We thank the reviewers for their constructive comments, which we have addressed in the revised manuscript. Specifically, we restructured the introduction to place less emphasis on coral ecology and focus more on seep biogeochemistry. Throughout the manuscript, we rephrased sections to ensure that no unsupported biological implications regarding corals are mentioned. Additionally, we simplified and reduced the number of figures, while including a supplementary file that contains all pore water plots (retaining only the most representative ones in the main text), gas data, and the biofilm image. We also edited Figures 2, 3, 4, and 5 (previous Figure 6). We added a discussion section to integrate our study findings with previous works in this area.

REVIEWER1

R1: I appreciate the opportunity to review the manuscript by Argentino et al., which documents the seafloor features and sediment/porewater geochemistry of a cold seep and cold-water coral site off Vesterålen, northern Norway. The authors present ROV observations together with geochemical and microbial analyses of the sediments and porewaters. The study compiles a diverse dataset, including seafloor imagery and mosaics, porewater concentrations and isotopic compositions, foraminiferal and bulk sediment geochemistry, as well as DNA data.

However, the conclusions drawn from these various datasets appear rather fragmented…

Authors reply: We restructured the introduction and emphasized the scope and context of this study as part of a larger multidisciplinary EMAN7 project. We specify that "This study provides a comprehensive overview of the biogeochemical context and seafloor environment where the Vesterålen CWCs have developed, thus aiding in the assessment of the potential influence of methane seepage on corals. Additionally, the generated seafloor orthomosaics and habitat maps will support the monitoring of habitat changes in response to future climate-induced environmental perturbations."

R1: In addition, some datasets reported do not appear to contribute meaningfully to the main objectives (for instance, the rationale for including foraminiferal stable isotope data is not explained).

Authors reply: In the revised version we explained more clearly the use of foraminifera as proxy for AOM-induced carbonate precipitation (reconstruction of fluctuations of the SMTZ), we specify :" The isotopic composition of foraminifera was used to trace MDAC precipitation as secondary overgrowth. We combined foraminifera geochemistry with pore water data to determine whether isotopic anomalies in foraminifera are associated with modern or ancient Sulfate-Methane Transition Zones (SMTZ)."

R1: When examining the specific conclusions more closely, not all are fully supported by the data presented. Additional considerations and possibly further analyses may be needed to substantiate some interpretations. Certain analytical methods should be verified, and the principles and assumptions underlying the calculations or models should be described explicitly. In particular, where correlations between microbial DNA data and environmental parameters are discussed, the authors often infer causation from correlation.

Authors reply: in the revised version we made sure that we clearly separate between direct interpretations and suggestions/unverified hypothesis. We removed the modeling regarding the biofilm CN endmember extrapolation. We added information to the methods.

R1: These interpretations should be revisited and revised as necessary. Detailed comments are provided in the annotated PDF file attached.

Authors reply: we followed the reviewers annotations and edited the manuscript accordingly.

Reviewer 2

REVIEWER 2

R2: The manuscript by Argentino et al. presents a compilation of several data sets (seafloor imaging, sediment solid-phase and pore water analysis from push cores, as well as genetic information from one of them, including a white biofilm dominantly composed of anaerobic methanotrophs) from sediment habitats at/near cold seeps in the Atlantic Ocean off northern Norway, which are located near a cold-water coral (CWC) site.

The idea behind this is that there is an interrelationship between the CWCs and the cold seeps, but I see no evidence of this in the current version.

I also do not understand the addition of foraminiferal data (including d18O, d15N, and C/N), as these are not discussed at all.

Authors reply: In the current version of the manuscript we made clear that this study is not dealing with corals biogeochemistry but on the seep-impacted geochemical substrate on which they superimpose. We also explained why the samplings were conducted at some distance from the main mounds. We justified the use and inclusion of all the proxies.

R2: There is no data shown for the CWC site except for the image in Figure 1.

Authors reply: Corals form two main mounds shown in fig. 1 but minor coral build-ups are also present over a larger area hosting gas seeps (Ferre' et al., 2024 https://doi.org/10.1029/2024JC020949). Sediment samplings in correspondence of the mounds and adjacent sediment were hindered by the presence of a stiff layer of coral debris and carbonate pavements partially exposed. Moving away from the mounds, we obtained cores for biogeochemical investigations which are used here to determine the lateral variations of subsurface biogeochemical processes and methane dynamics, which explain microhabitat distribution seen at the seafloor. The corals settled inside the seepage area that we characterized using several cores and different biogeochemical proxies. We revised the introduction and abstract to clarify that we do not present biological studies on corals but on the biogeochemical setting over which they thrive.

