

Review:

Paris et al. model the degassing scenario of CO₂ released by the CAMP. This is an a very important paper that makes an exciting contribution, and it should definitely be published. However, there are a couple problems I would like to see addressed first. I have only two main criticisms, and I feel if they are dealt with then I can fully recommend the paper for publication:

1. The author's seemed to choose a rather arbitrary release schedule – 10 pulses within the canonical 500 kyr degassing period (each of 100 yr duration), or a single pulse of longer duration. However, the observable is 4 major pulses of CO₂ observed in high-resolution studies from stratigraphically continuous sections (e.g., see Schaller, 2012; Schaller et al., 2011). Moreover, a maximum constraint can be placed on these magmatic pulses as having occurred within the confines of a single precession cycle. Why don't the authors use a release schedule that's more attuned to the observables, rather than one that's essentially made up, and does not match the observations?
2. The pCO₂ record, from sediments interbedded with the flow units of the CAMP is considered to be the same as/coincident with the carbon isotope excursions observed in the marine realm... This is a little tenuous, at best (really has not yet been demonstrated). But more importantly, a pCO₂ pulse need not coincide with an isotope excursion. In fact, there could be e.g., 4 pulses, only one of which intruded some widespread coals and produced the observed isotope excursion (or tapped an isotopically depleted reservoir in the mantle). I think the real value of what the authors have done here is to model the implications of the pCO₂ release, which should be the main thrust of the work. Anyone can model a δ¹³C excursion and propagate it through various reservoirs (e.g., Gerry Dickens and the PETM ad nauseum), but those models all require a suite of assumptions and made up stuff that just make the story muddy and don't apply here... They need not apply because, in this case for a change, we have a record of pCO₂ degassing due to the CAMP extrusion that's unambiguous. For that reason, this record and modeling effort has the potential to be even more meaningful than all that PETM, etc. work (particularly because the terrestrial CAMP/lacustrine sections have extremely high sedimentation rates and are continuous).

Otherwise, if the δ¹³C excursions are considered the “primary signal”, the authors end up with the same situation that we have at all the boundaries and all the other isotope excursions: An excursion of some differential total magnitude, depending on the reservoir being sampled, and a model dictating how much of what isotope composition C needs to be released. So, my caution is to not let the δ¹³C record dictate the thinking as much as perhaps the pCO₂ data (which, in the long run, contains both the organic and inorganic δ¹³C data anyway). This also frees the authors from having to release highly depleted CO₂ from the mantle. After all, we have no data that says the pCO₂ pulses are the same as the δ¹³C excursion (any one of them could have caused a single excursion!).

The δ¹³C excursions are a red-herring: they provide no information about the size of the pCO₂ slug that produced them: There has been 20-years of PETM research and we still have no idea how big the CO₂ pulse was that caused the event (nor can the source be deduced).

With these fixed, this would be an extremely useful piece of work.

Other comments (mostly related to point 1):

Maybe I missed it, but where is the discussion on the expected drastic increase in weathering due to the emplacement of the CAMP in the humid tropics?? I would think this would be a much more profound result than the simple degassing experiment. The GEOCLIM model is perfectly tuned to look at these paleo-geographic effects on CO₂ consumption over the long-term... Schaller et al (2012 EPSL) tried this, but the model they used (COPSE) needed parameterizations that would not be necessary with GEOCLIM.

2079-10 to 20 ish (lines 20)

Authors should use the actual record of degassing from the eastern North American sections – despite being continental, the record there is actually quite stunning! The “degassing model” proposed by Knight is not actually based on a data set that’s appropriate for that task. The problem is not answerable or even really addressable via 40-39 dating methods because the error associated with those measurements is larger than the target (i.e., duration of the volcanic pulses).

What Knight had done is perfect for reconstructing the local cooling history of the basalt flow units, but does not tell one much about the global duration of these magmatic events other than that these local manifestations probably occurred within x yrs of one another. It is an inference to the global extent and timing, although all indicators favor an extremely rapid (e.g., maybe less than 1kyr?) extrusion for the pulses...

Without the stratigraphic context afforded by some superposition with well-understood sedimentary units between the flow units themselves, Knight can only tell that the flows and flow units cooled within a short time of one another, but cannot determine how much time the entire lava pile represents (and for this they rely on Ar/Ar dates, which are possibly the worst tool for this job – see comments below).

What’s needed to solve that problem is higher resolution, continuous stratigraphy through the magmatic units.

