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 have provided a stronger geochemical context by adding porewater profiles and clearer site characterisation, and we will refine our interpretation of archaeal and GDGT signatures to ensure that observations, limitations, and inferred environmental controls are clearly distinguished. We have also improved 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 improved 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 the earlier version of our manuscript required a stronger geochemical basis for interpreting the archaeal communities. In the revised manuscript, we have therefore added porewater sulphate, methane, and chloride profiles for all studied stations (new Figs. 2 and 3) and used them to constrain the relative positions of the sulphate-methane transition zone, methane enrichment, and porewater freshening. We have also revised the interpretation of Methanosarcinia and other methane-cycling groups in light of these porewater data. The ammonia-oxidiser interpretation was also rephrased: we now distinguish between the sedimentary 16S rRNA signal and the lipid signal, and we state more explicitly that the strong crenarchaeol signal most likely reflects a pelagic, AOA-related contribution.

Changes made in the manuscript:

- Added new porewater SO_4^{2-}, CH_4, and Cl^- profiles and corresponding Results text (Section 3.1; Figs. 2 and 3).
- Revised the Discussion to link methane-cycling archaeal groups, including Methanosarcinia, to the geochemical profiles and site-specific activity status.
- Softened and clarified the AOA interpretation, distinguishing lipid-based inference from direct evidence of sedimentary activity.

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: The map and caption have been revised to make the activity/venting status of the pockmark sites clear and explained. The revised Fig. 1 now distinguishes the pockmark stations by activity status, and the caption explains that this classification is based on MBES/SBES observations from the present and previous campaigns, together with the supporting geochemical information presented in the revised manuscript.

- Changes made in the manuscript:
- Revised Fig. 1 and its caption to include pockmark category and activity status.
- Clarified in Section 2.2 and Supplementary Table S1, how pockmark and reference stations are defined and classified.

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 earlier figure did not clearly show the ANME-2a/2b pattern. In the revised manuscript, the archaeal community figure has been reorganised and expanded. In addition to the class-level profiles, we now include a family-level heatmap that resolves key methane-cycling groups, including ANME-2a/2b. The text now explicitly explains that ANME-2a/2b is not visible at the class level but is resolved at the family/genus level, and it identifies the horizons where this group is enriched, particularly in deeper pockmark intervals such as P/MET1-MP/7 and P/MET4/10.

Changes made in the manuscript:

- Revised the archaeal community figure (now Fig. 10) to include both class-level and family-level information.
- Added explanatory text linking the main figure with the supplementary family-, genus-, methanogen-, and methanotroph-level profiles.
- Clarified the occurrence of ANME-2a/2b in the Results section.

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 have clarified this point. In the manuscript, we state that the ammonia-oxidiser interpretation is based primarily on the dominance of crenarchaeol in the iGDGT pool, the strong covariance between OH-GDGTs and crenarchaeol, and the presence of Nitrososphaeria-related sequences. At the same time, we now emphasise that these observations do not, on their own, prove active ammonia oxidation within the sediment. The interpretation has therefore been rebalanced towards a likely AOA-related, pelagic source of crenarchaeol.

Changes made in the manuscript:

- Revised the Abstract, Results, and Discussion to state the basis of the AOA interpretation more explicitly.
- Added cautionary wording that separates lipid-source inference from direct evidence of *in situ* activity.

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 have retained the taxonomic names returned by the SILVA-based bioinformatic workflow to maintain consistency with the analysed dataset, but we have added a clarification that the Deep Sea Euryarchaeotic Group (DSEG) is an older database name and is classified within Thermoplasmatota in newer phylogenetic classifications.

Changes made in the manuscript:

- Added a taxonomy clarification in the Methods.
- Clarified the placement of DSEG within Thermoplasmatota when it is discussed in the Results/Discussion.

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 lipid distribution should not be interpreted as primarily methane-driven. In the manuscript, we now place greater emphasis on the dominance of crenarchaeol, the consistently low MI values, and the low GDGT-0/crenarchaeol and GDGT-2/crenarchaeol ratios. These patterns indicate that the bulk iGDGT pool is dominated by AOA-related and likely partly pelagic input rather than by a strong AOM or methanogenic lipid imprint. We have also added the BIT index to better constrain source contributions and to assess whether terrestrial or brGDGT-related input could affect the interpretation.

