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Discussions

Interactive comment on "Estimation of the global inventory of methane hydrates in marine sediments using transfer functions" *by* E. Piñero et al.

B. A. Buffett (Referee)

bbuffett@berkeley.edu

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The authors develop a parameterization to relate the local inventory of gas hydrate (in g/cmˆ2) to relevant input parameters (like water depth). These parameterizations are tuned to one-dimensional numerical models, which circumvents to the need to run numerical models with gridded data. Another approach is to use the numerical models to construct "look-up" tables for use with gridded data. The second approach eliminates the "parameterization" error that inevitably arises when the results of numerical solutions are fit to a specific parametric function. On the other hand, a transfer function (e.g. a parameterization) is easy to implement once a suitable parametric function is

found.

The main contribution of the present work is to find such a parametric function when the influence of fluid flow is taken into account in the numerical models. The authors obtain a good correlation between the transfer function and numerical solution, which is clearly demonstrated in one of the figures. Thus interested readers should be able to make quantitative estimates of the hydrate inventory. However, the estimates are only as good as the numerical results that guide the parameterization.

The estimates of hydrate inventory are not so different than some recent results, although the authors suggest that the results obtained in this study stand on a firmer foundation (see pg 601). This may be true, but the arguments used to support this claim are not overwhelming. For example, it is suggested that the parameterization reveals the important control parameters. Surely it is the other way around - the numerical results show which control parameters are needed in the parameterization. It would be surprising if the initial choice of control parameters was not motivated by the authors' numerical solutions. The authors also note that they use a more sophisticated kinetic model for methane generation. I think that this is true, although the importance of this added sophistication is not evident in the results. Do the details actually matter in this particular application? Finally, the authors point out that they have a "valid" mass balance approach for calculating the fluid flow. This is a nice idea, but I am not sure what "valid" means in this context. You would not expect the upward flux to exceed the downward burial of fluid in the pore space of compacted sediments. This provides an upper bound on FF, but there is no guarantee that all of the buried fluid returns to the surface through marine sediments. A lower bound would involve no upward flow, although various geological observations suggest otherwise. Previous studies have attempted to estimate the upward flow using various geochemical tracers at specific site where this information is available. Of course, it is difficult to know how these results should be extrapolated to other region.

The authors face a similar challenge. How should the total volume of fluid be distributed across the seafloor? I wonder if the authors could address this source of uncertainty in the hydrate inventory by considering different distributions of flow. For example, suppose that the upward flow was focused through 10% of the seafloor instead of 100% (as currently assumed). How would the inventory change? I think that the authors are in a position to quantify this uncertain and contribute something useful to our current understanding.

Specific Comments ——————

i) There were a few places where the text is unclear (to me) and some elaboration might be helpful. For example, I found the description of "Approach 2" on page 587 confusing. Equation (4) and (5) relates the carbon rain to POCar. However, equation (6) defines another relationship that accounts for remineralization. I understand why remineralization is important (otherwise there would be no dependence on benthic oxygen), but it is not clear from the text why you need equation (6) if equations (4) and (5) already give you POCar. It would also be helpful to be very explicit about which fields are evaluated from global data sets. Do not rely on the readers knowing what is contained in the cited references.

ii) the discussion of the pore water mass balance needs to have the unit corrected. This quantity should have units of volume per unit time. Immediately after equation (12), and subsequently, the units of PWf are gvien as kmˆ-3 /yrˆ-1. Presumably, the authors mean kmˆ3/yrˆ-1.

Interactive comment on Biogeosciences Discuss., 9, 581, 2012.