

Interactive comment on “Microbial activity and carbonate isotope signatures as a tool for identification of spatial differences in methane advection: a case study at the Pacific Costa Rican margin” by S. Krause et al.

Anonymous Referee #1

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Review "Microbial activity and carbonate isotope signatures as a tool for identification of spatial differences in methane advection: A case study at the Pacific Costa Rican margin" by Krause et al.

Krause and co-workers present measurements of microbial anaerobic oxidation of methane and sulfate reduction rates from sediments at the Pacific Coasta Rican Margin. In addition, they discuss the analysis of carbonate ^{18}O , $\delta^{13}C$, and $^{87}Sr/^{86}Sr$ signatures. They combine these measurements with numerical modelling to constrain

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the advection velocities and the origin of methane fluxes at these mounds. Results indicate pronounced differences between the two settings and thus reveal a strong temporal and spatial variability of methane charged fluid flow at the Pacific Costa Rican margin. The authors represent an interesting, multi-disciplinary analysis that integrates observations and numerical modelling techniques. The manuscript is generally well structured and well written. However, the authors need to emphasise the original aspects of their work. They should explain the differences between their study and previous studies. What is new? What distinguishes this study from the results presented in, for instance, Hensen et al., 2004; Mavromatis et al., 2012? How do their results fit into a regional/broader context? In addition, the description and rationale for the chosen model approach could be improved. In particular, simplifying assumptions (e.g. $CaCO_3$ treatment, FeS precipitation, pH dynamics, TA) should be better justified and critically discussed.

The presented manuscript could be considered for publication if the authors address these points and emphasise the originality of the presented research.

Specific Comments

p.2, l.24: a^{-1}

p.3, l.67: dissociate

p.9, l.228: remove brackets around Berner, 1980

p.9, l.228: The equation is only formulated for dissolved species. What about solid species?

p. 10, l. 250: What about bioturbation?

p. 10, l. 258: add with

p. 10, l. 261: mixed

p. 11, l. 271: justify the use of this equation for iron sulphide precipitation. Why don't you consider precipitation as a two step reversible process? What about the oxidation

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of FeS by O₂ and the oxidation of H₂S by Fe(OH)₃?

p. 11, l. 279: explain/justify why you use this simplified approach. Why is pH and the carbonate system not explicitly resolved? What are the errors associated with the chosen approach?

p.11, l. 289: how do you quantify HCO₃⁻, HS⁻ and CO₃²⁻ concentrations without resolving pH dynamics? For instance, the speciation of DIC and TH₂S released during AOM will depend on ambient pH and will also influence ambient pH.

p.11, l.290: B(OH)₄⁻ is an important component of TA in the marine environment.

p. 12, l. 293: What are the boundary conditions for Fe₂O₃ and CaCO₃?

p. 13, Result section: reference the figures more often

p.19-20, l. 495 onwards and l. 507: what is new?

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