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Discussions

Interactive comment on "Interaction between hydrocarbon seepage, chemosynthetic communities and bottom water redox at cold seeps of the Makran accretionary prism: insights from habitat-specific pore water sampling and modeling" *by* D. Fischer et al.

Anonymous Referee #2

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Natural methane seeps are highly interesting natural laboratories that are teeming with life due to the strong thermodynamic disequilibrium. Highly reduced seepage water, saturated with methane, contacts the oxidised biosphere, driving development of dense chemosynthetic communities. Anaerobic oxidation of methane (AOM) produces sulphide, that is then oxidised by sulphide oxidising bacteria, either free-living in mats or in the upper sediments, or in symbiosis with a high diversity of fauna, including bivalves, nematodes and polychaetes.

Seeps are often highly structured systems, both physically and biologically. Therefore they form natural laboratories for biologists to understand how complex transport processes determine conversion rates, and local diversity differences. The manuscript describes a study on how 3 transport mechanisms control AOM rates and the depth of the methane-sulphate transition zone. The mechanisms considered are diffusion, upward advection and bioirrigation. The study is based on camera observations on the occurrence of mats and chemotrophic (symbiontic) fauna, analyses on retrieved cores and modelling of porewater profiles, using transport-conversion modelling. The study area is interesting: methane seeps in and near the OMZ of the Makran area. In the central OMZ no bioirrigating macrofauna is observed. Sulphide oxidation is only possible by oxygen or nitrate, which are absent or extremely low in the OMZ. In the OMZ mats were observed of thiotrophs, but no benthic fauna. Outside the OMZ benthic fauna is found, except in the very center of seeps, where only mats are present. The data showed that with increasing distance from the center of seeps the depth of the Sulfate-Methane Transition Zone (SMTZ) increases. The SMTZ is operationally defined as the zone where sulphate profile levels off. The ecosystems typically form concentric circles with mats in the center, and symbiontic fauna on the outside. The depth of the SMTZ and the ecosystem structure are linked. Modeling showed that decreased seepage (upwardly directed advection) is needed to explain the increased depth of the SMTZ at further distance from the seep. Bioirrigating fauna cause a further deepening of the SMTZ, and induce a special kink in the sulphate profile. This is conceptually easy to understand: the depth to where the sediments are ventilated have a more or less level sulphate profile, below this diffusion and conversion will lead to a sharp decrease in sulphate concentration. The shape of the sulphate profiles could well be modelled when bioirrigation was included as transport mechanism. I cannot really judge the strength of the model, but it seems to me that the model has great value to explain the concepts, but not to provide quantitative predictions. It can be used to model the observed sulphate profiles, thus showing the importance of the 3 transport mechanisms, but it is not more than a fitting program. Parameters and constants are tuned till the profiles overlap. This brings me to the core of the problem with the paper: the calculated areal exchange rates, and conversion rates are based on porewater profiles from retrieved cores. During recovery of the cores from highly gaseous sediments severe disturbance will occur due to outgassing of methane. The sediments may contain 85 mM of methane, thus ca 80 mmol/L porewater will gas out (3-4 L gas/L sediment), during which significant exchange between seawater and sediment, and vertical mixing inside the cores will occur. This should really be discussed. The resulting spreading of the porewater profiles will lead to an underestimation of the advective flow. It also can explain the presence of sulphide in the watercolumn of some of the cores.

In short, the paper is excellent in concept. Even if none of the concepts are novel, it is a valuable contribution that summarizes the issue. I would very much like to use the data and model for teaching, to show the importance of the physical and chemical parameters that shape a seep ecosystem. But the reported exchange rates and upflow velocities are based on porewater water profiles that are disturbed by outgassing, and thus these can be underestimated.

Detailed comments: It would be good to explain the model better. It is a 1-D model, yet bioirrigation is 3-D. The total flux at each depth will be the sum of the advection, diffusion and bioventilation. Diffusion and advection can be approached as 1-D, upwards or down wards directed. Bioventilation is not. The same amount of water that is pumped downward will move up. The net areal velocity is thus 0. Yet, the depth integrated irrigation rates are expressed in velocity, m/year. The direction is not given. To include the bioirrigation in a budget we need an expression like Jtotal= J(diffusion)+J(advection)+J(bioirrigation). On p 9772 a mixing coefficient was mentioned. This will have a unit of m2/year? How does this translate to velocity? A spatial dimension seems to be lost. The authors should explain how this inconsistancy is solved.

It was argued that bioventilating fauna will eventually outcompete more simple sulphide

oxidising organisms, such as bactaria. This because they can mine deeper for reduced substances. This is a plausible hypothesis. Thus mats are seen as first colonizers, followed by fauna, that deepen the SMTZ, and make the reduced matter unavailable for bacteria that rely on an overlapping zone of oxygen and sulphide. However, clams can move rapidly, and thus would quickly invade the mat areas. Would there be an upper limit of upflow velocity, where fauna cannot sustain? The suggestion that fauna can outcompete thiotrophic bacteria should be supported by observations on seeps that have been regularly revisited.

On p9773 mats are described in the OMZ, where DO<1μM. This is based on older data. I do not believe that this low oxygen concentration is still there as the mats need electron acceptor. Are these really mats or precipitates? Such mistakes have been made, caution is needed.

P9784 The too low sulphide levels found are also due to loss during recovery, H2S will be subjected to outgassing.

P9785 Scenario A, so the total absence of upflow, is impossible, as then no supply of methane would occur. Same remark for L 8-9, P 9786.

P9787 An unlogical sentence: L8 ‘which induces’ should be ‘by inducing’, L9 ‘intense sulphide release’ should be ‘increasing AOM’.

P9787 I like L18-22.

The authors should explain where and when these massive pavements are formed. Also why it is striking that they are observed (P9788, L 6).

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