

Author's response - egusphere-2025-5350

We thank the associate editor and the referees for the positive reviews. Please find below the combined answers to the referees' comments.

Thölen et al.: Reply to reviewer RC1

We would like to thank RC1 for the careful and constructive comments on our manuscript. We greatly appreciate the detailed comments and suggestions, which have helped us to improve the readability and clarity of the manuscript.

Below, please find your original comments printed in black and our point-by-point responses printed in blue. Line numbers indicated here refer to the preprint manuscript submission.

Please find a list of references used within this answer at the bottom of this document.

RC1: Comment on egusphere-2025-5350, Anonymous Referee #2, 25.02.2026

This manuscript presents the time-course of the concentrations of the colored (CDOM) and fluorescent (FDOM) fractions of dissolved organic matter (DOM) in the sea-surface microlayer (SML) and the underlying water (UWL) during a controlled mesocosms experiment. The impact of a phytoplankton blooms stimulated by nutrient additions and photodegradation processes on the variability of the optical properties of dissolved organic matter in the two layers is assessed.

Overall, this is a well-designed study providing useful insights into the dynamics of colored and fluorescent dissolved organic matter in the SML, specifically regarding its biological production and photochemical decomposition under conditions that allow for isolating both driving forces from the multiple physical and biogeochemical factors involved.

Thank you very much for this positive feedback on our study design.

I find the manuscript to be very thorough, which makes it quite long and occasionally difficult to follow; however, I recognize that this level of detail may be beneficial for readers who are not experts in the optical properties of dissolved organic matter.

Thank you for the feedback on the manuscript length and readability.

Our intention for the manuscript as well as for our subproject in the BASS research group is to address an interdisciplinary readership. We want to show how optical measurements can be applied as proxies for organic matter transformation processes in the sea-surface microlayer. This requires a low-level introduction and method section. Nevertheless, we carefully went through the text and tried to shorten and clarify some of the sentences to ensure better readability and to generally shorten the manuscript without losing depth or information.

Something that strikingly caught my attention is the lack of data on dissolved organic carbon (DOC) concentrations. My recommendation is that, if these were measured, they should be included. If they were not, a brief explanation should be provided.

Thank you for bringing this to our attention. Thanks to your comment we recognize that the connection between CDOM/FDOM and DOC values was missing. DOC was measured during the mesocosm study. DOC data are shown and explained in Bibi et al. (2025a), as well as in the new preprint Zöbelein et al. (2026). In the supplements of Bibi et al. (2025a) you can find a detailed discussion of an interlab-comparison between the two parties that measured DOC during the mesocosm study. For our manuscript we will show the average DOC data. Using the DOC data we have calculated SUVA₂₅₄ from the absorption at 254 nm and the respective DOC values. We have added two plots to the appendix Figure A2: DOC and SUVA₂₅₄. They are explained in the results Sect. 3.4. When considering the SUVA₂₅₄ results we had to slightly revise the outcome of our second hypothesis as the significantly higher SUVA₂₅₄ in the ULW also supports stronger photodegradation effects in the SML. This is discussed in Sect. 4.2.

Please find a list of detailed comments and recommendations below:

Line 44 – replace 0.4 microm by 0.7 microm

Thank you, we replaced it.

Lines 70 – 74. This is not the right place for these sentences. They should be moved to the last paragraph of the Introduction.

Thank you, we moved the first sentence to the last paragraph of the introduction but rephrased the next sentence so the first hypothesis of the manuscript stays within this paragraph as the sentences before are leading up to it.

Lines 83 – 86. Same as above.

Thanks, we have removed this sentence from the introduction. The same information is already provided in the discussion section and should be sufficient there.

Line 93 – What do you mean with “high resolution” in this context?

Thank you for drawing attention to this phrasing. We have removed the wording “high-resolution” as the resolution of the bio-optical samples was the same as other samples taken during the study.

Nevertheless, it would have been beneficial for the study and the manuscript to have taken bio-optical samples in a higher frequency than the other parameters but unfortunately that was not done because of the limited availability of SML on the mesocosm surface. We had to keep sampling volume to a minimum in order to maintain the SML integrity.

Lines 96 – 98 (Figure 1). Processes excluded in mesocosms studies should be colored differently, e.g. inflow, upwelling, outflow, winds, mixing waves, etc...

