Response to the Comments of the Reviewers

Dear Editor and Reviewers,

We acknowledge the comments and encouragement of two reviewers, and are also grateful to the efficient serving of the editor. Here we submit our revised manuscript **"Measurement report: VOC characteristics at different land-use types in Shanghai: spatio-temporal variation, source apportionment, and impact on secondary formations of ozone and aerosol" (Manuscript number: acp-2022-250)**, as well as a thorough, point-by-point response to each point raised from the reviewers. The revisions to the manuscript are highlighted in blue words in the provided "Response to the Comments of the Reviewers". Additionally, there is a clean revised manuscript as required.

We greatly appreciate those comments and valuable suggestions from the reviewers. The manuscript has been greatly improved. We do feel that we have demonstrated our efforts in the revised manuscript.

Yours sincerely,

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Response to the Reviewer #2

General Comments:

The manuscript presents a study based on the concurrent observations of volatile organic compounds (VOCs) at three supersites sites in Shanghai. The characteristics of VOCs, ozone formation potential, secondary organic aerosol formation potential and emission sources are discussed. The influence of different land use type on VOC profiles and atmospheric oxidation capacity is worthy of study. However, the discussion in this manuscript is not enough to highlight this viewpoint. The author should focus more on the discussion of the influence of land use type on VOC profile and atmospheric oxidation capacity. Moreover, I find irrationality of the methods and inaccuracy of some conclusion. Also, the grammar needs to be thoroughly revised. Generally, under its current version, this paper needs substantial revision to reach the standard for publication. However, the scope of this manuscript is good and the measurements can provide deeper understanding of the influence of anthropogenic activities on megacities or city clusters. A revised edition is encouraged for resubmission.

Response: We would like to thank reviewer #2 for your carefully review and valuable and constructive comments. Promoted by your suggestions, we have made significant efforts to improve the logicality and readability of the paper. The details can be seen in the revised manuscript.

Comment 1: As the author have stated in Line 110-113: "limited knowledge is available on the multi-site research at a city level .....", authors should well explain how does multi-site observation bring us new insights different from single site observations. Furthermore, to my knowledge, there are a lot of studies about VOC characteristics in Shanghai during different measurement period, at different locations and at different years. The authors should also elucidate how this work bring new insights different from just comparing the reported results. Finally, the authors should rearrange the whole manuscript emphasizing on the difference of the observation results of three sites and the discussion about the influence of different land use type.

Response:

Thank you for your comment. We rewrote the description in the revised manuscript.

Lines 35-36:

> *"The findings here provide more information on the accurate air-quality control at a city level in China."*

The point in this study is to analyze the influence of land-use types on VOC concentrations, sources and ozone and SOA formation potentials in Shanghai. In detail, the long-term VOC emission inventory highlighted that the VOC emission varied with the land-use type (Li et al., 2019). The observation campaign also showed that VOC concentrations were largely influenced by the land-use type (Tang et al., 2008; Kumar et al., 2018; Zhang et al., 2018). Besides, the land-use types not only influence the VOC concentrations but also the sources especially the anthropogenic sources (Yoo et al., 2015; Chen et al., 2017; Wang et al., 2017; Jookjantra et al., 2022). Additionally, the diversities of VOC concentrations among the different land-use types could affect the ozone and SOA formation potentials (OFP and SOAFP), resulting in the variations of O_3 and SOA concentrations (Song et al., 2021; Zhan et al., 2021; Liu et al., 2022). Shanghai is regarded as an ideal area to perform atmospheric measurements with the different land-use types. However, many studies were mainly focused on the single-site measurements, particularly conducted at the urban site in Shanghai, resulting that the impact of land-use type on VOC characteristics is still unclear to date. Besides, they mainly concentrated on the O_3 characteristics, while the O_3 and SOA formations from VOCs and the relationship between VOCs together with $PM_{2.5}$ and O_3 were rarely analyzed. Given the factors mentioned above, in this study, the concurrent multiple-site and high time-resolution measurement of the VOCs with three typical land-use types in Shanghai for their characteristics, sources and ozone and SOA formation potentials was performed. The result therein could broaden the city-scale research from single-site measurement to multi-site observations, and simultaneously narrows the multi-site research from worldwide scale to city scale.

We added the discussion about the effects of land-use types on VOC concentrations, sources and ozone and SOA formation potentials in Sec. 4 (lines 515-650).

Comment 2: The instruments applied at three sites are different. I do not think the comparison is convincing, without any illustration about the data reconciliation. Since the main scope is to compare the VOC data observed at different sites, the data quality is of great importance to the final conclusion. The authors should discuss more about the detect of limit, accuracy of all measured VOC species.

Response:

Thank you for your comment. The three instruments (GC5000, GC580+TD300, GC866) all can be used to analysis VOCs. Their actual attainments were very similar in practice.

