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Climate of the Past Discussions

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

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This is a really nicely written review and synthesis of one of the leading models for carbon input to the ocean/atmosphere system during early Paleogene hyperthermals – namely the thermal destabilization and release of methane from seafloor hydrates. With a small number of exceptions it is comprehensive and for the very clearly presented, and represents a nice follow-on to the recent, more broadly focused review of McInerny and Wing. It raises some important points that should help to re-focus the community's evaluation of mechanisms underlying carbon cycle perturbations as-

sociated with PETM hyperthermals, most importantly with respect to the connection between carbon cycle and climate change and some of the basic parameters and constraints on the methane hydrate model. It left me interested and wanting to read more, which is a good sign for any such effort.

There are a handful of sections where the balance between the author's personal perspective based on his substantial track record of work and objective synthesis and presentation of the work of others was lost. The comment already offered by Matt Huber (which was spot on – although Dickens may not have suggested atmospheric CH4 forcing of climate both (Schmidt and Shindell, 2003) and (Renssen et al., 2004) have) is a relatively minor example. Probably more significant, I found some of the discussion on the CIE magnitude issue to be selective and the conclusions tenuous. I (obviously) agree that this is an important issue, and that various authors have selectively steered the discussion in one way or another depending on their perspectives. The current text does this as well, and while it might buck it clearly won't be the last word on this issue. A few interesting case studies and back-of-the-envelope calculations are presented, but no global model for the modulation of CIE amplitude in different environments is presented, and I'd like to see this acknowledged.

I enjoyed reading the speculative material on coupling of the methane hydrate capacitor and the Paleogene S cycle. I'm not sure I followed or am convinced of some of the conclusion advanced, however, and I think this section needs to be fleshed out or made a bit more quantitative if it's to be useful. . .the key ideas are floated and then kind of abandoned mid-stream. I think I see the challenge here – as I understand it the argument has 2 facets. 1) The decoupling of the C and S isotope cycles (as indicated by isotope trends) in the early Paleocene could be explained by the growth of a methane hydrate capacitor because this would divert CH4 from AOM that would otherwise support sulfide burial, whereas during the degradation of this capacitor (57 – 50 Ma) enhanced leakage of CH4 and AMO could drive the pyrite burial spike inferred by (Kurtz et al., 2003). 2) Massive CH4 release does not necessarily enhance sulfide

mineral burial because most of the methane bypasses AOM, so we wouldn't expect to see the same kind of catastrophic changes in the S isotope record as represented by the PETM, ELMO, etc. CIE's (additionally there is a reservoir size factor here). It seems to me that the argument hinges on the details of how the Paleocene capacitor was depleted. According to the model both catastrophic release (as argued for the hyperthermals) and subsequent replenishment of the capacitor have no net or negative effect on sulfide burial. So if a capacitor of ~6,000 gigatons was depleted by, say, 75% throughout this time interval, and the PETM, ETM1, and ETM2 required catastrophic release of 2,000, 1,000, and 500 gigatons, than leaves you with very little power to drive sulfide burial. . .basically all your net change in capacitor size is being driven by the catastrophic events. Clearly better models are needed and the author is not in a place to present a rigorous assessment of the model right yet, but the current presentation throws and idea out there and then 'bails' on it before (in my opinion) enough support is presented to justify it as an idea for serious consideration rather than just a 'wouldn't this be neat' type brainstorm. . .I'd like to see more.

A few minor comments:

Lines 16 – 20, p. 1160: I don't understand the conclusion drawn here that the numbers are in conflict. . .they seem pretty close given the uncertainty involved. Please elaborate or re-state.

Lines 24-27, p. 1162: The work of (Beerling, 2000) provides some context here.

Lines 8 – 11, p. 1156: I agree we shouldn't get out of hand with 'escalation' of C release numbers, but none of the recent assessments are consistent with 2,000 Gt.

Beerling, D.J., 2000. Increased terrestrial carbon storage across the Palaeocene-Eocene boundary. Palaeogeography, Palaeoclimatology, Palaeoecology, 161: 395-405.

Kurtz, A.C., Kump, L.R., Arthur, M.A., Zachos, J.C. and Paytan, A., 2003. Early Ceno-

zoic decoupling of the global carbon and sulfur cycles. Paleoceanography, 18, 1090, doi:10.1029/2003PA000908.

Renssen, H., Beets, C.J., Fichefet, T., Goosse, H. and Kroon, D., 2004. Modeling the climate response to a massive methane release from gas hydrates. Paleoceanography, 19, PA2010, doi:10.1029/2003PA000968.

Schmidt, G.A. and Shindell, D.T., 2003. Atmospheric composition, radiative forcing, and climate change as a consequence of a massive methane release from gas hydrates. Paleoceanography, 18(1): 1004, 10.1029/2002PA000757.

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