That is a good suggestion. We have edited the figure and changed the color of the excluded processes to a lighter blue grey. Additionally, we have printed all the included processes bold and underlined to better differentiate between them. This description was also added to the caption. We would like to include mixing, because of the flow pumps moving the water column in the mesocosm basin and to exclude waves, which is why we have edited their joint box to two separate boxes within the figure.

Line 101-102. Please specify what is the depth of the outdoor basin (0.8 m). What is the color of the basin? (relevant issue in a study about photodegradation).

Thank you for pointing out that the dimensions could be misleading. We have clarified the dimensions of the basin: [...] The facility contains an 8 m long, 1.5 m wide and 0.8 m deep outdoor basin [...].

We have interpreted your question about the basins color in two ways and hope one of our two responses sufficiently answers the question.

1. The color of the basin itself is a bright concrete grey, which possibly can promote the penetration of sunlight into the basin. We have added this information to the text in the methods and discussion sections.
2. The color of the basin's water changed with the progression of the phytoplankton bloom. Unfortunately, there is no systematic photographic or objective documentation of the color. The information we can provide comes from sporadic pictures and personal memory. The water was greenish, but clear at the beginning of the study and turned milky after the second bloom peak. Bibi et al. (2025a) include a figure of the basin's turbidity measurements. The turbidity increased after the bloom as the coccolithophores shed their coccoliths and turned the water milky.

Lines 104-105. How was constant mixing of the ULW achieved?

That's a good point to add. The mixing was achieved by placing eight flow pumps in the basin. We have added this information to the text.

Line 123. It surprises me that such large water volumes are needed to count **bacterial abundance**.

You are right to wonder about the volume. The colleagues responsible for the bacterial cell count also conducted several other measurements. The large sample volume was required for filtration to accurately characterize microbial community diversity and relative abundances via 16S rRNA amplicon sequencing. They used 400 ml sample volume from SML and ULW each, from which 300 ml was used for filtration, and the rest for enzyme analysis, flow cytometry, and Fluorescence In-

Situ Hybridization (FISH). The bacterial cell count method requires 2 ml (specifically 1730 μ l) sample volume.

To clarify this in the text, we have omitted the “because of the large sample volume needed” part of the sentence and just stated the frequency of measurements.

Line 135-136. While storing filtered samples at 4°C is standard practice for FDOM, the duration here extends to several months. Could you specify the exact timeframe? Additionally, were any tests performed to ensure sample integrity after such prolonged storage?

Thank you for raising this point regarding sample storage. After carefully revisiting the measurement documentation we can clarify that all samples were measured within days of being collected, with the exception for the final pair of samples collected on June 16. This pair of SML/ULW samples was measured in September of 2023 together with additional samples collected after the mesocosm study. These additional samples were initially considered to be included in the analysis which led to the original phrasing in the manuscript of being measured “within a few month”. The reason for the delayed measurement was temporary issues with the instrument and another overlapping sampling campaign.

Since it cannot be sufficiently confirmed that the prolonged storage time did not alter the sample composition, we decided to remove the sample pair from 16 June from the data set. According to this we have repeated the analysis, including PARAFAC, to get the accurate data within the given timeframe of 18 May - 15 June. Therefore, the respective figures and tables have been updated (Figures: 2, 4, 5, 6, 7, A1, A2, A3, A4; Tables: 2, 3, 4, 5)

The time frame was indicated in the beginning of the methods section, and the original phrasing was changed accordingly as well to “The samples were stored dark at 4 °C and measured within a few days of the study”.

Line 137. Please, specify the excitation and emission slit widths.

Thank you for drawing attention to this missing information. The Aqualog manual states: [...] the fixed optical geometry of the Aqualog® lends itself to the simple solution above because neither the slit-widths that determine the beam geometry, nor the path-lengths or overlap volume of the absorbance and emission paths are user-adjustable. [...]

The Aqualog manual further specifies a bandpass of 5 nm for the slit width. This information was added to the methods in line 137.

Line 149. No need to include the equation for inner filter correction.

We agree and have removed this equation and its explanation from the text.

Lines 154 and 157. No need to include the equation for Raman normalization.

We agree and have removed this equation and its explanation from the text.

Line 180 – Table 1. IC1, IC2 and IC3 not described yet. Explain meaning in the table caption.

Thank you for pointing that out. We have added their description to the table's caption (Table 1).

