

Interactive comment on “A two-dimensional model of the passive coastal margin deep sedimentary carbon and methane cycles” by D. E. Archer et al.

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Received and published: 16 March 2012

Buried within the text and behind the figures is the backbone to a really interesting and important paper. Archer and colleagues present model results that systematically show how various factors affect the amount of methane in sediment across a passive continental margin (ostensibly that off the eastern United States). Crucially and entirely new, the modeling includes sediment accumulation, fluid transport, and carbon cycling within sediment over a very long time (140 million years). This is the path, in my opinion, to understanding methane on continental margins. The cross-sectional (rather than planar) 2-D approach perpendicular to a margin, although advocated over ten years ago (Dickens, *Org. Geochem.*, 2001), is also fairly novel. Additionally, the timing of the manuscript is excellent: through the model results, it touches upon several

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current and controversial issues.

Unfortunately, the manuscript is exceedingly difficult to read and review for multiple reasons.

(1) It has very little background. In particular, after reading the Introduction, it is not clear why the study was conducted, what new aspects the manuscript brings to the table, or how the work will move things forward. This problem, which in part stems from other issues (below), pervades the manuscript. It is very much like diving into an unknown pool and dogpaddling in the dark. Yes, after multiple readings and struggles with the figures, key points eventually emerge, although often with questions that are not discussed.

(2) It contains excessive jargon and convoluted sentences. Indeed, for 30-50% of the paragraphs, I had to read multiple times, bouncing back and forth between figures and other paragraphs, just to understand what the authors are trying to say. Seriously, and for example, take either of the following paragraphs – [p.2924](#), [p.2930](#) – give to a colleague, and ask for a summary.

(3) It has numerous incomplete thoughts. A statement is made but not explained (and typically without reference). I have noted several examples below. Basically, it touches on many sub-topics with little depth, and I found myself continually asking “why?”

(4) It has several “off-hand” comments (effectively “asides”) with little context or support. I have noted some examples below. Either refrain or explain.

(5) It often omits comparison and discussion of field data (i.e., the observables from which to frame and assess the modeling). This comment pertains to model assumptions as well as to model results. Some omission is fine, given that the main goal of the study seems to be an examination of how various parameters affect methane accumulation in sediment over geological time. However, in some places, it is problematic because field data indicate fundamental flaws in the modeling. This has to

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be addressed, ideally with commentary as to how changes in assumptions will impact model results.

(6) It is poorly referenced. Key literature, well at least in my opinion, is missing. More importantly, numerous statements are asserted as fact with no acknowledgement to where the concept derived. For better or worse, this leaves a false impression that various ideas are well known facts, or that the authors conceived them. This is particularly problematic for relatively new concepts, especially ones that are controversial. I have noted several examples below.

(7) Discussion of the results, including implications, is mostly absent.

(8) It comes with many figures that seem very good and straightforward at first glance, but then become problematic and intriguing in the details. Indeed, the gloss on this manuscript, from text through figures, jeopardizes its significance considerably.

As to a recommendation on publication: the authors are top-notch scientists with outstanding track records; ideally, they would fix the manuscript and make it shine. Yes, this would take work, including some rethinking and significant rewriting. However, they would have a far superior and more enlightening paper in the end. Alternatively, if they want to convey an appearance of pedestal arrogance – I know it's "cool" in some socially backward circles, such as geochemists in academia at elite institutions – the current manuscript is a moderately good effort and mostly okay, well, except for a few major problems (below).

Sincerely,

Gerald Dickens

I first tried plowing through the manuscript, and frankly got frustrated for reasons 1-7 noted above. I then attempted to understand the work through the figures alone. This is like eating Vegemite for the first time with no warning: dense, interesting, complicated, confusing. Do I like or not? It's hard to tell without accompaniments.

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First, the use of Myr in several of the figures is awkward. This is because modeling pushes things with time moving forward, and geology places things with time moving backward. It does not help matters that Myr, Ma, and other such abbreviations have different meanings to different communities. In a nutshell, the problem is that 140 Myr in the model results means present-day (i.e., 0 million years ago) while 0 Myr in the model results means 140 million years ago. I would fix this in the figures (and text) so it is crystal clear to even the most casual reader.

