An itemized response (blue words) to reviewers' comments and suggestions

We sincerely thank the reviewers for their valuable comments on our manuscript entitled "Impact of anticyclonic eddies on the spatial distribution and emission of non-methane hydrocarbons in the northern South China Sea" [Paper # egusphere-2026-1212]. In the revised manuscript, we have supplemented critical methodological descriptions and statistical details, expanded the background information, corrected unit inconsistencies in the equations, and discussed the limitations of this study. Imprecise or redundant statements have been revised throughout, and figures and tables have been refined to improve clarity and consistency. All modifications are highlighted in blue, and our specific responses to the reviewers' comments are presented as follows.

Reviewer 1:

General comments: This study was designed to investigate the impact of anticyclonic eddy systems on the distribution and oceanic efflux of an array of non-methane hydrocarbon compounds (NMHCs) and the projected atmospheric impact in the South China Sea. The measurements in this manuscript show that 1) the concentrations and oceanic emissions of NMHCs were significantly lower in eddy-influenced stations compared to the reference stations; 2) the photoproduction of NMHCs was slower in eddy-influenced waters compared to the reference waters. This work is of significance because the biogeochemical cycling of NMHCs and other volatile organic compounds (VOCs) has not been thoroughly studied in mesoscale eddies. However, the most critical issue with this work, in my opinion, is the lack of clarification on the method of statistical analysis on multiple occasions that has compromised the quality of this manuscript in telling a compelling story. That's being said, statistical analyses must be performed throughout the manuscript to truly tell if these differences are significant, particularly when error propagation is involved in this work. Please see the texts below for the major and minor issues that I believe need to be addressed for the manuscript to be of a better quality. Overall, I think this work is well designed and carries significant scientific merits by providing new knowledge to the study of oceanic NMHCs in the system of mesoscale eddies. Thereby I believe it's suited for publication in *Biogeosciences* after revision.

Reply:

We sincerely thank the reviewer for the positive assessment of our work. We have revised and improved the entire manuscript. Specifically, we have added a dedicated Statistical Analysis section (Section 2.7) describing the statistical methods used in this study. For all comparative statements, we now clearly indicate which statistical test was performed, the test statistic, and the *p*-value. Uncertainty reporting has been improved throughout, with all mean values now accompanied by standard deviations (SD) derived from either triplicate incubations or between-station variability.

Major issues and recommendation: 1) This study relies on the comparison made for the measurements between eddy-impacted water and reference waters. However, the uncertainty associated with these measurements and the method of statistical analysis are missing on many occasions in the manuscript. For instance, in the Abstract (Lines 16-19), the authors claimed that the difference between the two means of NMHC concentrations was significant. What statistical analysis was performed that led to this conclusion? What is the sample number? Can authors clarify if the data are normally distributed? This is just an example. Authors should check through manuscripts and ensure proper statistical analyses are coupled with comparisons of means.

Reply:

We thank the reviewer for raising this critical issue regarding statistical analysis and uncertainty reporting. For the comparison of NMHC concentrations between the eddy-controlled region ($n = 8$) and reference sites ($n = 17$), we first assessed data normality using the Shapiro-Wilk test and homogeneity of variances using Levene's test. Both assumptions were satisfied, and therefore a parametric independent-samples *t*-test was employed to compare the two means. The result showed a statistically significant difference between the two groups (t = 5.645, p < 0.001). In the revised Abstract, we have now included the *t*-value to provide greater transparency. Additionally, we have also reviewed the entire manuscript to ensure that all statistical comparisons are accompanied by appropriate information regarding specific statistical tests used. A detailed description of our statistical approach has been added to the Materials and methods section (Section 2.7 Statistical analysis).

Line 17-19:

"Significantly lower NMHC concentrations were observed in surface seawater within the eddy-controlled region (201 ± 101 pmol L^{-1}) relative to the reference sites (433 ± 62.5 pmol L^{-1}) (*t*-test: t = 5.645, p < 0.001)."

Line 217-226:

2.7 Statistical analysis

Normality of the data was assessed using the Shapiro-Wilk test, and homogeneity of variances was evaluated using Levene's test. For comparisons between two groups, parametric *t*-tests were applied when both normality and homoscedasticity assumptions were satisfied; otherwise, non-parametric Mann-Whitney *U*-tests were used. For comparisons involving more than two groups, one-way ANOVA was used when assumptions were met, followed by Tukey's post-hoc test for multiple comparisons; otherwise, the Kruskal-Wallis test was employed. For comparisons where sample sizes were too small for meaningful statistical significance testing, Cohen's d was calculated as a measure of effect size to quantify the magnitude of observed differences. A significance threshold of p < 0.05 was used in all analyses. Statistical analyses were performed via SPSS 25.

2) The authors should acknowledge that only four photochemical experiments were done in the field that a robust assessment cannot be made here in any capacity for the photoproduction rates of NMHCs between eddy-influenced and reference stations. I understand that field work is labor intense and it's of great value to collect preliminary results. For future, 1) more photochemical experiments should be carried out; 2) I recommend authors think about what the rationales are that a difference is expected for their photoproduction in eddy vs reference stations; 3) Authors should also measure the CDOM absorbance in these waters to see if there is a difference in absorbance; 4) The most effective approach to study spectral dependence of a photochemical process is to determine wavelength-dependent apparent quantum yields. The crude spectral study, using optical treatments, is less valuable as it's well known that marine photochemistry is a UV-controlled process particularly during field work at sea. I'd rather invest the valuable sea time in performing more photochemical experiments that would afford one a larger dataset.

Reply:

We thank the reviewer for these thoughtful and constructive comments. In the revised manuscript, we have added a paragraph in Section 4.2 to acknowledge the study limitations and address the reviewer's suggestions for future research. Specifically, we acknowledge that the observed spectral response patterns and comparisons of photoproduction rates should be considered preliminary, and we state that future investigations with a larger number of incubation experiments are needed to validate these findings. Regarding the rationales for expecting differences in photoproduction between eddy and reference stations, we think that anticyclonic eddies can alter the concentration and composition of chromophoric dissolved organic matter (CDOM) through physical processes such as horizontal advection and vertical mixing, and that eddy-induced variations in CDOM concentrations are expected to influence NMHC photoproduction rates. Therefore, we agree that future studies should measure CDOM concentrations to better understand the optical properties of eddy-influenced waters. Finally, we acknowledge that the most effective approach to studying spectral dependence is to determine wavelength-dependent apparent quantum yields, and we state that future research should employ monochromatic irradiation systems to allow a more rigorous assessment of spectral dependencies.

Line 359-371:

"We acknowledge that only four photochemical incubation experiments were conducted in this study, which limits the robust assessment of NMHC photoproduction rates between eddy-controlled region and reference stations. Consequently, the observed spectral response patterns and comparisons of photoproduction rates should be considered preliminary. Future investigations with a larger number of incubation experiments are needed to validate these findings. Furthermore, mesoscale eddies can alter the concentration and composition of chromophoric dissolved organic matter (CDOM) through physical processes such as horizontal advection and vertical mixing (Zarokanellos and Jones, 2021; García et al., 2026). Since CDOM is a key substrate for photochemical reactions, eddy-induced variations in CDOM concentrations are expected to influence NMHC photoproduction rates. Therefore, future studies should measure CDOM concentrations to better understand the optical properties of water masses influenced by eddies. Moreover, future research should employ monochromatic irradiation systems to determine wavelength-dependent apparent quantum yields, allowing a more rigorous assessment of spectral dependencies."

References:

García, J. L., Arístegui, J., Pazó, M. J., Vieitez dos Santos, V., Hernández-Hernández, N., Nieto-Cid, M., Gelado-Caballero, M. D., Martínez-Marrero, A., and Álvarez-Salgado, X. A.: Mesoscale and submesoscale variability of organic matter in island-induced eddies across their life cycle: implications for the biological carbon pump, Front. Mar. Sci., 13, 1711689, https://doi.org/10.3389/fmars.2026.1711689, 2026.

