

Response to reviewer R2: minor revision

Manuscript egusphere-2026-1720, "DMS, MeSH and nanoparticles in semi-controlled deck-borne experiments using Antarctic seawater: on the effect of UV light", by Chamba et al. All line numbers refer to egusphere-2026-1720-manuscript-version2.pdf.

The paper describes paired incubations of Antarctic seawater in two on-deck Air-Sea Interface Tanks during the POLAR-CHANGE cruise, one under a lid transparent to the whole spectrum and the other under a lid that cuts off wavelengths below about 375 nm. DMS and MeSH fluxes and aqueous concentrations are derived from the headspace measurements, the nanoparticle population of each headspace is followed at the same time, and both are compared with the plankton and viral assemblages sampled inside the tanks. The main results, that removing UV increases DMS fluxes by about a quarter and MeSH fluxes by close to 60%, that the effect is largest at night, and that new particle formation is almost restricted to the tank without UV, are unusual and worth publishing. The paired design is what makes them credible, and experiments A and B, in which the same water was kept for two days, give the biological comparisons something to work with.

I thank the authors for their careful reply and for the revised version of the manuscript. They have answered most of the points I raised in the first round. On the main point, the absence of statistical support for most of the differences reported, the revision goes a good way: figure 5 now carries the fitted relationships with their equation, R^2 and p value, which is what was needed and makes the figure much more useful to other groups working in the region, and the comparisons of the MeSH/(DMS+MeSH) ratios and of the N1 concentrations between the two tanks are now supported by paired Wilcoxon signed-rank tests with effect sizes, with the software, the library versions and the testing procedure stated. The new material on the pumping system, on the cytometric detection of the large viruses typical of Southern Ocean waters, on the meaning of Fv/Fm and on polar diatoms as DMSP producers also improves the paper, and I think the decision to drop the regression forced through the origin was the right one. Little of what is left requires new analysis. Three statements survive in the manuscript that the revised evidence no longer supports, and a set of comparisons is still described as significant with no test attached. See my comments below for details.

We would like to begin by thanking the reviewer for their careful and thorough reading of the article, which enabled us to correct several errors that had gone unnoticed, as well as sentences that were no longer relevant. Furthermore, the requests for statistical tests to substantiate certain conclusions and discussions in the article have, indeed, significantly improved the quality of the article and the robustness of the results presented.

Finally, I would like to point out that I was not previously very familiar with the various statistical tests and that the reviewer's requests enabled me to examine them in depth. I hope that the additions and clarifications made now fully address the issues raised.

Statistical treatment

The tests that were added are done properly, which makes the remaining gaps more visible.

First, the description of the statistics at lines 694-697 sits under Code availability, and it would be easier to find as a short subsection of the methods, if not at least place a call in the Code availability so readers know where to look for the statistical details.

Second, that description covers the Wilcoxon signed-rank test only, while a Mann-Whitney U test is used at line 352 and in the reply; please describe both, with the reason for choosing a paired or an unpaired test in each case, and give the statistic, the p value and the sample size every time a test is mentioned, as is done correctly at lines 401-403 and 425-426.

Third, several of the comparisons across tanks and water types involve many simultaneous tests and no allowance is made for this; a false discovery rate control such as the one proposed by Benjamini and Hochberg (1995) is the least intrusive option and would suit the correlation matrices of figure S8 particularly well.

Fourth, comparisons that are still made with no test at all include the water type contrasts of figure 6, the responses of the phytoplankton groups to UV in figures 10 and S.4, the Fv/Fm patterns of figure 11, and the ozone differences at lines 421-422. Some of these may not survive a test, and figure 11 in particular shows boxes that overlap almost completely. Where that happens it is better to describe the pattern as apparent and to say that it is not statistically resolved than to keep the present wording.

The statistical test description was moved to the new section 2.4 in material and methods.

