of the present model: the GCM module allows us to take into account the weathering feedback on climate while the oceanic box model allows us to work on both DIC speciation and differential behavior between different boxes of the model. The question might be however difficult to solve because it might require calibration for the modern weathering rates of the CAMP basalts. This is a very interesting suggestion, but further thinking is required to evaluate its feasibility.

In conclusion, we agree that the article can be more rooted to the geological and geochemical data and refer more to previous works on other events, mainly the PETM which is the most intensive work on negative CIEs. However, the carbon cycle was different at the TJB and the volcanic event likely more intense, which is why it seems reasonable to approach this modeling with different assumptions than the conclusions from the PETM or even previous modeling work for the TJB. A further degassing scenario based on Schaller et al. works would improve the present article together with consideration for lithology changes due to the CAMP emplacement.

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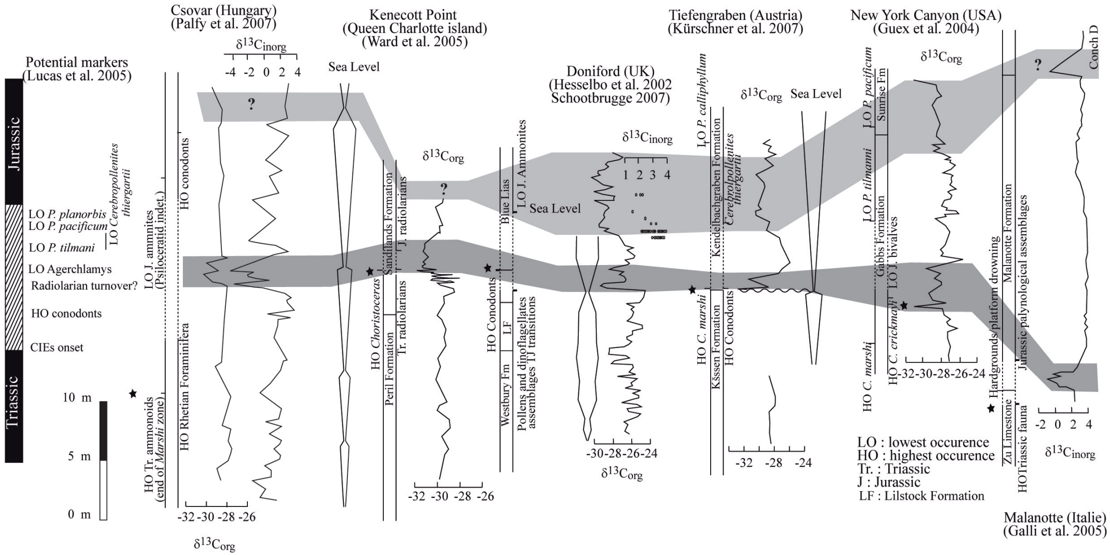

Fig. 1. Compilations of d13C data across the TJB

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Interactive Discussion

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