Changes made in the manuscript:

- Added the BIT index to the Methods, Table 1, Results, and Discussion.
- Expanded the Discussion of crenarchaeol dominance, AOA-related input, and the limited expression of methane-related GDGT signatures.
- Revised statements that previously could imply an exclusively methane-driven iGDGT signal.

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: Agreed. Because direct elemental TOC measurements were not available for all cores, we have revised this part of the manuscript to avoid presenting LOI-derived organic matter as equivalent to measured TOC. The manuscript now includes a validation comparison of LOI with directly measured TOC for two representative cores, followed by downcore LOI profiles for all cores. We explicitly describe LOI as a qualitative, screening-level proxy for variability in bulk organic matter, not as a direct TOC measurement.

Changes made in the manuscript:

- Added LOI-TOC comparison for representative cores (new Fig. 4).
- Added downcore LOI profiles for all studied cores (new Fig. 5).
- Revised the Methods and figure captions to clarify the limits of LOI-derived organic-matter estimates.

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: The original presentation was replaced by a more consistent figure structure. Now, the LOI-TOC validation plot is separated from the full-core LOI profiles, and comparable profiles are plotted in a consistent style across sites.

Changes made in the manuscript:

- Revised the organic-matter figures for consistent profile plotting.
- Removed/avoided the earlier inconsistent scatter-only presentation.

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 have simplified the statistical reporting throughout the manuscript. The Methods now specify the correlation approach, including the use of Pearson correlation to assess relationships among GDGT concentrations. In the Results, we report the essential statistics, mainly r and p values, and avoid excessive test-output detail.

Changes made in the manuscript:

- Moved methodological details of the correlation approach to the Methods.
- Simplified statistical reporting in the Results and Discussion.

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 have rechecked the GDGT-2/crenarchaeol values and corrected the wording to ensure that core-average values and depth-specific ranges are no longer conflated. Now, we report consistently low GDGT-2/crenarchaeol values and state that GDGT-2 is not the dominant compound in any sample.

Changes made in the manuscript:

- Corrected the GDGT-2/crenarchaeol values in the Abstract, Results, and Table 1.
- Clarified that GDGT-2/crenarchaeol remains low.

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 have clarified this comparison. The OH-GDGT% and RI-OH/RI-OH' values are compared with previously reported ranges for surface sediments from the Baltic Sea and the Baltic/Skagerrak. We have also simplified the interpretation so that the comparison is used only to support a broad Thaumarchaeal/AOA-related source of OH-GDGTs, not as a standalone environmental diagnosis.

Changes made in the manuscript:

- Clarified the basis and meaning of the OH-GDGT% comparison.
- Softened the interpretation to avoid overstatement.

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: Agreed. We have removed the abbreviation 'FA' to avoid confusion with fatty acids. Fractional abundance is now written explicitly or expressed using the percent symbol where appropriate.

Changes made in the manuscript:

- Removed the abbreviation 'FA' from the manuscript and figures.
- Revised labels to use fractional abundance or %.

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: The iGDGT figure has been simplified, and the axis scaling has been adjusted to better match the observed ranges. Minor compounds are now presented in a way that avoids overemphasising very small values, while the text describes the main pattern: dominance of crenarchaeol and GDGT-0, with GDGT-1 to -3 occurring only as minor components.

Changes made in the manuscript:

- Revised the iGDGT figure layout and scaling.
- Moved detailed individual compound patterns to supplementary material where appropriate.

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: Agreed. The OH-GDGT presentation has been simplified so that OH-GDGT-2 is no longer unnecessarily separated from the other OH-GDGT compounds. The revised figure and text emphasise the main distributional pattern, namely the dominance of OH-GDGT-0 and broadly similar OH-GDGT fractional abundances in pockmark and reference cores.

Changes made in the manuscript:

- Revised the OH-GDGT figure layout.
- Simplified the Results text on OH-GDGT fractional abundance.

