Comments from Andreas Teske

The current version of this manuscript is much improved compared to previous versions from one or two years ago. I noticed that the current version of the manuscript includes a detailed bathymetric map with clearly defined pockmarks, a much-needed improvement over the previous version that contained no bathymetry whatsoever. Also, the concentration profiles are now shown in a high-resolution figure where data points are color-coded by sampling region – a marked improvement over the previous version that showed only linearized functions (in greyscale) and no datapoints at all (and it was all hidden in the supplements, if I recall this correctly). So, I am happy to see these improvements that really make a difference.

We would like to thank Prof. Teske for his insightful comments that helped us improving our manuscript. In our point-by-point responses, we hope to demonstrate that we have addressed all comments and implemented the suggested revisions satisfactorily.

Lines 179ff: I guess that these sediment samples (3 cm^3) were used for methane and CO2 concentrations, as indicated by figure 1C? It would be helpful to mention this explicitly.

We added the information that the 3 cm^3 sediment samples were indeed used for methane and CO_2 concentration measurements.

Table 1. In terms of fluxes, there seems to be a gap of two orders of magnitude (1 mmol x m2 x d-1 to 100 mmol x m2 x d-1) between low flux seeps and moderate seeps. Is this a “grey zone” where current data are not sufficient to draw a clearer distinction?

Yes, the flux range between low- and moderate-flux seeps represents a conceptual transition zone. We have revised the caption of Table 1 to clarify this point.

In seep cores, do you observe covariance between iron and sulfur or sulfide concentrations in the seep cores, something to be expected since sulfide traps reactive iron and keeps it out of porewater? The manuscript text is specifically outlining the scenario in lines 550 ff, and discusses sulfide mineral formation in deeper sediments and at the SMTZ (below the range of these cores) in several paragraphs that follow; however I did not see any plots examining the Iron-sulfide relationship in any detail. Even the manuscript does not contain solid-phase data on sulfides, the porewater data are available (Fe concentrations in Figure 2h; H2S concentrations in Supplementary Figure S3), a clear case of low-hanging fruit. Other studies have examined the links between Fe and S concentrations in seep sediments (for subsurface cores, see Kars, M., Pastor, L., Burin, C., Koornneef, L.M.T., 2025. doi:10.14379/iodp.proc.385.207.2025; for push cores, see Rochelle-Bates et al. 2024, Doi:10.3389/fmicb.2025.1523696, Fig. S3). To summarize, frequently discussing metals and sulfides in the text calls for checking [and plotting] the data.

We fully agree with this suggestion and have added a new figure showing the relationship between dissolved Fe, sulfide, and sulfate concentrations for each site (see below). The new plot demonstrates that Fe and H_2S show clear inverse trends in seep-affected sediments, while sulfate generally decreases with depth.