Second, there are no figures that show or explain what they are trying to model. It would be very helpful to have some sort of conceptual framework that includes the basics.

Third, there is little comparison to field data. Ideally, they might have a few representative locations placed on most figures with a related table, so one can compare observables to model results.

With regards to specific figures: Figure 1. This is an interesting figure. However, the average geothermal gradient is about 20°C/km. This seems too low. I think the average might be above 30°C/km for most continental margins, even passive ones, but there are no references provided. In any case, on Blake Ridge, it is 30-35°C/km (e.g., Paull et al., Proc. ODP, Init. Rep., 164). Also, are the low but increasing temperatures outward across the shelf correct?

Figure 2. This is another interesting figure. However, I do not follow the b panels. What do the different curves represent? Following from above, are the thicknesses of sediment and grain sizes reasonable?

Figure 3. I am no longer clear what exactly is being modeled. I assumed at first that it was sediment accumulation above the J/K Boundary ca. 145 Ma as this is the approximate time scale relevant to the modeling. However, the modeled depths appear to represent sediment above basement, which is closer to 200 Ma. I doubt this makes a major difference to various problems addressed in the manuscript, but it

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needs clarification. Also, is this a representative cross-section of a passive margin? I suspect so, but there is no discussion in the text. (Of course, the limestone reef complex is an interesting and problematic twist to the modeling, but I think it can be ignored, perhaps with a brief comment).

Figure 4. This figure is hard to follow. First, the caption is oddly worded. What does the sentence actually mean? Second, what are the lines in panel a, and where do these come from? I assume these are sea-level curves, but they do not conform to recent ones that I am familiar with (e.g., Müller et al., *Science*, 2008). The numbering in panel b is uncertain. Do these numbers refer to different "oxic states" used in the modeling?

Figure 5. I do not follow panel b. What does a porosity of 200% mean?

Figure 6. I understand the general concept of permeable pathways, but the dimensions make little sense. Where is there evidence for >3 km wide permeable pathways (chimneys)? And what would be the mechanism for such chimneys? More crucially, does the width (horizontal area) of such chimneys affect model results? Although not referenced, there is definitely evidence for vertical permeable pathways in gas hydrate systems, including on Blake Ridge (e.g., Gorman et al., *Geology*, 2002). However, the horizontal scale is totally wrong.

Figure 7. I assume that velocity is such that up is positive (nb. the vector is not consistent across various modeling studies). So, why does the velocity appear to decrease in some chimneys? Alternatively, are these zones of downward flow between fluid conduits? This cannot be discerned from the figures.

Figure 9. The age labels seem incorrect. I think the top two are for 70 Myr, and the bottom two for 140 Myr. In any case, why does the H/C ratio landing on the seafloor change over time? Are the values reasonable for the time/depth of interest? Does the modeling include terrestrial and marine organic carbon?

Figure 10. This figure is totally unclear. What are the units for methane sources? Do

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these pertain to present-day sources or some integrated accumulation over time?

Figure 11. Why is the distribution of dissolved methane concentrations significantly different than that of methane sources? Does this reflect changes in the location of the (unspecified, above) sources over time or the net effects of formation, migration and consumption? Why are methane concentrations greater than saturation? It would very helpful (and intriguing) in this case to see the profiles at the half-way point (i.e., at 70 Ma).

Figure 12. This figure needs a blow-up of the upper part of the sediment column and modified scaling. Right now, as best as I can figure, only two grid cells have gas hydrate in one model run. Obviously, this raises some issues, considering that gas hydrate can be detected or recovered at multiple locations across the margin. The scale in panel e seems incorrect.

