

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: 21 April 2012

Dear David and Bruce,

Thank you for fixing some of the obvious problems with the modeling, and making the movies available for viewing. They are really interesting to watch, largely because they really make one think about the dynamic aspects of methane-charged systems from the big picture.

As for the movies, two immediate things would be very helpful: (1) Place units on the axes and scale bars for all the movies (right now, I'm guessing until I look at the text figures); (2) Show the BGHZ in all figures (I assume the black curve that shows in some movies).

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The actual manuscript figures are much better than before, although there are still some problems. For example, the age/labeling seems backwards in Figure 9; the depth axis in Figure 14d appears incorrect (I think it's supposed to be 0-500m and Sivan et al., 2007); I still do not follow the units mol hydrate/m in Figure 16. Maybe I am being stupid, but the denominator makes no sense to me; it's certainly something that I have no clue how to measure or to compare to other work.

In my opinion, at least one major problem raised in my previous review remains. This concerns the carbon mass balance.

Everywhere DIC is less than 30 mM, and it is almost universally highest in the uppermost grid cells. This is inconsistent with numerous drill sites, in fact in all methane charged sediment sections on the slopes of passive or active margins where advection is fairly low. Instead, DIC reaches a maxima well below the seafloor, often >100 mbsf. As but one example, consider Blake Ridge, where DIC is ~120mM at 300 mbsf. If you think Blake Ridge anomalous, look at Sites 902-906, which lie on the nominal transect. These sites do not have gas hydrate, but they do have modest amounts of methane, and the alkalinity generally reaches a peak (35 to 50 mM) below 100 mbsf.

In fact, I challenge you to find any site on the slope of any margin with high amounts of methane and low fluid advection where DIC does not exceed 30 mM at 100 m below the seafloor. Even the profile for Site 1081 (Sivan et al., 2007), which comes from sort of a random site as noted earlier, because it is not particularly rich in methane, highlights the problem (Figure 13).

Now consider the basic reaction of $2\text{CH}_2\text{O}$ going to CH_4 and DIC. Obviously, this is not the correct reaction, as methanogenesis involves intermediaries, but it should serve as a first order guideline. Indeed, this can be suggested through ammonium profiles, which generally reach subsurface peaks in concert with those of alkalinity, and often >100 mbsf.

The DIC profiles suggest to me that far too little particulate organic carbon is passing

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to methanogenesis in deeper sediment. That is, too much POC is being consumed by sulfate reduction, that too little methane is generated deeper in the sediment column, or some combination of both.

I then look at the $\delta^{13}\text{C}$ profile (Figure 14) and, assuming the depth axis is wrong, the low in $\delta^{13}\text{C}$ coincides with the high in alkalinity. This is absolutely incorrect at most locations; it only happens when there is major advection of methane from depth, but then the $\delta^{13}\text{C}$ should be about -40 per mil or less. Instead, as sort of shown by the Sivan profiles (which frankly are a bit unusual), most passive margin locations with significant methane have a $\delta^{13}\text{C}$ low that precisely marks the SMT, because this is where methane is being consumed, and an alkalinity high and $\delta^{13}\text{C}$ high at significantly deeper depth, because this is where methane is being generated. In fact, I think it is impossible to have a coinciding alkalinity peak with a $\delta^{13}\text{C}$ low of -10 per mil on the continental slope. So, I'll revise my question from before: how is this possible in the modeling? Specifically, if the alkalinity over the first grid cell is 25 mM, how can the $\delta^{13}\text{C}$ of DIC be -10 per mil and the CH_4 -50 per mil if the POC is coming in at -22 per mil? I believe the answer will raise some basic issues concerning the consumption of POC, the production of methane, and the depths of these reactions (i.e., the inference in the previous paragraph). This will of course, directly lead to questions regarding the relevant carbon fluxes (both inputs and outputs) which underpin the modeling.

I realize that some of this may be a grid cell issue near the seafloor. That is, it is hard to incorporate the SMT and the interval of methanogenesis given the vertical dimensions of grid cells. Nonetheless, there still appears to be a major carbon mass balance problem. Until this is solved satisfactorily, some key points to the manuscript fall apart.

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