“2.4 Statistical analysis

Statistical tests were performed using Python (version 3.11) with the SciPy library (version 1.16.2; Virtanen et al., 2020) at a significance level of $\alpha = 0.05$. For paired measurements, normality of paired differences was first assessed with the Shapiro-Wilk test; when normality was not rejected, the paired t-test was applied, otherwise the non-parametric Wilcoxon signed-rank test was used, with effect size quantified by the rank-biserial correlation coefficient r . For independent, non-concurrent measurements, normality was assessed for each group separately, the Welch's t-test was applied when normality held for both groups and otherwise the Mann-Whitney U test was used, with effect size similarly quantified by the rank-biserial correlation coefficient r . When multiple comparisons were performed on the same variable, p-values were adjusted using the Bonferroni correction to control the family-wise error rate. For the correlation matrices, p-values were instead adjusted using the Benjamini-Hochberg false discovery rate procedure (Benjamini and Hochberg, 1995).”

A call was kept in the Code availability: Statistical analyses were performed using Python (version 3.11) with the SciPy library (version 1.16.2; Virtanen et al., 2020) at a significance level of $\alpha = 0.05$, details concerning statistical analysis are presented in the section 2.4.

Following your recommendation, the p-values for the correlation matrices (Fig. S8) were adjusted using the Benjamini-Hochberg false discovery rate procedure (Benjamini and Hochberg, 1995) and we added this precision in the caption and the reference to the Supplementary.

For the fourth point we detail below all the changes in the specific comments section but we added tests for ozone differences, Fv/Fm difference between ASITs and UW and the phytoplankton groups.

We completed the text with the missing tests mentioned below (see specific comments).

Cryptophytes, nanophytoplankton and chlorophyll a

The cryptophyte result has been removed in some places and left in others, so the manuscript now disagrees with itself. The reply explains that the covariation was obtained with DMS and MeSH values that have since been corrected, and figure S8A shows no significant correlation between cryptophyte abundance and either DMSw or MeSHw. The abstract and one discussion paragraph were changed

accordingly, but the claim is still made at lines 613-615 and again at lines 668-669, in the conclusions. The sentence at line 450 about cryptophytes responding positively to UV towards the end of the experiments has lost the figure it used to point to, so a reader has no way of checking it. Please either take cryptophytes out of the first two statements or give the coefficients that do apply, and restore a figure call-out for the third.

Thank you for checking this carefully, we removed mentions of cryptophytes and also now nanophytoplanktons correlating with MeSHw and DMSw in the text. We also added cryptophytes time series in Figure S.4 in place of the old figure containing cryptophytes. Changes are detailed below in specific comments.

The claim that nanophytoplankton abundance predicts seawater DMS better than chlorophyll a is now made without any supporting analysis. It appears at lines 625-627 and, in a stronger form, at lines 669-673, where the ratio of DMSw to nanophytoplankton abundance is said to vary less than the ratio to chlorophyll a, a comparison that is shown nowhere in the paper. The analysis that supported it, the former figure 13, has been deleted, and the reply states that the regression is not significant when it is not forced through the origin. I see three ways out. The authors can report the ordinary least squares regression with its intercept, R² and p value and let readers judge; they can show the ratio comparison described in the conclusions as a figure or a table with the corresponding test; or they can reduce both statements to something the POLAR-CHANGE data are consistent with, mentioning that Rocco et al. (2025a) found the relationship in a different plankton community. Keeping the conclusion after removing the evidence is the one option I would advise against.

We decided to remove all the mentions of nanophytoplankton abundance being a better DMS proxy than chlorophyll-a as the correlation during experiment disappeared when we added the p value correction for the Benjamini-Hochberg false discovery rate procedure.

The inverse relation between chlorophyll a and the two sulfur volatiles is still not discussed. Figure S8A gives Spearman coefficients of about -0.8 for chlorophyll a against DMSw and -0.67 against MeSHw during the experiments, and two of the phytoplankton size classes behave in the same way. This is an interesting result and it deserves a couple of sentences, particularly because positive chlorophyll a to DMSP relations are commonly found in Antarctic coastal and glacially influenced waters, where the DMSP pool follows biomass while DMS does not (Li et al., 2024; Petrou et al., 2026). A decoupling between DMSP production and DMS accumulation through bacterial demethylation and photochemical loss would be a reasonable frame, and it would connect this result to the aqueous phase reactivity already discussed in section 4.1. The general caveat added at the beginning of section 4.2, that the correlations should be read individually and not as cause and effect, is fair, but it does not replace a discussion of the strongest signal in the correlation matrix of the authors themselves.