Figure 13. This figure is very hard to read, given the font size and lack of proper axis labels. In any case, what is the relevance of the Sivan et al. (2007) DIC/alkalinity profile, which as best as I can tell, pertains to Site 1081 at 800 m water depth off the coast of Namibia, a site with relatively low methane concentrations? Why not use and discuss the alkalinity profiles from sites on Blake Ridge, or other passive margin locations with high methane concentrations in sediment? These sites have much higher alkalinity concentrations over a more extended depth range; indeed, they are closer to the modeled profile. Far more important: the simulated DIC profile indicates some basic modeling problems. This is because it has ~100 mM at the seafloor and a maximum at ~700 m below the seafloor (mbsf). The first value should be ~2mM for all locations across the slope; the second value should be much higher in the sediment column. I think the latter may stem from my comment on Figure 1: the geothermal gradient is too low.

Figure 14. Again, this is very hard to read, given the font size. Similar to commentary on Figure 13, the relevance of the Sivan et al. (2007) $\delta^{13}\text{C}$ -DIC curve is not obvious,

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and the simulated d13C-DIC profile indicates a basic modeling problem. In this case, the problem lies in the sharp depletion of d13C, which, as it happens, corresponds to the sulfate-methane transition (SMT) at numerous sites with gas hydrate (e.g., see Figure 2 in Chatterjee et al., JGR, 2011). The modeling does not simulate this prominent drop in d13C, and yet, in my opinion, it is absolutely fundamental toward understanding methane-charged sediment sequences on the slope. There are multiple interpretations for the drop in d13C of DIC across the SMT (see discussion in the above paper). However, I think that, when one carefully examines and models the physics and chemistry of these systems, it represents the mixing of 13C-depleted DIC produced by AOM at the SMT and 13C-enriched DIC produced by methanogenesis and rising from depth (as noted by Sivan et al., 2007 and modeled in detail by Chatterjee et al., 2011). This contrasts significantly with the text on page 2939.

I assume that the mismatches between the modeled and observed DIC and d13C-DIC in Figures 13 and 14 are not a grid cell problem. So, why are these particularly illuminating? Somewhere, there is a serious carbon mass balance problem in the modeling. Basically, the upward fluxes of methane and DIC are considerably off target. Possibilities include that too little methane is accumulating and escaping over time, too much DIC is accumulating and escaping over time, or some combination of both. Alternatively, too much POC is being consumed by sulfate reduction in shallow sediment. I cannot tell from available information and model descriptions, but something is definitely incorrect. Until this issue is acknowledged and rectified, all results pertaining to carbon species (e.g., methane, DIC) – the main point of the manuscript – become suspect.

Figure 15. This figure is intriguing, especially because very few people have tried incorporating iodine cycling into the modeling of methane-charged sediment sequences. This then begs the question: how exactly is iodine being incorporated? Are dissolved iodine concentrations being tracked? If not, I do not see how the 129I profile can be used. Alternatively, if dissolved iodine concentrations are being tracked over time, how

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do the profiles compare to measured iodine profiles (e.g., Egeberg and Dickens, Chem. Geol., 1999)? [As an aside, colleagues and I have tried to simultaneously model profiles of dissolved I as well as 129I. We cannot get this work either. We get very nice I profiles that make sense, but the “ages” are way too young. It remains a puzzle and why we have not published].

Figure 16. I appreciate the sensitivity analysis regarding the gas hydrate inventory, but I do not understand the units (mol/m). Are these moles of gas hydrate (~124 g/mol), methane (16 g/mol) or carbon (12 g/mol)? And what does meter signify in the denominator? Indeed, how does one get m rather than m² or m³? Lastly, as noted above, the time scale is particularly awkward for panel c. I think many in the paleoceanographic community would view this as present-day on the left rather than on the right. Change “time-varying” to “time-dependent”.

Figure 17. Which base-case does this refer to: time-dependent or time uniform? More crucially, and following from comments on the last Figure, what does the gas hydrate inventory refer to? Is it for a cross-sectional slice of the margin with some thickness parallel to the coast? Lastly, I do not understand why a world with 10°C warmer bottom water (presumably relative to present-day) would have zero gas hydrate (presumably across the entire margin to 4 km water depth). This is not entirely consistent with 1-D modeling exercises (Gu et al., Nat. Geosci. 2011), although no comparison is made.

