Reviewer Comment 1

The manuscript by Schnabel et al. reports geochemical and microbiological analyses of sediment cores from an area with known subseafloor hydrocarbon reservoirs. The authors collected the cores on a grid and performed some geostatistical mapping.

I believe the manuscript has great potential based on the large number of investigated cores but the authors should work more on finding a novel aspect as well as implications.

We thank Reviewer 1 for taking the time to comment on our manuscript, providing constructive remarks that helped us improve the revised version submitted here. By providing point-by-point responses, we hope that Reviewer 1 will consider that we have addressed the comments and implemented the suggestions satisfactorily. A version of the revised manuscript with track changes is provided.

I have some concerns that can be grouped into three categories:

1) The term “seepage” is improperly used throughout the manuscript. Hydrocarbon seepage refers to areas where hydrocarbons are emitted from the seafloor. From what I read, the authors did not find any seepage (and the pore water profiles also show low gas fluxes and deep SMTZ) but rather show diffusive fluxes above the SMTZ (not intercepted) which are indirect evidence for diffusive fluxes of gas from deeper strata. That term is therefore, misleading, as I was looking for some water column “flare” or other evidence of seepage which I could not find. Moreover, in the text it is always mentioned the term “hydrocarbon” without defining whether it refers to just gas or oil. This must be clarified. The term “inconspicuous” is not explained, does it refer to ephemeral fluxes? or just to low fluxes?

We thank reviewer 1 for their insightful comments that helped us reflecting on our work. With some comments and suggestions, we fully agree, with others not so much. Regarding the usage of the word “seepage”, we feel that the reviewer’s perspective comes too much from systems with high rates of discharge. This perspective becomes particularly visible in their comment for Line 125, in which Haakon Mosby Mud Volcano (HMMV) is considered low-intensity seepage. HMMV is a structure that rises about 100 m above the seafloor and has a diameter of over 2 km while our sites have no surface expression at all. Seepage rates of HMMV are many orders of magnitude higher than in our study areas. Although our sites do not have any surface expressions, still there are hydrocarbons migrating upwards. In this study, we use the term ‘inconspicuous seepage’ to describe diffuse, low-flux upward migration of hydrocarbons within the sediment column that lacks any visible seabed expressions or water-column anomalies (e.g., flares), and therefore must be inferred from subtle geochemical and microbiological indicators. A clarifying sentence was added in the introduction: “According to our definition inconspicuous HC seeps are seeps where the seeping HCs lack visible surface manifestations. Instead, the HCs spread diffusely and are used up by biogeochemical processes so that the HCs do not reach the SWI.”

Regarding the term “hydrocarbon,” the reservoirs in the study area contain both oil and gas, so that is what we refer to. However, the weak and diffuse nature of the seepage observed here suggests that upward migration is dominated by gasses and/or low viscosity or dissolved *hydrocarbons. A more detailed analysis of the hydrocarbons was beyond the scope of this study.*

2) From the highlights, I see that all the bullet points are actually well-known processes, so there is no novelty or insights. We already know the biogeochemical processes associated with hydrocarbon oxidation and the consequent precipitation of minerals at or around the SMTZ. Also the spatial heterogeneity in those processes and pore water fluxes is common in gas charged sites.

We agree that the fundamental biogeochemical processes associated with hydrocarbon oxidation and authigenic mineral formation at, or around the SMTZ are well established. The novelty of our study does not lie in the discovery of new processes, but in demonstrating how these processes operate under extremely low-flux, non-expressive ("inconspicuous") seepage conditions.
Our results show that, even in the absence of visible seafloor expressions, water-column anomalies, or shallow SMTZs, which are features typically associated with active seepage; subtle and spatially consistent geochemical as well as microbiological signals persist in the sediment column. Identifying and quantifying such weak expressions has rarely been demonstrated in organic-lean, glacially influenced sediments anywhere, particularly not for the southwestern Barents Sea.
Thus, the contribution of this study is to illustrate the diagnostic sensitivity and spatial behavior of known biogeochemical indicators under seepage conditions which are difficult to detect and easily overlooked. This has direct relevance for seep detection in frontier basins, environmental baselines, and offshore resource assessments.

3) some methodological issues for pore water fixation

Please see comment for line 194

Detailed comments:

Line 65: add reference after "coastal areas"

We added a citation.

Chavez, F. P., Messie, M., and Pennington, J. T.: Marine primary production in relation to climate variability and change, Ann Rev Mar Sci, 3, 227-260, 10.1146/annurev.marine.010908.163917, 2011.

Line 122: seeps can be classified, instead of "are classified". The classification in table 1 based on Judd and Etiope's works cannot be applied worldwide, for example the Arctic fauna present much less diverse ecosystems, pockmarks are not "per se" proof of ongoing seepage but can be fossil structures now inactive. The methane fluxes reported in the table, do they refer to fluxes at the sediment/water interface or what? I suggest removing this table and related terminology.