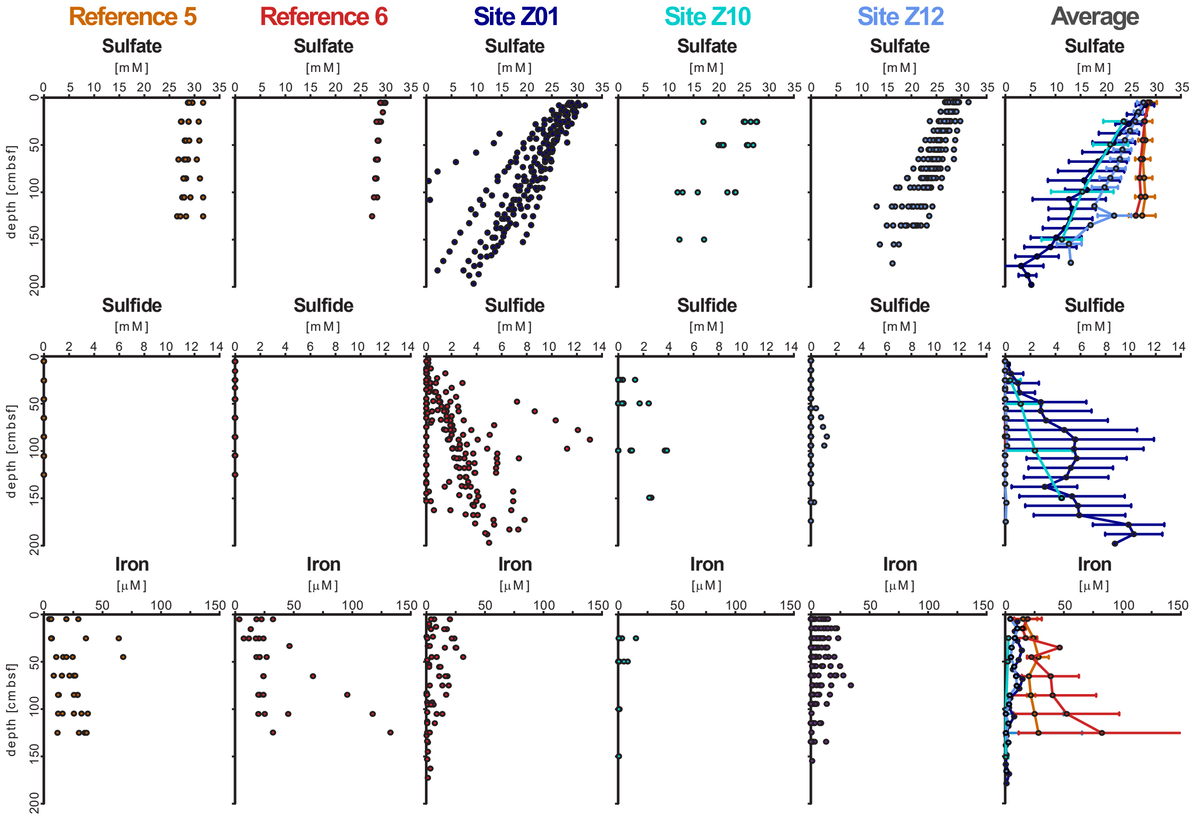

Figure 6. Porewater profiles of sulfate, sulfide, and dissolved iron across five sites, including site averages. In seep-affected cores, iron concentrations decrease where sulfide increases, indicating removal of reactive iron through sulfide precipitation. Sulfate generally decreases with depth across all sites. Error bars represent site-specific averages where applicable.

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.

Reviewer 2

This study investigated the impact of low-intensity hydrocarbon seepage on the geochemistry of the solid phase and porewater in organically impoverished sub-seafloor sediments of the Southwest Barren Shelf. By analyzing 50 gravity cores, the research combined organic and inorganic geochemical analyses. It found that while seepage areas lacked significant organic geochemical signals, inorganic indicators (e.g., the formation of carbonate and sulfide minerals) and porewater profiles (e.g., gradient changes in sulfate, alkalinity, and calcium ions) exhibited clear seepage imprints. The results suggest that even if hydrocarbons are completely degraded in deeper layers, shallow sediments can retain evidence of authigenic mineral precipitation driven by redox processes, providing complementary indicators for identifying past and present seepage activities. The significance of this study lies in revealing the subtle impacts of low-intensity seepage on the biogeochemical cycling of marine sediments, holding academic value for understanding carbon cycling, microbial activity, and hydrocarbon exploration. Overall, the study design is sound, the data is rich, and the conclusions support the research hypotheses. However, there are some methodological and interpretational shortcomings that need to be addressed before publication. I believe the manuscript is worthy of publication after appropriate revisions, as it offers novel insights, especially for seepage detection in organically impoverished environments.

We thank Reviewer 2 for taking the time to comment on our manuscript, providing constructive remarks that helped us improve the manuscript. By providing point-by-point responses, we hope that Reviewer 2 will consider that we have addressed the comments and implemented the suggestions satisfactorily.

Major issues:

The main problems in the manuscript are concentrated in methodological limitations, depth of data interpretation, and statistical processing of some results. For example, the organic geochemical analysis failed to effectively capture the seepage signal, the sampling depth might not have covered key biogeochemical zones, and the statistical methods are somewhat simplified in explaining spatial heterogeneity. These issues affect the comprehensiveness and persuasiveness of the results.

Deficiencies and suggested revisions:

1)Limitations of organic geochemical analysis (Lines 30-31):

The manuscript states that "organic geochemistry analysis provided limited insights" (Lines 30-31) but does not fully explain why FT-ICR-MS failed to detect seepage-related compounds. This could lead readers to question the applicability of the method.

Suggestion: In the discussion section (e.g., Lines 495-504), deeply analyze the reasons for failure, such as whether the degradation products of hydrocarbons had too low molecular weights or were affected by background signal interference. Compare with successful cases in similar environments from the literature to enhance critical thinking in the methods section.

