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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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Comments by Ellen Thomas on 'Methane Release from Gas Hydrate Systems during the Paleocene-Eocene Thermal Maximum and other past Hyperthermal Events: setting appropriate parameters for discussion', submitted to Climate of the Past by G. R. Dickens

Overall, I liked to read this paper and not only because I agree with the author that we should not yet decide that there is no role for gas hydrate dissociation in the expla-

nation of hyperthermal events. I think this is an interesting contribution to the overall hyperthermal debate and should be published. I do have some remarks, however.

The size of the Carbon Isotope Excursion (CIE) during the Paleocene Eocene Thermal maximum (PETM) in various carbonate and organic components, as mentioned in lines 40-44, 212-227. As the authors says, the CIE has been seen in pretty much all components of the carbon cycle, but with different magnitude, and it is not easy to derive the 'true, globally averaged' CIE, due to the complex interactions and feedback within the biosphere, as well as problems with preservation of the maximum signal. At least some benthic foraminiferal records from open ocean show a larger magnitude than 2.5-3 per mille: see McCarren et al., 2008 (G3, vol. 9 no 10, Q10008) who argue for ' a relatively rapid excursion of at least 3.5 o/oo but no more than 5.0 o/oo', rather similar to the numbers in Handley et al., 2008. See this article also for comparison of carbonate and organic records from the same location. It is certainly true that this value may not be representative of whole-ocean values, because it might reflect a mid-water plume of some sort, but it is a benthic, non-coastal value.

In the coastal waters of New Jersey the CIE was very large (up to 5 per mille or so), also in benthic foraminifers, and not only in planktic foraminifera, so you would have to argue for lowered salinities not only in the surface waters, but also at the bottom. These carbon isotope values were not the most extreme at the more seaward Bass River, they were more extreme at more landwards sites such as Wilson lake (see e.g. John et al., 2008, Paleoceanography23, PA 2217; compilation in fig. 1 in Cramer & Kent 2005, Palaeo3 224, 144-166; Zachos et al. 2006 Geology 34, 737-740). This difference between Wilson Lake and Bass River is confirmed by a set of measurements (not yet published) on quite a few additional species of benthic foraminifera (Stassen, P. Thomas, E. and Speijer, R. P., 2011. Dissecting the PETM along the New Jersey Coastal Plain. Climate and Biota of the early Paleogene (CBEP 2011), 5th-8th June 2011, Salzburg, Austria). The land-sea difference may indeed be seen as indicative of influx of fresh water with isotopically light DIC. Sluijs et al argued that there was

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little particulate terrigenous material in the fresh water supplied to the New Jersey sections (not many pollen grains in the dinoflagellate samples), but Kopp et al. 2007 (Paleoceanography 22, PA4103) indeed think that there was major river influence. In my opinion, however, one should not argue that a 5 psu unit drop in salinity reflects a 1-2 per mille decrease in d13C of DIC: this clearly depends upon the d13C value of the river water under discussion as well as of the DIC concentration in the rivers relative to these in the oceans (see e.g. Thomas et al., 2000, Journal of Coastal Research, 16: 641-655, fig. 7; see http://ethomas.web.wesleyan.edu/lisweb for mixing model for Long Island Sound. In this present coastal water body there IS a salinity effect, but there is a much larger effect on benthic foram d13C due to organic carbon oxidation during eutrophication - maybe also during PETM?).

Lines 135-137: 'CH4 is rapidly oxidized to CO2 in the ocean and atmosphere. There has never been a suggestion that CH4 inputs from the seafloor entered the atmosphere..'. Like Matt Huber, I disagree with that statement. After all, Dickens et al. 1995 include the sentence 'the 4oC increase in bottom water temperature led to a significant change in sediment thermal gradients, dissociation of large quantitities of oceanic hydrate, and release of CH4 to the ocean/atmosphere inorganic carbon reservoir'. There have been many more such suggestions, and it has been argued that how much of the CH4 entered the atmosphere depends upon the process of dissociation (rapid or slow dissociation). It is true that the bubbles in fig. 1 in Katz et al (1999, Science 286, 1531-1533) do not reach the sea surface, but in the present oceans CH4 from gas hydrates may reach the atmosphere (e.g., Leifer & MacDonald 2003, EPSL 210 411-424). Bubble dynamics have been discussed and a way of transferring methane to the atmosphere has likewise been discussed (e.g, the 'methane-driven ocean eruptions' similar to the gas eruptions from stratified volcanic lakes such as lake Nyos and Kivu) by Zhang & Kling (2006, Annu. Rev. Earth Planet. Sci. 34, 293-324), who conclude 'Therefore, if there is a large amount of methane hydrate, and methane gas is released to the seawater column, under the right conditions it is possible that a large and concentrated bubbly plume could form at about 500 m water depth and lead to a

