Dear Prof. Liss,

I will continue to handle correspondence until the birth of Dr. Amavi Silva's child and until she is allowed to return to work after her maternity leave. Below you will find information that complements our original responses to the reviewers' comments. For each response (marked in blue), I have briefly added where and in what form of changes have been made. These are all marked in red color. I hope this helps you navigate the changes efficiently.

Thank you for your time and efforts.
With kind regards,
Markus

Reviewer #1

General assessment: I have reviewed this manuscript as a non-expert in meta-analysis; therefore, I am not in a position to fully assess the methodological rigor or potential limitations associated with the meta-analytical approach and related statistical procedures. My comments should therefore be considered primarily from the standpoint of subject-matter expertise rather than methodological specialization.

This study presents a meta-analysis-based approach to previously published sea surface microlayer (SML) research, aiming to quantitatively assess the distributional properties of selected organic carbon and nitrogen compounds. Overall, this manuscript presents a valuable effort to integrate existing SML data, with potential relevance for future modeling studies aimed at elucidating the role of the SML in biogeochemical cycles and climate-related processes. Overall, the manuscript aligns well with the interdisciplinary aims of Biogeosciences, providing findings and perspectives that would likely be of interest to the diverse journal's readership.

The results indicate that nitrogen-rich compounds and particulate organic matter are generally more enriched in the SML compared to carbon-rich compounds and dissolved organic matter. The authors further demonstrate that enrichment levels for individual compounds vary under different internal and external conditions. By examining enrichment factors (EFs) for a range of measurable compounds, the study provides updated estimates of their typical values and ranges. The discussion also offers a critical evaluation of EFs, underscoring their utility in describing the partitioning of organic matter within the SML while acknowledging their limitations in reflecting trophic conditions and absolute concentrations of either SML or ULW ("actual" enrichment).

Comment 1: However, despite the application of a novel data-processing approach, the study does not appear to yield substantially new findings beyond those already established in previous research. Overall, the results largely confirm existing knowledge regarding SML properties and organic matter enrichment patterns, without providing significant new insights. Therefore, I suggest that the authors revise the concluding statements throughout the manuscript, including Abstract, to more clearly reflect the study's contributions and its relationship to existing knowledge. This would help to strengthen the manuscript's impact and clarify its significance for the field.

We thank the reviewer for this thoughtful comment. Although we respectfully disagree that our results only confirm existing knowledge, we agree that our initial presentation did not sufficiently highlight the significance of our study in advancing the existing knowledge on the SML. Our meta-analytical findings on the general patterns of organic matter enrichment in the SML appear to be consistent with previous observations, which we believe should be a natural outcome, and would be concerning if they were not. However, our study moves beyond confirming previous findings and yields several novel insights that warrant particular attention:

- **First meta-analysis of SML research:** Our study integrates data across multiple investigations, addressing inconsistencies and method-specific biases in previous work, and advances a more unified understanding of SML dynamics. Engel *et al.* (2017) highlighted the lack of agreement on which biogenic compounds become selectively enriched in the SML, and under what conditions. By synthesizing datasets through such a meta-analytical approach, we identify cross-study patterns that depend on how compounds are grouped and interpreted. The inferred selective enrichment of nitrogen-rich, particulate compounds in the SML relative to carbon-rich, dissolved compounds emerges from aggregating data into "nitrogen-enriched" and "carbon-enriched" categories. While this framework is informative, it represents one analytical perspective and may bias interpretation of the underlying chemical diversity. This issue we now better address in the discussion section.

- **A reliable reference for future studies:** We present the full range of typical EF values for various compounds, offering a clear and comprehensive reference that allows future studies to determine whether new measurements fall within typical ranges or represent extreme cases. This underscores the effectiveness of meta-analysis in resolving long-standing ambiguities, and provide a solid foundation for future studies such as air-sea gas exchange modeling. Also, we specify the highest concentrations of a broader group of substances found in the SML from a large number of independent measurements. If such maximum concentrations are identified, it is advisable to investigate more closely whether these high concentrations can be attributed to specific conditions or compounds.

- **Surprising trends of enrichment:** While most of our results confirmed existing knowledge on SML enrichment patterns, also surprising trends were revealed, such as overall moderate/poor TEP enrichment or the differences in the enrichment of POC that depend on wind conditions. This meta-analysis thus provides a common ground for directing future research questions, such as further exploring conditional probabilities of EF, which are beyond the scope of the data provision and analysis presented here. Exploring potential enrichment patterns further, for instance, with regards to sampling season or wind regimes, could be of great value for modeling a global and dynamic SML.