Zarokanellos, N. D. and Jones, B. H.: Influences of physical and biogeochemical variability of the central Red Sea during winter, J. Geophys. Res. Oceans, 126(3), e2020JC016714, https://doi.org/10.1029/2020JC016714, 2021.

Minor issues: The writing of this manuscript in English language could benefit from further polishing for a more concise and accurate communication of your research.

Reply:

Thank you for this helpful suggestion. We have carefully re-read the entire manuscript and made revisions to enhance the language quality. Specifically, we have removed vague or redundant expressions, simplified overly complex sentences, and improved the text logic.

Specific comments:

Line 16: Get rid of "particular"

Reply:

We thank the reviewer for the suggestion. We have deleted the word "particular" as suggested.

Line 15-17:

"Herein, we characterized the distributions and emissions of eight C_2–C_5 NMHCs in the South China Sea, with emphasis on the impacts of an anticyclonic eddy."

Line 17: Were these concentrations determined in water samples from the near-surface?

Reply:

Thank you for highlighting this ambiguity. The concentrations of NMHCs were determined in surface seawater samples at a depth of 5 m. We have revised the manuscript to specify the sampling layer.

Line 17-19:

"Significantly lower NMHC concentrations were observed in surface seawater within the eddy-controlled region (201 ± 101 pmol L^{-1}) relative to the reference sites (433 ± 62.5 pmol L^{-1}) (t-test: $t = 5.645$, $p < 0.001$)."

Lines 21-22: It's not that the flux dropped by 56% in the eddies. I believe you meant that the flux in the eddies is 56% lower than reference sites. Please have this sentence rewritten if that's true.

Reply:

Thanks for this comment. We have rewritten the sentence as follows:

Line 22-24:

"The sea-to-air fluxes of NMHCs within the eddy were 56% lower than reference sites, which reduce their potential contributions to ozone and secondary organic aerosol by 59% and 60%, respectively."

Line 29: Add a dot (•) to the side of OH to make a radical.

Reply:

Thank you for this careful correction. We have added a dot (•) to the side of OH to make a radical.

Line 30-31:

"Atmospheric NMHCs can participate in reactions with hydroxyl radicals (•OH) and significantly contribute to tropospheric ozone (O_3) production (Atkinson, 2000; Tran et al., 2013)."

Line 74: Get rid of "systematically", and I'd avoid this type of "empty" words in scientific writing.

Reply:

Thanks for this suggestion. We have deleted the word "systematically".

Line 85-86:

"Here, we investigated NMHC spatial distributions and emissions in the northern SCS, and specifically focused on their response to an anticyclonic eddy."

Line 76: Get rid of "formation".

Reply:

We thank the reviewer for the suggestion. We have deleted the word "formation".

Line 86-88:

"By further assessing the contribution of NMHCs to O_3 and SOA production, we evaluate the potential climate effects induced by mesoscale eddies through their modulation of oceanic NMHC fluxes."

Line 95: Add "photochemical" before incubation experiments.

Reply:

Thank you for this suggestion. We have added the word "photochemical" before "incubation experiments" in the caption of Figure 1.

Line 109-114:

"Figure 1. (a) Geographic location of the study area. (b) Locations of sampling stations during the cruise. Yellow squares indicate the stations for photochemical incubation experiments. The background color and vector arrows represent sea level anomaly (SLA, m) and the derived surface geostrophic currents (m s^{-1}) on 21 August 2021, respectively. Transect C that cutting through the core of anticyclonic eddy (AE) was chosen to analyze the sectional distributions of NMHCs. The data for SLA and surface geostrophic current were obtained from Copernicus Marine Service (https://marine.copernicus.eu)."

Line 93: Can you clarify what that 0.5 m/s on the figure to the right shows?

Reply:

Thank you for this suggestion. The arrow labeled "0.5 m/s" in the right panel of Figure 1 reflects flow rates of surface geostrophic currents. We have revised the figure caption to clarify this information.

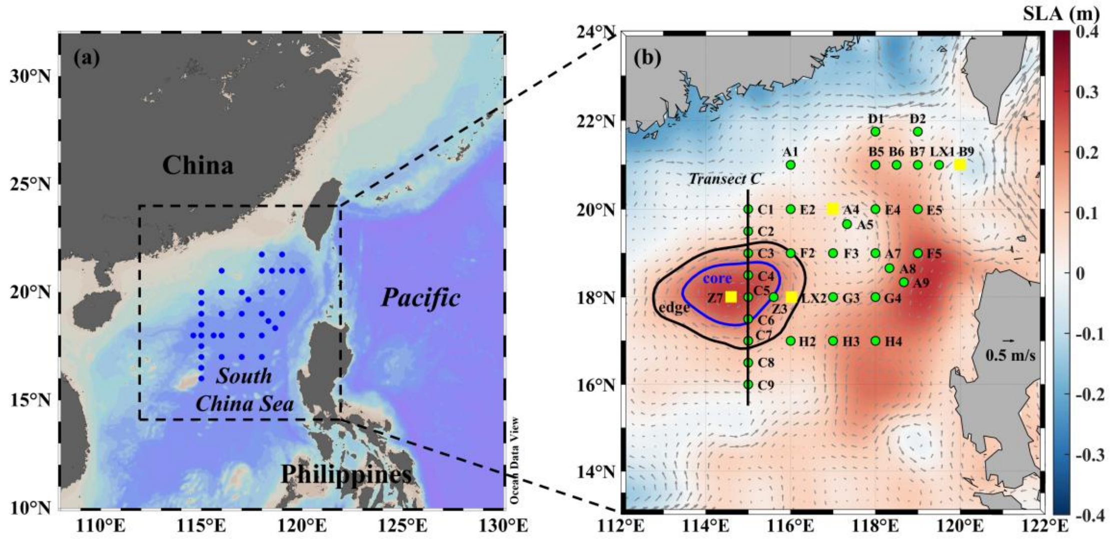

Figure 1. (a) Geographic location of the study area. (b) Locations of sampling stations during the cruise. Yellow squares indicate the stations for photochemical incubation experiments. The background color and vector arrows represent sea level anomaly (SLA, m) and the derived surface geostrophic currents (m s^{-1}) on 21 August 2021, respectively. Transect C that cutting through the core of anticyclonic eddy (AE) was chosen to analyze the sectional distributions of NMHCs. The data for SLA and surface geostrophic current were obtained from Copernicus Marine Service (https://marine.copernicus.eu).

Line 100: Just simply say "samples were collected in duplicate at 4 stations".

Reply:

We thank the reviewer for the suggestion. We have revised the sentence as recommended.

Line 116-117:

"To assess analytical variability, samples were collected in duplicate at 4 stations."

Line 144: Change "light" to optical.

Reply:

We thank the reviewer for the suggestion. We have changed "light" to "optical".

Line 160-161:

"These quartz tubes were subjected to four distinct optical treatments:"

Line 145: To my knowledge, the term “visible light” rather than PAR is used more often in photochemical studies.

Reply:

Thank you for this precise terminological suggestion. We have replaced “photosynthetically active radiation (PAR)” with “visible light”.

Line 163-164:

“(ii) wrapped with Mylar-D film to transmit radiation in the visible light (400–700 nm) and ultraviolet A (UVA, 320–400 nm) spectral bands.”

Lines 159-160: Are there any studies supporting this statement about photolysis of NMHCs in seawater? If so, provide the references. If not, get rid of this statement since it’s meaningless. I highly doubt any of these NMHCs photolyze in ambient seawater. What would be the photolysis mechanism(s)?

Reply:

We thank the reviewer for this comment. There are no relevant literatures reporting photolysis of NMHCs in seawater. Thus, we have removed the statement about “photodegradation of NMHCs may have occurred”.

Line 211: Authors should take time to verify the scales on the y-axes of these plots. As an example, the scale for the DOC plot seems to be too broad if you look at the color scheme in the plot. There is no dark red in the plot.

Reply:

Thank you for this comment. We have adjusted the color scale for the DOC plot to better match the data range. We have also verified the y-axis scales for all other plots in other figures to ensure that each scale appropriately reflects the range of the data.