By using GC580+TD300, all PAMS substances meet the standard "curve correlation coefficient ≥ 0.995", all substances meet the standard "precision ≤ 10%", more than 95% of the target compounds meet the standard "accuracy ≤ ± 20%", all target compounds meet the standard "detection limit ≤ 0.15ppb", and more than 90% of the target compounds have blank response less than 0.1 ppb.

By using GC 5000 BTX/VOC, more than 90% of PAMS substances meet the standard "curve correlation coefficient ≥ 0.995", all substances meet the standard "precision ≤ 10%", more than 95% of the target compounds meet the standard "accuracy ≤ ± 20%", more than 98% of the target compounds meet the standard "detection limit ≤ 0.15ppb", and more than 95% of the target compounds have blank response less than 0.1 ppb.

By using GC866, more than 95% of PAMS substances meet the standard "curve correlation coefficient ≥ 0.995", all substances meet the standard "precision ≤ 10%", more than 95% of the target compounds meet the standard "accuracy ≤ ± 20%", more than 90% of the target compounds meet the standard "detection limit ≤ 0.15ppb", and more than 90% of the target compounds have blank response less than 0.1 ppb.

Because VOCs in three sites were collected and analyzed separately, we used the most suitable detection instruments for the three stations.

Comment 3: The authors should discuss more on how to determine the final PMF solution before discussing the PMF results.

Response:

Thank you for your comment. We added the description in the revised manuscript.

Lines 174-180:

"where MDL is the minimum detection limit, EF is the error fraction and can be set to 0.05-0.2 (Song et al., 2007). It was 0.1 in this study. In this study, four to eleven factors were utilized to determine the option solution. Q_{true}/Q_{robust} and and $Q_{true}/Q_{expected}$ are important parameters for characterizing the rationality of the PMF results (Brown et al., 2015). Seven factors were regarded as the optimal solution, comparing the ratios of Q_{true}/Q_{robust}, $Q_{true}/Q_{expected}$ and PMF results. The Q_{true}/Q_{robust} values were set to 1.0 at the three sampling sites. The $Q_{true}/Q_{expected}$ values were 1.3, 1.1, and 1.0 at the JS, PD and QP sites, respectively."

Comment 4: The term" secondary formation potentials" in the title is confusing, it will be better to use "ozone and SOA formation potential".

Response:

Thank you for your comment. We revised the title in the new manuscript.

"Measurement report: VOC characteristics at different land-use types in Shanghai: spatio-temporal variation, source apportionment, and impact on secondary formations of ozone and aerosol"

Comment 5: Line 29-30: The sentence "The VOCs-O_3 sensitivity indicated that VOCs-S_{O3} values varied at the different sites and were primarily controlled by the alkenerelated reactions". It is confusing at this place to see VOCs-SO_3 without illustrations of the methodologies. It will be better to just state the main collusion here.

Response:

Thank you for your comment. The calculation of VOC-O_3 sensitivity (VOCs-S_{O3}) was deleted after consideration.

Comment 6: Line 31-34: How the findings provide new insights into the accurate control of different land-use type? Moreover, how the results highlight the importance of multiple-site measurements?

Response:

Thank you for your comment. We rewrote the description in the revised manuscript.

Lines 35-36:

"The findings here provide more information on the accurate air-quality control at a city level in China."

The results herein could provide scientific-based information for policymakers to establish targeted strategies of alleviating VOC pollution at the different land-use types. For example, the JS site exhibited higher fractions of aromatics and alkenes, particularly toluene and propylene, than those at the PD and QP sites. The VOC concentration in the early morning (5:00 LT) at the JS site was higher than those at the other two sites. This result did necessarily correlate with the fact that the JS site is close to the industrial area with heavy industrial emissions, suggesting that industrial activities were key factors of VOC pollution at the JS site. Moreover, the industrial emission and biogenic source showed slight contributions to VOC concentrations at the QP and JS/PD sites, respectively. It was consistent with the regional characteristics of anthrogopenic activities dominated by land-use types. Additionally, the results of VOCs-$S_{PM2.5}$ varied with the land-use types. The aromatics at the JS and PD sites, as well as alkanes at the QP site played crucial roles in the VOC-induced haze pollution. The relevant emission sources, which are thought to be the industrial production at the JS/PD sites and vehicle exhaust at the QP site, should be controlled in priority. Therefore, these findings could provide more information on the accurate VOCs control. The results shown herein highlight that the simultaneous multiple-site measurements with the different land-use type in the megacity or city cluster could be more appropriate to fully understand the VOC characteristics relative to a single-site measurement performed normally.

Comment 7: Line 35-139 I do not think it is necessary to put some many basic knowledge in the introduction part.

Response:

Thank you for your comment. We rewrote the introduction in the revised manuscript.