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: The GDGT profiles are now combined with the new sulphate, methane, and chloride profiles, and they distinguish between active, weakly active, and inactive pockmark settings. We have also included the standard deviation alongside the mean values of the GDGT-based indices in Table 1. However, we have decided not to include the profiles of GDGT-derived proxies due to the overall small variations in values. We have expanded the text discussing the low MI, GDGT-0/crenarchaeol, GDGT-2/crenarchaeol, BIT, OH-GDGT%, RI-OH, and RI-OH', and added value ranges. This revised text shows that the highest lipid concentrations occur mainly at the Gdańsk Deep sites, particularly MET3 and MET4, whereas the strongest porewater freshening occurs at MET1-MP and MET4. The lipid pool therefore responds in a site-specific manner.

Changes made in the manuscript:

- Integrated the new porewater profiles into the interpretation of spatial GDGT variability.
- Added mean ± SD values for GDGT-based indices in Table 1 and ranges of the values in Results.
- Expanded the Results and Discussion to separate observed spatial patterns from inferred controls.

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

Author response 16: Corrected.

Changes made in the manuscript:

- Corrected 'varrying' to 'varying'.

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

Author response 17: Corrected.

Changes made in the manuscript:

- Corrected 'nutrients availability' to 'nutrient availability'.

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

Author response 18: We have reviewed and corrected the erroneous reference citations and cross-references in the manuscript.

Changes made in the manuscript:

- Corrected the cited references and cross-references.

Response to Reviewer #2

We thank Reviewer #2 for the careful and constructive evaluation of our manuscript. We agree that several aspects of the manuscript require clearer geochemical support and more precise terminology, particularly regarding pockmark activity, porewater freshening, and the interpretation of GDGT distributions. In response, we have revised the manuscript to strengthen site characterisation, incorporate porewater profiles of chloride, sulphate, and methane, and distinguish more clearly between direct observations and interpretation.

Reviewer comment 1: As noted by the authors in the introduction, pockmarks can be both active and non-active. Apart from Supplementary Table 1 where a qualitative description is given for the sites, quantitative constraints to what extent the pockmark sites display porewater freshening, SGD, methane ebullition or dissolved gas seepage are lacking. Furthermore, as it seems that some of these sites have been investigated previously, it would benefit the study if some data is presented to confirm the duration of methane seepage or SGD over the years. Such porewater chemistry data is rather essential to support statements in the manuscript such as "which may reflect the dynamic geochemical conditions characteristic of these gas systems" (line 698), "likely result from SGD-marine water mixing within a shallow redox-transition layer" (line 822), "Results suggest that submarine groundwater discharge and pockmark activity drive geochemical conditions and microbial distribution." (line 876) and "site-specific hydrographic conditions associated with SGD" (line 885).

Author response 1: We agree that the earlier version of the manuscript did not provide sufficient quantitative site characterisation. The revised manuscript now includes a substantially expanded geochemical context and some geophysical context. We have added sulphate, methane, and chloride porewater profiles for all studied sites, revised the site descriptions, and explicitly classified the pockmarks as active, weakly active, or inactive, based on published studies, MBES/SBES observations, porewater data, and supplementary echograms (P/MET3 and P/MET4). We also make clear where the evidence is strong, especially for MET1, and where published constraints remain more limited, especially for MET3 and MET4.

Changes made in the manuscript:

- Expanded the Study area and Sampling sections with site-specific geophysical and geochemical information.
- Added porewater SO_4, CH_4, and chloride profiles (Figs. 2 and 3).
- Revised Fig. 1, Supplementary Table S1, and supporting text to include activity status and porewater freshening information.
- Added/used supplementary echograms for MET3 and MET4 (Fig. S1) and stated limitations where evidence remains incomplete.

Reviewer comment 2: To what extent do the authors think that the considerable variability and lack of compositional differences (as described in Figure 3) can be explained by accounting for present-day porewater chemistry. For example, reference sites might experience methane production and seepage without pockmark formation, and pockmark sites might have been inactive for a long time. Do significant differences arise when active vs non-active seepage sites are compared?

