

Interactive comment on “Temperature characteristics of bacterial sulfate reduction in continental shelf and slope sediments” by J. E. Sawicka et al.

Anonymous Referee #2

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In the manuscript “Temperature characteristics of bacterial sulfate reduction in continental shelf and slope sediments” by Sawicka et al., we authors presented the sulfate reduction temperature response in sediments collected in different regions. The authors bring again the topic of thermal adaptation of sulfate reducing communities by using well-established temperature gradient incubation experiments. The novelty of this study with respect to previous ones by the same authors is the survey of sediments of different regions to unravel short-term thermal adaptations. My main criticism is the suggestion that temperature response profiles can be used to fingerprint the origin of sulfate reducing bacteria populations from different origins. The authors should decrease the tone of this affirmation as it generates a lot of speculation in the dis-

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ussion. If the main goal of this survey in different sediments would have been to have a quick assessment of microbial fingerprinting, these analyses should have been complemented by molecular-based analyses based on specific 16S rRNA of SRB or specific genes (e.g. *dsrA*) (in fact, something so simple as DGGE analysis on RNA with well-known primers for SRB would have greatly support the conclusions derived from this study). In addition, if the idea of the authors was to survey temperature responses in sediments from different locations I would have expected more samples taken in the same sampling points and even a seasonal sampling. Despite of these comments the manuscript is well written, the methodology is correct and I am certain that the topic is of interest for the scope of the journal. But again, I would recommend reducing the tone of the “fingerprinting” assessment (or provide another method to support those conclusion if possible) and a discussion of why not more samples were analyzed.

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