- **Critical evaluation of 'EF' metric:** Our work carefully examines the strengths and limitations of EF as a metric. Although a few studies have pointed out its weaknesses, we are the first to offer alternatives to the ratio-based EF approach, introducing tools that allow researchers to explore the problem from multiple perspectives. For instance, we emphasize the value of considering the absolute differences between concentrations in the SML and ULW, rather than relying on the EF exclusively as the primary measure.

- **Proper treatment of data scale:** Existing SML studies often use linear scales for concentration data spanning several orders of magnitude, which can introduce biases, especially when paired with inappropriate central tendency estimates. We are the first to highlight this issue and propose improved scale choices and central tendency measures, helping reduce statistical artifacts in future SML research.

Together, these contributions provide practical guidance and novel insights for future SML research. We have now revised the Abstract, Methodology and Discussion of the manuscript, to ensure that the points outlined above are communicated more clearly.

In response to this comment, we have revised text in several paragraphs of the manuscript. In line with the above points, we consider three aspects to be particularly important:

1) The SML-OM global data collection (now official name, as available at PANGAEA) provides insights into the total concentration ranges of OM compounds and of the bulk variables found in the underlying water and in the SML, which is now explicitly stated in the Abstract.

2) The variability of enrichment factors (EF) and the selection of the appropriate statistics for evaluation and comparison. This is now already addressed in the last paragraphs of the Introduction section, and then later in the Methods section (2.2.1) and in the Discussion section.

3) The added value of looking at wide range of observations, which is now the entry sentence of the Discussion: "*A major strength of the SML-OM dataset is its broad coverage of OM concentrations, whereas individual studies are typically restricted to a narrow range of similar ULW conditions*." The Conclusion section was revised completely, also clarifying therein that: "*…, we provide reliable distributional estimates and redefine typical EF ranges for 12 organic compounds, offering a comprehensive reference for assessing whether new observations fall within expected conditions or reflect unusual enrichment*."

Comment 2: In the introduction, the authors provide a thorough overview of the specific characteristics of the sea surface microlayer (SML) and the general tendency for organic matter to accumulate within this layer. The focus on surfactants as a key organic fraction—both as components forming surface films and as important contributors to gas exchange—is appropriate and well justified.

We thank the reviewer for this positive comment and are pleased that she or he found the introduction detailed, appropriate and well-justified.

The Introduction section has been revised substantially, mainly in response to Comment 3.

Comment 3: However, the introduction does not provide sufficient context regarding previous research on the surface activity, selective transport, and enrichment of specific organic compounds within the SML, which are later discussed in the manuscript.

We appreciate the reviewer pointing this out. In the introduction of the revised manuscript, we have added a concise summary of previous research on the surface activity, selective transport, and enrichment of key organic compounds in the SML. The Discussion still contain the full detailed analysis and comparison of these findings across studies, as this is essential for interpreting our meta-analytical results. By summarizing the literature in the introduction and providing detailed discussion later, we now provide the necessary context upfront while avoiding excessive repetition.

The Introduction section has been revised substantially, see major changes in the second, third and fourth paragraph. Previous findings on differences in EF are now included in the Introduction section.

Comment 4: Additionally, the authors do not address prior findings on the contribution of these compounds' surface activity to the overall SML surfactant pool, despite the strong emphasis on surfactants in the introduction.

We thank the reviewer for this comment. We note that the surface activity of individual compounds is inherently reflected in our data and contributes to the observed variability in their enrichment in the SML. However, directly linking each compound's surface activity to its specific signal in the SML goes beyond the scope of the present study. While this is an important aspect, addressing it would require additional analyses and information not considered in the current data set.

Our meta-analysis addresses mass concentrations of organic compounds and does not include measurements of their surface activities or their effects on the physico-chemical properties of the SML or its uppermost monolayer. Therefore, translating our data into surfactant units or effects is not meaningful here. However, we have added a concise discussion in the introduction summarizing prior findings on how the compounds included in our study (i.e. Target Compounds), contribute to the overall SML surfactant pool, clarifying the functional relevance of these compounds to surface activity.

