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Biogeosciences Discussions

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

Anonymous Referee #1

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The manuscript "Temperature characteristics of bacterial sulfate reduction in continental shelf and slope sediments" by Sawicka et al. presents the temperature response of sulfate reduction in a variety of sediments, measured in a temperature gradient block. Ten different sediments from three different regions (the coast of Svalbard, and the southeast and southwest Atlantic) were investigated. Beside direct measurements of sulfate reduction at different temperatures, apparent activation energies from the obtained data were calculated. Furthermore in situ sulfate reduction rates in the uppermost 10 cm of the studied sediments are presented. The study is based on a sound methodology which is well established. The authors extend our current knowledge on temperature adaptation of sulfate reduction with new temperature profiles from new

sites. Furthermore, they introduce new and interesting interpretations of the measured temperature response of sulfate reduction. The questions addressed in the manuscript fits well to the scope of the journal.

However, the authors present only one temperature gradient block measurement of sulfate reduction per site investigated. Unfortunately, they have not sampled at least some of the sites repeatedly to test for the reproducibility and representativeness of their results. Nevertheless, they deduce from these single point/time measurements general conclusions, e.g. that the T-response of a sediment can be used as a "fingerprint to infer mixing of bacterial communities of different origins". To my understanding, this interpretation goes too far, as long as it is unclear how representative the measurements are for the sites instigated. The very different T-profiles in the near-by arctic fjord sediments indicate a significant small-scale heterogeneity and the authors did not give a convincing explanation for this heterogeneity (see below). A comparison of repeated measurements, e.g. from some of the arctic sites, would significantly improve the validity of the presented conclusions. If this is not possible, the authors should carefully revise the interpretation of their data and comment on the reproducibility of their results.

General comments:

The interpretation of the obtained temperature profiles in the discussion (4.1) is not consistent. The authors hypothesize that environmental temperatures select for specific microbial populations. The high temperature optima of respiration in some of the arctic sediments, which apparently contradict latter hypothesis, are explained (P684, L.22) by a significant temperature offset between microbial respiration (measured in their experiment) and growth (relevant for T adaptation). In contrast, the low temperature optima for microbial sulfate reduction (respiration) measured in the deep-sea sediments and another arctic sediment (Kongsfjorden) are seen as indication for the low temperature optima for growth of the microbial population (P685, L.12ff). It is not convincing that the temperature response of respiration should significantly deviate from the temperature response of microbial growth only in some of the arctic fjords (e.g. Smeerenburgfjorden) but not in others (Kongsfjorden). Hence, no convincing explanation for the >10°C difference in temperature optima of near-by arctic shelf sediments is given. The authors should give a consistent interpretation of the T-profiles in the arctic sediments or comment on the limits of their interpretation.

Furthermore, the discussion on the effects of sediment transport (discussion 4.3) on the temperature response of the microbial population is confusing. In a fist statement, the authors describe the Argentinean slope sediments as psychrophilic/psychrotolerant and explain the higher T-optimum in the Namibian slope with a down slope deposition of mesophilic sulfate reducers (P687, L. 7ff). In the next sentence, the authors describe that also the Argentinean and Uruguayan slope sediments are affected by down slope sediment transport, but in this case the deposition of sediment with mesophilic organisms results in broadening the temperature curve. The authors should explain, why in Namibia sediment transport results in a higher T-optimum and in Argentinia in a broader T curve with a lower T-optimum. As long as no repeated measurements of the T-response of SRR in the same sediments are available, it's hard to judge if the T-response of the Namiban slope (Fig. 1 panel c) and the Argentina slope (Fig. 1 panel d,f) are significantly different.

Specific comments:

The Reference list needs a thorough revision, some of the references in the text are not listed, and some in the list are in the wrong order.

P674, l17-25: Not only the continental slope but also permanently cold arctic sites showed a broad T optimum. Hence, the interpretation given is not sufficiently supported by the presented data and should be revised. The temperature response profiles of sulfate reduction cannot be used as a "fingerprint" for bacterial communities as long as the reproducibility of the measurements remains unclear. I recommend not using latter term.

P675, l. 24: Jørgensen and Kasten (2006) is missing

P677 L. 10: When did you sample the Svalbard sediments? 2007 or 2008 as given in P.679 l.6?

P677, L. 21: Carr 2002 is missing

P679 l.13: Did you also analyze the samples from 1998/1999 with the method of Kallmeyer et al. 2004?

P679, L.22ff and Fig. 1: Did you incubate the samples in replicates at the single temperatures? Give error bars in Fig. 1.

P.684L.28: Replace allow by prevent

P 686. L. 26ff: Please give a reference for these Ea values. Sagemann 1998 reports values of 38-74 kJ mol-1 for permanently cold arctic sediments.

P687, L.2f:This sentence is misplaced.

Interactive comment on Biogeosciences Discuss., 9, 673, 2012.