Dear Editor and Reviewers,

Thank you for handling our manuscript and for the reviewers' thoughtful comments. We appreciate the time and attention invested in evaluating our work. We have revised the manuscript accordingly and believe the changes will improve its clarity and quality.

Below, we provide a detailed, point-by-point response to all reviewer comments. Each comment is reproduced in full, followed by our corresponding response. Because a revised manuscript has not yet been requested at this stage, page and line numbers for a revised version are not included here. Comments were sectioned into categories and responses are concluded at the end.

Sincerely,
Izabela De Mey-Śnieżyńska on behalf of all the co-authors

RESPONSE TO REVIEWER #1

We thank the reviewer for the thoughtful comments, which have helped us identify several areas where the manuscript can be clarified and strengthened. In the revised manuscript, we will provide a stronger geochemical context through the addition of porewater profiles and clearer site characterisation, and we will refine our interpretation of archaeal and GDGT signatures so that observations, limitations, and inferred environmental controls are more clearly separated. We will also improve the presentation of the data by revising figures, expanding downcore profiles for relevant indices and organic matter, simplifying statistical reporting, and correcting terminology, citations, and minor language issues. Together, these changes will improve the clarity, consistency, and interpretive balance of the manuscript.

Reviewer comment 1: How do methane seepage and submarine groundwater discharge (SGD) affect archaeal community composition? The research finds that pockmark sediments harbor substantially higher archaeal diversity and abundance (up to three times higher) compared to non-pockmark reference sediments. Pockmarks function as tightly coupled metabolic systems where diverse groups likely cooperate in methanogenesis and ammonia oxidation, whereas reference sites contain more independent, niche-partitioned communities. The profiles of sulfate and methane are necessary to determine whether the methanogenic zone is located at greater sediment depths, particularly given the relatively high abundance of Methanosarcina observed at pockmark sites (Fig. 6). In addition, ammonia oxidation is inferred based on the dominance of Nitrososphaeria; however, the pattern is not clearly evident in Fig. 6. This may be related to color resolution or figure presentation. The authors are encouraged to carefully review and possibly revise the figure to improve clarity.

Author response 1: We agree that a clearer geochemical context is essential for interpreting the composition of archaeal communities. In the final revised manuscript, we will add sulfate, methane, and chloride profiles, and we will use these profiles more explicitly to discuss the position of the sulfate-methane transition, the likely depth of methanogenic zones, and the influence of submarine groundwater discharge on archaeal community composition. We will also clarify the ammonia-oxidation interpretation based on the lipid evidence to soften and balance the claim regarding the

Nitrososphaeria contribution as contrasted with the contribution of methanogens; we will strive to clearly distinguish between the processes occurring in the water column and those occurring in the pockmark sediment, since the current presentation of the topic could lead to misleading conclusions. We will also temper statements that were previously stronger than the available porewater evidence allowed, and we will distinguish more carefully between direct geochemical observations and broader ecological interpretation, which is also based on literature.

Author changes in the manuscript:

-We will add porewater sulphate, methane, and chloride profiles for the studied stations.

-We will revise the Discussion linking Methanosarcina occurrence and archaeal structure to the geochemical profiles.

-We will add an explicit limitation statement where porewater constraints remain incomplete.

Reviewer comment 2: Fig. 1: It would be better to list the venting status in the figure for comparing the difference. For example, Ebullition, methangenesiis and refe for MET1, MET3, and MET4.

Author response 2: Thank you for this suggestion. We will revise Fig. 1 so that the venting/activity status of each site is shown directly in the figure and/or legend. This makes the comparison among MET1, MET3, MET4, and their reference sites much clearer.

Author changes in the manuscript:

-We will update Fig. 1 to include venting/activity status for each site.

Reviewer comment 3: Fig. 6: It makes me confused about the presence of ANME-2a/2b. In the main text, it is stated that the higher ANME-2a/2b in the main text, Fig. 7, and supplementary Fig. S7 and S8. Why ANME-2a/2b abundance is not shown in Fig. 6.

Author response 3: We agree that the original figure presentation made the ANME-2a/2b pattern difficult to follow. We will revise Fig. 6 to improve readability and to make the contribution of ANME-2a/2b evident at the taxonomic level used in the corresponding discussion, and to ensure the relative abundances of class-level and family-level taxonomy are clear, so that the contribution of ANME-2a/2b is evident in the main text. We will also clarify in the text how Fig. 6 relates to Fig. 7 and Figs. S7-S8.