Author response 2: We agree that present-day porewater chemistry explains part, but not all, of the compositional variability. Methane occurrence is now treated as a broader feature of the Gdańsk Basin sediments, and methane-bearing sediments are no longer equated exclusively with pockmarks. At the same time, the new profiles show that pockmark cores generally have higher methane concentrations, stronger sulphate depletion, and lower chloride concentrations than reference cores, although the strength of these contrasts is site-specific. The text now distinguishes persistent/active pockmarks from weakly active and currently inactive structures, and explicitly recognises that reference sediments may contain methane without forming pockmarks or showing observed ebullition.

Changes made in the manuscript:

- Revised the interpretation of site-to-site variability using the new porewater profiles.
- Clarified that methane-bearing reference sediments occur in the study area.
- Distinguished active, weakly active, inactive, and reference settings throughout the manuscript.

Reviewer comment 3: Line 60: remove "worldwide".

Author response 3: Corrected. The word 'worldwide' was removed.

Changes made in the manuscript:

- Removed 'worldwide'.

Reviewer comment 4: Line 60: Lithosphere could imply that pockmarks can also be formed in non-sediment parts of the lithosphere. Would be more specific here, e.g., use "sediments".

Author response 4: We thank the reviewer for pointing this out. The wording has been corrected to refer specifically to sediments rather than to the broader lithosphere.

Changes made in the manuscript:

- Replaced the broader/imprecise wording with 'sediments'.

Reviewer comment 5: Line 62: "fluid emanation"; perhaps clarify that pockmarks can be formed both due to fluid seepage as well gas seepage?

Author response 5: We agree that 'fluid emanation' could be ambiguous in this context. The revised wording now explicitly states that pockmarks can form through gas and/or water seepage from sediments into the hydrosphere.

Changes made in the manuscript:

- Revised the sentence to specify gas and/or water seepage.

Reviewer comment 6: Line 68: Suggest to replace "Fluid types" with methane sources or something equivalent.

Author response 6: We have revised the wording to make the intended meaning clearer. The relevant passage now refers more explicitly to seepage types and methane sources, rather than using the ambiguous phrase 'fluid types'.

Changes made in the manuscript:

- Replaced the imprecise term 'fluid types' with more specific wording where necessary.

Reviewer comment 7: Line 90: "(from initiation through expansion to stabilisation)" it is It is unclear to me what the authors intend to say here.

Author response 7: We agree that this phrase was unclear. The sentence has been rewritten so that the introduction no longer contains this ambiguous parenthetical phrase.

Changes made in the manuscript:

- Removed the phrase 'from initiation through expansion to stabilisation'. Rewritten the Introduction for clarity.

Reviewer comment 8: Line 94: "Under such conditions" the causal relation between these two sentences is unclear to me. Can the authors elaborate on the link between sediment trapping, porewater freshening and shallow methanogenesis?

Author response 8: We have revised these sentences to clarify the causal link. The revised Introduction now explains more cautiously that sediment and organic-matter trapping, together with freshened porewater discharge and altered sulphate availability, can shift redox zonation and create conditions favourable to shallow methanogenesis. The wording is now framed as a possible mechanism.

Changes made in the manuscript:

- Revised the Introduction to clarify the relationships among sediment trapping, porewater freshening, sulphate depletion, redox zonation, and shallow methanogenesis.

Reviewer comment 9: Line 188: I suggest to replace "methane bubbling" with methane ebullition.

Author response 9: Corrected. We have replaced 'methane bubbling' with 'methane ebullition' wherever the text refers to upward gas emission from the sediment.

Changes made in the manuscript:

- Replaced 'methane bubbling' with 'methane ebullition' where appropriate.

Reviewer comment 10: Lines 364-366: report the STDV along with the median and means.

Author response 10: We have added standard deviations where summary statistics are reported. In the revised Results, means are now reported alongside SDs for sulphate, methane, chloride, lipid concentrations, fractional abundances, and selected archaeal relative abundances, where applicable. Medians are retained where they help describe skewed distributions.