Now a return to the text . . .

Specific Comments – Page 2922 – Line 5: It seems to me 1-2 sentences are missing here that explain the new and basic features of the model. Line 25: A paragraph is missing that explains why methane cycling along continental margins is interesting, especially in the time domain.

– Page 2923 – Line 1 (and from last page): This is neither a good nor complete description of current 1-D modeling of gas hydrate systems. Indeed, this is precisely where I think the paper misses some very important points, ones that haunt the paper

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later (see comments above concerning Figures 13 and 14). If one takes conventional views for modeling early diagenesis, certain geochemical profiles in methane-rich systems on continental slopes cannot be explained. At the most basic level, and in my opinion, this is for three reasons. The systems are open; organic carbon degrades along a geotherm in rapidly accumulating sediment (Burdige, *EPSL*, 2011; Gu et al., *Nat. Geosci.*, 2011), in striking contrast to way most geochemical studies model early diagenesis (e.g., Middleburg, *GCA*, 1989); AOM dominates net sulfate consumption in shallow sediment deposited with high organic carbon at low seafloor temperatures (e.g., Chatterjee et al., *JGR*, 2011). These notions remain heresy in many geochemical circles, although the current manuscript seems to accept them, at least partly, *carte blanche* with no discussion or reference. So, why then are the modeled DIC and $\delta^{13}\text{C}$ -DIC profiles in the manuscript obviously wrong? I will take a guess: they have not fully incorporated these ideas, which stem from 1-D modeling. However, there are other possibilities, such as inappropriate grid cell dimensions. Lines 4-7: Why does the depth of methanogenesis impact 1-D modeling? And why do “relevant chemical processes” clearly show the impact of migration in more than one dimension? I fully agree that moving into 2-dimensions is an important step, especially over the space-time scales examined in the current manuscript. But the necessity is not for the stated reasons. Line 9: Many authors have discussed this, both before and after 2004. For example, Xu and Ruppel (*JGR*) clearly highlighted this point in 1999.

Lines 9-12 (and elsewhere): This sentence is amusing. One of my primary criticisms of manuscripts by the first two authors (e.g., Dickens, *Clim. Past*, 2011) is that they have tried to drive present-day methane distributions on continental slopes with Holocene organic carbon inputs and Holocene oceanographic conditions (Archer, 2007; Archer et al., 2009), which makes little sense given variable supply of organic carbon to slopes and the cycling time in methane systems. One of my biggest frustrations over the last few years has been the blind acceptance and referencing to their works as to why there must be very little methane in marine sediment at present-day, and especially during past times of higher Earth surface temperature. I guess it's cool that the authors now

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appreciate the problem more fully. I leave it to them as to whether they want to place things in perspective, which would be useful. Lines 13-14: This needs clarification considering Figure 3. Clearly, either is not to basement (= bedrock?) or the time limits are wrong. Line 15: Probably should have references. Several papers, going back almost 10 years, have specifically emphasized that gas hydrates are but one component of a larger methane cycle along continental margins (e.g., Dickens, *EPSL*, 2003). Line 24: Remove second “ridge”. Line 27: Should have a reference. There are several good ones summarized in ODP Leg 164 papers.

– Page 2924 – Lines 1-2 (and from last page): This is an undocumented and unexplained aside. What does it actually mean? Yes, Blake Ridge has characteristics for sediment accumulation that differ from other locations. However, the pore water profiles at multiple sites on Blake Ridge have remarkably similar shapes to locations where sediment accumulation is completely different (e.g., Site 1230 on Peru margin). So what does this tell us? That the mode of sediment accumulation is largely irrelevant to understanding seafloor methane cycling on slopes? Line 4: processes. Lines 9-18: I really cannot follow this paragraph. The basic problem is that too much information has been concatenated into too few words and wrapped in jargon. Lines 19-23: This is (potentially) problematic. For many locations, a grid cell of 200 m depth seems way too large to capture methane cycling in shallow sediment. I appreciate the necessity to reduce computation time, and I am not sure how one might tackle this issue other than nested programming, which probably lies beyond the current work. However, it has to be addressed, even if to comment how it may impact results.

