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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.

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Referee #2 has difficulties with three main aspects of our manuscript concerning the biological significance, the spatial coverage of sampling sites, and the new discoveries of our manuscript in contrast to earlier studies.

First, the referee claims that the biological aspects of our study are too small to get published in Biogeosciences.

The main findings and conclusions of our study are based on biological aspects, like the microbial signature of δ13C data and the AOM-dominating biomarkers identified in the

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carbonate. The detected microbial signatures helped to identify that deep processes are extinct nowadays. The biological results support our geochemical and geophysical observations and form a key point of our discussion. Additionally, we understand that the objective of this journal is to publish research which combines biological, chemical, and physical investigations and which highlights the interaction between them (see homepage Biogeosciences). Our manuscript combines all three aspects and emphasizes the importance of an interdisciplinary research approach to draw the best possible conclusions.

Secondly, the referee expresses his concerns that the spatial coverage of our sampling sites is not sufficient to infer basin-wide phenomena.

Sample locations were chosen based on findings by Lizarralde et al. (2010) who describes sill intrusions associated with hydrocarbon gas emissions, biological communities, and authigenic carbonates. In this study, we investigated 3 seepage sites at various distances from the hydrothermal vent field based on locations identified by Lizarralde et al. (2010) as areas of active methane release. Additionally, a reference site, smoker sites as well as the water column have been sampled. With the exception of the active smoker site, there is no indication for a deep fluid advection and methane δ13C data are predominantly of microbial origin (see Fig. 4 and section 4.4). Despite the fact that no deep fluids were detected at the seepage sites, an active methane flux was present, indicating that we hit the currently active sites described in Lizarralde et al. (2010). The detected methane was predominantly of microbial origin and no active thermogenic methane is released nowadays at the investigated sites as claimed by Lizarralde et al. (2010). We cannot exclude the possibility that thermogenic methane is still released in other areas of the basin, but the lack of evidence for high temperature geochemical processes at the investigated sites contradicts with Lizarralde's et al. (2010) conclusions. The seismic evidence of seep-induced hydrothermal systems alone is not sufficient for projecting methane emissions for the whole basin at present (see also comment to referee#1). Thermogenic methane release induced by off-axis

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sill intrusions is still a likely process to occur, but our study suggests that the lifetime of these systems is limited and has to be taken into account for budget calculations. Hence, the study of Lizarralde remains valid and is highly valuable in terms of describing the general process and the potential magnitude, but care has to be taken concerning the longevity of the hydrothermal systems and associated thermogenic methane release after the occurrence of sill intrusions.

The last point of criticism by the referee is that it is not clear how the findings of this study differ from those of Lizarralde et al. (2010) and Berndt et al. (2016).

If the main findings of this study are not clear to the referee then we indeed have to improve this part of the manuscript and clarify how our findings differ from those of earlier studies. While Berndt et al. (2016) focused on characterizing the geophysical and geochemical characteristics of the Smoker area, Lizarralde et al. (2010) investigated geophysical aspects of the wider basin and the water column. Our study is the first one to look at geochemical, biological, and geophysical characteristics of seepage sites and the water column above. Main findings are the decoupling of gas and fluid phases, the microbial origin of methane, and the detection of sediment layers above extinct fluid conduits. We used the sediment thickness to infer an age at which deep fluid and gas flow induced by magmatic intrusions must have ceased. Our results contrast with findings by Lizarralde et al. (2010) who claim that thermogenic methane is still actively released in all places presented in their study. As detailed above, we do not disagree with Lizarralde et al. (2010) about the general mechanism. However, we disagree that all of the off-axis sites are presently active in the sense of hydrothermal systems (we discovered none) and that their lifetime has to be taken into account. We claim that this process only occurs during and for a certain time (depending on the lifetime of a sill-driven hydrothermal system) after the magmatic intrusions intruded in the sediment. How long this process really occurs still needs further investigation.

Specific comments:

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We value the specific comments of the referee and would be glad to improve wording, definitions, and figures in case of a positive evaluation.

To the referee, the purpose of biomarker and heat flow analyses is unclear (L150 and L255 in referee comment).

The analyses of biomarkers and heat flow delivered fundamental knowledge about the origin of the carbonate and the heat distribution in the basin, respectively. Both analyses helped to reach the conclusion that high-temperature processes at the investigated sites are extinct.

In L491 the referee comments on the way of sediment-sill interaction.

We agree that after sill-emplacement, heat is the driving force to induce chemical reactions, as observed also in other regions (Cruse and Seewald, 2006; Ishibashi et al., 2014). If that conclusion is not understandable from our manuscript, we will clarify it.

In L599 the referee criticizes the section about timing in our manuscript.

Indeed, ages are only available for the carbonate sample and from the sedimentation rate. However, we approached the cessation of active thermogenic methane release by taking the sediment thickness above extinct conduits into account. Of course, the resulting time is only an approximation. As we stated in our manuscript and in the comments above, the lifetime of a magmatic system needs further investigation before conclusions of the timing of active methane release can be drawn.

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