Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-12-AC1, 2018

Interactive comment on "Transition from hydrothermal vents to cold seeps records timing of carbon release in the Guaymas Basin, Gulf of California" *by* Sonja Geilert et al.

Sonja Geilert et al.

sgeilert@geomar.de

Received and published: 16 May 2018

The referee criticizes in his comment mostly our conclusion that hydrothermal activity has ceased in the Guaymas Basin deduced from the observation of dominantly seawater signatures in the pore fluids and biogenic methane emissions. The referee proposes that our data could also be interpreted in an opposite direction, namely that recent hydrothermal activity drives a shallow convection cell that draws seawater into the sediment.

At first, we do agree with Lizarralde et al. (2010) that hydrothermal activity in the Guaymas Basin was once driving seepage and an elevated thermogenic methane flux to the water column. We can also not exclude that there is still thermogenic methane released into the basin driven by off-axis sills. However, as all seep sites investigated in this study show predominantly seawater composition, a simple correlation of detected sills and active thermogenic methane release as done by Lizarralde et al. (2010) appears not to be feasible. Our data set shows that deep processes are extinct, at least at the investigated sites so that it is not unlikely that at other places deep processes are extinct as well. At least, it does not seem valid to assume that all other off-axis sills represent active hydrothermal systems. In order to calculate accurate thermogenic carbon fluxes, sill emplacement mechanisms like longevity and spatial distribution need further investigation. It appears that this aspect is not well understood from our manuscript and will be edited if it is accepted for further revisions.

The referee claims that the heat provided by the Black Smoker field might 'decompose the organic matter and therefore explains the mostly biogenic methane source'.

However, if organic matter is decomposed by an elevated heat source than the isotopic signal would be indicative of this thermogenic source. The thermogenic δ13CCH4 signal is relatively heavy (about -40 to -20‰ compared to the biogenic signal (< -55‰. All our (unaltered) δ13CCH4 data falls in the biogenic field (see Fig. 8) and are thus not decomposed by thermogenic alteration. The lateral heat from the Smoker field might support and enhance biological processes but is not responsible for the isotopic signal as proposed by the referee.

The referee further criticizes that we just might not have detected the deep fluid phase as it is decoupled from the gas phase and might arrive later or at a different location.

We do not think that this is a likely alternative to the presented hypothesis. The known active hydrothermal systems from the Southern (Von Damm et al., 1985) and Northern (Berndt et al., 2016) rift axis in the Guaymas Basin emit hot fluids with clear evidence for high temperature fluid-rock interactions and thermogenic gas production. Such a fluid is not found at any of the seeps. Instead, we found pore water containing predominantly biogenic methane, but which is otherwise only slightly diagenetically altered from seawater. Biogenic methane formation is expected to occur within the uppermost tens to a few hundreds of meters below the seafloor at low temperatures. Methane-enriched pore water sourced in those depths should be likewise enriched in other products of organic matter degradation (e.g. NH4). Such a fluid composition was found at the Slope Site, where organic matter turnover is extremely accelerated by high accumulation rates of organic material. The fact that only biogenic methane is significantly enriched at the seep locations lets us conclude that methane gas is percolating through shallow sediments (even forming gas hydrates as observed at North Site) rising along pre-existing low permeability pathways formed by previous hydrothermal activity.

We argue that such a high-temperature geochemical signal might have been present during and for a certain time after sill emplacement, but as there are no obvious pore water signatures today, we conclude that deep processes are extinct at the investigated sites (see also section 4.4). The young age of the carbonate supports our hypothesis of a decoupled gas and fluid phase as only ascending gas is needed to drive AOM and the formation of authigenic carbonates. Biomarker, δ13CCH4, δ18OCaCO3 and 87Sr/86Sr signatures clearly point to a formation in seawater at ambient temperatures. We agree with the referee of a recent seepage event, however, mainly driven by shallow-sources, biogenic gas and not by deep-sourced, hydrothermal processes.

Overall, we do agree with Lizarralde et al. (2010) that during and for a certain time after sill emplacement large amounts of CH4 and CO2 are emitted as the heat released by magmatic intrusions induced thermogenic decomposition of organic matter. However the activity of such a process appears to be limited by the lifetime of a sill-induced hydrothermal system (time required to cool down). From sediment thicknesses above extinct fluid conduits we estimated that the processes must have stopped more than 7kyrs ago at least at the places investigated so far. We cannot exclude that there are still areas in the Guaymas Basin with active sill-induced methane release. At our investigated sites though, we have not found any evidence of thermogenic methane release. A simple extrapolation as done by Lizarralde et al. (2010) in which they compile all sills and estimate the potential methane release appears not applicable.

The referee claims that the title of our manuscript might be misleading as it appears to argue for the process of active thermogenic methane release.

The title of the manuscript might indeed be misleading. We referred to the processes itself and not to the activity. We are willing to change the title to a more general meaning, like ‘On the development of hydrothermal vents and cold seeps in the Guaymas Basin, Gulf of California’.

Specific comments:

We also appreciated the helpful and detailed specific comments about language, grammar, and general clarification needs, which we will gladly correct in case of a positive evaluation. In L434-435, L445-446, L461, and L473-474 in the referee comment the referee criticizes our interpretation of seawater and hydrothermal signals in the pore fluids.

We did find hydrothermal signatures but only in the deep core section of GC09 (>4m) as reported in lines 461-474 in the manuscript. The remaining pore fluids show seawater composition and are interpreted as shallow convection cell drawing seawater into the sediment. We propose to rearrange this section to clarify our interpretation of the data.

In L502 the referee notes that the word ‘active?’ appears without context.

The blanking of the seismic profile indicates a fluid conduit, but the profile cannot differentiate between active fluid and / or gas flow. As we only conclude later in this paragraph that the fluid and gas phases must have been decoupled we decided to put the ‘active’ in question.

In L526 the referee criticizes that Li is named a major pore water component.

Li is considered as a major indicator for high-temperature sediment-water interactions, as are Mg and Cl. We agree that it might be confusing in this context and are willing to rearrange the sentence.

In L565 and following the referee requests a definition for the type of oxidation.

The samples were taken in the AOM zone and the oxidation is therefore anaerobic. We will gladly clarify this section during the revision process.

In L601-629 the referee claims that the shallow convection cell surrounding the hydrothermal area contradicts with our conclusion.

We do not deny the activity of the hydrothermal system in general. We just state that at the investigated seep sites no deep signal is detected and there are no indications of actively released thermogenic methane. We cannot exclude that this process occurs in other areas of the basin, however Lizarralde et al. (2010) calculated methane flux might be excessive (see also comments above).

In L677-680 the referee wonders how the young age of the carbonate argues for a cessation of a deep signal.

The δ13CCH4 data of the bulk carbonate overlaps with the δ13CCH4 values in the associated pore fluids. No indicators of a deep signal have been found in the carbonate. Indeed, carbonate formation requires a recent seepage event; therefore we concluded beneath others that the fluid and gas phase must have been decoupled (see also comment above).

References

Von Damm, K. L., Edmond, J. M., Measures, C. I. and Grant, B.: Chemistry of submarine hydrothermal solutions at Guaymas Basin, Gulf of California, Geochim. Cosmochim. Acta, 49(11), 2221–2237, 1985.

Lizarralde, D., Soule, S. A., Seewald, J. S. and Proskurowski, G.: Carbon release by off-axis magmatism in a young sedimented spreading centre, Nat. Geosci., 4(1), 50–

54, doi:10.1038/ngeo1006, 2010.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-12, 2018.