– Page 2926 – Lines 8-22: This approach for sedimentation across the margin is intriguing and seems okay at a basic level. However, it raises some obvious issues. Where and how does pelagic sediment falling through the water column become incorporated? Far more important in this regard, from where is organic carbon supplied? After reading the manuscript multiple times, I am still lost on this matter. With the current writing, organic carbon magically appears in a dynamic model for accumulation of

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sediment grains. Is organic carbon treated as sediment particles arriving off the shelf and subsequently settling? Alternatively, is it formed in the photic zone and rained through the water column to combine with sediment on the seafloor?

– Page 2927 – Lines 24-25. Where is the reference for sedimentation on (presumably outer) Blake Ridge? In any case, it is not correct as stated, when time is considered. The 2 cm/kyr is slightly less than that for the late Quaternary. It is much faster, exceeding 10 cm/yr, for multiple sites in the late Miocene-Pliocene (e.g., Paull et al., 1996). Here I note that these are compacted sedimentation rates; any comparisons to field data need to distinguish between original and compacted sedimentation.

– Page 2928 – Lines 2-3: I am not sure what this means. The described deposition (highest rates close to shore but in water that [is] not too shallow) seems independent of sea level change given the settling model and accommodation space. So, how do changes in sea level affect sedimentation? Lines 3-5: Is it the absolute value or the rate of change in sea level that is important? I would think that rapid, large amplitude changes during a time of relatively low sea level (i.e., the Quaternary) has a more significant impact on margin sedimentation than slow, small amplitude changes during a time of relatively high sea level (i.e., the Late Cretaceous). This is not clear. Lines 18-22: What are the two B values? I would rewrite the equation in terms of parameters, and then define these.

– Page 2929 – Lines 4-9: Although not referenced, the equation appears to be some hybrid form of the Kozeny Equation, where multiple parameters have been combined to give a value of 180. Where and how then does a stated anisotropy of 100 arrive? Is this a typo? Lines 15-23: I understand the rationale for including vertical chimneys, but this needs rethinking and rewriting considering the scale (see comment on Figure 6). Clearly, they are not representing faults. I also appreciate that such modeling is “crude” with an “intent to gauge in a qualitative way...”. However, without discussion as to whether the horizontal scale of such conduits is important, this modeling gives limited insight.

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– Page 2931 – Lines 12-14: I am uncertain how to read the clause “for the formation of methane hydrate discussion to come”. Does this mean the downward flow (somewhere) must occur to form gas hydrate in locations with permeable pathways and upward flow? It is an interesting idea, but needs better explanation. Lines 20-21: See above. It is not clear how the supply of POC occurs in the model. Lines 21-23: The coupling of POC accumulation and sea level is confusing, indeed misleading, without elaboration. Presumably, the implied link is that times of high sea level are times of greater Earth surface temperature, which affects the solubility of oxygen and leads to enhanced organic carbon burial on the slope. I appreciate the concept, but it is, by no means, widely accepted . . . nor should it be given the time-scales examined. During the Quaternary, POC supply to the slope is much greater during low stands (and cold “glacial” conditions), because the shelf is subaerially exposed. Indeed, as noted above, this is a major criticism of past efforts that have tried to model present-day gas hydrate distribution using the Holocene POC supply to the slope (Dickens, *Clim. Past*, 2011). During the Cretaceous and early Paleogene, the link is not obvious because the mechanism (changes in ice volume) appears to have been absent. In the big scheme of things, a primary point of this and other manuscripts (Buffett and Archer, 2004; Gu et al., 2011) remains: changes in POC accumulation to the slope over time significantly affects the amount and distribution of methane in sediment. However, the rationale for such change needs rethinking in the current manuscript. One means: divorce temperature and sea level; they are good friends but not married. Line 24: Why do the H/C and O/C ratios depend on depth and oxidation state? This needs additional information and referencing.

