

Interactive comment on “A transfer function for the prediction of gas hydrate inventories in marine sediments” by M. Marquardt et al.

Anonymous Referee #1

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The authors apply a 1D numerical model for the formation of gas hydrate in marine sediments to establish a correlation between the hydrate inventory and key input parameters. The 1D numerical model is based on previous work of the authors, so the main focus here is to use the model to fit a suite of observations from a number of study sites. A good fit to observations from the various study sites can be obtained without too much fine tuning for site-specific details (like time-dependent changes in the carbon input and fluid flow). One of the nice results of this work is the sensitivity to the input parameters. The authors make the case that POC input rate and the vertical extent of the hydrate stability zone are the key parameters. Using these results they construct a function that can reproduce the estimates from the numerical model. The effort is worthwhile because numerical calculations are cumbersome to apply in global studies; the computational requirements are too high. In addition, the correlation devel-

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oped here gives interested researcher the ability to make estimates of a local inventory without resorting to numerical calculations.

One of the shortcomings of the correlation is the neglect of any influences due to fluid flow. This shortcoming is acknowledged in the Conclusions and reflects an incomplete understanding of the processes that control fluid flow and other mechanisms of gas transport (e.g. transport of gas bubbles into the stability zone). These are areas where future progress will be required in order to improve our estimates of hydrate inventory. As a result, the authors caution that the correlation yields a lower bound on the hydrate inventory.

I have only minor suggestions for the authors to consider.

1. The recent paper of Archer et al. (2008) includes an estimate for the global inventory. This estimate doesn't fit the general trend toward high values in recent times, but it should be noted. An optimist might claim that the oscillating estimates are converging toward the truth.
2. Table 1 lists parameters for the time-varying carbon input, but it is not clear how these results are used to construct the function $f(t)$ in equation (2). A little more detail in the discussion of the specific sites should fix this problem.
3. The assumption that the POC concentration is only a function of sediment rate is surely a simplification. Given that the POC input rate (POC concentration \times sedimentation rate) is a key parameter in the correlation, it might be useful to comment on how the POC rate could be estimated in areas where direct observations in the top few cm are not available.

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