

Thölen et al.: Reply to reviewer Bita Sabbaghzadeh

We would like to thank reviewer Bita 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, Bita 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 Dragecivic 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: Bitia 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>