Also, in the Methods section we have clarified that we collected data on directly measured mass concentrations. Our dataset therefore does not include measurements of surface activities that have been converted into equivalent surfactant concentrations, such as those expressed in Triton X-100 equivalents.

Two significant additions were made to the text (also related to Comment 5 below).

Firstly, it is now clarified (at the end of the Introduction): "*The data collection presented here covers mass concentrations of OM compounds and does not include measurements of surface activities or effects on the physico-chemical properties of the uppermost monolayer of the SML. The primary objective is to provide an overview and specific insights into OM compounds that can accumulate within the SML and potentially be linked to biogeochemical processes occuring in the ULW. Accordingly, surfactant measurements of surface activities that have been converted into equivalent surfactant concentrations, such as those expressed as Triton X-100 equivalents, are not considered here. Ultimately, this data compilation, together with the knowledge derived from its initial meta-analysis, is intended to establish a robust foundation for subsequent studies that may support future modelling efforts linking biological processes to functions of the SML and their implications for biogeochemistry and climate.*"

Secondly, more details, including those relating to mechanisms, have been added to the Discussion section (4.1). In this context, we decided to add a new figure (now Fig. 8) to provide further insight into the EFs of specific groups of biosurfactants. This new Figure is also of great importance for a more careful interpretation of the EF, which ultimately led to a refinement of our conclusion statement. We therefore greatly appreciate this comment from the reviewer.

Comment 5: Consequently, it is unclear why certain compounds or organic fractions were selected for study, and why surfactants themselves—a highly relevant and previously measured organic fraction—were not included among the investigated compounds. Clarification of these points would strengthen the rationale for the study and better situate it within the context of existing literature.

The selected Target Compounds represent major organic carbon and nitrogen pools in the SML and are widely measured across diverse aquatic systems, providing sufficiently robust datasets for our meta-analysis. As one of our primary objectives was to distinguish between the organic carbon and nitrogen pools, these compounds provide a representative, interpretable coverage of both. This is now emphasized in the revised Methodology.

Also, the selected Target Compounds and their broad categories cover different groups of biosurfactants (dissoved hydrolyzable carbohydrates and lipids versus amino acids and proteinous compounds), but their relative proportions of the bulk cannot be inferred from the data collected.

- **Available mass concentration measurements:** Although surfactants play an important role in SML dynamics, they represent a highly heterogenous ‘bulk fraction’ of diverse compounds. Most surfactant measurements describe surface activity rather than enrichment or presence of certain compounds in the SML, and methodological differences in their quantification limit their suitability for cross-study synthesis. Therefore, we did not preselect surfactants as aseparate analytical category, but included all available bulk mass concentration measurements for which EF values were documented.

- **Link to phytoplankton dynamics:** Overall, by considering the broader pool of organic matter compounds, rather than focusing on specific operationally defined surfactant fractions, our current approach examines those observational types that are both feasible to measure and available in our datasets, and that are potentially also indicative of the state of the plankton ecosystem, while acknowledging that the exact mechanisms linking them to ecosystem processes are not well understood and are subject to ongoing research.

- **Comparison between effective biosurfactants:** In the end, we agree that the reviewer’s comment on surfactants raises a valid point. In response, we have expanded our analysis to examine the natural enrichment variability of major biosurfactants by directly comparing amino acid, fatty acid, and carbohydrate data from our dataset. These results are informative and have been included in the revised manuscript, in a newly added discussion section on surfactants. This way it is clarified that amino acid, despite being nitrogen-enriched, do not show the highest enrichment. Instead, their EF values fall between those of carbohydrates (lower end) and fatty acids (higher end). It indicates that structural and surfactant properties of specific compounds such as fatty acids are key to determining selective enrichment. The analysis thus highlights the need for more compound-specific approaches in future SML research and demonstrates the strength of meta-analysis in capturing real, emergent patterns.

An explanation has been added to the last paragraph of the Introduction section (please see our response and changes described above, Comment 4).

Comment 6: The authors should clearly define their search strategy, including the databases consulted, as well as the inclusion criteria, such as the specific keywords used, to ensure transparency and reproducibility of the study.

We thank the reviewer for this advice. In the revised Methods, we have included a description of our search strategy, specifying the platform used, the time frame, the keywords applied, and the inclusion criteria.

Information was added to the Methods section, see changes in the first paragraph of 2.1 Data collection and compilation.

