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Biogeosciences Discussions

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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Hej David and Bruce,

I'm not trying to be a thorn in your side . . . but to get you two to think about things differently and consistent with data.

The big picture problem: a series of papers (yours included) have modeled the distribution of gas hydrate and free gas at present day over the last few years. Almost invariably, these exercises come with very limited comparison to actual data, but when one examines things, methane concentrations seem too low for known grid cells. These unconstrained model results are then taken to make commentary, including by people who either have not looked at the data, or who do not understand methane cycling in

marine sediment. I think enough is enough, and the models and commentary should be consistent with data.

The same is true in the current effort. Here it manifests in the profiles of DIC and the d13C of DIC: the model simulations do not conform to abundant data. I suspect this is well beyond a grid cell issue. Without changing the grid cell dimensions, the appropriate test would lie in taking a few 250 m intervals (the apparent grid cell dimension) at multiple drill sites on continental slopes and integrating the DIC concentrations and the d13C of DIC over depth. I strongly suspect you will not get the values you have modeled . . . and the reason is because too little methane is being generated deeper in the sediment column. Basically, I do not think you will find a summed average of <30 mmol DIC with a d13C of -10 per mil over 250 m at almost any site on a continental slope with significant methane (with the caveat of sites with high fluid advection, which are not being emphasized).

The issue is not addressed by changing fluxes of chemical weathering. Frankly, I do not understand this response (although, as an aside, if one sets up a truly dynamic carbon cycle model with seafloor methane, weathering becomes less important, because fluxes into and out of the seafloor become very important, especially for carbon isotopes).

Instead, I think the issue lies at the heart of carbon cycling in methane systems on continental slopes.

I honestly do not care whether seafloor methane is important or not as to how the world works. Things are the way they are (and were). However, I do not like models that suggest one way without considering obvious issues and problems with actual data. Serious, as I read things right now (albeit without seeing the latest text), it's like: simulations are conducted for a passive margin over time, little methane is generated, this has implications, never mind that the end model results do not conform to present-day observations.

First aside: in what may have seemed a silly comment regarding moles in my first comment, please be careful with conversions between mass and moles. There are several papers that use total organic carbon (TOC) entering methane systems with a conversion of 30g/mol when it should be 12 g/mol, and conversion of carbon mass in hydrates to moles of hydrates without considering the water. (And in any case, fix the units on hydrate amounts in the end figures – they make no sense!).

Second aside: at the recent Gordon Conference, a few people commented to me "what's new about this? The petroleum industry has been doing basin modeling for a long time so why re-invent the wheel?" I sort of see the point on one hand, but think this very different. In any case, you might want to add this perspective to your broad audience.

In summary, I very much like the avenue that is being followed here. It is very cool and on the frontier and I would like to see it published. Just make sure it is reasonable, or at least the caveats are emphasized, so people don't spend time later explaining why it's wrong when (in my opinion) major flags were given to you here and in previous comments by me.

Assuming you fix the text so things are readable and referenced correctly, and you mention the problems in modeling and their implications, I'll be mostly okay with this manuscript.

Enough commentary from me: if you don't like my comments or you don't want to seriously address them, at least I tried.

Jerry

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