

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

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Response to the reviews of

“A transfer function for the prediction of gas hydrate inventories in marine sediments“

Please find below the response to the reviews of our manuscript “A transfer function for the prediction of gas hydrate inventories in marine sediments“. Overall, we appreciate the critical and thoughtful comments by Gerald Dickens and an anonymous reviewer, which were very helpful for the preparation of an improved version of the manuscript. Below we addressed each topic raised by the respective reviewer (respective passages in the new manuscript attached as a PDF-file are highlighted accordingly).

Sincerely (in behalf of all co-authors), Mathias Marquardt

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Specific comments of the reviewers

Anonymous reviewer

1. The estimate of Archer has been incorporated. Following the suggestions of reviewer 2, however, Figure 1 was removed and the overview about global estimates was shortened. See details below. 2. All functions for variable POC input are included in Table 1 (page 28). 3. Although it is possible that POC information in surface sediments might be unavailable at a specific site, this is generally a ubiquitously available parameter. There are a number of global compilations published, which enable a reasonable estimate at almost any location of the seafloor (e.g. Premuzic et al., 1982; Seiter et al., 2004; Romankevich et al., 2009). This explanation has been added on page 11, line 26.

Gerald Dickens

1. Introduction: We like the suggestions of the reviewer with respect to a restructuring of the introduction. We included all aspects listed from 1a-1d and completely rewrote and re-focussed this section, which is now shortened by about 50

2. Equation 1: RPOC was determined after Wallmann et al. (2006) who tested this model on an extensive data set from the Sea of Okhotsk and ODP site 997 (Blake Ridge). The idea was to test, if this model approach and parameterization (within certain limits) can be generalized by applying it to other geological/geochemical settings. This is clearly outlined in the text and our overall conclusion is that it can be used for systematic runs. For clarification some minor changes were added to the description on page 4, line 12. The C(CH₄) (as well as the C(DIC)) value is calculated by the model; no fixed concentrations were used (see text, page 4, line 6). Values for

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ageinit are difficult to constrain and obviously quite variable (Table 1, and Wallmann et al., 2006). In order to address the effect of ageinit on GH formation we performed a sensitivity analysis (page 9, lines 8-19 and Figure 8). For all subsequent runs an average value of 43,700 yr was used in order to minimize potential errors related to uncertainties of this parameter.

3. Steady state: We agree with reviewer's statement that the pore water profiles may not represent the long-term conditions of the system, however, the models are generally run into steady-state, because – in our opinion - there is no useful alternative to this approach: There is no information provided in the manuscript on the time that is necessary to reach steady-state conditions, because it highly depends on starting and boundary conditions. Specifically, the starting conditions of pore water concentrations and the development of pore water concentrations over time at the lower boundary are typically not known. Even for the best-studied sites this parameterization is difficult to constrain (if possible at all), and hence such an approach (although it may be desirable) is by all means not feasible, at least if one considers that the study focuses on a useful generalization of the processes. A short explanation is added in page 11, line 29. On the other hand, the fact that it is possible to create time independent ($dC/dt = 0$) scenarios that have been parameterized according to published data and produce good fits to measured data suggests that the steady-state approach is useful. Moreover, at three sites (Figs. 2, 4, and 6) non-steady state scenarios are considered in order to produce better fits to measured POC profiles (increasing and decreasing POC contents). However, the results show that the resulting effect on predicted GH abundances is quite low. In addition, the idea of “dying” system at Blake Ridge may be appropriate, however, the trend of decreasing POC concentrations at the Peru site is not that clear and the major reason for high GH abundances at this location are the overall higher POC levels compared to the Blake Ridge site.

4. Model data comparison: We assume that Gerald Dickens refers here to the results of the numerical model presented in Figures 2-7: It is true that independent methods

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indicate 4 vol.

Gerald Dickens again raises the problem of the steady-state approach here, which addressed above (3.). In addition, he criticizes that POC_{car} is derived from a general relationship between POC concentration and sedimentation rate (Fig. 10) although there may be significant deviations from this trend. We principally agree with this objection, because Figure 10 indicates a general trend as well as a considerable scatter of data. Nevertheless, numerous studies (e.g. Henrichs, 1992; Colman and Holland, 2000; Canfield, 1994) have clearly demonstrated that a general relationship between these two parameters exists. Hence, in order to provide a useful and generally applicable approach it was necessary to perform a justified simplification of the system. Overall, it is requested that these issues are discussed in more detail. We addressed various aspects on page 11, line 26 (derivation of the transfer function), page 8, line 6 (Blake Ridge), page 15, line 8 (Test and application), and page 15, line 29 (Conclusions).

5. Depth of SMT and gas hydrate abundance: The presentation of results by Bhatnagar et al. (2008) and Dickens and Snyder (2009) with respect to the link between SMT-depth and gas hydrate abundance were not correct in the original manuscript. We exchanged the respective paragraph according to the reviewer's comments (see page 10, line 10 and 14).

Editorial issues:

- page 2, line 18 : Nankai -> Nankai Trough

- page 2, line 20 : New Reference for Thermogenic enriched CH₄: Sassen et al., 2001, Gulf of Mexico

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- page 1060: (original Manuscript) text is deleted completely
- page 4, line 18 : e.g. -> , for example
- page 4, line 21 : including: GH-containing sedimentary strata with varying thickness of 200 to 800 m
- page 6, line 3: The result is a better fit of POC and PON data than in the a-model, which, however, only affects estimated GH concentrations by about 20
- page 6, line 7: since -> because
- page 6, line 12 : 5000 -> 5100
- page 10, line 14: Schneider -> Snyder
- page 10, line 28: e.g. -> , for example
- page 12, line 26: e.g. -> , for example

References not mentioned in the original manuscript text:

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- Seiter, K., Hensen, C., Zabel, M.: Benthic carbon mineralization on a global scale, *Global Biogeochem Cy*, GB1010, doi:10.1029/2004GB002225, 1 - 26, 2005.

Additional changes

- The values of the sensitivity analyses of the standardised model in the text (page 9, line 23 and 25) and in Table 2 have been corrected in order to be consistent with the used ranges (Figure 9).
- For site 1041 an erroneous porosity value was used for calculation of the GHI. This value has been corrected in Tables 3 and 4 and in the text on page 15, line 1.
- We also added an explanation how Equation 6 was used to obtain the values listed

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in Table 3 and 4 (caption in Tab 3 and 4, pages 30 and 31) and in the text (page 14, line 11).

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/7/C831/2010/bgd-7-C831-2010-supplement.pdf>

Interactive comment on Biogeosciences Discuss., 7, 1057, 2010.