Line 207. Refer to the paper by Bibbi et al. (2025) for nutrient concentrations. A sentence indicating the levels observed and the would help the readers to follow the evolution of the mesocosm.

Adding nutrient information to the results is a good suggestion, thank you for that. We restructured the beginning of the Sect. 3.1, including a new sentence about the nutrient concentrations, and referred to Bibi et al. (2025a).

Line 2014. Which paper of Bibi et al (2025)? 2025a?

Thank you for pointing that out. We missed adding the right reference with the citation program here. This refers to Bibi et al. (2025a). It was changed accordingly.

Line 223. These temperatures seems unrealistic for the Jade Bay. Please comment.

Thank you for drawing attention to the description of the temperature in the mesocosm. We would like to clarify the mesocosm setup: The mesocosm basin is located on-land on the institute's premises. The basin has concrete walls and is quite shallow (0.8 m deep). The incoming sun can reach up until the basins floor and heat up the water column. As there is no exchange with the adjacent Jade Bay during the mesocosm study the water column can warm faster than the bay. The temperature mentioned in this paragraph was measured within the mesocosm basin.

During the time of the mesocosm, we also conducted daily measurements on the Jade Bay. We used the autonomous catamaran HALOBATES (Wurl et al., 2024) to collect additional SML and ULW samples and measure environmental parameters. The location of each deployment of the catamaran was similar every day. Besides other parameters, temperature was measured at 1 m depth. The mean temperature measured by the catamaran HALOBATES rose from about 13.8 °C to 19.5 °C in the Jade Bay in the timeframe of the mesocosm study.

We have also clarified the mesocosms location in the methods: “[...] The on-land facility [...]”. For pictures and a more detailed description of the mesocosm set up we encourage the reader to refer to the paper of Bibi et al. (2025a) at the end of the methods section.

Line 276. It seems more logical to introduce first the variation of the CDOM and FDOM concentrations and later the %FDOM.

Thank you for this suggestion. When revisiting the manuscript, we realized that the information which is provided by Figure 5 is partially redundant to the PARAFAC component time series of

Figure 7 in the original submission, now Figure 6. In addition, its interpretation was not integrated into the discussion section and therefore not contributing to the overall narrative. For the sake of clarity and manuscript length, we have removed Figure 5 and its description in the text from the results section.

Line 347. Explain how the sum was calculated. Is just the sum of the Fmax of each component or the sum of the average fluorescence of each component?

Thank you for pointing out that this information could be misleading. The sum refers to the sum of the Fmax of each component. After receiving the comment of the second referee, we decided to remove this sentence here and just include the info in the figure caption. The caption adapted to make this point clearer (Figure 7, now Figure 6).

Line 405 – Figure 8. You may calculate the derivative of Chla in the panels on the right.

That's a good suggestion. After revisiting the figure, we have decided to shorten it, by not including the REPIX index and the C1/a312 index, as the slopes of these indices are neither strongly supporting nor contradicting the hypotheses. We adapted Figure A2 in the appendix to include the timeseries of REPIX and C1/a312 as well as the beforementioned SUVA₂₅₄. We have updated former Figure 8, now 7, to include a plot with the deviation of chlorophyll-a. We decided against showing it in every plot on the derivation side as it's very noisy during the bloom phase even after smoothing advances. We also followed up on the new indications provided in the text in the results and discussion section.

Lines 536-537. a₂₅₄ should not change substantially because natural UV of this wavelength does not arrive to the Earth Surface so it does not directly decompose chromophores absorbing at this wavelength.

We agree that solar radiation at 254 nm does not reach the Earth's surface and therefore does not directly induce photodegradation of chromophores absorbing at this wavelength. However, changes in a₂₅₄ do not necessarily require direct irradiation at 254 nm. CDOM absorption arises from a continuum of electronically coupled chromophores rather than independent absorbers, as shown by Del Vecchio and Blough (2004). In such systems, photochemical alteration at longer wavelengths (e.g., UV-A and visible) can induce changes across the full absorption spectrum.

Accordingly, the observed changes in a₂₅₄ in our study are interpreted as the result of indirect photochemical transformation processes affecting the overall DOM pool, rather than direct photodegradation at 254 nm.

Line 542-543. This sentence is repeated in the previous paragraph. It could be erased.