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surface eruption.' Nisbet et al. 2009 (Nature Geoscience 2, 156-159) want to trigger the main PETM by just such an 'ocean eruption' in the North Atlantic Kilda Basin, and say 'Therefore a methane burst capable of triggering the PETM is needed to cause warming over a period of well over a century. During this initial period immediately after the trigger, the atmospheric methane residence time must have increased manyfold'. The recent paper by Carozza et al. (2001, GRL 38, L05702) says 'Among hypotheses to explain the massive carbon input at the start of the PETM, the methane hydrate hypothesis [Dickens et al., 1995] and the thermogenic CH4 hypothesis [Svensen et al., 2004] involve the release of CH4 into the ocean and/or atmosphere.' Schmidt and Shindell (2003, Paleoceanography 18, 1004) discuss the effects of CH4 in the atmosphere after has hydrate dissociation, as do Renssen et al., 2004, Paleoceanography 19, PA 2010. Finally, in some scenarios there is so much methane emitted that complete oxidation within the oceans must have led to whole-ocean anoxia, which did not occur as far as we can see from the sedimentary record.

Lines 149: 'model simulations have shown that intermediate waters can suddenly warm; McCarren et al. 2008 (G3, vol. 9 no 10, Q10008) show evidence that they did, at Walvis Ridge.

Lines 153-163: for more discussion of the comparison between warming and magnitude of the CIE during ETM2, see also Stap et al., 2010 a (Geology 38, 607-610), e.g. The magnitudes of the d13C and d18O excursions of both events (ETM2, H2) are significantly smaller than those during the PETM, but their coherent relation indicates that the d13C change of the exogenic carbon pool was similarly related to warming during these events, despite the much more gradual and transitioned onset of ETM2 and H2) and Stap et al. 2010b (G3, vol 11, no 11, Q11011) 'Coherent pattern and timing of the carbon isotope excursion and warming during Eocene Thermal Maximum 2 as recorded in planktic and benthic foraminifera'.

Line 175: see also Nisbet et al. 2009 as cited above.

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Lines 190-210: I certainly agree that CaCO3 dissolution during the PETM was heterogeneous, with more severe dissolution in the North Atlantic (although also at some South Atlantic locations), see Thomas 1998 In Aubry et al. Late Paleocene-early Eocene Biotic and Climatic Events in the Marine and Terrestrial Records, Columbia University Press, 214-243. However, dissolution patterns in Dickens et al 1997 indicate much less dissolution than observed in all ocean basins.

Lines 270-272: There are indeed difficulties in explaining the isotope records of Acarinina and Subbotina due to possible effects due to changes in habitat of these organisms; see Stap et al., 2010 b cited above for a discussion of this topic with regard to ETM2.

Lines 358-388: the estimate of gas hydrate volumes present during the Paleocene rests on assumptions about the supply of organic matter to the seafloor (line 355). I have made a speculative argument that during the Paleogene Greenhouse, organic matter supplied to the seafloor further offshore was more similar to organic matter supplied on continental margins than it is today. Today, there are very clear differences between continental margin and open-ocean deep-sea benthic foraminiferal faunas, with margin faunas supposedly reflecting a larger supply of organic matter (food). In the warm Paleogene, the differences in faunal composition between deep-sea open-ocean benthic assemblages and continental margin assemblages was much less pronounced, suggesting that the food supply to these two environments was more similar (Thomas et al., 2000 in Huber et al., eds., Warm Climates in Earth History, Cambridge University Press, 132-160; Thomas 2007 Geological Society of America Special Paper, 424: 1-24.

Lines 425-434: in my opinion the argument in Higgins & Schrag 2006 is indeed not very strong: their figure 3 shows various possible epicontinental basins which might have dried up so that organic-carbon rich sediment could have been oxidized, but in most of these proposed basins (e.g. Egypt, Tunisia, Caucasus) the PETM is recovered in marine sections (e.g., the type section for the P/E boundary, Ouda et al., 2003, Micropaleontology Special volume 49), so that these basins clearly did not dry out, as

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also argued by the author. In fact, in many of the Caucasian basins from the Black Sea to the Sea of Aral (e.g. Gavrilov et al 2003 in GSA Spec Pap 369) and not only in the Turgay Straits were extremely organic rich sediments deposited during the PETM.

Lines 457-459: for the age of the PETM see also Charles et al., in press on the numerical age of the Paleocene/Eocene in Svalbard, in press G3.

467-468: for link between North Atlantic volcanism and the PETM in an early paper, see Eldholm & Thomas 1993, EPSL117, 319-329.

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