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.