We agree with this observation. We have added the information of this sentence to its appearance in the previous paragraph to refrain from doubling information but keeping the indication that the increase in SR values suggests that photodegradation outweighed microbial alteration as a sink.

Lines 555-561. This paragraph about a paper in preparation could be omitted.

Thank you, we agree that a paper in preparation should be omitted, when the manuscript was first written the submission of Zöbelein et al. (2026) was very close, so we decided to add its info to the text. In the meantime, the paper Zöbelein et al. (2026) was also posted as a preprint at EGU sphere. As its results underline the findings in our paper, we would much like to include it.

Line 574. The conclusions may be drastically reduced. Lines 576–581 and 591–594 can be omitted.

Thank you for this suggestion. Those sentences were removed from the conclusions.

Additional references

Bibi, R., Ribas-Ribas, M., Jaeger, L., Lehnert, C., Gassen, L., Cortés-Espinoza, E. F., Wollschläger, J., Thölen, C., Waska, H., Zöbelein, J., Brinkhoff, T., Athale, I., Röttgers, R., Novak, M., Engel, A., Barthelmeß, T., Karnatz, J., Reinthaler, T., Spriahailo, D., ... Wurl, O. (2025). Biogeochemical dynamics of the sea-surface microlayer in a multidisciplinary mesocosm study. *Biogeosciences*, 22(23), 7563–7589. <https://doi.org/10.5194/bg-22-7563-2025>

Del Vecchio, R., & Blough, N. V. (2004). On the Origin of the Optical Properties of Humic Substances. *Environmental Science & Technology*, 38(14), 3885–3891. <https://doi.org/10.1021/es049912h>

Wurl, O., Gassen, L., Badewien, T. H., Braun, A., Emig, S., Holthausen, L. A., Lehnert, C., Meyerjürgens, J., & Ribas-Ribas, M. (2024). HALOBATES: An autonomous surface vehicle for high-resolution mapping of the sea-surface microlayer and near-surface layer on essential climate variables. *Journal of Atmospheric and Oceanic Technology*. <https://doi.org/10.1175/JTECH-D-24-0021.1>

Zöbelein, J., Sawle, S., Friedrichs, G., Ribas-Ribas, M., Lehnert, C., Paetz, K., Pflaum, M., & Waska, H. (2026). *Buoyancy and polarity driven accumulation of dissolved organic matter in the sea surface microlayer during a phytoplankton bloom*. Biogeochemistry: Organic Biogeochemistry. <https://doi.org/10.5194/egusphere-2025-6563>

Thölen et al.: Reply to reviewer Bitá Sabbaghzadeh

We would like to thank reviewer Bitá Sabbaghzadeh for the detailed and helpful comments on our manuscript. They have helped us to clarify the manuscript and greatly improved the readability.

Below, please find your original comments printed in black and our point-by-point responses printed in blue. Line numbers indicated here refer to the preprint manuscript submission.

Please find a list of references used within this answer at the bottom of this document.

RC2: Comment on egusphere-2025-5350, Bitá Sabbaghzadeh, 07.04.2026

This manuscript presents a month-long mesocosm study investigating the effects of phytoplankton bloom dynamics and photodegradation on C/FDOM in the SML and ULW. By isolating biogeochemical processes from natural environmental variability and conducting daily sampling in both layers, the study provides a valuable dataset covering the onset, peak, and decay phases of two partially overlapping phytoplankton blooms. The inclusion of morning and afternoon sampling to account for variable UV exposure is a notable strength and enhances the interpretation of photochemical effects

However, some limitations inherent to the experimental design should be more critically addressed when interpreting the results. In particular, the use of a mesocosm system, while advantageous for controlling environmental variability, may not fully capture the complexity of natural conditions. Processes such as atmospheric deposition (e.g. precipitation inputs) were excluded, and physical dynamics may differ substantially from in situ environments. As a result, it remains uncertain to what extent the observed microbial and biogeochemical responses in the SML are representative of natural systems.

Furthermore, while the study aims to investigate temporal mechanisms, the relatively short duration of approximately one month may limit the ability to fully resolve longer-term dynamics and feedbacks in C/FDOM variability. It is possible that the constraints of the mesocosm setup have dampened potential layer-specific differences, particularly in the SML, where environmental gradients are often more pronounced under natural conditions. These aspects should be discussed more explicitly to better contextualize the findings and their broader applicability.