R2: Overall, this paper attempts to summarize all the data in a single manuscript, which in most cases is not a good decision, as it causes the context to be lost and the reader to lose track of the big picture.

Authors reply: The strength of our interdisciplinary study is the fact of having different datasets depicting different biogeochemical aspects of the investigated environment. We agree that these datasets should be better integrated, and the interpretations combined into a cohesive narrative. We simplified the data presentation by leaving the most representative cores in the figures and moving the rest (which is still important to discuss in the main text) to a supplementary figure. The biofilm figure was also moved to the supplementary materials and presented mainly as observational data.

R2: The current version results in a highly fragmented manuscript that does not even provide clues as to the interrelationship between cold seeps and CWCs, except that they are in close vicinity. I would rather suggest removing unnecessary data and pursuing the "one article, one story" approach. Therefore, I recommend that the authors fundamentally revise the current submission and reduce the data sets to those that are relevant to the most important interpretations supported by the results.

Authors reply: please see previous comments.

R2: Title: By adding CWCs, the authors attempt to construct an argument that is not subsequently presented. Based on what I have read, such a combination is not necessary. Although CWCs are evident in this region (and there is a push core, but which was not used), the discussion of the data does not benefit from this fact. Removing this construction will increase the likelihood of arriving at some kind of context.

Authors reply: We restructured and rephrased the text.

R2: Paragraph starting at line 40: Is it really necessary to expand the focus of the manuscript to include ocean acidification and coral reefs/CWCs (see further comments below)?

Authors reply: We understand the reviewer's perspective. This paragraph was meant to put the study into the context of why we are interested in this place, which is the EMAN7 project. This paragraph was intended to provide context for why we are interested in this location, specifically in relation to the EMAN7 project. We revised the introduction to place less emphasis on the corals and focus more on seep biogeochemistry.

R2: Paragraph from line 50 (to line 58): Again, not much data on this topic is presented later on. Is this part necessary?

Authors reply: The link between CWCs and seep carbonates is fundamental to understanding their occurrence in this seepage area. Our manuscript is a follow-up of others (Ferre' et al., 2024, Serth et al., 2025) integrating the deeper subsurface to the seafloor.

R2: Figure 1 : The CWCs are outside the range of the push cores used later for interpretation. I do not understand the general correlation announced in the title.

Authors reply: now we explained more clearly that : "Sediment samplings near the mounds and adjacent sediment were hindered by the presence of a stiff layer of coral debris and/or hardgrounds partially exposed at the seafloor. Moving away from the mounds, we managed to obtain cores for biogeochemical investigations and decided to focus on these areas for samplings and orthomosaicking. " And made clear that we are not studying corals biogeochemistry but rather the biogeochemical context in which they settled.

R2: Lines 87 and 119: How can I obtain information about methane dynamics in a core if this is not provided? Simply by measuring sulfate and assuming that this is caused exclusively by AOM? No data on concentration profiles or d13C of methane in the core are provided.

Authors reply: We added gas data to the supplementary table. The gas isotopes were presented in a previous study Sauer et al., 2015 showing a thermogenic origin.

R2: Line 123: Where can I find information on this in the current version? Please provide an echo sounder image.

Authors reply: Multibeam echosounder maps are shown in Fig. 1 and uploaded to a database.

R2: Line 190: An accuracy of 1.2 mM for the DIC concentration seems rather high. This leads to a certain degree of uncertainty in the definition of the diagenetic zones in the cores.

Authors reply: This is not an accuracy. This is the highest value of repeatability (precision) obtained on 5 duplicates, following the terminology recommended by the International Association of Geoanalysts (IAG). This is a conservative value to report, as is commonly accepted in geoscientific metrology. DIC values in our dataset range from 2 to 23 mM (PusC04), the magnitude of our repeatability does not compromise biogeochemical zonation interpretations.

R2: Lines 193–197: Where can I find the data on methane concentration in the core? These are not displayed.

Authors reply: The data was added to the supplementary file.

R2: Line 199: The data from this is not used later in the discussion, but only displayed in the results section.

Authors reply: We acknowledge that the foraminiferal data (δ13C, δ18O) were not clearly introduced or fully integrated in the previous version of the manuscript. More explanation has been added to the revised version.

R2: Line 208: d13C of the three internal reference materials?

Authors reply: we added the values.

R2: Line 212: d15N of the three internal reference materials?

Authors reply: we added the values.