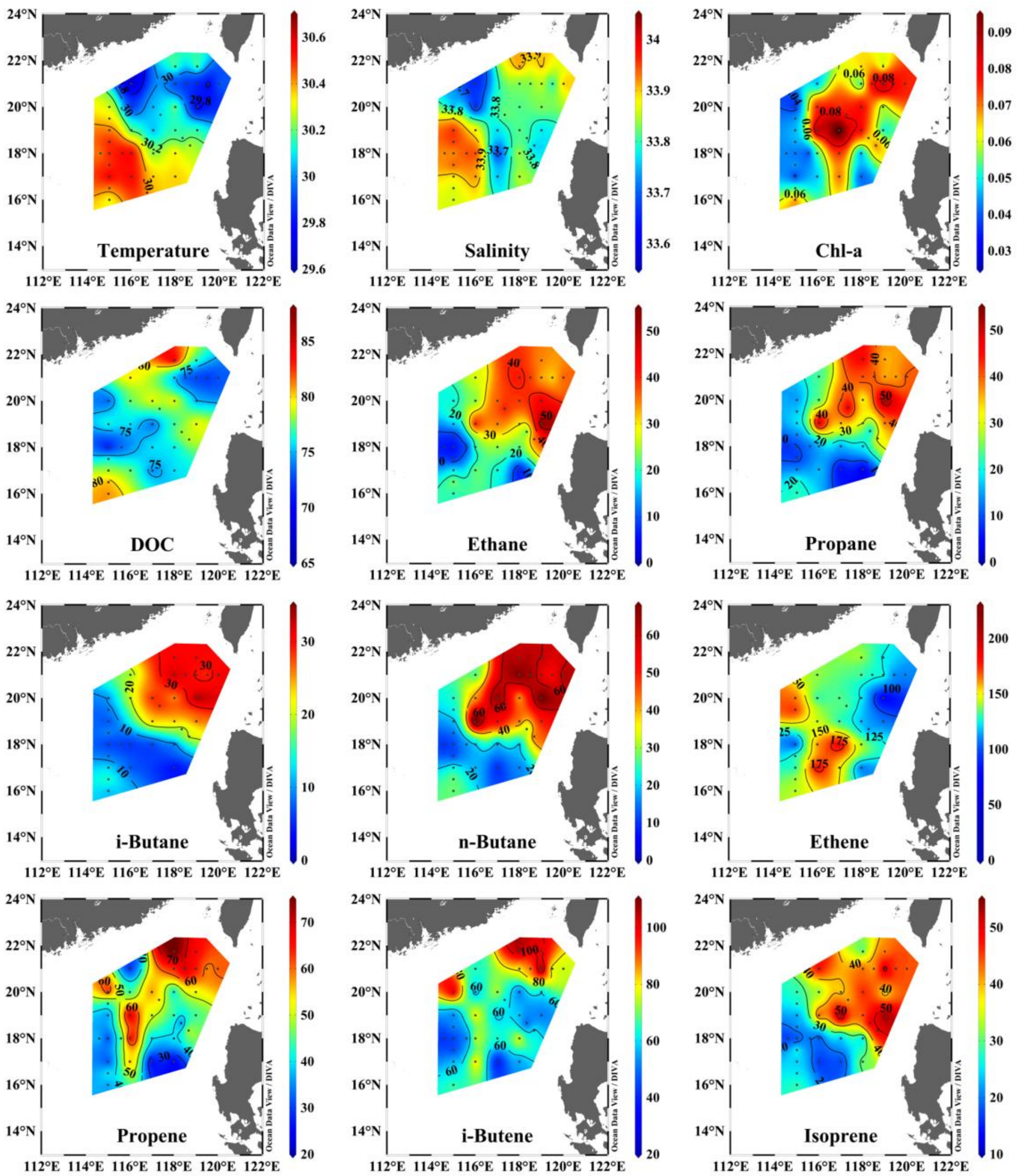

Figure 2. Horizontal distributions of temperature (°C), salinity, Chl-*a* ($\mu g\ L^{-1}$), DOC ($\mu mol\ L^{-1}$), and NMHCs ($pmol\ L^{-1}$) in surface seawater of the northern SCS.

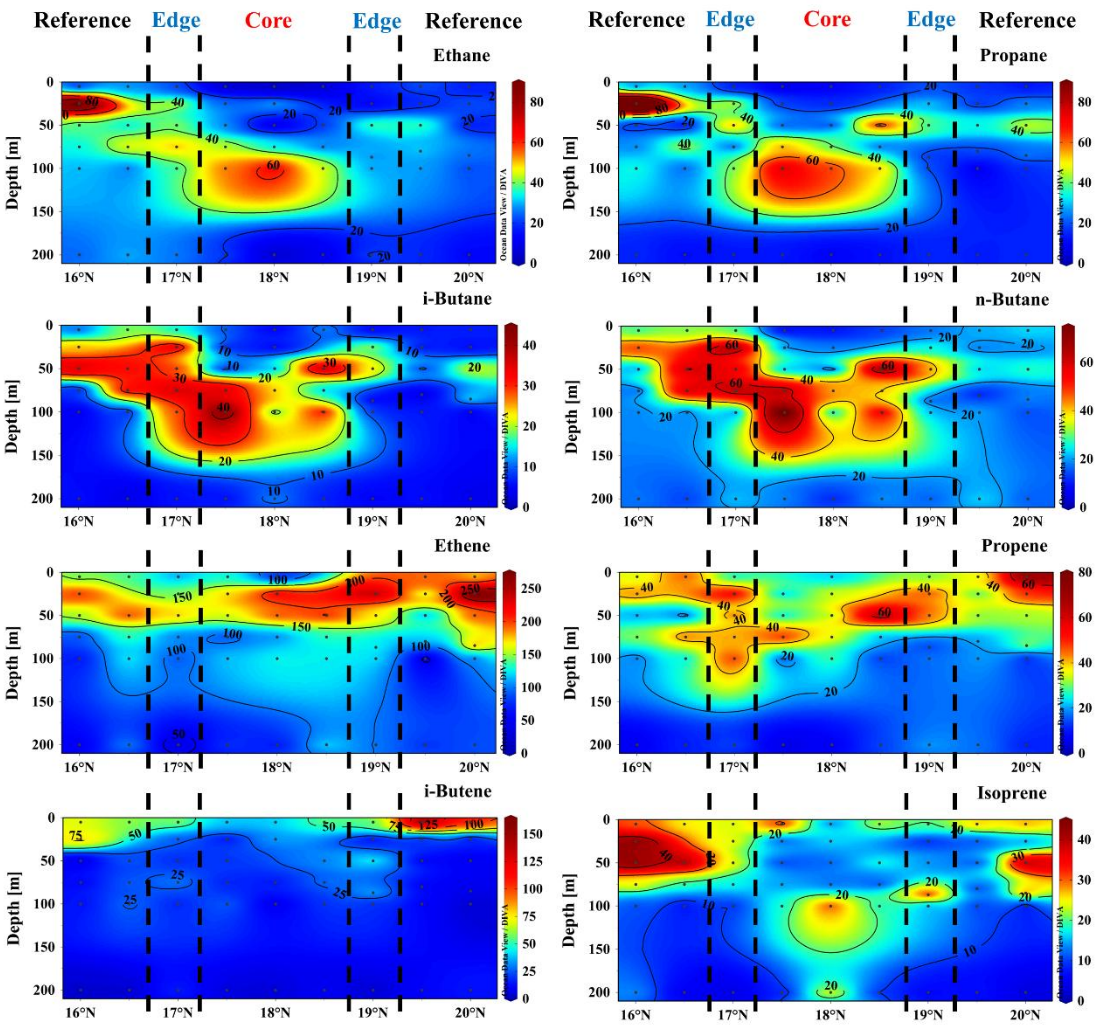

Figure 3. Vertical profiles of NMHCs (pmol L^{-1}) along transect C in the northern SCS.

Lines 218-219: please provide statistics for the DOC comparison like you did for Chl a. Are these two means truly statistically different?