Changes made in the manuscript:

- Added SD values to the relevant Results sections.
- Revised summary-statistic reporting for porewater, lipid, and community data.

Reviewer comment 11: Figure 2: i) To facilitate easier comparison of the different cores please use the same ranges and ticks for the x-axes wherever possible without loosing the downcore trends. ii) For the MET3 TOC figure, why is there no line for the pockmark datapoints? iii) Please elaborate on the error ranges or uncertainties for these plots. iv) In the figure caption "TOC concentrations are consistently elevated in pockmark sediments" > this does not seem to be true for MET3. v) Put the MET1-BH iGDGT plot y-axis to 95 cm to keep it consistent with other subplots.

Author response 11: We thank the reviewer for these detailed comments on the figures. We have substantially revised the figure set. The organic matter presentation has been reorganised into a LOI-TOC validation figure and a separate LOI downcore profile figure. The GDGT downcore profile figures have been revised for comparability, and the text and captions no longer state that TOC or organic matter is consistently elevated across all pockmark cores. We have also added an explicit statement in the Methods that individual horizons were analysed only once, so sample-specific analytical uncertainty and error bars cannot be estimated; instead, method uncertainty is now described.

Changes made in the manuscript:

- Revised the relevant figures for more consistent plotting and clearer comparison among cores.
- Removed generalised caption wording concerning consistently elevated TOC/organic matter in pockmarks.
- Added an explicit uncertainty statement in the Methods and relevant captions.
- Aligned profile depth ranges where applicable.

Reviewer comment 12: Table 1: If I understand correctly, these GDGT-based indices are downcore means per core. If that is the case, please also report the STDEV or something similar to demonstrate the variability.

Author response 12: Agreed. Table 1 has been revised so that the GDGT-based indices are reported as mean ± sample standard deviation, enabling readers to assess downcore variability within each core.

Changes made in the manuscript:

- Revised Table 1 to include mean ± SD for GDGT-based indices.

Reviewer comment 13: Line 473, 528 and throughout manuscript: "sediment types" the phrasing of sediment types may be misleading, I suggest to be specific and say "between pockmark and non-pockmark cores/sites" or anything equivalent.

Author response 13: We agree that 'sediment types' was misleading. We have revised the terminology throughout the manuscript and now refer more specifically to pockmark cores/sites and to reference or non-pockmark cores/sites.

Changes made in the manuscript:

- Replaced 'sediment types' with pockmark/reference or pockmark/non-pockmark cores/sites wording throughout the manuscript.

Reviewer comment 14: Line 571: "methane-bearing sediments (pockmarks)" > is it confirmed that all pockmark sites have active methane seepage? Similar comment for line 613 "methane-rich sediments".

Author response 14: In the revised manuscript, we no longer equate all pockmarks with active methane seepage. We now distinguish between active, weakly active, and currently inactive pockmarks, as well as reference sites. We also clarify that methane-bearing sediments occur more broadly across the Gdańsk Basin, including in reference areas, whereas observed ebullition and the strongest geochemical contrasts are associated with pockmark (morphological) structures.

Changes made in the manuscript:

- Revised wording such as 'methane-bearing sediments (pockmarks)' and 'methane-rich sediments' when referring to pockmark cores.
- Clarified activity status and methane occurrence in Fig. 1, Sect. 2.2 and the Discussion.

Reviewer comment 15: Line 637: "in the absence of fluid seepage", do the authors refer here to methane seepage or SGD?

Author response 15: We have revised the ambiguous phrasing and now specify the relevant process in each context. Where the text refers to gas release, we use 'methane seepage' or 'methane ebullition'; where it refers to the influence of freshened water, we use 'freshened porewater discharge' or 'porewater freshening'. We avoid the term 'fluid seepage' unless both gas and water seepage are intended (this has also been explicitly highlighted).

Changes made in the manuscript:

- Replaced ambiguous uses of 'fluid seepage' with process-specific wording.