We appreciate the suggestion and understand the importance of providing a clear explanation. However, we feel that we have sufficiently mentioned the reasons why FT-ICR-

MS did not yield any useful findings. In brief, the subtle nature of hydrocarbon seepage in our samples, as well as potential degradation of hydrocarbons below the sampling depth, likely limited detectability. Regarding the literature, we do not see suitable studies to cite for our specific setting. While FT-ICR-MS results from strong seeps with surface manifestations do show clear signals, these are not comparable to our organic-lean, low-seepage samples. We hope this explanation helps to clarify the limitations of our approach.

2)Potentially insufficient sampling depth (Lines 39-40 and 533-534):

The manuscript acknowledges that microbial hydrocarbon degradation may occur below the sampled interval (Lines 39-40) and mentions the fluctuation of the SMZT (Sulfate-Methane Transition Zone) in the discussion (Lines 590-597). However, it fails to assess whether the sampling depth (maximum 3 meters) was sufficient to capture key processes, which limits the representativeness of the results for active seepage.

Suggestion: Add a discussion on the rationale for sampling depth in the methods section (Lines 132-135), for example, by citing regional SMZT depth data. In the discussion (Lines 610-617), recommend future studies using deeper sampling or model inference to compensate for spatial limitations.

Sampling was focused on the upper 3 meters of sediment to capture the biogeochemical and microbial signatures of hydrocarbon seepage. While we acknowledge that microbial hydrocarbon degradation may occur below the sampled interval, longer cores could not be obtained due to technical limitations. The sediment consists of extremely sticky silty clay that does not allow for any deeper penetration, unless with much larger equipment that would require a larger research vessel with dedicated gravity/piston coring capabilities. This was beyond the scope and financial means of this project. We knew from previous projects in the area (Nickel et al., 2012, 2013) that 3 m penetration will be about the maximum we can achieve with the given equipment.In microseepage systems, hydrocarbons may be fully metabolized before reaching the sediment–water interface, so we aimed to detect indirect effects rather than the hydrocarbons themselves. This strategy allows identification of seepage impact while minimizing sampling effort. We have modified the text in the introduction accordingly: "We analyzed 50 gravity cores collected from the southwestern Barents Sea, including 40 cores from zones affected by low-intensity seepage and 10 from unaffected reference zones. Given the challenges of deeper coring and the goal of minimizing environmental impact, we focused on the upper 3 meters of sediment to determine whether direct or indirect effects of seepage could be detected."

3) Insufficient detail in statistical method descriptions (Lines 263-286):

The statistical section (e.g., PCA and Mann-Whitney U test) is described very briefly. Lines 273-286 mention the use of R software but omit crucial parameters (e.g., criteria for selecting variogram models), which could affect the reproducibility of the results.

Suggestion: Supplement statistical details in the methods section (near Line 286), such as PCA loadings or the goodness-of-fit for kriging models. Include code snippets in the supplementary materials to enhance transparency.

We have expanded the section "Statistical and geostatistical analyses" to provide additional details on the procedures used. Since the methods employed (PCA, Mann–Whitney U test, *Brown–Forsythe test, Pearson correlation, and ordinary kriging) follow standard statistical workflows as implemented in commonly used R packages, and all required parameters and decision criteria are now described in the manuscript, we therefore consider the inclusion of code snippets in the Supplementary Materials unnecessary.*

4) Linearity assumption in pore water data Interpretation (Lines 352-355 and 392-393):

The manuscript assumes linear changes in porewater profiles (e.g., sulfate and alkalinity) (Lines 352-355), but mentions non-linear manganese concentration profiles (Line 392-393). This has not been adequately addressed in the statistics, potentially leading to biases in flux calculations.

Suggestion: Add a comparison of non-linear models (e.g., exponential fitting) in the results section (Lines 350-355). In the discussion (Lines 587-590), explain the limitations of the linearity assumption and suggest the use of more complex diffusion models.

Deviations from linearity are now explicitly referenced with Fig. 2, Supplementary Fig. S4, and the porewater profile descriptions in Section 3.1. We already stated that the results for manganese should be considered as approximations. The linear fits were applied as a first-order approach to estimate concentration gradients and fluxes. While more complex non-linear or reactive transport models could better capture the curvature in some profiles, they were not applied here in order to maintain clarity and focus.

5) Overly general conclusions (Lines 655-665):

The conclusions reiterate the main findings of the results but fail to highlight the novelty of the study (e.g., the significance of spatial heterogeneity). Lines 661-662 mention that "the FT-ICR-MS-based approach was unsuccessful" but do not elaborate on the methodological implications.