Reply:

Thanks for the valuable comment. The DOC data in the eddy core include only two samples, which is insufficient for *t*-test. Therefore, we used Cohen's d to evaluate the magnitude of the difference. The calculated Cohen's d was 0.92 (large effect size), which suggests a substantial difference in DOC concentrations between the eddy core and the reference sites. This has been revised in the manuscript.

Line 244-246:

"Similarly, the distribution of DOC was also influenced by the AE, with lower values in the core of AE (72.2 ± 9.6 μmol L^{-1}) and higher concentrations at the reference sites (80.5 ± 8.9 μmol L^{-1}) (Cohen's d = 0.92, large effect size)."

Line 22, section 3.2: As a general comment, the concentration or standing stock of an NMHC species measured is always a net result of its production and removal. Are there any previous studies that investigated the impact of eddies on bacterial activity or more specifically on the consumption rates of NMHCs or any other biogenic products?

Reply:

We thank the reviewer for the insightful comment. We have added a discussion in the revised manuscript addressing the impact of eddies on bacterial activity and its potential effects on NMHC consumption rates. Specifically, previous studies demonstrated that mesoscale eddies can significantly alter bacterial community structure and activity (Sun et al., 2022; Villegas-Mendoza et al., 2022). More directly, studies from the South China Sea demonstrate that anticyclonic eddies significantly influence microbial consumption rates of biogenic sulfides and methane (Li et al., 2025; Wu et al., 2025). Therefore, the eddy-driven variations in bacterial community structure and activity may potentially impact the microbial consumption of NMHCs.

Line 328-334:

"It should be noted that the NMHC concentrations represent net values resulting from the balance between biological production and microbial consumption. Previous studies have shown that mesoscale eddies can significantly alter bacterial community structure and activity (Sun et al., 2022; Villegas-Mendoza et al., 2022), thereby influencing the microbial consumption rates of marine reactive gases such as dimethylsulfide and methane (Li et al., 2025; Wu et al., 2025). Therefore, the eddy-driven variations in bacterial community structure and activity may potentially impact the microbial consumption of NMHCs, and also contribute to the observed spatial patterns of NMHCs."

Reference:

Li, X. J., Li, S., Gao, X. X., Li, J. L., Xu, F., Zhou, C., Zhang, H. H., and Zhuang, G. C.: Regulation of mesoscale eddies on oceanic methane production, oxidation, and emissions, Glob. Biogeochem. Cycles, 39(10), e2025GB008500, https://doi.org/10.1029/2025GB008500, 2025.

Sun, F., Xia, X., Simon, M., Wang, Y., Zhao, H., Sun, C., Cheng, H., Wang, Y., Hu, S., Fei, J., and Wu, M.: Anticyclonic eddy driving significant changes in prokaryotic and eukaryotic communities in the South China Sea, Front. Mar. Sci., 9, 773548, https://doi.org/10.3389/fmars.2022.773548, 2022.

Villegas-Mendoza, J., Gómez-Ocampo, E., Velásquez-Aristizábal, J., and Rodríguez-Escobar, D.: Microbial metabolic activity in two basins of the Gulf of Mexico influenced by mesoscale structures, J. Mar. Syst., 234, 103781, https://doi.org/10.1016/j.jmarsys.2022.103781, 2022.

Wu, J. W., Xu, F., Yan, S. B., Xu, G. B., Jiang, Y. C., Li, X. R., Zhai, X., Zhou, L. M., Zhang, H. H., and Chen, Z. H.: Effect of anticyclonic eddies on the production and emission of marine dimethylsulfide in the northern South China Sea, Glob. Planet. Change, 252, 104883, https://doi.org/10.1016/j.gloplacha.2025.104883, 2025.

Lines 247-248: Make your writing concise by simply saying “In 0.2μm-filtered seawater”. In the method, you already clarified about the purpose of filtration.

Reply:

We thank the reviewer for this helpful suggestion to improve conciseness. We have revised the sentence as follows:

Line 274-275:

“In 0.2 μm-filtered seawater, the concentrations of NMHCs increased after 6 hours of solar irradiation (Fig. S2).”

Lines 252-254: what is the sample number n?

Reply:

Thank you for pointing out this missing information. The photochemical production rates were determined from incubation experiments conducted at four stations. We have revised the sentence to include the sample size as follows:

Line 277-281:

“The photochemical production rates of ethane, propane, i-butane, n-butane, ethene, propene, i-butene, and isoprene ranged from 2.5–3.1, 4.3–4.4, 2.4–3.4, 4.0–7.0, 24.1–38.8, 5.3–22.9, 12.2–15.3 and 2.6–4.7 pmol L^{-1} h^{-1}, with mean values of 2.8 ± 0.4, 4.4 ± 0.1, 2.9 ± 0.7, 5.5 ± 2.2, 32.2 ± 6.2, 18.1 ± 8.6, 13.8 ± 1.7, and 3.7 ± 1.5 pmol L^{-1} h^{-1}, respectively (n = 4).”

Lines 257-260: what is the sample number for eddy, eddy edge, and refence stations? Very important to show these information.

Reply:

Thank you for this important comment. The incubation experiments were conducted at four stations, comprising one eddy-core station, one eddy-edge station, and two reference stations. We have clarified this information in the text.

Line 284-289:

“The rates of ethene, propene, and i-butene in the eddy core (24.1 ± 2.6, 5.3 ± 0.6, and 12.2 ± 1.2 pmol L^{-1} h^{-1}, respectively; from one station with triplicate incubations) were lower than those at the eddy edge (ethene: 34.6 ± 2.2 pmol L^{-1} h^{-1}; propene: 21.5 ± 1.3 pmol L^{-1} h^{-1}; i-butene: 15.3 ± 0.7 pmol L^{-1} h^{-1}; from one station with triplicate incubations) and at the reference sites (ethene: 35.0 ± 5.4 pmol L^{-1} h^{-1}; propene: 22.8 ± 0.2 pmol L^{-1} h^{-1}; i-butene: 13.9 ± 2.1 pmol L^{-1} h^{-1}; mean ± SD from two stations) (Table 1).”

Line 266, Table 1: Change “natural light” to Full spectrum because that’s the comparison you are showing Full spectrum vs Specific spectral bands (e.g. UVB). Please clarify why there are these blank spaces. Experiments were not performed at these stations or data were problematic? Also, please provide the concentrations in your dark controls as it's so important to know the level of [ ]s in the dark. You said that triplicate quartz tubes were irradiated. Can you provide the uncertainty associated with these measurements? Can you actually say with confidence that the production in PAR is significant since the production is so low?

Reply:

We thank the reviewer for these constructive comments on Table 1. We have changed "natural light" to "Full spectrum". The blank spaces indicate that the photochemical incubation experiments with distinct spectral band treatments (visible light, UVA, UVB) were not performed at these stations. In the revised manuscript, we have replaced the blank spaces with "-" and added a footnote below Table 1. We have provided the concentrations in the dark controls and irradiated groups after 6 hours of incubation in the revised Table 1, and provided the uncertainty associated with triplicate measurements. Given the small sample size, we calculated Cohen's d as a measure of effect size to quantify the magnitude of the difference between visible light treatment and dark controls. The Cohen's d values ranged from 2.10 to 8.43 (large effect size) across different NMHC compounds, indicating substantial differences between the two treatments. Therefore, the observed production under visible light was significantly higher than that in the dark control.

Table 1. Concentrations of C_2–C_4 alkenes in 0.2 μm-filtered seawater from the northern SCS under dark control and irradiated conditions after 6 hours of solar exposure, and the calculated photochemical production rates (mean ± SD, n = 3).