Author changes in the manuscript:

-We will revise Fig. 6 to improve visibility and taxonomic clarity.

-We will add explanatory text linking Fig. 6 with Fig. 7 and the supplementary figures.

Reviewer comment 4: Line 50: what results are inferred for ammonia-oxidiser? Because high Crenarchaeol and thaumarchaeota genes? It should be stated clearly.

Author response 4: We will clarify this point in the revised manuscript. The inference of ammonia-oxidiser contribution will now be stated explicitly as being based on the dominance of crenarchaeol and the co-occurrence of Nitrososphaeria-related sequences, while also noting that these signals do not by themselves prove in situ activity. We will discuss this interpretation more cautiously and distinguish potential pelagic input from sedimentary production.

Author changes in the manuscript:

-We will revise the Introduction/Discussion to state explicitly the basis for the ammonia-oxidizer interpretation.

-We will soften the wording so that inferred function is not overstated relative to the available evidence.

Reviewer comment 5: Line 683: DSEG is an older phylogenetic name and now belong Thermoplasmatota. Please check the new phylogenetic lineages.

Author response 5: We thank the reviewer for pointing out that DSEG is indeed an older lineage name. In the final revised manuscript, we will retain the taxonomy originally produced by the reference database used in our bioinformatic workflow in order to preserve consistency of the analysis and classification, but we will clarify that DSEG is treated within Thermoplasmatota in newer phylogenetic classifications.

Author changes in the manuscript:

-We will add a clarification on the current placement of DSEG within newer phylogenetic schemes.

Reviewer comment 6: Does the lipid distribution in these gas systems reflect methane-driven processes or ammonia oxidation? Although methane concentrations are reported to be high, the iGDGT distributions appear to primarily reflect ammonia-oxidizing archaea (AOA) rather than methane-driven processes. Crenarchaeol—commonly associated with AOA such as Ca. Nitrosopumilus—is the dominant lipid in all cores. This may indicate a strong pelagic contribution from settling AOA-derived lipids from the water column. To better constrain potential terrestrial input and source contributions, the authors could consider providing BIT index values or related proxies.

Author response 6: We agree that the dominance of crenarchaeol requires a more cautious interpretation of the iGDGT data. In the final revised manuscript, we will discuss more explicitly the likely contribution from ammonia-oxidizing archaea and pelagic input, and we will avoid presenting the iGDGT signal as exclusively methane-driven. We will also include BIT index information to better constrain source contributions and to separate methane-related interpretations from possible terrestrial/organic-matter inputs.

Author changes in the manuscript:

-We will expand the Discussion of crenarchaeol dominance and likely AOA/pelagic contirbution.

-We will add BIT index value to better constrain source contributions.

Reviewer comment 7: Line 353: it would be better to show the TOC results among different sediment cores. It is not sufficient to state the approach limit in Result section.

Author response 7: We agree with the reviewer, and we are grateful for this suggestion. In the revised manuscript, we will now show the organic matter profiles across the investigated sediment cores more clearly, allowing direct comparison among sites. As direct elemental TOC measurements were not available for the entire core set, the reported values are identified explicitly as LOI-derived where applicable. This will now be clarified in both the Methods and the figure captions.

Author changes in the manuscript:

-We will add the expanded presentation of organic matter profiles across the sediment cores.

-We will clarify in the Methods and figure captions that these values are LOI-derived where applicable.

Reviewer comment 8: Fig. 2: why it a scatter plot for the TOC of MET3 rather than line curves like others? Please use a consistent plotting way.

Author response 8: We thank the reviewer for this suggestion. We will correct this plotting inconsistency and will use a consistent presentation style for all the profiles across comparable panels.

Author changes in the manuscript:

-We will revise Fig. 2 so the MET3 panel is plotted consistently with the other cores.

Reviewer comment 9: Line 384: It is not necessary to show so many parameters in statistics. Only show r and p values and regardring Peasron or Spearman can be introduced in the Method section. Please check through the whole text.

Author response 9: We agree and will simplify the statistical reporting throughout the manuscript. We will report only the key correlation metrics in the Results, while the choice of Pearson or Spearman correlation will be described in the Methods.

Author changes in the manuscript:

-We will simplify statistical reporting throughout the manuscript.

-We will specify the correlation approach in the Methods.