Comment 7: The manuscript mentions extracting “secondary data” (e.g., environmental variables, sampling factors) when reported. This implies that metadata coverage is inconsistent across the dataset. I recommend quantifying the proportion of records with complete metadata and discussing potential selection bias in analyses that rely on the subset of data with full metadata coverage.

The availability of secondary (complementary) data has been quantified in the Supplementary Information using pie charts (i.e., Supplementary Figure 2), which show the proportion of records with available metadata. This is highlighted in the revised Discussion. Also, we note that all analyses were conducted on the full metadata set, regardless of the availability of the secondary data. The secondary data were only summarized (i.e., via pie charts) to highlight existing research gaps in SML studies and that subsequent investigations are possible, e.g. deriving conditional probability densities. Potential selection bias from incomplete metadata does not affect the results present in our study. This clarification is added to the revised Methodology.

We added on the third paragraph of the Method Section 2.1 "*All analyses were performed using the complete primary data set, independent of whether secondary data were available. The secondary data were summarized only to illustrate existing research gaps in SML studies*."
Relevant meta data information can be found in the Supplementary material. These are also flagged in the data provided (SML-OM global data collection).

Comment 8: L491-494 Considering previous work (as discussed by authors L304-L309), the concluding statements should be revised to explicitly emphasize the unique contributions and novel findings provided by this study in comparison to earlier work.

We agree with the reviewer's comment. The concluding statements have now been revised to emphasize the importance and novelty of our data collection and clarify how our initial meta-analysis, and possibly subsequent analyses, can contribute to advancing our understanding of SML dynamics.

The conclusion section has been revised accordingly.

Reviewer #2

General Assessment: This manuscript presents a novel and comprehensive meta-analysis of organic matter enrichment in the sea surface microlayer (SML). The study synthesizes a large dataset (2055 data points from 30 publications) to provide a statistically robust, cross-compound assessment of enrichment factors (EFs). The manuscript is generally well-structured, the methodology and the statistical treatment (e.g., boot-strapped KDE, log-transformation) is technically sound. The findings on preferential enrichment of nitrogen-enriched and particulate OM, alongside insights into EF limitations, are valuable for advancing SML biogeochemistry and climate-related models.

Comment 1: However, while the study effectively describes SML enrichment patterns (e.g., N-rich > C-rich, particulate > dissolved), it lacks a deeper mechanistic explanation for their existences. The discussion primarily correlates findings with previous studies but does not fully leverage the power of the meta-dataset to synthesize and propose unified physicochemical or biological mechanisms governing the observed selective enrichment. A more in-depth exploration of the underlying drivers (e.g., molecular hydrophobicity, particle buoyancy and aggregation, microbial biofilm formation, photochemical processing) would significantly enhance the conceptual contribution of this work.

In addition to the major issues mentioned above, moderate revisions are needed to enhance clarity, statistical rigor, and contextualization of results.

The authors appreciate the interest in the underlying processes that may explain some of the results of this meta-analysis. We share this perspective and agree that elucidating the mechanistic basis of selective enrichment would further enhance the contribution of our study. The meta-datasets available in the literature originate from diverse ecological settings, which complicate the identification of universal mechanisms. The study presented here essentially comprises the findings that the large amount of data has provided us with so far.

More far-reaching insights into the mechanisms that lead to the outcomes presented here are part of ongoing research, the preliminary results of which cannot yet be stated with certainty and which would also go beyond the scope of this meta-analysis. We therefore focus here on what is discernible and avoid premature interpretations of the underlying processes among the many different environmental conditions covered by this data set.

However, in response to the reviewer's suggestion, we agree to elaborate further on the discussion of categorized biosurfactants. Following this, the discussion was revised accordingly. We also added a new comparative analysis of three major biosurfactant classes, which reveals a consistent EF hierarchy and highlights the importance of compound-specific molecular properties in controlling the organic matter enrichment in the SML. In the end, the reviewer's comment helped us to present a clearer and somewhat refined view on how the EFs of nitrogen- versus carbon-enriched compound categorizations should be interpreted.

In response to this comment, we expanded the Discussion and included the more detailed comparison between the three biosurfactants: Amino acids, fatty acids and carbohyrates. We believe this is important, because it helped us refine our conclusion statement.