Thank you for this interesting suggestion, we agree that these negative correlations that stayed significant even after recalculating p value with Benjamini-Hochberg false discovery rate procedure are worth mentioning. We included the discussion of this part and modified the paragraph following your suggestion:

“None of the biological variables available showed a strong correlation with aqueous DMS and MeSH concentrations throughout the entire campaign when considering the continuous flow mode of our ASITs set-up, except with Chl-*a* and bacterial abundances during the open ocean period. The non-systematic

measurement of diatoms and dinoflagellates, known emitters of DMSP (Wittek et al., 2020; Sheehan and Petrou, 2020; Shaw et al., 2022), could explain this lack of correlations. During the experiments, Chl-a was inversely correlated with both DMSw (Spearman $\rho = -0.81$) and MeSHw ($\rho = -0.67$), as were 1-2 and 15-20 μm SED nanophytoplankton size classes (Fig. S8A). This contrasts with the positive associations between chlorophyll a and DMSP, the main DMS precursor, reported in Antarctic coastal and glacially influenced waters (Li et al., 2024; Petrou et al., 2026), and points to a decoupling between phytoplankton biomass and dissolved volatile sulfur compounds, whose concentrations depend on the balance between DMSP release, cleavage and subsequent removal as discussed earlier. Given the absence of DMSP measurements in the ASITs, this interpretation remains hypothetical but it must be noted that significant positive correlation was observed between DMSP and nanophytoplankton (particularly cryptophytes) in sea surface samples of the same cruise (Wohl & Williams, 2026).”

Plankton groups that were not measured in the tanks

The material on the micro-phytoplankton community given in the reply is more informative than what reached the manuscript. The reply says that diatoms accounted on average for less than 40% of the micro-phytoplankton carbon while dinoflagellates accounted for more than 60%, and that the cruise probably took place during a diatom post-bloom, which would make dinoflagellates the likely dominant DMSP source in the tanks. That is a substantive argument and it belongs in the paper. The methods paragraph at lines 243-248 describes the Utermöhl analysis but gives none of its results, and the discussion still contains no statement of the limitation, namely that micro-phytoplankton was characterised in the underway samples and not inside the ASITs, so that the biological comparisons reported here concern the pico- and nanophytoplankton fractions only. A short paragraph in section 4.2 would settle this, and the wording of the abstract and of the conclusions should follow.

Thank you for your suggestion, we changed the materiel and methods to include the fact that the micro-plankton community was not consistently characterized in the ASITs.

“Pico-and nanophytoplankton abundance. Samples for photosynthetic pico- and nanophytoplankton abundances were collected on 5 mL cryovials, fixed with glutaraldehyde (1% final concentration) and frozen in liquid nitrogen for cell enumeration by Flow Cytometry following Vaultot et al. (1989). Cell abundances were detected with a 488 nm laser beam from their signatures in a plot of side scatter (SSC) versus green fluorescence (FL3), separating the picophytoplankton fraction of 1–2 μm (sphere equivalent diameter, SED), the nanophytoplankton fractions of SEDs of 2–7 μm , 7–15 μm , 15–20 μm and the cryptophytes size classes (*Cryptomonas* spp., identified by their phycoerythrin signal in the FL3 vs orange fluorescence FL2 plots).

Microplankton abundance. Some samples for the characterization of the microplankton (20 - 200 μm cell size; mainly diatoms and dinoflagellates) community were collected from the underway system (Rocchi et al., 2025) along the period where the ASITs experiments were run. Unfortunately, the samples were not collected consistently to track the changes inside the ASIT, and can only provide a rough idea of the microplankton taxa in the seawater during the POLAR CHANGE cruise. This does not allow us to take into account the contribution of microplankton to VOC emissions and the links to new particle formation. Samples were processed using the Utermöhl method on 125mL neutral Lugol fixed seawater (Edler and Elbrächter, 2010) and are presented in details in Rocchi et al. (2025). »

These results are for now only included in the correlation matrix. We added a sentence in section 4.2 to assess this problem:

“The non-systematic measurement of diatoms and dinoflagellates, known emitters of DMSP (Wittek et al., 2020; Sheehan and Petrou, 2020; Shaw et al., 2022), could explain this lack of correlations.”