We agree that the seep classification from Judd & Etiope cannot be universally applied, particularly in the Arctic. We have therefore changed the phrasing to "can be classified" and clarified which aspects of the classification are relevant for our study area.

We did not remove the table because we believe that it gives an overview of what we consider inconspicuous seep compared to seeps with surface manifestations. In addition, we explain what the term "seep site" encompasses in this manuscript, based on applying selective criteria to differentiate sampling sites rather than global seep categories.

Furthermore, we have incorporated references to studies by Nickel et al. (2012, 2013), which specifically discuss pockmarks in the Barents Sea. These studies demonstrate that pockmarks in this region are often ancient or inactive features and may not indicate active seepage. We believe that these works provide additional context to support our classification approach and the interpretation of seep features in our study area.

Line 125: actually, we see in the Arctic that low-intensity seepage (emissions) can be associated with widespread chemosynthetic habitats (mats), e.g. HMMV

This again reflects different definitions of seepage. We now clarify in the introduction of the manuscript that our study focuses on non-expressive, low-flux seepage within sediments rather than visually detectable emissions (also see answer to major concern No.1).

134: please clarify what you use to distinguish between low-intensity seepage and unaffected zones.

Our classification of stations as "seep" or "reference" follows the site characterization provided by our industrial project partner Aker BP and is in line with our definition of seepage.The reference zone has no underlying hydrocarbon anomaly.

137-140: implications? This is already known

We agree that the general effects of hydrocarbon seepage on pore water chemistry are well known. However, the implications of our study differ because we investigate these processes under extremely low-flux, non-expressive seepage conditions, which are rarely documented and poorly constrained.

187: define how many hours after retrieval

Done.

191: why have you also filtered at 0.22 um? The rhizon mesh is already 0.15 um so that would be useless. But it does not affect the data.

You are correct that rhizon samplers have a 0.15 µm pore size. We filtered samples at 0.22 µm because this is standard practice in our workflow, particularly when pore waters are obtained using other methods, e.g pore water hydraulic press. For the samples discussed here, this additional filtration step had no analytical impact. We kept the statement for consistency with our standard laboratory procedures.

194: why haven't you acidified the pore water sample for metals? That's an important preservation step to avoid scavenging onto newly-formed precipitated particles. The metals data can be altered.

Pore water samples were kept anoxic and only acidified before analysis, which we omitted to mention in the method section. Because pore water samples were kept anoxic we are confident that exposure to oxygen did not occur and that our geochemical analyses were not affected by precipitation.

232: 3% is referring to a RSD or what parameter?

Yes, changed accordingly

240: measurement uncertainty, what metrical parameter does it refer to? Repeatability or what? Please check the guidelines by the International Association of Geoanalysts.

We revised the text to specify whether we refer to precision, repeatability, or reproducibility.

467-469 and 495-496: so no seepage

As mentioned in our previous answer, this derives from different definitions of seepage. We now clearly state in the introduction how we define inconspicuous seepage.

520-523: low-Mg Calcite? Strange, generally methane-derived carbonates are aragonite or high-Mg calcites.

We agree with the Reviewer that methane-derived carbonates are typically associated with high-Mg calcites or aragonite. In the absence of direct measurements of carbonate phases, we based our interpretations on concomitant changes observed for both the solid-phase and pore water geochemistry. We inferred the presence of likely diagenetic low-Mg calcites based on pore water profiles which reflect preferential incorporation of Ca^{2+} over Mg^{2+} or Sr^{2+}. The absence of significant changes in Mg^{2+} concentrations in the pore water thereby suggests limited substitution of Ca^{2+} by Mg^{2+} during precipitation, which is consistent with faster, localized precipitation under active seepage conditions. We acknowledge that the exact nature of the carbonates requires further investigation, and we have adjusted the wording in the manuscript to reflect this uncertainty more clearly.

558: please add ref to the sentence "…SO3 concentrations in the solid phase,"

There is a misunderstanding caused by an imprecise formulation. We are referring to our own results, so we do not need a reference her. We have clarified this accordingly in the text. The revised section now reads as follows: "Because higher pore water sulfide concentrations in our samples generally correlate with elevated sulfur (i.e. SO_3) concentrations in the solid phase, sulfide precipitation and sulfur accumulation allow us to distinguish seepage sites from reference sites, where they remain systematically low."

669: variable seepage intensity, again this is not what you are referring to, which is diffusive fluxes within the sediment

We refer the Reviewer to our previous answers. Our definition of seepage differs from the one of the reviewer. In the introduction of this manuscript we provide a clear definition of inconspicuous seepage.