We agree with these observations on the limitation of the mesocosm design and are thankful for the comment. We dedicated the beginning of the discussion to addressing the limitations more critically (Sect. 4.1 in the revised version of the manuscript).

Overall, the manuscript addresses an important topic and provides meaningful contributions to our understanding of DOM dynamics at the air–sea interface. With a more thorough discussion of the study's limitations and their implications, the manuscript would be further strengthened.

The manuscript would also benefit from careful proofreading, particularly in the results section. The results require revision and rephrasing to improve clarity, readability, and overall coherence.

Thank you for your careful consideration of this section and all the following helpful comments. We have reworked the results section intensively to clarify the phrasing. Please find point-by-point responses below.

Comments

Line 12: It would be helpful to clarify what aspect of CDOM and FDOM is being influenced here (e.g., concentration, composition, enrichment, or degradation), as this is not entirely clear from the current phrasing.

Thank you for pointing that out. We agree and have edited the sentence in line 12.

Line 33: Several studies have addressed the role of the SML and its chemical properties in air–sea gas exchange. The authors could also refer to additional relevant work for a more comprehensive perspective, such as Pereira et al. (2018), which specifically discusses the influence of the SML on gas.

Thanks for the good suggestion, we have added Pereira et al. (2018) as a reference.

Line 38-40: There appears to be insufficient linkage between the two statements. The authors could cite additional studies, for example Sabbaghzadeh et al. (2017), to support the idea that the SML can re-establish after disruption. These regenerated bubbles bring organic matter, including surface-active compounds, back to the SML, contributing to its enrichment. Including such references would strengthen the mechanistic explanation for SML resilience and OM accumulation.

We agree that the sentences sounded a bit disconnected before. We have edited the phrasing and included your recommended study as well as the study by Dragevic and Pravdic (1981).

Line 58: A full stop is missing at the end of the sentence.

Good catch, we have edited the sentence while refining the introduction.

Line 68: SML should be used here, as the abbreviation has already been defined earlier.

Thank you, we changed it to “SML”.

Line 91: Regarding the figure caption: If the figure represents a closed system, this should be explicitly stated in the figure caption. Otherwise, if the intention is to show all sources and sinks, some pathways, such as downwelling, appear to be missing. Regarding Figure 1 labelling: In Figure 1, “evaporation” should be indicated as a sink rather than a source.

Our intention was to show all sources and sinks. Based on the comment of anonymous referee #2 we have modified the figure to highlight the processes discussed in our manuscript. Thanks to your comment, we also included downwelling. After looking deeper into the topic of volatility and dissolved organic matter, we conclude that evaporation removes water from the SML, which concentrates non-volatile DOM fractions within the layer, acting as a source of DOM (Jiang et al., 2025; Moody, 2020). However, evaporation simultaneously acts as a sink by driving mixing, which disrupts the SML and promotes exchange with the ULW, diluting the enriched surface layer.

Additionally, a small semi-volatile fraction of DOM may be lost directly with evaporating water (Materić et al., 2017). Given this complexity, we have chosen to represent evaporation as both a source and a sink in the revised figure and have clarified this dual role in the figure caption.

Line 109: Since this sampling approach for the ULW is not standard, it would be helpful to include a photograph of the sampling device in the appendix for clarity and reproducibility.

That is a good suggestion. We have included a scheme of the ULW and SML sampling method in the appendix in Figure A5.

Line 114: Specify which pigments are being referred to.

We decided to remove the phrase “and other pigments” as it doesn’t support the sentences intention. Other pigments (Chlorophyll-b, Chlorophyll-c and β -carotene) which were measured were used to help identify the bloom forming species and were explained in Bibi et al., (2025).

Line 121: The specific sampling depths should be clearly indicated.

Thank you, we included the depths, 2 and 40 cm, in the methods section.

Line 123-124: The manuscript should provide sufficient information for readers to understand the methodology. Please specify how surfactants in the SML and ULW were measured, even briefly, and refer to Bibi et al. (2025) for further details.

That is a good suggestion. We have added information about the voltammetry method and the corresponding device and reference in the methods section.

Line 134-135: Please rephrase this sentence to improve clarity and readability.

Thanks, we rephrased it to “The FDOM samples were filtered into clear 40 ml SUPELCO bottles, which were acid-washed twice and combusted at 500 °C for 5 h following the protocol of Ferdinand (personal communication).”