Planktons mentions in the abstract and conclusion now mention that it refers to nanophytoplankton

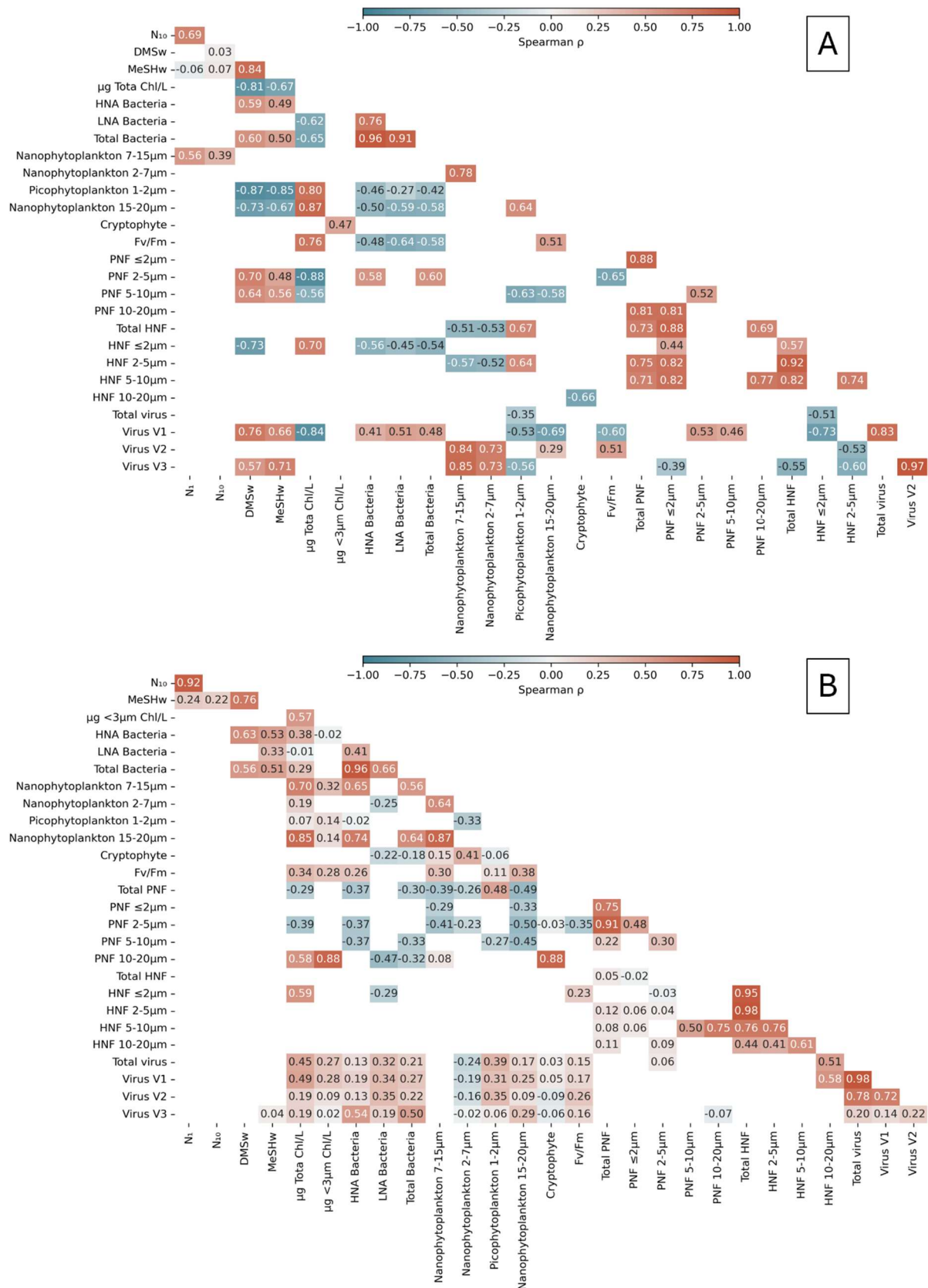
Derived concentrations and figure S8

The rewritten text at lines 194-206, explaining how the departures from equilibrium of 2.6 and 7.2 were obtained, is a clear improvement and was worth doing. Since every correlation in section 4.2 and in figure 12 uses DMS_w and MeSH_w calculated from headspace mole fractions rather than measured in the tanks, one sentence acknowledging that the uncertainty of these values is larger than that of a direct measurement, and that figure S7 shows offsets against the underway values at particular times, would set the reader's expectations correctly without weakening the analysis.