Category	**Station**	**Species**	**Concentration (pmol L^{-1})**				**Photochemical production rate (pmol L^{-1} h^{-1})**			
			Dark	**Full spectrum**	**Visible light + UVA**	**Visible light**	**Full spectrum**	**UVB**	**UVA**	**Visible light**
Eddy core	Z7	Ethene	80.3 ± 1.9	225 ± 14.2	122 ± 8.5	85.1 ± 1.7	24.1 ± 2.6	17.1 ± 1.3	6.1 ± 1.7	0.8 ± 0.2
		Propene	47.9 ± 2.8	79.5 ± 6.5	64.4 ± 3.3	53.2 ± 2.2	5.3 ± 0.6	2.5 ± 0.9	1.9 ± 0.8	0.9 ± 0.4
		i-Butene	37.9 ± 1.4	111 ± 7.9	64.7 ± 4.0	46.9 ± 0.8	12.2 ± 1.2	7.7 ± 0.7	3.0 ± 0.8	1.5 ± 0.3
Eddy edge	LX2	Ethene	108 ± 3.6	315 ± 10.0	163 ± 7.9	120 ± 4.0	34.6 ± 2.2	25.4 ± 2.6	7.1 ± 1.5	2.1 ± 0.6
		Propene	69.0 ± 1.2	198 ± 8.9	120 ± 9.8	82.4 ± 1.9	21.5 ± 1.3	13.0 ± 1.5	6.3 ± 2.0	2.2 ± 0.4
		i-Butene	93.6 ± 4.4	185 ± 7.6	146 ± 8.7	108 ± 4.5	15.3 ± 0.7	6.5 ± 1.4	6.3 ± 0.7	2.5 ± 0.9
Reference site	A4	Ethene	111 ± 4.2	344 ± 14.3	–	–	38.8 ± 3.0	–	–	–
		Propene	83.8 ± 2.3	219 ± 7.5	–	–	22.6 ± 1.5	–	–	–
		i-Butene	80.3 ± 3.6	155 ± 17.9	–	–	12.4 ± 3.0	–	–	–
Reference site	B9	Ethene	91.9 ± 3.5	279 ± 13.0	–	–	31.2 ± 2.6	–	–	–
		Propene	77.0 ± 6.7	215 ± 9.3	–	–	22.9 ± 2.1	–	–	–
		i-Butene	68.8 ± 4.8	161 ± 15.0	–	–	15.3 ± 2.7	–	–	–

"–" indicates that the corresponding experiments were not conducted.

Line 281: these differences in Chl a are statistically different? They are definitely not substantially different.

Reply:

We thank the reviewer for this important statistical comment. We have calculated Cohen's d to quantify the magnitude of the observed differences in Chl-*a* concentrations. The results showed a Cohen's d of 1.08 between the eddy core and reference sites (large effect), and a Cohen's d of 0.55 between the eddy core and eddy edge (medium to large effect). These effect sizes indicate that the observed differences are indeed substantial. In the revised manuscript, we have reported the effect sizes.

Line 310-313:

"Moreover, Chl-*a* concentrations within the DCM layer of the eddy core (0.25 ± 0.11 μg L^{-1}) were substantially lower than those at the eddy edge (0.31 ± 0.09 μg L^{-1}) (Cohen's d = 0.55, medium effect size) and reference sites (0.34 ± 0.06 μg L^{-1}) (Cohen's d = 1.08, large effect size)."

Line 298: It's not entirely accurate to say "wavelength dependence" because the way you did in this work can only give you an idea of the crude spectral dependence. The best way to do it is using a monochromatic system or even a polychromatic system with narrower spectral range.

Reply:

We thank the reviewer for this methodological critique. We have revised the section title accordingly to better reflect what was actually done in this study. Additionally, we also acknowledge that a more precise determination of wavelength dependence would require monochromatic or polychromatic systems with narrower spectral ranges. In the future photochemical experiments, we will incorporate such improvements to resolve the wavelength-dependent production rates of C_2-C_4 alkenes more accurately.

Line 335:

4.2 Eddy-driven changes in alkene photochemical production and their spectral dependence

Reviewer 2:

This manuscript discussed the impact of an anticyclonic eddy on the distribution, production, and emission of NMHCs in the South China Sea. The study combines field observations and deck incubation experiments, providing valuable insights into the coupling between physical oceanographic processes and marine trace gas variability. However, several issues need to be addressed, particularly regarding the clarity of interpretation, the description of methodological details, and redundancy in certain parts of the text. Some conclusions require more precise wording to avoid potential overinterpretation. Overall, the manuscript is well structured, and the results are potentially of interest to the community. After these issues are carefully addressed, it could be suitable for publication.

Reply:

We sincerely thank the reviewer for the positive and constructive evaluation of our manuscript. We have carefully addressed all the issues raised by the reviewer, including clarifying interpretations, improving methodological descriptions, eliminating redundancy, and refining wording to avoid overinterpretation. We believe these revisions have significantly improved the quality of the manuscript.

Major comments:

Line 22: "which further diminished ozone and secondary organic aerosol generation by 59% and 60%" it may be misleading and could be interpreted as referring to total atmospheric O_3 and SOA production. It is more likely that these reductions apply only to the fraction attributable to NMHC emissions from the ocean. The authors should revise this statement to explicitly indicate that the reported decreases refer to NMHC-derived contributions, in order to avoid overinterpretation.

Reply:

Thank you for this important clarification. To avoid overinterpretation, we have revised the sentence to explicitly indicate that the reported reductions apply specifically to the NMHC-derived contributions.

Line 22-24:

"The sea-to-air fluxes of NMHCs within the eddy were 56% lower than reference sites, which reduce their potential contributions to ozone and secondary organic aerosol by 59% and 60%, respectively."

Line 49: "exerting substantial impacts on atmospheric chemical processes within the marine boundary layer." are there any specific impacts about atmospheric chemical processes from the reference? it seems repetitive of the statement in Line 28 the "exert substantial influences on atmospheric reactivity and global climate patterns". The authors are encouraged to either specify the distinct processes (e.g., OH reactivity, ozone formation pathways, etc.)

Reply:

Thanks for this constructive comment. We have added a brief description of how NMHCs affect atmospheric chemical processes in the marine boundary layer.

Line 51-55:

"Specifically, once released from the ocean into the atmosphere, NMHCs rapidly react with •OH, thereby influencing the overall oxidizing capacity of the MBL (Elshorbany et al., 2022). Furthermore, in the MBL of remote marine regions, NMHCs play an important role in the formation of organic aerosol and contribute to the occurrence of new particle formation events (Tripathi et al., 2024)."

References:

Elshorbany, Y., Zhu, Y., Wang, Y., Zhou, X., Sanderfield, S., Ye, C., Hayden, M., and Peters, A. J.: Seasonal dependency of the atmospheric oxidizing capacity of the marine boundary layer of Bermuda, Atmos. Environ., 289, 119326, https://doi.org/10.1016/j.atmosenv.2022.119326, 2022.

Tripathi, N., Girach, I. A., Kompalli, S. K., Murari, V., Nair, P. R., Babu, S. S., and Sahu, L. K.: Sources and distribution of light nmhcs in the marine boundary layer of the northern Indian Ocean during winter: implications to aerosol formation, J. Geophys. Res. Atmos., 129(3), e2023JD039433, https://doi.org/10.1029/2023JD039433, 2024.

Line 67-79: The background description of mesoscale eddies in the South China Sea is somewhat limited. Authors should provide more information about the eddies in SCS, like numbers, types, frequent, already reported eddies impacts on marine biogeochemistry, etc.

Reply:

We thank the reviewer for this constructive comment. The impacts of eddies on marine biogeochemistry have already been discussed in the preceding paragraph (Line 62-68). Therefore, we have added information on the numbers, types, and spatial distribution of eddies in the SCS, while avoiding repetition of biogeochemical impacts.

Line 79-85:

"Statistical analyses based on satellite altimetry data spanning nearly three decades have revealed that approximately 230 mesoscale eddies occur annually in the SCS, with cyclonic eddies (52.2%) slightly outnumbering anticyclonic eddies (47.8%) (Jin et al., 2024). Eddy activity exhibits notable spatial heterogeneity. Particularly active regions include the area west of the Luzon Strait, and the offshore region east of Vietnam (Wang et al., 2003). Some eddies in these regions are recurrent, appearing at similar locations and during similar months each year, and are classified as persistent strong eddies (Jin et al., 2024)."