Reviewer comment 16: Lines 653, 666, 679, 765: „add reference" for statements: "which are characterised by strong SGD", "pockmark MET4 is characterized by low-intensity SGD", "the MET3 and MET4 study sites were characterised by stable, non-ebullitive methane emissions", "the inactive pockmark P/MET3", and "low-SGD pockmark MET4"

Author response 16: We agree that these statements required stronger support and more careful wording. In the revised manuscript, we have replaced broad claims such as 'strong SGD' with more precise terms such as 'porewater freshening' where supported by chloride and sulphate depletion. MET4 is now described based on the chloride profile and supplementary echogram evidence, rather than as a low-SGD site without qualification. The revised text also distinguishes the lack of gaseous activity at MET3 from the single/localised bubbles observed above MET4, and it makes explicit where published support is limited.

Changes made in the manuscript:

- Revised site-activity and SGD-related wording throughout the manuscript.
- Added/retained appropriate references for MET1 freshening and activity.
- Supported MET3/MET4 descriptions with chloride profiles and supplementary echograms.
- Added cautionary wording where evidence remains limited.

Reviewer comment 17: Line 834: "advective flushing sediment layer" it is unclear to me what the authors mean here.

Author response 17: We agree that the phrase was imprecise. The wording has been revised so that the manuscript no longer uses 'advective flushing sediment layer' without explanation. Where relevant, we now refer more specifically to advective methane flow, porewater transport, or freshened porewater discharge, depending on the process under discussion. However, due to a lack of data, we do not make any definitive statements about the studied pockmarks without references.

Changes made in the manuscript:

- Rephrased the unclear phrase and replaced it with process-specific wording where applicable, in line with previous research (added references).

Reviewer comment 18: In Supplementary Table 1 it seems that a decrease in chloride is used as an indicator for SGD. Are these profiles diffusive or advective? And for the sites with no porewater freshening, does saline SGD occur? Even in the absence of freshwater, advection of saline porewater could still have an important control on the depth of different redox zones, and thus archaeal composition.

Author response 18: We appreciate this point. Chloride depletion is now presented as evidence of freshened porewater influence, rather than as a universal diagnostic of SGD. We also acknowledge that the absence of chloride depletion does not rule out saline porewater advection, as saline SGD or recirculated porewater may not produce a clear chloride signal. Since the available data do not allow a robust quantitative separation of diffusive and advective transport regimes, we avoid classifying the profiles and instead describe the observed chloride patterns and their limitations.

Changes made in the manuscript:

- Clarified the interpretation of chloride depletion.
- Broader SGD description is implemented in the Introduction.
- Revised terminology from 'SGD' to 'freshened porewater discharge/porewater freshening' where chloride evidence underpins the interpretation.
- Added cautionary wording regarding transport-regime interpretation, applied only when supported by references.

Reviewer comment 19: "Lines 648, 651, 674 > reference sources not found" and "Line 705: Table number is missing."

Author response 19: Corrected. We have checked the missing or incorrect reference sources and the table citation, and corrected the technical issues in the revised manuscript.

Changes made in the manuscript:

- Corrected missing references and the missing table number/cross-reference.

Reviewer comment 20: There seems to be an inconsistency on a few occasions where SGD is used interchangeably to describe fSGD, while in lines 82 to 84 it is acknowledged that SGD can be both fresh or saline (which I fully agree with, although it is naturally a matter of definition). An example is: "SGD alters porewater chemistry through decreased salinity and chloride depletion" (line 98), which would imply that SGD is always fSGD.

Author response 20: We agree that the original manuscript used SGD too broadly in places. The revised Introduction now defines SGD as including discharge of fresh, brackish, or saline groundwater or porewater, while later interpretations use the narrower terms 'freshened porewater discharge' or 'porewater freshening' when the evidence is based on chloride or salinity depletion. This avoids implying that all SGD necessarily decreases salinity or chloride concentrations.

Changes made in the manuscript:

- Revised SGD terminology throughout the manuscript.
- Distinguished broad SGD from freshened porewater discharge and from chloride-based freshening.

Reviewer comment 21: "Freshwater infiltration" (line 32); I suggest to replace this with freshened porewater discharge, freshwater presence or an equivalent, as freshwater infiltration could be interpreted as freshwater infiltrating from the water column down into the core.