We added the following sentence: “Seawater concentrations of DMS in the ASIT (DMS_w) are calculated from headspace concentrations by using an average departure to the thermodynamical equilibrium, and therefore are more uncertain than direct measurements, as shown by offsets of the calculated values against the measured ones in the underway figure S7.”

Figure S8 carries a good part of the argument and needs a fuller caption: which coefficient is plotted, Spearman judging from the colour bar, the sample size behind each panel, what a blank cell means, the significance threshold used, and whether the p values are adjusted for multiple testing. As things stand a reader cannot tell whether a blank cell means that the correlation was not significant or that it was not computed, and that ambiguity is what produced the contradiction discussed above.

Thank you for this suggestion, the p-values for the correlation matrices (Fig. S8) were adjusted using the Benjamini-Hochberg false discovery rate procedure and the caption of Figure S8 was completed accordingly: “Spearman's rank correlation coefficient matrix obtained in both ASITs during experiments (A) and continuous flow mode (B). All measurements presented are taken in the water except for N1 and N10 particles (measured in the ASIT headspace). Cells are shown in white when the p-value is greater than 0.05 after correction for multiple comparisons using the Benjamini-Hochberg false discovery rate procedure (Benjamini and Hochberg, 1995).”



Benjamini, Y. and Hochberg, Y.: Controlling the False Discovery Rate: A Practical and Powerful Approach to Multiple Testing, Journal of the Royal Statistical Society Series B: Statistical Methodology, 57, 289–300, <https://doi.org/10.1111/j.2517-6161.1995.tb02031.x>, 1995.

Specific comments

Line 21. "are are of particular interest".

Now corrected, thank you.

Line 50. Raised in the first round and not addressed: if "the marine atmosphere" means the marine boundary layer here, please say so.

It here refers not only to marine boundary layer but also to all air-masses with marine origins. We changed this part of the sentence:

“Also, the role of DMS in marine aerosol formation processes via its oxidation to form sulfur dioxide (SO₂), sulfuric acid (H₂SO₄) and methanesulfonic acid (MSA), make it a key compound in the study of marine air masses and in the link to the formation of new particles in the CLAW hypothesis (Charlson et al., 1987).”

Line 95 and figure 1. The caption now explains that each point is an hourly position of the ship, which helps. My earlier request still stands, though. Since the track crosses itself several times, a supplementary table with the dates and the coordinates of the periods shown would let readers recover what the overlapping symbols hide.

The full track of the ship is available in the data and we didn't find a way to make it clearly readable in the figure when track crosses itself so we add in the caption this information: “The journey along the west coast of the Antarctic Peninsula was a round trip between the 21 and 26 of February 2023.”

Lines 243-248. The Utermöhl analysis and the carbon conversion are described but no result from them is given, and Rocchi et al. (2025) is cited here without appearing in the reference list.

We changed the material and methods paragraph to add that these analyses were not systematically done in the ASITs and the discussion on the limitation. These results are only included in the correlation Matrix (Fig. S8) for now. We also added Rocchi et al. (2025) to the reference list:

“Microplankton abundance. Some samples for the characterization of the microplankton (20 - 200 µm cell size; mainly diatoms and dinoflagellates) community were collected from the underway system (Rocchi et al., 2025) along the period where the ASITs experiments were run. Unfortunately, the samples were not collected consistently to track the changes inside the ASIT, and can only provide a rough idea of the microplankton taxa in the seawater during the POLAR CHANGE cruise. This does not allow us to take into account the contribution of microplankton to VOC emissions and the links to new particle formation. Samples were processed using the Utermöhl method on 125mL neutral Lugol fixed seawater (Edler and Elbrächter, 2010) and are presented in details in Rocchi et al. (2025). “

Rocchi, A., Fitzsimons, M. F., Akenga, P., Sotomayor, A., Sà, E. L., Güell-Bujons, Q., Vila, M., Castillo, Y. M., Dall'Osto, M., Vaqué, D., Wohl, C., Simó, R., and Berdalet, E.: Distribution of alkylamines in surface waters around the Antarctic Peninsula and Weddell Sea, <https://doi.org/10.5194/egusphere-2025-407>, 12 February 2025.

Line 244. The text cites Edler and Elbrächter (2010) while the reference list gives Karlson et al. (2010) for what seems to be the same IOC manual. Please reconcile the two.