The reasons for the limited applicability of FT-ICR-MS are discussed in detail in section 4.1 of the discussion. However, for better clarity, we have revised the conclusion to more clearly reflect this limitation. The updated conclusion now reads as follows:" Extremely low element fluxes with often complete degradation of HCs in sediment layers below the sampling depth of this study posed significant challenges in the detection of active seepage. This explains why the FT-ICR-MS-based approach was unsuccessful in identifying diagnostic organic compounds, as volatile and short-chained organic compounds cannot be resolved."

Suggestion: Rewrite the conclusion section to emphasize the innovative aspects of this study in detecting low-intensity seepage.

We added a sentence to emphasize the difference between our study and studies on strong seeps.

Additionally, please cite the following literature to enhance the logical flow of the study's background and discussion.

Lines 63-70: When introducing microbial activity controlling sedimentary cycles, you can cite this literature (Yang et al., 2025a) to support the role of organic matter input. When outlining the sedimentary carbon cycle, cite literature (Wang et al., 2025) as background.

Lines 81-85: When analyzing the contribution of hydrocarbon seepage to methanogenesis pathways, cite literature (Cai et al., 2025) to explain the key role of substrate availability in regulating microbial responses.

Lines 480-487: When comparing hydrocarbon seepage with background organic matter mineralization, cite literature (Wang et al., 2025) to support the influence of substrate characteristics on temperature sensitivity.

Lines 510-517: When explaining how the anoxic conditions caused by hydrocarbon seepage enhance carbon mineralization, cite literature (Zhang et al., 2025) as supporting evidence.

Lines 184-189: When discussing how hydrocarbon seepage might induce a priming effect through methanogenesis, you should cite literature (Yang et al. 2025b) to strengthen the conclusion.

Lines 132-137: When comparing the priming effect of different organic matter sources, you can cite literature (Yang et al. 2023) to highlight the uniqueness of hydrocarbon seepage.

Lines 498-502: When speculating on the fate of hydrocarbon degradation products, you can cite literature (Yang et al. 2020) to illustrate the potential for microbial utilization of allochthonous DOM.

Refs suggested:

Yang et al., 2025a Bacterial biomass-derived organic matter triggers nitrous oxide production and positive priming effect in lake sediments. Geochim Cosmochim Acta 408: 190-200, https://doi.org/10.1016/j.gca.2025.08.030

Cai et al., 2025 Substrate Availability Controls the Temperature Sensitivity of Methanogenesis in Lake Sediments. Water Research 289: 124836, https://doi.org/10.1016/j.watres.2025.124836

Wang et al., 2025 Substrate chemistry trumps mineral protection in governing temperature sensitivity of organic carbon mineralization in saline lake sediments. Geochim Cosmochim Acta 407: 81-90. https://doi.org/10.1016/j.gca.2025.08.040

Zhang et al., 2025 Increased anoxia promotes organic carbon mineralization in surface sediments of saline lakes. Journal of Earth Science 36: 2240–2250, https://doi.org/10.1007/s12583-024-0155-4

Yang et al. 2025b Methanogenesis rather than carbon dioxide production frives positive priming effects in anoxic sediments of saline lakes. Chemical Geology 678: 122680, https://doi.org/10.1016/j.chemgeo.2025.122680

Yang et al. 2023 Predominance of positive priming effects induced by algal and terrestrial organic matter input in saline lake sediments. Geochimica et Cosmochimica Acta 349: 126–134, https://doi.org/10.1016/j.gca.2023.04.005

Yang et al. 2020, Potential utilization of terrestrially derived dissolved organic matter by aquatic microbial communities in saline lakes. The ISME Journal 14(9): 2313-2324. 11.217 https://doi.org/10.1038/s41396-020-0689-0

We carefully reviewed each recommendation to ensure that the literature cited in our manuscript remains relevant and directly supports the arguments presented. While we appreciate the suggestions, most of the proposed articles focus on limnic or saline lake systems, considering the effect of terrestrial OM input and/or methanogenesis. Although this is not inherently problematic and some concepts may be transferable, we do not consider the suggested citations relevant to the context of our manuscript. Therefore, we concluded that these references are not suitable in this context. Apart from content-related reasons, we also note that all articles originate from the same working groups and we do not consider it appropriate to emphasize the work of a group of authors so much.