Line 135: The sentence could be replaced with: “The samples were stored in the dark”.

We agree that this would be a good alternative. In response to a comment of anonymous referee #2 we refined the description of the time frame in which the samples were measured. The following is a segment of our answer to anonymous referee #2’s wish to specify the exact timeframe:

“After carefully revisiting the measurement documentation we can clarify that all samples were measured within days of being collected, with the exception for the final pair of samples collected on June 16. This pair of SML/ULW samples was measured in September of 2023 together with

additional samples collected after the mesocosm study. These additional samples were initially considered to be included in the analysis which led to the original phrasing in the manuscript of being measured “within a few month”. The reason for the delayed measurement was temporary issues with the instrument and another overlapping sampling campaign.

Since it cannot be sufficiently confirmed that the prolonged storage time did not alter the sample composition, we decided to remove the sample pair from 16 June from the data set. According to this we have repeated the analysis, including PARAFAC, to get the accurate data within the given timeframe of 18 May - 15 June. Therefore, the respective figures and tables have been updated (Figures: 2, 4, 5, 6, 7, A1, A2, A3, A4; Tables: 2, 3, 4, 5)

The time frame was indicated in the beginning of the methods section, and the original phrasing was changed accordingly as well to “The samples were stored dark at 4 °C and measured within a few days of the study”.

Line 145: Ensure that the opening parenthesis follows directly after the date.

Thank you for the suggestion. We have omitted the ‘supplementary material, appendix A’ note as this should already be referenced with just Murphy et al. (2013).

Line 169-171: Rephrase this sentence to improve clarity and readability.

Thank you, we have added the preparation steps in parenthesis (blank subtraction, inner-filter effect, Rayleigh-masking, Raman-normalization) and believe this improved the sentence clarity and readability.

Line 174:for an overview please refer to Table 1..... This part should be placed within parentheses

Done. Good point.

Line 174-175: Rephrase this sentence to improve clarity and readability.

Thank you for pointing this out. We have rephrased this sentence to “Metrics calculated from specific FDOM components were obtained using their PARAFAC derived equivalents.”

Line 176: Delete “E.g.”.

Done. Good point.

Sub-Section 2.3. In the Statistics section, it is unclear whether all the current details are necessary. Many of these points could be summarized for the reader with an appropriate reference. At present, the section appears to contain excessive detail that could be streamlined by citing relevant sources.

We agree and have removed the detailed explanation of the PERMANOVA method and generally went through this subsection to clarify the language and removed some of the details.

Line 187: Provide a reference for the “Bray” method.

Added it. Good point.

Line 189-190: Rephrase this sentence to improve clarity and readability.

Please see above at the sub-section 2.3 comment.

Section 3: Overall, the manuscript is well-written and clear; however, the Results section could be improved by moving some details currently in the main text into the figure captions. The Results should be written more clearly, and the figures should be properly referenced within the text to enhance readability and coherence.

Thank you for your careful consideration of this paragraph and the detailed suggestions for clarifying it. We have reworked the entire section, rephrasing and clarifying the text.

Line 211: What is meant by “SML values”? Please clarify.

Thank you for pointing out that this phrasing needed clarification. We changed it to SML chlorophyll-a concentrations.

Line 233: The word “say” should be replaced with “stays” in this sentence for accuracy.

Good catch, that was a typo. We changed it.

Line 259: The term “named” should be replaced with “named as” for clarity.

Done. Good point.

Table 2: Remove the word “existing” from the caption.

Thanks for pointing this out. We agreed and removed it.

Line 293-295: Rephrase this sentence.

We have clarified these two sentences to read “For the Layer × Phase run, it was tested which influence the layer (SML or ULW) and the bloom phase (onset, peak, decay) had on the FDOM component composition (Table 3). In the Environmental variables run a set of environmental variables was tested for their influence on the composition (Table 4).”

Line 297-298: The term “significant but negligible” is potentially confusing. Please clarify the distinction between statistical significance and the small effect size indicated by the low R^2 value. E.g. The layer had a statistically significant but small influence ($R^2 = 0.08$, $p < 0.001$) on the composition.

Thank you. We have adapted the sentence according to your suggestion.

Line 339-342: This information would be better suited for the figure caption rather than the main manuscript.