Edler and Elbrächter (2010) is now correctly cited, thank you for reading this carefully.

Lines 284 and 286. Crabeck and Rocchi (in press) and Ferracci et al. (2024) are cited but not listed.

Both are now listed, thank you.

Crabeck, O., Rocchi, A., Wohl, C., Fitzsimons, M.F., Akenga, P.C., Sotomayor, A., Vila, M., Vaqué, D., Sà, E.L., Delille, B., Muller, S., Castillo, Y.M., Barthelmeß, T., Engel, A., Dall'Osto, M., Berdalet, E., R. Simó. 2025. Biogenic aerosol precursors in decaying Antarctic sea ice and brines in late summer. *Elementa: Science of the Anthropocene*, in press.

Ferracci, V., Weber, J., Bolas, C. G., Robinson, A. D., Tummon, F., Rodríguez-Ros, P., Cortés-Greus, P., Baccarini, A., Jones, R. L., Galí, M., Simó, R., Schmale, J., and Harris, Neil. R. P.: Atmospheric isoprene measurements reveal larger-than-expected Southern Ocean emissions, *Nat Commun*, 15, 2571, <https://doi.org/10.1038/s41467-024-46744-4>, 2024.

Lines 295, 491, 493 and 506. Salignat et al. (in press) is cited four times and the reference list contains only Salignat et al. (2025), which is the WRF-Chem study and a different paper. Please add the missing entry with its status at the time of submission.

Salignat et al. (2026) is now published, cited properly in the text and listed.

Salignat, R., Chamba, G., Paillet, L., Colomb, A., Leriche, M., Deguillaume, L., Rose, C., and Sellegri, K.: New Insights Into Nighttime Marine Sulfur and Nitrogen Chemistry and Related New Particle Formation Using the CLEPS Model, *JGR Atmospheres*, 131, e2025JD044824, <https://doi.org/10.1029/2025JD044824>, 2026.

Lines 327-333. Also raised in the first round. This passage on why the ASIT fluxes and the ambient air concentrations need not follow one another is interpretation rather than result, and would sit better in section 4.1, where it could be expanded and referenced.

We agree that these sentences are more discussion related than results but we kept them here because we think it's explaining why we displayed Ambient and ASIT molar fraction as well as fluxes and aqueous concentration in the same panel which might catch the reader's attention.

Lines 343-345 and figure 5. The text gives $R^2 = 0.88$ for the Control-ASIT and 0.81 for the noUV-ASIT while the fits now printed in panels A and B give 0.77 and 0.66. Please check which is right and make the two agree. The value quoted at line 514 for Rocco et al. (2025a) should be checked at the same time, since 0.81 also appears there.

Thank you for your attention: 0.88 and 0.81 were referring to correlation not including every period of measurement and were changed to 0.77 and 0.66.

The 0.81 value given at line 514 refer to the R^2 found by Rocco et al. (2025) in this previous experiment but refer to correlation in measured concentration and not fluxes. Text has been changed accordingly:

“A strong positive correlation was found in DMS and MeSH fluxes (Fig. 5), as also previously reported earlier by Novak et al. (2022) over non-enclosed coastal waters ($R^2=0.65$) and for molar ratio in previous ASIT measurements by Rocco et al. (2025a) ($R^2=0.81$ in the Control-ASIT). »

Line 352. "significantly different with Mann-Whitney U test". Please give U, the p value and the sample sizes, and say which pairs were compared, since four water types appear in figure 6.

We now provide U, p value and sample size focusing on comparison to expA/expB for both ASIT to compare series one by one with Mann-Whitney U test:

“However, the average $F_{ASIT-MeSH}/(F_{ASIT-DMS}+F_{ASIT-MeSH})$ in experiment B (with open ocean waters) was significantly higher than in experiment B (with coastal water types) in Control-ASIT (Mann-Whitney $U = 192$, $n_{expA} = 29$, $n_{expB} = 33$, $p = 5.46 \times 10^{-5}$, $r = 0.599$) and in noUV-ASIT (Mann-Whitney $U = 47$, $n_{expA} = 39$, $n_{expB} = 32$, $p = 2.70 \times 10^{-11}$, $r = 0.925$).”