Author response 21: We agree and have replaced this wording. The revised manuscript avoids 'freshwater infiltration' where it could imply downward infiltration from the water column, and instead uses terms such as 'freshened porewater discharge' or 'freshened porewater influence'.

Changes made in the manuscript:

- Replaced 'freshwater infiltration' with more precise terminology, i.e., 'freshened porewater discharge' or 'freshened porewater influence'.

Reviewer comment 22: Line 83: Replace "freshwater groundwater" with freshened groundwater.

Author response 22: Corrected. The phrase has been changed to 'freshened groundwater'.

Changes made in the manuscript:

- Replaced 'freshwater groundwater' with 'freshened groundwater'.

Reviewer comment 23: In the introduction both SGD and methane seepage are discussed and the processes are described. When moving into the GDGT section of the introduction (lines 107-163), only methane and nitrate related GDGTs are described and SGD is disregarded. Is there anything known from literature regarding SGD-derived or influenced GDGTs? Or is there any hypothesis how SGD may impact GDGT presence? If little is known, it would be good to specify that, which only strengthens the motivation for the study.

Author response 23: We thank the reviewer for this helpful suggestion. We have expanded the GDGT-related section of the Introduction to include the potential relevance of SGD and groundwater-seawater mixing zones. The revised text now notes that direct studies of GDGTs in SGD-affected marine sediments remain scarce, but that groundwater and subterranean-estuary studies indicate that iGDGTs can be produced by indigenous subsurface/groundwater archaeal communities and may be influenced by salinity and redox gradients. We use this to motivate the study rather than to claim a well-established SGD-specific GDGT signal.

Changes made in the manuscript:

- Added a paragraph on SGD/subterranean-estuary context and GDGTs in groundwater-influenced systems (Introduction).
- Clarified that direct evidence for SGD-specific GDGT signatures in marine pockmark sediments remains scarce.
- Linked this literature gap to the motivation for the present study.

Reviewer comment 24: Line 373: "When normalised to TOC"; which TOC? The LOI-derived TOC?

Author response 24: We agree that this was unclear. The revised manuscript no longer treats LOI-derived organic matter as a direct TOC measurement. We now distinguish direct TOC measurements, available for selected representative cores, from LOI-derived organic matter estimates, which are used as a screening-level proxy. The wording regarding normalisation/comparison was revised accordingly.

Changes made in the manuscript:

- Clarified in the Methods and Results that LOI is a qualitative proxy, not direct TOC.
- Revised the wording regarding TOC/LOI comparison and GDGT interpretation.

Reviewer comment 25: Line 373: To what extent (quantitatively) is elevated summed iGDGT explained by elevated TOC contents in general?

Author response 25: We have addressed this by comparing GDGT profiles with LOI-derived organic matter trends. The elevated summed iGDGT and OH-GDGT concentrations are not solely attributable to higher bulk organic matter content. For example, MET1-BH shows relatively high LOI and clear methane enrichment but not the strongest GDGT accumulation, whereas MET3 and MET4 show high GDGT concentrations despite different organic matter and porewater patterns. We therefore interpret the GDGT signal as reflecting a combination of archaeal source, pelagic input, porewater chemistry, the ebullitive nature of methane seepage, and preservation, rather than bulk organic matter alone.

Changes made in the manuscript:

- Added a comparison of GDGT distributions with LOI-derived organic matter trends.
- Revised the Discussion to state that GDGT enrichment is not tightly coupled to bulk organic matter.

Reviewer comment 26: Line 680: “which could have contributed to the better preservation of iGDGTs” can the authors elaborate on through which mechanism?

Author response 26: We have revised and softened the preservation statement. We no longer rely on an unsupported general claim of 'better preservation' as the main explanation. Where preservation is discussed, it is now framed cautiously as one possible influence, linked to anoxic, fine-grained, organic-rich sediments and reduced degradation, while the main interpretation emphasises source contributions and site-specific geochemical conditions.

Changes made in the manuscript:

- Revised the preservation-related wording in the Discussion.