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Interactive comment on “Modelling the impact of Siboglinids on the biogeochemistry of the Captain Arutyunov mud volcano (Gulf of Cadiz)” *by* K. Soetaert et al.

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Rebuttal of the reviews on the MS Modelling the impact of Siboglinids on the biogeochemistry of the Captain Arutyunov mud volcano (Gulf of Cadiz).

We thank the two reviewers for their helpful comments. In our detailed response we repeat the concerns of the reviewers and then provide our rebuttal. We refer to page + line numbers in the original manuscript for any changes that we have made.

Reviewer 1 - minor comments ** P6685 L2: I suggest specifying in which sediments significant amounts of methane are produced, as this is not a general feature of aquatic sediments.

Reply: on page 1 the sentence “Sediments produce a significant amount ...” was changed to : “Sediments overlying gas hydrate deposits or experiencing high organic matter deposition produce a significant amount...”

** P6685 L22ff: I have the impression that the possibility of anaerobic sulfide oxidation with nitrate is played down a bit – maybe because the corresponding microbial pathways were not included in the model presented here. Beggiatoa (mentioned in L24) and other large sulfur bacteria are well known to use intracellularly stored nitrate for sulfide oxidation (e.g. Schulz & Jørgensen 2001).

Reply: We did not add too much information about the use of nitrate as electron acceptor as this was not the focus of the MS. However, in the updated version, we have added somewhat more information.

We added the sentence: “Some bacteria, such as Beggiatoa can use use intracellularly stored nitrate for sulfide oxidation, either by dissimilatory nitrate reduction to ammonium (DNRA) \cite{Jorgensen and Nelson 2004} or by chemolithotrophic oxidation of reduced sulfur compounds using nitrate as electron acceptor and concommitant release of N2 or NO2 (Cardoso et al., 2006).”

** I also don’t agree with the statement that free-living bacteria are generally unable to bridge the gap between the concentration fronts of oxygen/nitrate and sulfide. Thioploca, Beggiatoa and others show intense and large-scale vertical migration activity (e.g. Huettel et al. 1996, Kamp et al. 2006)

reply: It is true that certain bacteria are able to bridge the gap between concentration fronts of oxygen/nitrate and sulfide. This is particularly true for the Thioploca rather than for Beggiatoa, and is mostly found in environments characterized with sharp gradients of oxygen and sulfide close to the sediment surface. During investigations at the Captain Arutyunov mud volcano a comprehensive sea floor imaging study was conducted but extended microbial mats were not observed at the sediment surface. There was one small patch (Sommer et al. 2009, Fig. 1,4). Hence, for the sulfide turnover of the

Captain Arutyunov MV microbial mats are not important. We moderated our statement though, by now writing: “Where sharp gradients of oxygen and sulfide are separated but close to the sediment surface, certain bacteria such as Thioploca can still bridge the gap by intense and large-scale vertical migration activity. However, when sulfide and oxygen are physically separated over larger distances, it will be more difficult for free-living bacteria to bridge the oxic-anoxic interface. . .”

** P6686 L20ff: I suggest including a little more detail on the biology of Siboglinum. Is it, for instance, known whether the individuals indeed vary in body length or even have the means to “adjust” their body length to the biogeochemical needs in their sediment environment? If you do not give the details here, it will later sound like a modeler’s trick to vary the length of the worms (i.e. “playing around with the length of the worms, until the model fits”).

Reply: We did not want to imply that single specimens of Siboglinids vary in body length during their adult stage. But - of course - they do during their development. There are certainly differences in body length between the different species present at the Captain Arutynov mud volcano. A length of 10 cm was mentioned in Sommer et al paper; the model used 11 cm, which gave a slightly better fit.

** P6687 L9: I suggest using the term “bio-ventilation” again. Done P6689 L4: I guess again due to “bio-ventilation”. Done

** P6691 L17: The Results and Discussion section starts with the weaknesses of the model in a very explicit way. I appreciate that the authors discuss the limitations of their model, but suggest doing this at a later stage of the Discussion. This section is almost a bit discouraging and will prevent some people from reading the rest of the manuscript.

Reply: This is quite standard in modeling; however, in order not to discourage people, this section has been slightly rewritten. For instance, we removed the sentence One weakness of the modelling presented here is our oversimplification of the worm physiology. We now start by dealing with how the model represented the physiology. We also removed the sentence: “There are also several pitfalls in using biogeochemical profile data for flux estimation, not in the least related to the overinterpretation of the data. Because of that, the model results we present should be considered with caution. Notwithstanding these uncertainties, we do feel that the model and tentative budgets provide some interesting results.”

** P6692 L6: One may want to combine this section with the last paragraph of the Introduction. Done P6692 L21: I find “where sulfate consumes methane” a bit too sloppy. Rephrased as: where the AOM process takes place. P6698 L13: “for worms of 11 cm in length” Done P6700 L10ff: The model might also be applied to benthic macrofauna species that stimulate the efflux of N2O or CH4 from aquatic sediments AND emit N2O or CH4 due to bacterial activities directly associated with their body (Stief et al. 2009, Figueiredo- Barros et al. 2009). Thanks for the suggestion - added

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Reviewer 2.