– Page 2932 – Lines 14-15: As mentioned above, the relationship between POC accumulation and sea level needs clarification. For the slope, records on multiple margins are opposite to that suggested for the Quaternary (see for example references in Dickens, *Clim. Past*, 2011). Lines 18- 22: This section needs rewriting given current controversy on these topics. First, what is meant by biological degradation? Is this the classical “Berne” sequence of microbially-mediated reactions in shallow sediment

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that omits AOM, or a modified sequence with upward transport of methane through open systems with AOM? Second, how does one suggest that organic carbon reactivity increases down the geotherm when this goes against convention (e.g., Middelberg, GCA, 1989)? These are important points because they change the perspective of early diagenesis of POC on slopes significantly. The recent paper by Burdige (EPSL, 2011), which should be referenced, articulates both issues. Basically, I think I agree on the model framework for diagenesis (although see below): large fractions of POC bypass conventional sulfate reduction in rapidly accumulating sediment on cold seafloors of continental slopes to produce methane in the upper few hundreds of meters along the geotherm; the sulfate profiles in shallow sediment are driven largely by the return flux of methane and AOM. I state this because it explains key field data that are difficult to reconcile otherwise (e.g., Chatterjee et al., JGR, 2011). However, this is a minority view circa 2012, and should not enter the modeling blithely.

– Page 2933 – Lines 1-11. This needs rewriting. What is trying to be explained? An additional figure may help; changes to the text and other references would definitely help. Part of the problem is that, following Boudreau and Ruddick (AJS, 1991), I think the age concept for organic carbon degradation is conceptually flawed. It seems to me a case of different organic compounds with different reaction rates, which also explains why total POC reactivity can increase down the geotherm in rapidly accumulating sediment. See also Burdige (EPSL, 2011). Line 12: A comparison to field data would be helpful here, as might another sensitivity analysis. I think it will be realized that if geothermal gradients are increased, the microbial respiration zone will migrate upward, and both borehole temperatures and pore water profiles pertaining to respiration (probably better expressed as fermentation) will be more compatible with observations. Lines 15-17: Seemingly, this is a really important concept, missing in almost all literature (notably including past work by the authors, as mentioned above). However, it is not exactly clear how sea level variations were conducted in the time domain. From the assumed curves (see comments on Figure 4 above) and text (comments above), there seems to be little consideration of collective magnitude and rate (i.e., fast large amplitude

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changes during relatively low sea level versus slow small amplitude changes during relatively high sea level). Also, what does “burns out” mean? Is POC supply shifted to the slope or is it simply cut-off from the shelf? Lines 20-12 (next page): It needs to be emphasized that this is a sum approximation of methanogenesis (e.g., Conrad, Org. Geochem., 2005). The reaction proceeds through a series of intermediate steps beginning with hydrolysis. Furthermore, I am not convinced that the dominant pathway is through CO₂ and H₂ (which is assumed but without reference).

– Page 2936 – Lines 10-11: Why does gas need to escape the sediment column rather than forming gas hydrate? Lines 11-14: Reference? Line 19: Reference? Line 24: Tall bubble column?

– Page 2937 – Lines 6-12: Rewrite. This is not clear.

– Page 2938 – Line 2: Ten times DIC values in sediment pore water compared to the ocean? Maybe I am getting old and I cannot see and perform simple math, or maybe the results have been plotted incorrectly. But when I look at Figure 7, after I zoom in on the ridiculously small font and infer units from the caption, I see 100 moles/m³ at the seafloor and a maximum of 270 moles/m³ at ~700 mbsf. I assume this is for pore space, in which case the units really should be in mM (with the implied conversion to kg of water). The Sivan et al. DIC curve appears correct in these units, assuming that it is that from Site 1081 (not discussed). So . . . it seems to me that the modeled DIC is more like 50-350 times the DIC in ocean water (more below). Line 5: I do not follow the reference to Sivan (et al.) here. How does this pertain to the modeling? Lines 10-25 (next page): I am surprised by this discussion of DIC and d¹³C-DIC given the authorship. First, the units need to be checked (above). Second, the modeled profiles should be compared to appropriate sites (e.g., Blake Ridge). I think it will be seen that the profiles are not so problematic, especially if the geotherm and microbial rates are amended. Once this is done, it needs to make sense (see comments on Figures 13, 14). I hate to say it, but right now it reads as if they are dreaming up ideas and explanations because they have not followed and read pertinent literature thoroughly.

