

REVIEWER1

The revised version of the manuscript by Argentino et al. goes one step further, but does not fully meet the requirements I addressed in my first round of comments.

Firstly, I still believe that the title is not representative of the content of the manuscript. Since the manuscript is very descriptive, the title should reflect this as well. I would suggest a title along the lines of “Cold-water habitats, sediment microbiomes, and biogeochemical zoning at a cold-water coral site off the coast of Vesterålen, northern Norway.”

Besides, I welcome the fact that the authors have added further information on the foraminifera data, but this is not comprehensively represented in the discussion and is missing from the abstract and conclusion. Please amend this.

Authors: We thank the reviewer for the constructive feedback. We appreciated the general and detailed comments, which we carefully evaluated and integrated into our text. We tried to reply whenever in disagreement, hoping that the final version still satisfies everyone's perspectives. Regarding the title, we appreciated the suggestion of the alternative title “Cold-water habitats, sediment microbiomes, and biogeochemical zoning at ...”. However, the peculiarity of these habitats is that they are seep-related and chemosynthetic. We believe that the term “seafloor chemosynthetic habitats” in the original title implicitly encompasses the distribution and biogeochemistry, while “AOM-influenced sediment microbiome” is sufficiently similar to the suggested phrasing. We would prefer keeping the original title. We now added information on foraminifera data in the text (abstract, discussion and conclusion).

Specific comments:

Line 19: The authors emphasize the biofilm, but omit information about foraminifera in the summary. Please adjust. Perhaps by deleting the details about biofilm in lines 19–21.

Authors: We added information of foraminifera in the abstract and preferred to keep the biofilm information, since it is quite a rare feature worth emphasizing.

Line 21: What makes this particular core so special? Why were genetic analyses only performed on this one?

Authors: We now clarified this in the methods (line 184), we selected this core based on the microhabitat and on the presence of the biofilm.

Line 23: Again, something about biofilm.

Line 24: “which leads to ...”?

Authors: we slightly rephrased to improve readability.

Line 27: The abbreviation CWCs has not yet been introduced.

Authors: we corrected accordingly.

Line 202: Please add information on how the methane concentration analyses were performed (GC, T-program, injection volume, calibration, etc.).

Authors: we added the suggested details about analytical session, calibrators and volumes.

Line 293: PusC 04 and 07 do not need to be repeated in Fig. S1 of the supplements.

Authors: we removed the two cores from Fig. S1 as suggested.

Line 339: Please explain why this particular core was selected for gene sequencing. Why only the upper 10 cm? How representative is this core compared to the others? Please specify.

Authors: In the methods we edited previous sentence to become: “Preliminary microbiological analyses were only performed on PusC-08 *after discovering* a biofilm at 10 cm, *suggesting strong biogeochemical and microbiological gradients.*” And in 3.6 DNA extractions and sequencing, we also edited a sentence to become: “Samples were collected every cm in the interval 0-10 cm, *to characterize any vertical gradient from the seafloor down to the biofilm-rich interval at 10 cm.*”

We only compared the pore water datasets and habitats, because the core-specific organic geochemistry and the microbiomes are expected to show heterogeneities, both

laterally and stratigraphically. Having measured only one core for microbiology, we do not draw any microbiological extrapolations for other cores.

Line 410: The authors should explain the foraminifera data in more detail instead of just saying that this data is used for paleo-seep reconstructions. In Figure 4B, it looks as if this signal has not been preserved in all species. Why?