Schaller et al. (2012) has shown that there are 4 major pulses of CO₂ within a 750kyr time period, and with constant sedimentation and a complete section, it is unlikely that any significant CO₂ producing magmatic events were missed. The observed CO₂ pulses themselves, if indeed associated with the magmatic pulses (also an inference) are much better global indicators because we know the atmosphere to be homogeneous on that timescale.

2081-10 – Does this hold true for when you add the CAMP extrusives to the continents (e.g., Dessert et al. 2001)?

Spatially averaging the change in lithology over every grid cell may not be an appropriate way to deal with the potential change in lithology here – the tropics are disproportionately important (e.g., Godderis et al., 2008), and the CAMP adds a huge amount of very reactive basalt right to that equatorial humid belt.

2082-20 – Authors are allowing the $\delta^{13}\text{C}$ records here to have too much influence over the modeling! Think about it – if we knew the actual atmospheric pCO₂ concentrations at the PETM, we’d have figured that problem out years ago. An isotope excursion probably means a pCO₂ pulse, but NOT

vice versa. A pCO₂ pulse can be independent (having a value the same as the atmosphere), and therefore go unnoticed isotopically.

Moreover, when measured anywhere on earth's surface, pCO₂ is more or less relevant to the globe and is not a local phenomena. This paper will be cited much more if the pCO₂ pulses themselves (as have been observed in Greenland/Sweeden (McElwain et al., 1999; Steinthorsdottir et al., 2011), and especially in Eastern North America (Schaller et al., 2011; 2012). Schaller et al. even has CAMP lava flow units in superposition with those pCO₂ estimates in the Newark, so they unambiguously may be related to CAMP degassing, with a cycle-stratigraphic timescale that puts some firm constraints on the "long" release suggested here. No need to fiddle with these silly δ¹³C excursions. They are great when we lack measurements of the actual concentration, but are not very useful in light of the actual observed pCO₂ record other than as auxiliary data, simply because they provide no information about the size of the pCO₂ slug that produced them.

2083- 5 to 20: The authors could actually figure all this out if they simply inverted the problem, as I've suggested. Use the observed pulses to model the outcome, and try to fit these observations (many of them are very tenuous and not deserving of the discussion here)

2083-10: Ar/Ar Cannot constrain the duration of the CAMP because the target is well within the error on these measurements. The only relevant radiometric means useful for determining duration are U/Pb – see Schone et al. (2010), for superior dates. FYI, a paper by Sam Bowring's group presented at AGU in 2012 shows high-precision U-Pb ages that confirm the cycle-stratigraphic model of Olsen to within 10 kyr(!):

Blackburn, T.J., Olsen, P.E., Bowring, S.A., McLean, N.M., Kent, D.V., Puffer, J.H., McHone, G., Rasbury, T., High-precision U-Pb zircon geochronological constraints on the End-Triassic Mass Extinction, the late Triassic Astronomical Time Scale and geochemical evolution of CAMP magmatism. AGU, 2012 Fall Meeting, Abstract U51A-11

Maybe we can finally put some of the severe bias introduced by 40-39 dating to bed, but for the time being those radiometric dates are most often used in defense of a protracted duration for each pulse, and a smearing of the individual pulses together, both of which are simply artifacts of an using a sledge hammer where tweezers are more appropriate. I urge the authors not to bother with the Ar-Ar literature other than to provide overall context.

2084 – section 3.2: As I've commented, why not use observational evidence to model a more rational scenario in addition to the 10-pulses or a single pulse? I see the merit in both approaches, but it makes for a much more interesting piece of work if at least one of those runs models the observations (and the other two can remain one pulse vs. 10 pulses).

2085 – you cite Schaller – his 2011, 2012 records are perfect for this approach, why the need for hypothetical's?

2087-20: As an isotope geochemist, this is perfectly reasonable (and supported by evidence), but it should be noted that these values are not necessary because the δ¹³C excursion could due to one or all of them... the stratigraphy allows both interpretations.

2092-5: (Martindale et al., 2010) has observed a marked decrease in reef productivity at this time. How do these observations fit with the model output of increased productivity? Can a 30% decrease in carbonate productivity account for these data?

2092-22-26: Why is it unreasonable that this degassing scenario occurred in 1000-years or less? The 21,220 GtC released over 4 pulses of less than 100-years each would have a much more profound effect than divided over 10 pulses... There's no real justification for this (or a long timescale)...

References.

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