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To jump-start things, I suggest the authors read Chatterjee et al. (JGR, 2011). In any case, the considerable differences between measured and modeled DIC and $\delta^{13}\text{C}$ -DIC present a major scientific hurdle in the manuscript, one that has to be crossed. Otherwise, the seafloor methane/carbon cycle so crucial to the model falls apart.

– Page 2940 – Line 4: Reference? Line 6: Why give an e-folding time rather than half-life? And reference? I would remove the iodine cycle from manuscript because it neither enhances nor detracts from current discussion. It remains a puzzle, and not one solved in the present manuscript.

– Page 2941 – Lines 17-18: The temperature change is relative but also should be presented in absolute terms, because this is important. Lines 20-22: I do not fully follow these sentences as written. What does “these” refer to? And which study compares to the Buffett and Archer (1994) study? Line 26: What does “that model” refer to?

– Page 2942 – Lines 1-3: This is not correct as written. Unless I am mistaking things, a problem with the Buffett and Archer (2004) model was that microbial degradation down the geotherm was not considered. I also suspect that AOM was not properly modeled. However, both concepts are really hard to evaluate from past works and documentation (and hence, in part, why a very long review comes this time in the hope the authors clarify things this time around). Lines 3-7: What does this mean? Why is the mechanism different? Section 3.2 also lacks context with the current Introduction and ensuing text. Basically, why would the manuscript present and discuss changes in gas hydrate across a margin depending on ocean temperature? Of course, there is a very good reason: a series of papers (none referenced) have discussed the issue and its complexities. Line 15: The referencing to concepts presented by Gu et al. (2011) are sort of correct. Changes in the gas hydrate inventory with variations in temperature and POC accumulation are complex. At the most basic level, and assuming that our modeling is generally correct, it strongly depends on water depth and the magnitude of temperature. It should be noted, though, that these inferences are for long-term variations in ocean temperature, not short-term variations, which may have

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a completely different response, because of possible conversion of gas hydrate to free gas. Lines 16-24: This paragraph needs rewriting, once the diagenetic modeling is explained and cemented. At the most basic level, a series of organic compounds land on the seafloor; some fraction of this passes through the SMT. Of this, some portion is available for methanogenesis.

– Page 2943 – Lines 6-16: I agree and this is a really important concept.

It's 8:38 PM and I would rather spend time with my family than further waste a day pointing out issues on a manuscript where the authors are smart and clever and know better than to submit cool stuff but as a messy pile. How about they take all the above, fix things, and extend such a treatment to the last parts? I'll make comments on a revised version (ideally much improved) if asked. Seems like a fair deal?

***Other random comments, ones that would take the manuscript to another level: - The modeling, if correct, will necessarily impact carbon isotope records of the ocean. They have not discussed, but obviously important considering the time domain. - The modeling implies a seafloor methane residence time. They have not discussed, although obviously interesting. - Although largely outside the domain of the current manuscript, the parameterization and discussion of methane bubble transport may lie at the heart of future work. Actually, it may lie embedded in the current work (see random comments at the end). Seemingly, the only means to get massive quantities of methane out of slope sediment fast (i.e., on the <100 kyr scale) is through conversion of gas hydrate to free gas, passage of a critical threshold on free gas accumulation, and venting of free gas to the water column (Dickens, EPSL, 2003). So, if they are truly modeling things correctly, they presumably have times of greatly accelerated methane input to the ocean.

Interactive comment on Biogeosciences Discuss., 9, 2921, 2012.

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