R2: Line 273: I find it difficult to discern this clear definition of curvatures and interpretations from the diagrams.

Authors reply: We agree that some profiles with fewer data cannot be accurately described in terms of shapes, so we reprhased and only discussed the more reliable profiles.

R2: Line 286: What can the reader deduce from these concentrations? Are these integrated values over the entire core length? Maxima in a specific horizon? Which one exactly? Depth profiles should be specified.

Authors reply: We added information and used the data in terms of spatial heterogeneity in gas concentrations (and fluxes, indirectly)

R2: Figure 3: The names of the push cores are difficult to read in the images. The compilation is good, but do we need them all? Reducing the number of figures would improve readability. Adding methane profiles seems reasonable. This would define the SMTZ horizon much better. If there is a dominant advective component, this is also good to know. Why are there question marks? What do they mean?

Authors reply: We improved the readability of this figure and added the information in the caption.

R2: Figure 4: These data with information on the deeper sections are not discussed at all in the manuscript. I think they can be deleted or moved to the supplementary information. As in Figure 3, the details are difficult to read if the size is retained.

Authors reply: We simplified the figure and discussed the data.

R2: Figure 5: The caption with information on sampling can be deleted. The figure may be good information that can be easily included in the SI.

Authors reply: we moved the figure as suggested.

R2: Figure 6: Differences in color coding and the associated microbes are difficult to discern. Please change.

Authors reply: We modified color palette of this figure.

R2: Lines 349 to 370: This is a general and lengthy introductory section that is unnecessary here. Why should this be specific to CWC-associated cold seep habitats?

Authors reply: We reduced the introductory part and modified the title from "Biogeochemistry of CWC-associated cold seep habitats " to "Cold seep biogeochemistry and microhabitats"

R2: Line 371: What other parameters did the other studies use to identify the SMTZ?

Authors reply: The approach used to determine the SMTZ can vary depending on whether you conduct modeling or just track the SMTZ stratigraphic location. For example, Lee et al., 2019 published on Biogeosciences used only sulfate profiles to determine SMTZs; Sen et al., 2018 on Biogeosciences used several parameters to conduct modeling. Fischer et al., 2012 used sulfide and sulfate. Sulfate is the key parameter when enough data points to plot.

R2: Lines 384 to 387: I do not see any connection to the data obtained in the current study.

Authors reply: We rephrased this as "Regarding the spatial distribution of microhabitats, the seep carbonate pavements in the CWC area act as a barrier to upward migrating

sulfide-rich fluids, controlling the distribution of mats around the edges of carbonate crusts or in apparently carbonate-free seafloor areas (Fig. 2)."

R2: Line 392: How high is the "methane charge" in mM? Must be defined or deleted.

Authors reply: We added information.

R2: Line 396: This statement contradicts any previously established connection to CWC. Delete this line and adjust the entire manuscript accordingly.

Authors reply: We modified the text.

R2: Lines 402 to 413: Another introductory section that is not needed here.

Authors reply: We believe that providing more context of isotopic thresholds helps to interpret the results.

R2: Figure 8 : Is this figure really helpful? Is there a correlation with depth/core position?

Authors reply: We kept the figure as we think it is useful discussion of the results.

R2: Line 461: To my knowledge, ANMEs predominantly use DIC and not OM.

Authors reply: we corrected

R2: Line 468: What "heavier" carbon sources do the authors mean? Please clarify this. Does this make sense and is it supported by previous literature?

Authors reply: We edited the sentence.

R2: Line 474: How can greater incorporation of carbon derived from methane, i.e., DIC, be tested? Isn't this simply a pure biofilm without a sediment matrix from which ANMEs cannot be easily extracted?

Authors reply: we removed the biofilm modeling part.

R2: Line 482: The only point at which the authors return to the foraminifera data.

Authors reply: we added more discussion of foram result to the manuscript.

R2: Line 483: The general statement is not justified given the location studied by the authors.

Authors reply: we removed the sentence.

R2: Line 490: Proximity does not imply interdependence.

Authors reply: we removed that part of sentence and rephrased it.

R2: Line 492: Is there any new data on carbonate crusts other than those from OBIA in this article?

Authors reply: The carbonate crusts geochemistry will be covered by a separate manuscript focused on geochemistry. The reason for this choice is that carbonate pavements d13C and d18O from this site were already reported by Cremiere et al., 2016 and already proved the methane-derived origin of these crusts. We added this information to the main text.

R2: Lines 502 to 505: This general conclusion is not supported by the current data and should be removed.

Authors reply: We edited the conclusions.