References:

Jin, Y., Jin, M., Wang, D., and Dong, C.: Statistical analysis of multi-year South China Sea eddies and exploration of eddy classification, Remote Sens., 16(10), 1818, https://doi.org/10.3390/rs16101818, 2024.

Wang, G., Su, J., and Chu, P. C.: Mesoscale eddies in the South China Sea observed with altimeter data, Geophys. Res. Lett., 30(21), 2121, https://doi.org/10.1029/2003GL018532, 2003.

Line 89: more information about AE is needed. Clarify how to determined its life? What kind of data is depending on? “sampled at the end of its intensification” how many stages are typically defined in eddy evolution, and what criteria were used to define these stages in this study? A clearer description of the formation, evolution, and decay of the AE would strengthen the interpretation of the results.

Reply:

Thank you for this constructive suggestion. We have revised the manuscript to provide a clearer description of the formation, evolution, and decay of the AE, and have explicitly provided the criteria used to define its life stages.

Line 99-107:

“The AE was identified and tracked using satellite altimetry data from the Copernicus Marine Environment Monitoring Service (CMEMS), following the closed-contour method (Chelton et al., 2011), which defines an eddy by its outermost closed SLA contour. The eddy formed through a merger event at the end of July, when a closed SLA contour appeared. The AE was tracked continuously until its SLA signature could no longer be distinguished from the background field, yielding a total lifespan of approximately 100 days before its dissipation in early November. According to established eddy stage classifications defined by lifespan, for example, < 1/3, 1/3–2/3, and 2/3–1 for the intensification, mature, and decay stage, respectively (Sweeney et al., 2003; Zhou et al., 2020), AE was sampled at the end of its intensification stage.”

References:

Chelton, D. B., Schlax, M. G., and Samelson, R. M.: Global observations of nonlinear mesoscale eddies, Prog. Oceanogr., 91(2), 167–216, https://doi.org/10.1016/j.pocean.2011.01.002, 2011.

Sweeney, E. N., McGillicuddy, D. J., and Buesseler, K. O.: Biogeochemical impacts due to mesoscale eddy activity in the Sargasso Sea as measured at the Bermuda Atlantic Time-series Study (BATS), Deep-Sea Res. Pt. II, 50(22–26), 3017–3039, https://doi.org/10.1016/j.dsr2.2003.07.008, 2003.

Zhou, K., Dai, M., Xiu, P., Wang, L., Hu, J., and Benitez-Nelson, C. R.: Transient enhancement and decoupling of carbon and opal export in cyclonic eddies, J. Geophys. Res. Oceans, 125(9), e2020JC016372, https://doi.org/10.1029/2020JC016372, 2020.

Line 109: Only 8 species of NMHCs were measurement. I think it is crucial to point this study focus on limited species in the abstract, e. g., “eight C2-C5 NMHCs”, so that readers can better understand the scope of the results.

Reply:

Thank you for this helpful suggestion. We have revised the abstract to specify that our study focused on eight C_2–C_5 NMHC species.

Line 15-17:

“Herein, we characterized the distributions and emissions of eight C_2–C_5 NMHCs in the South China Sea, with emphasis on the impacts of an anticyclonic eddy.”

Minor issues:

Line 25: change “consequences” to effects.

Reply:

We thank the reviewer for the suggestion. We have changed “consequences” to “effects”.

Line 24-26:

“Overall, our findings elucidate the regulatory role of mesoscale eddies in NMHC dynamics, highlighting their critical function in shaping marine trace gas cycling and associated environmental effects.”

Line 53: “well established”? I think it is only well recognized that eddies have important influence on marine biogeochemistry.

Reply:

We thank the reviewer for this comment. We have revised the sentence by changing “well established” to “well recognized”.

Line 58-60:

“It is well recognized that mesoscale eddies significantly influence ocean biogeochemistry (McGillicuddy et al., 1998; Dai et al., 2020; Liu et al., 2020; Zhang et al., 2023; Zhou et al., 2023).”

Line 60: how to understand “reshape phytoplankton community dynamics”, delete “dynamics”

Reply:

Thanks for this suggestion. We have deleted the word “dynamics”.

Line 63-65:

“Conversely, by driving upwelling and transporting subsurface nutrients to the euphotic zone, cyclonic eddies may enhance primary productivity and reshape phytoplankton community (Zhou et al., 2020; An et al., 2024).”

Line 61: change “phytoplankton productivity” to “primary production” or “phytoplankton biomass”

Reply:

Thank you for this precise terminological suggestion. We have changed “phytoplankton productivity” to “primary production”.

Line 66-68:

“Therefore, mesoscale eddies can significantly modulate primary production within the euphotic zone and influence DOM concentrations and their associated carbon export flux (Zhou et al., 2013, 2020; Liang et al., 2025).”

Line 84: how can “survey period” revealed those? Rewrite this sentence.

Reply:

Thanks for this comment. We have rewritten the sentence as follows:

Line 94-95:

“During the survey period, sea surface height was higher in the southeastern and western areas than that in the central and northern areas.”

Line 119: provide the calibration curves in the SI

Reply:

Thanks for this suggestion. We have added the calibration curves for all eight target NMHCs to the supplementary information as Figure S1. Each curve shows the linear regression of peak area against concentration, with the corresponding regression equation and correlation coefficient.