Lines 354-357, caption of figure 6. The caption defines the boxplot conventions but says nothing about testing. If the differences among the four observational periods were tested, the test and the correction for multiple comparisons should appear in the caption; if they were not, the text should not call them significant.

The text was changed accordingly as answered to previous comment.

Line 361. "significantly stronger diel contrast in the Control-ASIT", with no test given here. The reply reports $p = 0.048$ with $r = 0.152$ for the day to night comparison, which is marginal and has a small effect size. If that is the test behind this sentence, it should be reported with those values and the wording adjusted accordingly.

We are not sure how to test the significance of differences in diurnal cycle so we presented the test for the comparison of both DMS and MeSH normalized concentration and calculated the hours of the days where the differences between both ASITs are the strongest with size effect and significant p value.

“The diel variation of the DMS mole fraction appear clearly when the fractions are normalized to the daily maxima molar fraction (Fig. S.5 A) with a decrease between 12:00 and 19:00 LT and an increase of the molar fractions in the hours following the decline of radiation level, with significantly stronger diel contrast in the Control-ASIT (paired t-test, $n=193$, $t=-4.95$, $p=1.6 \times 10^{-6}$), most notably around midnight (00:00 LT: mean difference = 0.13 ± 0.12 , $r=0.94$, $p=0.016$). The diel variation of MeSH molar fraction was even clearer with high fraction at night and decrease through the day (Fig. 7B and Fig S.5B). This pattern was marked in the noUV-ASIT both during day and night in molar fraction but is clearer for Control-ASIT when the fractions are normalized with significant differences (Wilcoxon signed-rank test, $n=193$, $W=6118$, $p=6.7 \times 10^{-5}$, $r=0.35$), particularly around 14:00 and 16:00 LT ($r=0.944$ and 1.00 , $p<0.05$) (Fig. S.5 B).”

Lines 390-395, figure 9. Thank you for stating in the caption that panels B and C use different axes.

The comparison that still has no test is the one between water types within each tank, which the between-tank test at lines 425-426 does not cover.

We added Mann-Whitney U test for this part, the tests were found significant even if the number of hours of measurement during exp B is low (10 hours):

“Moreover, experiments A and B showed significantly lower particle concentrations than with their respective seawater outside of the experiments (Mann-Whitney U = 3336, n1=96, n2=42, $p = 1.02 \times 10^{-9}$, $r = -0.655$ for difference between coastal seawaters and exp B; and Mann-Whitney U = 299, n1=41, n2=10, $p = 2.65 \times 10^{-2}$, $r = -0.459$ for difference between open ocean seawaters and exp B).”

Lines 421-422. The ozone differences of 37% and 53% between the two tanks during experiments A and B are given with no test and no measure of variability.

We completed the sentence with average ($\pm$ std) and a statistical test to assess that the difference was significant:

“During Experiment A, the noUV-ASIT exhibited on average 38% ($\pm 17\%$) lower ozone concentrations than the Control-ASIT (paired t-test, n=40, $t=13.99$, $p<0.001$), and during Experiment B this difference increased to 50% ($\pm 16\%$) (paired t-test, n=35, $t=18.92$, $p<0.001$).”

Lines 438-451. The responses of the phytoplankton groups to UV, including the different timing for the 2-7 μ m and the 7-20 μ m fractions, are described from figure 10 alone. Given how close the two curves run in several of the panels, these statements need testing before they can be phrased as they are.

This is true that no test can really support the statement for timing so we removed this sentence. We added Wilcoxon signed-rank test for differences between Control and noUV and completed with a description of UV light effect on phytoplankton by size, based on the statistical test we did both by experiment and with both experiments combined:

“Regarding the sensitivity to UV light, phytoplankton cell counts were significantly lower under UV exposure (Control-ASIT) than in the noUV-ASIT for the 1–2 μ m picoplankton and for the 2–7 μ m and 7–15 μ m nanophytoplankton, both when experiments were analysed individually (Wilcoxon signed-rank test, n=9; 1–2 μ m: $r=1.00$, $p=0.008$ in both experiments; 2–7 μ m: Exp A $r=0.91$, $p=0.023$, Exp B $r=0.87$, $p=0.039$; 7–15 μ m: Exp A $r=0.87$, $p=0.039$, Exp B $r=1.00$, $p=0.008$) or with both experiment combined (n=18; 1–2 μ m: $r=1.00$, $p<0.001$; 2–7 μ m: $r=0.88$, $p=0.003$; 7–15 μ m: $r=0.97$, $p<0.001$). In contrast, the 15–20 μ m nanophytoplankton size class showed no significant difference between treatments, suggesting that UV sensitivity was strongly size-dependent, with the smallest size classes being consistently and most strongly affected.”