Comment 2:

The methodology for the literature search and study selection is briefly mentioned. A more detailed description (e.g., search databases, keywords, inclusion/exclusion criteria) would strengthen the reproducibility and rigor of this meta-analysis. The provided Supplementary Table S1 is crucial, but its description in the main text is minimal.

We have added a description of our search strategy to the main text, including the platform used, the time frame of search, the keywords applied, and the inclusion criteria, in the revised Methods (as also noted in our response to Reviewer 1).

Details were added to the first paragraph of 2.1, as mentioned before.

Comment 3:

The discussion is thorough in comparing results with past work, but the connection between the observed enrichment patterns and the underlying physicochemical or biological mechanisms should be more explicitly developed. The manuscript would benefit from a dedicated paragraph that synthesizes potential mechanisms for the dominant patterns (N-rich, particulate enrichment) revealed by the meta-analysis.

The authors generally agree with the reviewer's comment. However, as noted in our response to Comment 1, the underlying mechanisms cannot be derived easily and remain highly uncertain, due to the large variability in measurements of both bulk and specific compounds. Shedding further light on this issue is the focus of our subsequent analyses. In the Discussion section we have emphasized the value of drawing inferences about the potential mechanisms underlying the observed enrichment patterns, while clearly highlighting the limitations imposed by all the variations in the ecological settings and the constraints of the available data.

This has been addressed in the Discussion subsections 4.1.1 and in 4.1.3.

Comment 4:

The manuscript states that "fixed optimal bandwidth was applied" for log KDEs but does not specify the value or justification.

We value the reviewer's insight on this matter. A clarification has been included in the revised manuscript. In our analysis, we apply the plug-in method to derive the optimal bandwidth for log-transformed data. This is meaningful because the raw (non-transformed) data can span one order of magnitude and are associated with skewed propability densities. Using the original scale would produce optimal bandwidth estimates that are dominated by the largest (EF) values, which are not directly comparable across the data subsets. By log-transforming the EF data first, the variability is compressed, resulting in bandwidth estimates that are more balanced, interpretable, better resolve skewed probability densities, and are comparable across different compounds or conditions. Additionally, we note that the optimal bandwidth is derived for each log-transformed subsampled EF data individually, and that the resulting bandwidths remain consistent across subsamples, see Comment 5 below.

This is actually quite complex, and I have a KDE method at hand that is more advanced and eliminates the common problem of bandwidth. However, that would have been too complicated here, and we agreed to use the more common approach for now, which is easy to reproduce and understand. For reproducability, the codes used in this study are provided in a repository (GEOMAR OceanRep, https://oceanrep.geomar.de/id/eprint/63615/). We added to the end of the second paragraph of 2.2.1: "*Log-transformations, unlike the linear scale, produce similar distributions with comparable spreads across variables, allowing a single fixed bandwidth to produce stable and consistent smoothing for all data*."

Comment 5:

Similarly, the 67% subsampling rate for bootstrap resampling lacks rationale—was this based on sample size distribution or statistical convention? These details are critical for reproducibility.

We used a 67% (two-thirds) subsampling rate because it strikes a balance between retaining enough data to generate a stable KDE while still introducing sufficient variability to test robustness. There is no strict statistical rule, but this proportion is commonly used in bootstrap-style validation approaches. In principle, subsamples of smaller size (33% and below) can also be used, but this carries a higher risk of obtaining biased estimates, while the margins of uncertainty are overestimated at the same time. Another, more pragmatic reason is that some categories contain few data points, so subsample sizes would become too small to derive reliable KDEs.

Ideally, the subsample size would be chosen so that the derived optimal bandwidths for the KDE are comparable across subsamples. Achieving this would then require different subsample size selections. By choosing the 67% approach, we are able to cover all data types consistently. We have added this explanation to the manuscript to enhance clarity and reproducibility.

We added information in the third paragraph of 2.1.1: "*Robustness of the KDE method decreases at low sample size. Since the SML-OM contains variables with sample sizes as low as 16 (for proteins), a bootstrap resampling approach was adopted where 67% of the original data (i.e., 2/3 of the sample) were randomly subsampled. This proportion balances the need for sufficient data to generate stable KDEs while still introducing variability for robustness testing. By allowing consistent treatment across all data types, this approach maintains comparable KDE bandwidth behavior among subsamples*."