Reviewer comment 10: Line 723: Table 1 shows the GDGT-2/cren of 0.02 on average, but here it is maxim 0.4. It means some depth should be much lower than 0.02. please check the statement.

Author response 10: We thank the reviewer for noticing this inconsistency. We will check the values and the statement carefully, and we will revise the text so that downcore maxima are clearly distinguished from core-average values reported in Table 1.

Author changes in the manuscript:

-We will check and correct the wording and average values around GDGT-2/cren, distinguishing the average values from depth-specific maxima.

Reviewer comment 11: Line 48: what is the implication of OH-GDGT% values consistent with those of Baltic Sea surface? What is the ratio and compared with what sampling sites? It is not clear for the OH-GDGT%

Author response 11: We will clarify this comparison in the revised manuscript by specifying which Baltic Sea studies and ratios are being referred to and what the comparison is intended to show. Where the comparison remained insufficiently informative, we will simplify the wording to avoid overinterpretation.

Author changes in the manuscript:

-We will clarify the OH-GDGT comparison with Baltic Sea surface data and specify the basis of the comparison.

-We will simplify the statement where the comparison was not sufficiently informative.

Reviewer comment 12: Line 379: FA is not necessary to be abbreviated from fractional abundance since it is not widely used, and it is easily confused with fatty acids (FA). In addition, fraction abundance can be marked with the symbol %.

Author response 12: We agree with the reviewer. We will remove the abbreviation 'FA' and now will report fractional abundance directly as '%', which is clearer and avoids confusion with fatty acids.

Author changes in the manuscript:

-We will remove the abbreviation 'FA' throughout the text and figures, and we will replace it with '%'.

Reviewer comment 13: Fig. 3: please adjust the scale of y axis in the down panel since the upper limit is not arrived to 0.06 and it doesn't make to keep higher value of 0.08. It seems not necessary to plot minor compounds separately because it is still not clear.

Author response 13: We thank the reviewer for this suggestion. We will revise Fig. 3 to better match the observed value range, and we will simplify the figure layout so that the presentation of minor compounds is easier to follow.

Author changes in the manuscript:

-We will adjust the y-axis scaling and simplify the Fig. 3 panel layout.

Reviewer comment 14: Fig. 5. Same issue as Fig. 4. It is not necessary to separate OH-2 from other OH-GDGTs.

Author response 14: We agree and will simplify the OH-GDGT presentation accordingly so that the figure is easier to interpret.

Author changes in the manuscript:

-We will revise Fig. 5 to simplify the OH-GDGT presentation.

Reviewer comment 15: What is the impact of spatial and environmental variability (e.g., SGD) on these signatures? Spatial differences are pronounced; the highest lipid concentrations were found in the northern Gdańsk Deep (MET3, MET4), whereas sites in the MET1 area showed more influence from active advective flushing and freshwater infiltration. The authors can provide the profiles of GDGT-derived proxies, e.g., MI, RI-OH, RI-OH', GDGT-0/Cren. Presenting only average values in a table makes it difficult to assess vertical trends and site-specific variability.

Author response 15: We agree with the reviewer that reporting only core-average GDGT proxy values does not adequately capture downcore or site-specific variability. In the revised manuscript, we will therefore include depth profiles of the main GDGT-derived indices to allow direct comparison of vertical trends among cores. We will also revise the Results and Discussion to distinguish more clearly between the observed spatial patterns and our interpretation of the processes that may influence them, including possible effects of SGD and site-specific hydrographic conditions. Where direct geochemical constraints remain limited, we will state this explicitly and avoid over-interpretation.

Author changes in the manuscript:

-We will add downcore profiles of the key GDGT-derived indices.

-We will revise the Results and Discussion to separate observed spatial patterns from inferred controls, referring to newly added sulphate, methane, and chloride profiles.

Reviewer comment 16: Line 583: varrying should be varying.

Author response 16: We thank the reviewer for point this error out. It will be corrected.

Author changes in the manuscript:

-We will correct the typographical error.

Reviewer comment 17: Line 643: nutrients availability should be nutrient availability.

Author response 17: We thank the reviewer for point this error out. It will be corrected.

Author changes in the manuscript:

-We will correct the wording.

Reviewer comment 18: Line 648, Line 651, reference citations are wrong. Please check through accordingly.

Author response 18: We thank the reviewer for indicating this mistake. We will check the references cited in this section and will correct the erroneous/inactive citations.

Author changes in the manuscript:

-We will correct the references and cross-checks in this part of the manuscript.