

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.

Anonymous Referee #2

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Soetaert et al, Impact of Siboglinids on mud volcano biogeochemistry.

This is a very interesting study of the influence of frenulate siboglinids on the biogeochemistry of an active mud volcano. The authors demonstrate the alteration of oxygen, sulfide, sulfate, and methane profiles and flux rates by the activity of the frenulate worms. It also makes some strong predictions about the controls on the worm's growth by the geochemical conditions it inhabits. This is a novel finding and approach and represents a significant contribution to the field.

I have a few concerns about some of the details of the paper, but I feel that these are relatively easy to address. First, it is unclear to me how the sulfide oxidation rate of

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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. 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.

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 discussing 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.

Specific comments follow:

P6686 L2: Should be “symbiotic bacteria”

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.

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.

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

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justified somehow here.

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.

P6694 L8-9: Was release of sulfate by the frenulates modeled explicitly? This could explain the additional sulfate at depth here. See the models of Cordes et al 2005 (cited) and Dattagupta et al 2006, JEB.

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

P6695 L16: A revision of the permeability coefficient might improve this.

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. . .

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