Specific Comments:

Comment 6:

Page 2, Line 31: The definition of SML thickness as "1-1000 μm" is very broad. It would be helpful to briefly mention that this range reflects different operational definitions and sampling techniques. Consider adding: "...which has an operationally defined thickness typically ranging from 1-1000 μm, depending on the sampling method used (e.g., screen, drum, plate)."

We have revised the sentence as suggested to clarify that the SML thickness is operationally defined.

Done

Comment 7:

Page 3, Line 87: The definition of EF is clear. However, the sentence "This equation proposes that..." is slightly awkward.

We have revised the sentence for clarity, as follows: "According to this equation, when the concentration of x is higher in the SML than in the ULW, the EF value rises above 1; when it is lower, the EF drops below 1".

Done (see first sentence after Equation 1)

Comment 8:

Page 5, Line 131-132: The use of PlotDigitizer and GraphClick is noted. It would be good practice to state the estimated error or uncertainty associated with digitizing data from figures, or to mention that data points were cross-checked for accuracy where possible.

We thank the reviewer for this suggestion. We have estimatd the associated uncertainty of data digitization by adopting repeated digitization approach. Methodology has been revised accordingly to include the resulting error estimate.

The following was done, as described in the third paragraph of 2.1 Data collection and compilation: "*To estimate digitization uncertainty, TOC data (40 datapoints) from Baastrup-Spohr and Staehr (2009) were digitized five times (200 values in total). The standard deviation of repeated measurements was calculated for each point and expressed relative to its average. Across all points, the median relative uncertainty was 0.29%, indicating that digitization introduced minimal error*."

Comment 9:

Page 6, Line 156: The bootstrap resampling approach using 67% of the data as a solution for small sample sizes. However, the choice of 67% is not justified.

This point has been addressed in our response to C5.

Yes, done.

Comment 10:

Page 13, Line 270: "FA ... fail to display conspicuous consistent trends" – awkward; rephrase to "FA exhibit no clear trend".

Thank you for this helpful comment. In the revised manuscript, we have rephrased the sentence as suggested.

Done

Comment 11:

Page 16, Line 353-365: The analysis of factor-specific variability (e.g., location, season, method) is a highlight of the discussion. This section could be strengthened by more explicitly stating the main takeaway from each panel in Fig. 8. For example, for panel (b), what is the key difference in DOC enrichment between oceans, coasts, estuaries, and freshwater?

We thank the reviewer for this valuable suggestion. We have revised the discussion of Fig. 8 (Fig. 9 in revised manuscript) to more explicitly highlight the key takeaways from each panel to improve the interpretability of the figure and strengthen the overall discussion of factor-specific variability.

This has been revised (structural changes and more details in 4.1.3).

Comment 12:

Page 20, Line 441-447: The terms "trophic conditions," "trophic status," and "ecological setting" are used frequently in the discussion of EF limitations. However, the manuscript does not explicitly define what is meant by these terms in this context.

We thank the reviewer for this helpful observation. We agree that these terms were used in the manuscript without any explicit definition. In response, we have revised the text to provide clear definitions at their first mention. In the revised manuscript, we have specified that:

- Trophic conditions / trophic status refers to the nutrient/productivity characteristics of the water body (i.e., oligotrophic, mesotrophic, eutrophic)
- Ecological setting refers more broadly to the biological and environmental context in which SML samples were collected

This has been addressed in the first paragraph of the Discussion section 4.3.

Comment 13:

Page 25, Line 552-554: The statement that "computational codes... are available upon request" is no longer considered best practice in scientific publishing. To ensure full transparency and reproducibility, the authors should deposit the code in a permanent, publicly accessible repository.

Yes, of course. We thank the reviewer for highlighting the importance of transparency and reproducibility. In line with current best practices, and should our study be accepted, we will deposit the codes used in this work in a permanent, publicly accessible repository. A link to the repository will be added.

Done.

"**Code availability**

Computational codes used in this study are available at at OceanRep GEOMAR [https://oceanrep.geomar.de/id/eprint/63615/]. The repository includes the full implementation of the KDE method, representative example scripts demonstrating its application for generating probability density functions and cumulative density functions, and a script to reproduce the correlation plots presented in the manuscript."

and

"**Data availability**

All data used in this study were extracted from previously published peer-reviewed sources and are publicly available through the PANGAEA data repository [https://doi.pangaea.de/10.1594/PANGAEA.990017]. Full citations for all the datasets are provided in supplementary information. No new data were generated for this study."