

## ***Interactive comment on “Methane release from gas hydrate systems during the Paleocene-Eocene thermal maximum and other past hyperthermal events: setting appropriate parameters for discussion” by G. R. Dickens***

**G. Dickens**

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Dear Gabe,

Thank you for the review and comments. It delights me immensely that the manuscript “left (you) interested and wanting to read more” – the underlying purpose behind the submission was to get people from several Earth science communities collectively thinking.

You raise some very good points. I have tried to dissect and number them, and respond

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thusly.

1/ With regards to objectivity, I am very glad that the submitted version passed by Jan Backman’s desk a few times first! Basically, the manuscript began as a rant after reading and reviewing numerous (15+) papers and proposals in the last year. As I see things, a series of poorly conceived arguments against gas hydrate dissociation [as a cause of the d13C excursion across the PETM] have emerged over the last 8 years; all were presented in earlier work, along with caveats; many recent papers and proposals seem to skip a careful reading, referencing and digesting of existing literature and ideas; things have reached the point where papers outright dismiss gas hydrate dissociation on unjustified grounds of mass balance and then offer plausible alternatives with much worse mass balance considerations. The initial drafts were far less diplomatic, and it’s always good to pass such writings through a Swede first!

2/ I am not sure how to best include the Schmidt and Shindell (2003) and Rensson et al. (2004) papers. I know both papers very well; however, they neither add nor subtract anything to the present discussion.

3/ I have rewritten the paragraph that you, Matt, Ellen and others found “objectionable”. The key point is that seafloor methane cannot be the sole primary cause of the PETM, despite numerous papers that begin with this notion (often incorrectly citing my work). This does not discount the concept that methane may be a very important response and feedback to environmental perturbations across the PETM (and other hyperthermal events).

4/ I am unsure how best to respond to the issue of carbon isotope excursion magnitudes. I think that, in many places, one or more processes have modified the amplitude. As an intriguing example, we are stuck with our New Zealand sections, because the PETM is beautifully expressed, but with only a -1.5 per mil excursion in d13C. Even more interesting, the other hyperthermal events are consistently damped by the same proportion, so it appears that the same mechanism affecting the d13C record

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in these locations occurred multiple times!

A model showing how carbon isotopes of CO<sub>2</sub> and DIC should be distributed across the globe before, during, and after the PETM (and other hyperthermal events) would be tremendously helpful! I will add this point in the hope that it will prompt someone (you – ha ha) to generate this.

5/ The speculation on the sulfur cycle is probably the most interesting, and if right, most enlightening part of the paper. All inferences you state are correct, except the last one: the recharge depends on the fluxes. In particular, if the “quasi steady-state” inputs and outputs of methane are on the order of  $1 \times 10^{12}$  mol/yr, and the outputs are diminished (say by half) over 200,000 years of cooling following a hyperthermal event, then 1200 Gt C can be added back to the reservoir.

However, I agree with the assessment, and I thus have clarified the text, and have added some simple mass balance calculations from a series of numerical modeling exercises.

I will be honest: I can get things to generally conform to available records (well I think so) in independent models for the sulfur, carbon and “capacitor” boxes; however, I have been unable to get everything working in a truly coupled model. It’s frustrating! On the other hand, I don’t think anyone else has ever tried to couple the two cycles through AOM.

Will correct the minor comments.

Jerry

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Interactive comment on Clim. Past Discuss., 7, 1139, 2011.