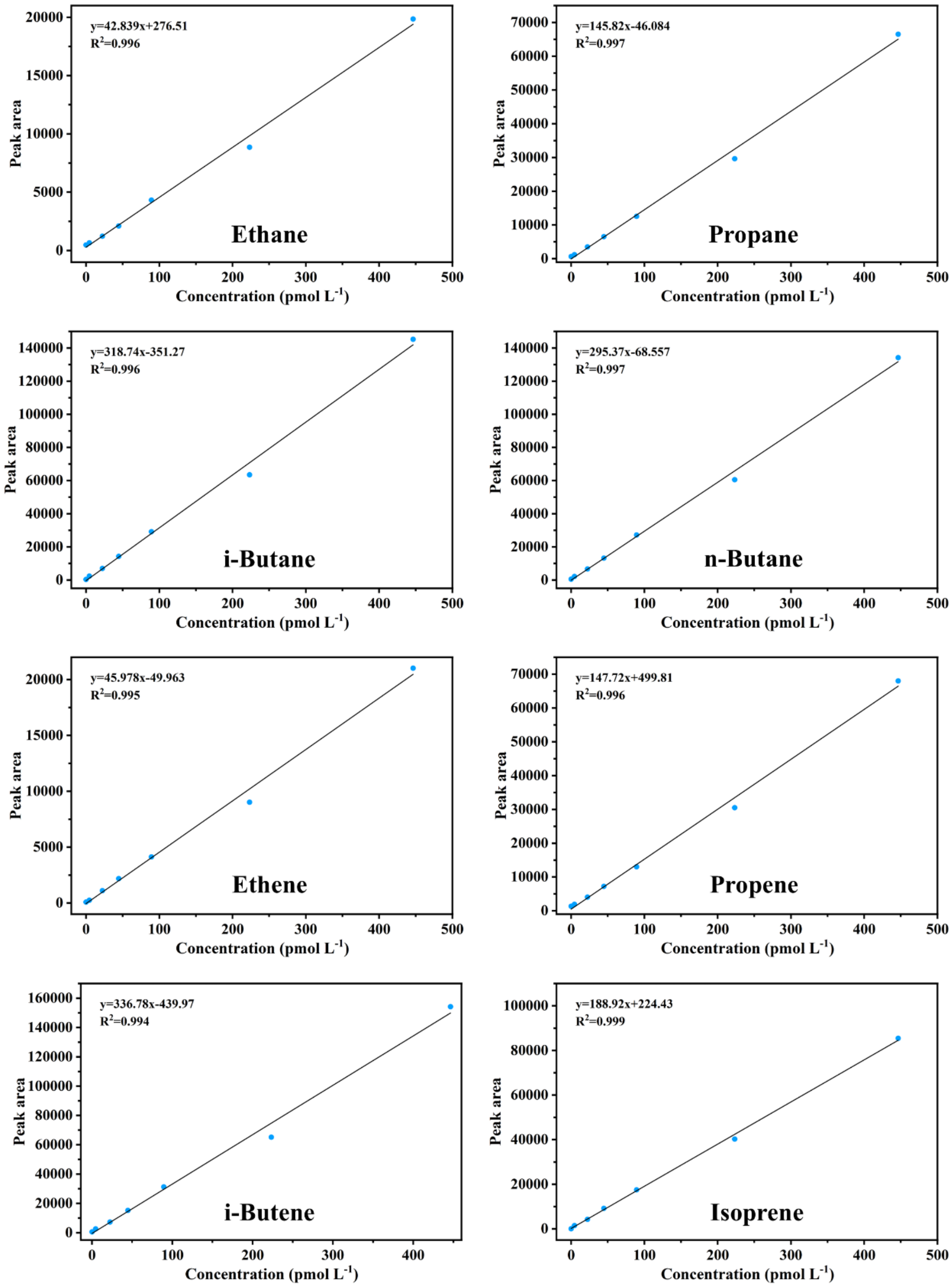

Figure S1. Calibration curves for the eight target NMHCs.

Line 223: "37.3 ± 25.1 129 ± 47.1," missed a ,

Reply:

We thank the reviewer for catching this typographical error. We have added the missing comma in the revised manuscript.

Line 250-251:

"with mean values of 26.7 ± 15.4, 26.1 ± 17.4, 19.5 ± 13.6, 37.3 ± 25.1, 129 ± 47.1, 48.6 ± 19.3, 69.7 ± 30.3, and 34.1 ± 15.3 pmol L^{-1}, respectively."

Line 225: "85.8%–383% higher than those of alkanes" chang it to "xx-xx times higher than"

Reply:

Thanks for this helpful suggestion. We have revised the sentence by changing "85.8%–383% higher than those of alkanes" to "0.9–3.8 times higher than those of alkanes".

Line 251-253:

"Alkenes accounted for a relatively large proportion of total NMHCs, with concentrations being 0.9–3.8 times higher than those of alkanes with the same carbon number."

Line 227: change "AE-dominated" to "AE-affected"

Reply:

Thanks for this suggestion. We have changed "AE-dominated" to "AE-affected".

Line 253-255:

"The distribution of NMHCs was also significantly influenced by the eddy, with their mean concentrations in the AE-affected area (201 ± 101 pmol L^{-1}) being notably lower than those at the reference sites (433 ± 62.5 pmol L^{-1}) (t-test: $t = 5.645$, $p < 0.001$)."

Line 250: "far exceeding the changes observed for other compounds" list the specific changes of other compounds.

Reply:

We thank the reviewer for this constructive suggestion. We have revised the sentence by adding the specific increase ranges for other compounds.

Line 275-277:

"C_2–C_4 alkenes exhibited the largest enhancement, with increases of 63%–210%, far exceeding the changes observed for other compounds (which increased by 25%–55%)."

Line 267: why are there so many empty cells in the table 1? Authors should provide the rates as mean ± standard deviation based on triplicate experiments.

Reply:

We thank the reviewer for these detailed and constructive comments on Table 1. The blank spaces indicate that the photochemical incubation experiments with distinct spectral band treatments (visible light, UVA, UVB) were not performed at these stations. In the revised manuscript, we have replaced the blank spaces with "-" and added a footnote below Table 1. For the experiments that were performed, we have now provided the rates as mean ± standard deviation based on triplicate incubations.

Table 1. Concentrations of C_2–C_4 alkenes in 0.2 μm-filtered seawater from the northern SCS under dark control and irradiated conditions after 6 hours of solar exposure, and the calculated photochemical production rates (mean ± SD, n = 3).

Category	Station	Species	Concentration (pmol L^{-1})				Photochemical production rate (pmol L^{-1} h^{-1})			
			Dark	Full spectrum	Visible light + UVA	Visible light	Full spectrum	UVB	UVA	Visible light
Eddy core	Z7	Ethene	80.3 ± 1.9	225 ± 14.2	122 ± 8.5	85.1 ± 1.7	24.1 ± 2.6	17.1 ± 1.3	6.1 ± 1.7	0.8 ± 0.2
		Propene	47.9 ± 2.8	79.5 ± 6.5	64.4 ± 3.3	53.2 ± 2.2	5.3 ± 0.6	2.5 ± 0.9	1.9 ± 0.8	0.9 ± 0.4
		i-Butene	37.9 ± 1.4	111 ± 7.9	64.7 ± 4.0	46.9 ± 0.8	12.2 ± 1.2	7.7 ± 0.7	3.0 ± 0.8	1.5 ± 0.3
Eddy edge	LX2	Ethene	108 ± 3.6	315 ± 10.0	163 ± 7.9	120 ± 4.0	34.6 ± 2.2	25.4 ± 2.6	7.1 ± 1.5	2.1 ± 0.6
		Propene	69.0 ± 1.2	198 ± 8.9	120 ± 9.8	82.4 ± 1.9	21.5 ± 1.3	13.0 ± 1.5	6.3 ± 2.0	2.2 ± 0.4
		i-Butene	93.6 ± 4.4	185 ± 7.6	146 ± 8.7	108 ± 4.5	15.3 ± 0.7	6.5 ± 1.4	6.3 ± 0.7	2.5 ± 0.9
Reference site	A4	Ethene	111 ± 4.2	344 ± 14.3	–	–	38.8 ± 3.0	–	–	–
		Propene	83.8 ± 2.3	219 ± 7.5	–	–	22.6 ± 1.5	–	–	–
		i-Butene	80.3 ± 3.6	155 ± 17.9	–	–	12.4 ± 3.0	–	–	–
Reference site	B9	Ethene	91.9 ± 3.5	279 ± 13.0	–	–	31.2 ± 2.6	–	–	–
		Propene	77.0 ± 6.7	215 ± 9.3	–	–	22.9 ± 2.1	–	–	–
		i-Butene	68.8 ± 4.8	161 ± 15.0	–	–	15.3 ± 2.7	–	–	–

"–" indicates that the corresponding experiments were not conducted.

Line 273: "Surface nutrients were nearly undetectable within the AE," if undetectable, say undetectable, else show the data

Thanks for this comment. We have revised the sentence by replacing "nearly undetectable" with the specific concentrations.

Line 302-303:

"Surface nutrients within the AE were very low (DIN: 0.30 ± 0.06 μmol L^{-1}; phosphate: 0.04 ± 0.03 μmol L^{-1}) but increased gradually with depth."

Line 281: provide the t-test results

We thank the reviewer for this important statistical comment. The Chl-*a* data for the DCM layer in the eddy edge and eddy core comprised only two and three samples, respectively, which are insufficient to perform a *t*-test. Therefore, we used Cohen's d to assess statistical significance. The results showed a Cohen's d of 1.08 (large effect size) between the eddy core and reference sites, and a Cohen's d of 0.55 (medium effect size) between the eddy core and eddy edge. These effect sizes indicate that the observed differences are indeed substantial. In the revised manuscript, we have reported the effect sizes.

Line 310-313:

"Moreover, Chl-*a* concentrations within the DCM layer of the eddy core (0.25 ± 0.11 μg L^{-1}) were substantially lower than those at the eddy edge (0.31 ± 0.09 μg L^{-1}) (Cohen's d = 0.55, medium effect size) and reference sites (0.34 ± 0.06 μg L^{-1}) (Cohen's d = 1.08, large effect size)."

Line 300:" because the zonal extent of the study region was only 4°" It would be more intuitive to express the zonal extent in kilometers or nautical miles rather than 4°.

Thanks for this helpful comment. We have rewritten the sentence as follows:

Line 336-339:

"However, because the maximum latitudinal extent between the incubation stations was only approximately 330 km, the variations in solar irradiance among stations during the incubation period were minimal (852–895 W m^{-2}), which was insufficient to explain the inter-station differences."

