

Interactive comment on “Hydrothermal activity lowers trophic diversity in Antarctic sedimented hydrothermal vents” by James B. Bell et al.

Anonymous Referee #3

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General comments This paper reports microbial and biogeochemical data obtained from the recovered sediments at near hydrothermal vent and non-vent fields in Bransfield Basin. Such data is quite limited from the basin, even Southern Ocean, so this is a possible first systematic report. The authors mentioned that such sedimented hydrothermal systems (or commonly called sediment-hosted hydrothermal system, not only vent!) are the least studied deep-sea ecosystems, however, Okinawa Trough, which is a similar sedimented basin involved hydrothermal activities, have been studied for a long time. I can not understand why authors ignore a lot of previous studies performed in Okinawa Trough, Southwestern Japan. For example, recently many related studies have been published as an open access book from Springer (<http://www.springer.com/jp/book/9784431548645>). Sedimentary fatty acids (not only PLFAs) were also studied by Yamanaka and Sakata (Org. Geochem. 35: 573-582,

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2004). Authors should compare their data with those previous studies. I think the data reported in this study is still valuable, but it is not suitable for publication at this time.

Specific comments In the sediment-covered hydrothermal field the physicochemical condition of surface sediment is quite heterogeneous. So more careful consideration is required. In addition, description of core samples was almost lack. Authors cited previous study (Bell et al., 2016), but some parameters such as Cl⁻, H₂S, and methane should be provided for reader. Those informations are related to evaluation of sample heterogeneity. And also please show bathymetric map of the Bransfield Basin with sampling sites.

Authors displayed the isotopic data to two decimal place, but S.D. of instrumental analysis is not so small. The last decimal is so significant?

For sulfur isotope analysis of organisms and sediments it is quite important for complete removal of seawater sulfate. I could not find any description about sample preparation for sulfur isotope analysis in the manuscript. Sulfur data in this study, especially sediment data, is incredible for me.

Authors performed PLFA analysis and identified many PLFAs, but discussion of those origins is insufficient. I think this data contain some important information of organic matter sources necessary to discuss.

Line 328: Authors could not avoid the possibility of inorganic carbonate contamination, why did not authors treat the samples with acid?

Line 435: Do authors have any other evidence of nitrogen fixation? Such negative values is often found in chemosynthesis-based animals.

Line 438-439: Really SRB facilitate nitrogen fixation? Please indicate reference.

Line 444-453: Discussion about carbon isotope ratio of DIC is thrown into confusion.

Line 455-458: re-dissolved sulfide means the following reaction? $2\text{FeS}_2 + \text{organic}$

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matter (CH₂O) + 2H₂O = 2FeS + 2H₂S + H₂CO₃ This reaction is expected to occur at high temperature (>300-degree C)(Seewald et al., 1994 GCA 58: 5065-5082). So it is expected high temperature fluid discharging near the sampling point. Maybe hydrothermal precipitate which have quite low δ³⁴S values (< -5‰ originate in bacterial pyrite dissolution, but. . .

Line 474-475: Carbon isotope ratio of methane is easily changed by bacterial consumption (enriched in ¹³C). Those methane values were reported from the same core samples?

Line 482: sulfate reducer? Really??

Fig. 3: This figure make no sense to me. It is difficult to compare the difference because X-Y scales vary among species.

Line 607: What is the cause of the environmental toxicity? Hydrogen sulfide? Low DO? Heavy metals?

Other comments This manuscript contains many typos. Please check carefully. In section 1, I can not find subsection 1.1 and 1.2. Line 416: 19:1ω8 PLFA is not PUFA (poly-unsaturated fatty acid). It is monounsaturated fatty acid. And also indicate PUFA stands for. . .

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