Line 347: Please see the previous comment.

Line 364: The first sentence may not be necessary in the main text and could be omitted.

Line 365-366: This could be more suitably placed in the figure caption.

We agree with the four comments above. We have omitted these sentences in the main text and moved the information to the figure captions in cases where it was not already mentioned there.

Line 377: However, all EF values in Table 5 are very close to or greater than 1!

Thank you for pointing out that this might be misleading. Yes, you are right. In this sentence we are only referring to the HIX enrichment factor. We have adapted the sentence to clarify this: “The average EF for HIX is slightly lower than 1, indicating a higher humification in the ULW (Table 5).”

Line 377 and 380: The figure should be cited in parentheses at the end of the sentence.

We agree and have adapted this for these sentences and for the entire results section.

Line 382: Which values are being referred to? Please specify clearly.

Line 382: Please clarify. Variance of what exactly.

Line 385: Clarify which parameter the time series values refer to.

Thank you for pointing out that these sentences needed refinement. We have clarified which values we wanted to refer to in this entire section.

Line 389: This information is not essential in the main text and could be moved to the figure caption.

Line 415-424: Most of this section could be moved to the figure captions and removed from the Results section.

Thanks for drawing attention to this paragraph, we agree and have moved the description to the figure caption and formulated the results from these figures more clearly (Figure 8 caption, now Figure 7 in the revised version).

Line 442: The authors should clarify the magnitudes of the values reported.

We agree. When revisiting the named references, we dug a little deeper, found another good reference with enrichment factors in similar magnitudes like the mesocosm study (Carlson, 1982) and changed this sentence accordingly: “Chlorophyll-a concentrations in the SML were distinctly higher ($\bar{\theta}$ EF = 47.95, Table A1) and more delayed than in the ULW (Figure 2a), consistent with EF of 10-100, reported for very productive and slicky conditions in the ocean (Antonowicz, 2018; Carlson, 1982; Hardy and Apts, 1984).”

Line 444: Please replace “...rather than active growth” with “...rather than active growth of phytoneuston.

Thank you. That improved the clarity of the sentence.

Line 450-452: The authors are encouraged to rephrase this sentence to improve clarity.

Yes, we agree with your comment on the lack of clarity in this sentence. When revisiting it, we have decided to fully remove it as it did not add needed information to the text.

Line 539: Consider revising the text to read: “As previously mentioned (Sect. 4.1).

Good suggestion. This was changed accordingly.

Figure A4: It appears that some axes are missing in Figure A4. The authors should ensure that all intended content is included.

Thank you for noticing. We updated Figure A4 and made sure all axes are visible.

Line 641-642: This statement is new to me. I would like to clarify whether it is necessary to mention this here and whether it is part of the journal’s policy.

We agree that this kind of statement has only recently come of use in scientific publications. The EGU Biogeoscience submission guidelines state the following: “*Should you have used AI tools to*

generate (parts of) your manuscript, please describe the usage either in the Methods section or the Acknowledgements.” Therefore, we decided to mention the AI tool used in the acknowledgements.

General comment: The dates throughout the manuscript should be formatted consistently and correctly. For example, “May 1” could be changed to “May 1st” where appropriate.

Thank you for pointing this out. This was a misconception and we have edited all dates to read e.g. 1 May as stated in the submission guidelines.

Reviewer: Bita Sabbaghzadeh

Additional references

Jiang, W., Li, L., Yu, L., Kim, H., Sun, Y., & Zhang, Q. (2025). Photochemical processing of dissolved organic matter in fog water: Oxidation and functionalization pathways driving organic aerosol evolution. *Atmospheric Chemistry and Physics*, 25(22), 16817–16832. <https://doi.org/10.5194/acp-25-16817-2025>

Materić, D., Peacock, M., Kent, M., Cook, S., Gauci, V., Röckmann, T., & Holzinger, R. (2017). Characterisation of the semi-volatile component of Dissolved Organic Matter by Thermal Desorption – Proton Transfer Reaction – Mass Spectrometry. *Scientific Reports*, 7(1), 15936. <https://doi.org/10.1038/s41598-017-16256-x>

Moody, C. S. (2020). A comparison of methods for the extraction of dissolved organic matter from freshwaters. *Water Research*, 184, 116114. <https://doi.org/10.1016/j.watres.2020.116114>