** First, it is unclear to me how the sulfide oxidation rate of the worms is calculated. The rate kinetics of sulfide oxidation are quite different for chemical oxidation, microbial oxidation, and siboglinid-facilitated oxidation by the intact symbiosis. These should probably be separated in the model, or at least analyzed and discussed (see additional specific comments below). Furthermore, the sulfide oxidation rate of the worms is presented as a model result rather than an input. It is unclear to me where this is coming from.

Reply: In table 2 it is shown that sulfide oxidation is modeled as kHSox * [O2] *[HS], i.e. it is a function of the ambient oxygen and sulfide concentration, using a bimolecular rate coefficient (kHSox). This rate coefficient is representative for a rate occurring in an organisms body, and we used the same bimolecular rate also for the surrounding sediments, for simplicity. This assumes that in the sediments, sulfide oxidation would

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occur in the body of bacteria. We could add different rate coefficients for all the processes enumerated here, but their values are generally not known. The effect of having a lower sulfide oxidation rate in the sediment is that the region over which the process occurs becomes larger; it does not alter the main conclusions.

** It would also be very informative if some sort of range was put on these values. What is the error associated with this determination? A sensitivity analysis might be helpful here. Reply: It is difficult to put a range on this value; it just has to be large enough, such that the worm can harvest enough energy from the oxidation. However, that we are not very far off is clear from the organism's mass-specific oxidation rates that result from the model, and which fall within the ranges from literature (p 6695, L1-5). When reducing the rate parameter kHSox with a factor 10, the oxidation rate in the worm falls with a factor 2, and the worms contribution to total HS oxidation is reduced with a factor 2.

** It is my opinion that the discussion could use a broader context. I felt that it focused too much on the Captain Arutyunov mud volcano habitat. The authors set up the paper by discussion anaerobic oxidation of methane quite a bit, but I found it largely lacking in the discussion. Are there broader implications for the findings of the paper on the biogeochemistry of mud volcanoes in general? All cold seeps? The global carbon budget? The addition of a paragraph discussing some of these possibilities at the end of the paper might increase its relevance to the broad reader base of Biogeosciences.

Reply: We added in the conclusions: "Our findings are relevant for all seep habitats which comprise similar tube worms occurring at high densities. Siboglinids, as studied here, are prominent members of many cold seep ecosystems such as the Skagerrak (e.g., Dando et al. 1994) or the Hakon Mosby mud volcano (e.g. de Beer et al. 2006). We have shown that these organisms act as "geo-engineers" by depressing the sulfate-methane transition zone. This may potentially allow other eukaryotic heterotrophic infauna that are less tolerant to high porewater sulfide levels to colonize these sediments. Therefore, the Siboglinids may represent a key species that affects successional patterns at cold seep communities."

** P6686 L2: Should be "symbiotic bacteria" Done

** P6686 L9-11: Sulfide oxidation provides a significantly higher amount of energy per molecule than methane oxidation. I believe that many (most?) of the cold seeps of the world rely more on sulfide as an overall energy source than methane oxidation.

We rephrased "In these systems the oxidation of methane provides a source of carbon and energy, with another energy source provided by the high sulfide concentrations that are used by sulfide-oxidising bacteria \citep{Levin2005}".

** P6689 L11-12: The permeability of the worm's body wall and tube is very unlikely to be the same as the surrounding sediments. The estimates for Lamellibrachia found in the Julien et al 1999 JEB paper might be helpful. Again, a sensitivity analysis will tell you if this is important.

Reply: The model is relatively unsensitive to the permeability of the integument. Assuming this is only 1% of the sediment diffusion coefficient, then the mean concentration in the upper 10cm is still about 97% of the concentration at the nominal run. For D = Ds/1000, this is still 70%.

** P6690 L10: Why do you assume that OSR only occurs below the depth of the worms? It won't matter since this is an insignificant term in your model, but this should be justified somehow here. Reply In more complex diagenetic modeling, we would describe also nitrate, and iron and manganese oxides, and let the OSR start when these substances are exhausted. We added: "As we do not explicitly model nitrate, manganese- or iron oxides, which would be consumed before sulfate, we assume that the OSR occurs in the zone below the worm."

** P6693 L13: Complete consumption of sulfate by methane has been noted in many other habitats. Perhaps adding a ref or two here would be good. Reply:We already refer to Dale (2010) on the same page.

** P6694 L8-9: Was release of sulfate by the frenulates modeled explicitly Reply: No, it is a consequence of the model, mainly due to the permeability of the wall.

** P6694 L27: How is the 71% figure calculated? Please explain.

Reply: The model has grid cells within the worm body and outside. It is a matter of integrating the sulfide oxidation rates within the worm body and dividing this over the integrated rate in the worm+sediment.

** P6695 L16: A revision of the permeability coefficient might improve this. Reply: It has almost no effect (see above).

** P6700 L17-18: I am not convinced that high sulfide levels would kill the worms. The binding capacity of the hemoglobins are likely to be quite high. As long as they do not run out of oxygen, they should survive. Is there any empirical evidence for their absence in high sulfide environments? I can't think of any. . .

Reply: This may be true, but if the concentrations are sufficiently high already for short worms, why would they invest energy in becoming larger?. That sentence was removed; also deleted from the conclusions the sentence : "Worms that extend too deep in the sediment probably experience too high sulfide concentrations for them to survive. "

Interactive comment on Biogeosciences Discuss., 9, 6683, 2012.