Line 306: provide the t-test results

We thank the reviewer for this comment. The DOC concentrations reported here are single-point measurements at each station, which precludes appropriate significance testing. Therefore, we have deleted the word "markedly", and the sentence now simply reports the observed values without statistical claims.

Line 342-344:

"DOC concentrations at the eddy core (Z7: 62.6 μmol L^{-1}) were lower than those at the eddy edge (LX2: 89.8 μmol L^{-1}) and the reference sites (A4: 95.5 μmol L^{-1}; B9: 84.6 μmol L^{-1})."

Line 323: "Assessing the impact of NMHCs on global climate feedback requires precise estimates of their sea-to-air exchange fluxes." This sentence is meaningless in the beginning of the paragraph.

Thanks for this comment. We have deleted this sentence in the revised manuscript.

Line 348: change to "demonstrating that the anticyclonic eddy suppressed the release of NMHCs."

Thank you for this precise suggestion. We have revised the sentence accordingly.

Line 388-391:

"Calculations revealed a mean NMHC sea-to-air flux of 196 ± 170 nmol m^{-2} d^{-1} within the AE, representing a 56% reduction relative to the reference sites (447 ± 289 nmol m^{-2} d^{-1}; Mann-Whitney *U*-tests, $p < 0.05$), demonstrating that the anticyclonic eddy suppressed the release of NMHCs."

Reviewer 3:

This paper reported the observation results of NMHCs in the south China Sea. Observation was well organized. NMHCs were lower concentration within the eddy-controlled region. This would be precious observation results.

Reply:

We sincerely thank the reviewer for the positive and encouraging evaluation of our work. We have carefully addressed all the comments.

OH reactivity, Ozone formation potential, and particle formation from the atmospheric light NMHCs were discussed. But the calculated values (L_{OH}, OFP, P_{SOAP}) were just compared with other observation sites. I think if the estimation of these values of DMS would be informative, especially P_{SOAP}. If DMS observation was not available, just assume suitable DMS concentration and compare these values. Then we can evaluate the importance of light NMHCs over the surface ocean.

Reply:

Thanks for this insightful suggestion. Based on the sea-to-air flux of DMS (3.66 ± 4.30 μmol m^{-2} d^{-1}) reported by Wu et al. (2025) and applying the same atmospheric lifetime approach used for NMHCs, we calculated the L_{OH} of DMS in the marine boundary layer. The calculation shows that the average L_{OH} of the eight NMHCs (0.00863 s^{-1}) is approximately 10.5% of that of DMS (0.0825 s^{-1}). However, due to the absence of *MIR* and *SOAP* values for DMS in the existing literature, we were unable to further calculate the *OFP* and P_{SOAP} for DMS using the same methodology applied to NMHCs. Thus, we added a comparison of L_{OH} between NMHCs and DMS in the revised manuscript.

Line 413-418:

"To evaluate the relative importance of NMHCs in MBL chemistry, we compared their •OH reactivity with that of dimethylsulfide (DMS), the most abundant biogenic sulfur compound emitted from the ocean. Based on the DMS sea-to-air flux (3.66 ± 4.30 μmol m^{-2} d^{-1}) reported by Wu et al. (2025) for the northern SCS and applying the same atmospheric lifetime approach used for NMHCs, we calculated the average L_{OH} of DMS to be 0.0825 s^{-1}." In comparison, the average L_{OH} of the eight NMHCs measured in this study was 0.00863 s^{-1}, which is approximately 10.5% of the DMS value."

References:

Wu, J. W., Xu, F., Yan, S. B., Xu, G. B., Jiang, Y. C., Li, X. R., Zhai, X., Zhou, L. M., Zhang, H. H., and Chen, Z. H.: Effect of anticyclonic eddies on the production and emission of marine dimethylsulfide in the northern South China Sea, Glob. Planet. Change, 252, 104883, https://doi.org/10.1016/j.gloplacha.2025.104883, 2025.

Line 164-167: In the explanation of Eq. (1), units of Fux, k, Cw, Ca, (and H) are given (pptv etc.). These are correct for dimension. But these are not directly to use for calculation.

Reply:

We thank the reviewer for this technical comment. We have revised the manuscript by converting all parameters into consistent units that can be directly used in the calculation. Specifically:

C_w (seawater concentration): 1 pmol L^{-1} = 1 nmol m^{-3}, so C_w in pmol L^{-1} is numerically equal to C_w in nmol m^{-3}.

C_a (atmospheric mixing ratio): C_a (pptv) is converted to nmol m^{-3} using the ideal gas law ($PV = nRT$). Under standard atmospheric conditions (P = 1 atm, T = 298.15 K), 1 pptv = 0.0409 nmol m^{-3}. Thus, C_a (nmol m^{-3}) = C_a (pptv) × 0.0409.

H (Henry's law constant): When C_w and C_a are both expressed in nmol m^{-3}, H must be dimensionless. The dimensionless Henry's law constant (H) is calculated as $H = H_0 \times R \times T$, where H_0 (M atm^{-1}) is the original Henry's law constant, R = 0.082057 L atm mol^{-1} K^{-1}, and T is temperature (K). For T = 298.15 K, $H = H_0 \times 24.465$.

k (gas transfer velocity): k (cm h^{-1}) is converted to m d^{-1} by multiplying by 0.24 (since 1 cm h^{-1} = 0.01 m h^{-1} × 24 h d^{-1} = 0.24 m d^{-1}).

With these conversions, Eq. (1) becomes: F (nmol m^{-2} d^{-1}) = k (m d^{-1}) × [C_w (nmol m^{-3}) - C_a (nmol m^{-3}) × H], where all units are directly compatible for calculation.

Line 183-184:

"where k (m d^{-1}) is the gas transfer velocity; C_w (nmol m^{-3}) is the seawater NMHC concentration; C_a (nmol m^{-3}) is the atmospheric NMHC concentration; and H (dimensionless) is Henry's law constant."

This is also for Eq. (6). (NMHC concentration would be molecules/cm^3 for calculation.)

Reply:

Thanks for this comment. We have revised the manuscript by converting the NMHC concentration from mol m^{-3} to molecules cm^{-3}. The conversion is performed as follows: 1 mol m^{-3} = 10^{-6} mol cm^{-3} = 10^{-6} × 6.022 × 10^{23} molecules cm^{-3} = 6.022 × 10^{17} molecules cm^{-3}.

Line 204-205:

"$NMHC_i$ (molecules cm^{-3}) is the concentration of atmospheric NMHCs"

Also same for Eq (7), Eq (8). (the unit of NMHC concentration should be micro g/m^3 for OFP etc.)

Reply:

Thanks for this comment. We have revised the manuscript by converting the NMHC concentration from mol m^{-3} to μg m^{-3}. The conversion is performed as follows: 1 mol

m^{-3} = M g m^{-3} = M × 10^6 μg m^{-3}, where M represents the molecular weight of each individual NMHC species.

Line 213:

"where *$NMHC_i$* (μg m^{-3}) is the concentration of atmospheric NMHCs"

Question about Deck incubation experiments.

In Table 1, there are four "Photochemical production rates": natural light, UVB, UVA, PAR

"natural light" seems to be the sum of UVB + UVA + PAR.

There are four experiments (Line 144-148).

(1) Natural light

(2) PAR + UVA

(3) PAR

(4) (dark)

Do you deduce the "Photochemical production rates" as follows?

Natural light: (1)-(4)

UVB: [(1)-(4)] – [(2)-(4)]

UVA: [(2)-(4)] – [(3)-(4)]

PAR: (3)-(4)

Reply:

Thanks for the question regarding the calculation of photochemical production rates from the deck incubation experiments. We confirm that the reviewer's understanding is exactly correct. We have added a clear description of this calculation method in the revised manuscript.

Line 172-175:

"Specifically, the visible light production rate was obtained as (iii) − (iv); the UVA production rate was obtained as (ii) − (iii); the UVB production rate was obtained as (i) − (ii); and the full spectrum production rate was obtained as (i) − (iv)."