Authors: *We moved the sentence “This geochemical proxy is widely used in paleo-seep reconstructions (Argentino et al., 2021, 2024; Boretto et al., 2026; Schneider et al., 2018; Yao et al., 2020).” To the methods and rephrased the foraminifera paragraph at 410 as : “Geochemical anomalies in $\delta^{13}\text{C}$ of foraminifera in PusC08 appear below ~18 cm, with remarkably negative isotopic values compared to the reference core. Those anomalies match the modern SMTZ as indicated by DIC data and the sharp drop in sulfate concentration right above (Fig. 4). Based on these observations, we ascribe the foraminiferal anomalies to precipitation of methane-derived carbonate as secondary overgrowth on the tests. Oxygen isotopes in PusC-08 show high $\delta^{18}\text{O}$ values (up to 4.6‰) and similarly high values seem recorded at the bottom of the reference core BlaC-01 (Fig. 4A). Previous measurements on pure carbonate crusts from this site (Crémière et al., 2016; Sauer et al., 2017) indicated seep carbonate precipitation in isotopic equilibrium with seawater, with no influence from gas hydrates, at least during Holocene. Additional foraminiferal investigations on longer sediment cores are needed to interpret the oxygen trends and reconstruct the history of seepage.”*

In Fig.4B both foraminifera show anomalies in the same interval but maybe L. lobatula is just more evident due to sampling resolutions and/or gaps. In fact MDAC precipitation would affect all tests present in the sedimentary layer hosting AOM.

Line 431: Yoshinaga et al., 2015.

Authors: *We double-checked this reference but “2014” seems correct for this paper <https://doi.org/10.1038/NGEO2069>*

Line 435: Better AOM consortium biomass, as methanotrophic organisms can alternatively be interpreted as aerobic.

Authors: We corrected accordingly.

Line 473: AOM has already been introduced.

Authors: We corrected accordingly.

Line 506: The sentence about DOC does not fit in the context and should be deleted.

Authors: We would prefer to keep the ending paragraph of the section titled “Geomorphological and biogeochemical factors influencing the distribution of CWCs at the Vesterålen site”: “At the CWC mounds, cold seeps influence the chemical and isotopic composition of dissolved organic carbon (Sert et al., 2025). Whether there is any seep-derived carbon uptake from corals remains unclear. Future results from ongoing coral transplantation experiments at this site will offer insights into this and other biochemical processes.” because despite DOC biogeochemistry not being investigated in our work, it has been covered by other studies within EMAN7 project. In our opinion, it indicates a knowledge gap worth mentioning.

Conclusion: A statement about foraminifera is missing here.

Authors: we added missing sentence on foraminifera:” Remarkably negative $\delta^{13}\text{C}$ values (as low as -18.5‰) in foraminiferal tests were found in association with the modern SMTZ in one core, suggesting precipitation of MDAC as secondary overgrowth.”

REVIEWER2

General Comments :

Dr. Argentino and his co-workers present a substantial body of data on seafloor chemosynthetic habitats and microbiomes, along with associated seafloor topography and geochemical measurements, and attempts to link these with cold-water corals. While the reported data are notable for their broad coverage, this comes at the expense of depth in certain areas. Nevertheless, the study reports a wealth of valuable data, which lays an important foundation for understanding cold-water corals associated with cold seeps and for conducting more in-depth research in the future. Meanwhile, after carefully reviewing the revised manuscript and the authors' response letter, I believe they have made substantial improvements and significant enhancements to the manuscript, particularly in terms of focus—the paper now more sharply concentrates on presenting the characteristics of cold seeps off the Vesterålen coast, northern Norway. Therefore, I consider the manuscript, in its current form, meet the requirements for publication in Biogeosciences.

Special Comments :

In terms of writing conventions and formatting, I have a few specific comments.

For example, Line 460: in “Fischer et al., 2012; Sen et al., 2018...”, “et al.” should be set in non-italic type to maintain consistency with other instances in the text. In Line 635: the parentheses in “(Ferré et al., 2024)” should be removed.

I suspect there may be more minor issues of this kind throughout the manuscript, and I would therefore encourage the authors to carefully review the entire text and make further refinements accordingly.

Authors: We thank the reviewer for spotting this inconsistency in reporting the references. We checked and fixed throughout the manuscript. We thank the reviewer for taking the time to review the manuscript and previous response letters.