Line 450. The claim about cryptophytes responding positively to UV has lost its figure call-out. Please restore it or drop the claim.

We precise that it occurs in the experiment B and referred to the figure S.4 that now contain cryptophytes:

In contrast, only cryptophytes experienced an apparent positive response to the UV light towards the end of the experiment B (Figure S.4) even though, in both experiments, UV light neither significantly favored nor disadvantaged the cryptophytes over the period considered.

Lines 459-470 and figure 11, caption at lines 473-475. The differences in Fv/Fm between the two tanks, and between the tanks and the underway water, are the least convincing of the untested comparisons, since the boxes in figure 11 overlap to a large extent. Please test them, and if they are not resolved say so. The diel pattern itself is clear and does not depend on the between-treatment claim.

We tested the differences between the two tanks and UW water and found them both significant at 12h even if, with only two experiments of two days each, the sample size is small. We precised it in the text:

“However, Fv/Fm values within the ASITs exhibited less variability throughout the day and were overall higher compared to the seawater from the underway system outlet, particularly around noon, with larger size effect in both ASIT compare to UW even with small sample size (Mann-Whitney $U=382$, $n_1=62$, $n_2=18$, $p=0.043$, $r=0.315$) for Control-ASIT Exp; $U=326$, $n_1=62$, $n_2=18$, $p=0.008$, $r=0.417$ for noUV-ASIT Exp).”

We also tested the differences between both experiments and found it non-significant so we modified the sentence:

“UV exposure did not seem to significantly impact phytoplankton photosynthetic efficiency as both Experiments A and B progressed, leading to marginally lower Fv/Fm in the Control-ASIT by the end of the experiment period (Fig. S.4G, H).”

Lines 613-615. Cryptophytes should be removed here or the statement supported, as discussed above.

Cryptophytes are now removed from this sentence: “Since cryptophytes, nanophytoplankton, and bacterial abundances showed some co-variation with these sulfur volatiles during the experiments,...”

Lines 625-627 and lines 669-673. The nanophytoplankton against chlorophyll a claim, as discussed above.

We dropped both these sentences as we do not bring any argument saying that nanophytoplankton is a better biological proxy than Chl-a anymore.

Lines 668-669. Cryptophytes again, this time in the conclusions.

We also removed it now, thank you for noticing it.

Line 677. "the increase of airs MeSH concentrations".

Now corrected, thank you.

Lines 691-697. Please move the description of the statistical analyses to the methods and add the Mann-Whitney U test and any correction for multiple comparisons. Virtanen et al. (2020) is cited at line 694 but not listed.

Virtanen is now correctly listed and the statistical test description was moved to the new section 2.4 in material and methods.

“2.4 Statistical analysis

Statistical tests were performed using Python (version 3.11) with the SciPy library (version 1.16.2; Virtanen et al., 2020) at a significance level of $\alpha = 0.05$. For paired measurements, normality of paired differences was first assessed with the Shapiro-Wilk test; when normality was not rejected, the paired t-test was applied, otherwise the non-parametric Wilcoxon signed-rank test was used, with effect size quantified by the rank-biserial correlation coefficient r . For independent, non-concurrent measurements, normality was assessed for each group separately, the Welch's t-test was applied when normality held for both groups and otherwise the Mann-Whitney U test was used, with effect size similarly quantified by the rank-biserial correlation coefficient r . When multiple comparisons were performed on the same variable, p-values were adjusted using the Bonferroni correction to control the family-wise error rate.”

Supplement. Two different figures are numbered S.5, the cryptophyte time series and the new normalised diel profiles, and the text calls on figure S.5 for both. The supplementary reference list also gives Wohl and Williams twice, as a preprint and as the published ACP paper, and a tracked change note in French has been left in the file.

The confusion comes from the document that we sent with tracked change on. It is now corrected as the time series of cryptophyte concentrations with DMS and MeSH dissolved concentrations were removed. Cryptophytes time series have now been added to Fig S.4.

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