For example, lines 55-57:

"The long-term VOC emission inventory highlighted that there were significant spatial discrepancies of VOC emissions (Li et al., 2019a). The observation campaign also showed that VOC concentrations varied with the sampling sites.

These phenomena were attributed to the fact that VOC concentrations were closely correlated with the land-use types."

Lines 63-65:

"Besides, the land-use types not only influence the VOC concentrations but also the sources especially the anthropogenic sources (Yoo et al., 2015; Chen et al., 2017; Wang et al., 2017; Jookjantra et al., 2022)."

Lines 72-73:

"The diversities of VOC concentrations among the different land-use types could affect the ozone and SOA formation potentials (OFP and SOAFP), resulting in the variations of O_3 and SOA concentrations."

Lines 77-78:

"In detail, atmospheric VOCs undergoes degradation to produce oxidants (HO_2 and RO_2), which further oxidizes atmospheric NO, followed by producing NO_2 and the formation of O_3 finally via the photochemical pathways (Wang et al., 2017). "

Lines 81-85:

"As the key precursor of SOA, VOCs can be oxidized to produce the low VOCs, followed by the formation of SOA via homogeneous nucleation (Merikanto et al., 2009). Moreover, the partitioning of semi-volatile products from VOCs and oxidants gas-phase photochemical reactions to form SOA (Pankow, 1994; Lim et al., 2010). Additionally, low VOCs are produced via the aqueous-phase reactions in atmospheric waters e.g., clouds, fogs, and aerosol water which are largely retained in the particle-phase to generate SOA (Lim et al., 2010)."

Lines 87-94:

"The expanding urbanization and industrialization jointly aggravate the VOC pollution. Moreover, the O_3 concentration at the urban area in Shanghai increased by ~ 67 % from 2006 to 2015 with the growth rate of 1.1 ppbv pear year (Gao et al., 2017). The maximum 1-hour concentration of O_3 could exceed 380 μg m^{-3} during polluted days (Shi et al., 2015; Gu et al., 2020). Such scenario suggested that O_3 played an important role in atmospheric pollution and

Shanghai was suffering from heavy O_3 pollution. Additionally, the large changes of land-use occurred in Shanghai due to the rapid development e.g., many cultivated areas became urban and/or industrial zones, resulting in the diverse land-use types (Tian et al., 2017). Therefore, Shanghai is regarded as an ideal area to perform atmospheric measurements with the different land-use types."

Comment 8: Line 140-168: It will be much easier to compare the difference of three sites, if the authors can make a table about potential sources at different sites, and the results of published works conducted at the three sites (if there were).

Response:

Thank you for your comment. We added a table in the revised supporting information.

Table S1. The sources and land-use type at the sampling sites.

Sites	Land-use type	Details	References
Jinshan Site (JS)	Industrial district	Site surrounded by chemical factories	Zhang et al., (2018)
Pudong Site (PD)	Residential and commercial mixed districts	Site surrounded by residences and administrative areas	Cai et al., (2010b)
Qingpu Site (QP)	Background district	Site surrounded by farmland and forests	Zhang et al., (2020)

Comment 9: Line 177: what's TD300?

Response:

Thank you for your question. We added the illustration in the revised manuscript.

"At the PD site, VOCs was measured by gas chromatography (GC580-FID, PE, USA) and TD300 (a transformer driver)."

Comment 10: Line 181-183: R^2 of what? Calibration results? Please clarify. Odd expression about "accuracy of 95% of compounds". What about the accuracy of the left 5%?

Response:

Thank you for your question. R^2 is the curve correlation coefficient. We revised the sentence in the new manuscript.

Line 145:

"The curve correlation coefficient (R^2) of all of the VOCs were ≥ 0.995."

By using GC580+TD300, all PAMS substances meet the standard "curve correlation coefficient ≥ 0.995", all substances meet the standard "precision ≤ 10%", more than 95% of the target compounds meet the standard "accuracy ≤ ± 20%", all target compounds meet the standard "detection limit ≤ 0.15ppb", and more than 90% of the target compounds have blank response less than 0.1 ppb.

By using GC 5000 BTX/VOC, more than 90% of PAMS substances meet the standard "curve correlation coefficient ≥ 0.995", all substances meet the standard "precision ≤ 10%", more than 95% of the target compounds meet the standard "accuracy ≤ ± 20%", more than 98% of the target compounds meet the standard "detection limit ≤ 0.15ppb", and more than 95% of the target compounds have blank response less than 0.1 ppb.

By using GC866, more than 95% of PAMS substances meet the standard "curve correlation coefficient ≥ 0.995", all substances meet the standard "precision ≤ 10%", more than 95% of the target compounds meet the standard "accuracy ≤ ± 20%", more than 90% of the target compounds meet the standard "detection limit ≤ 0.15ppb", and more than 90% of the target compounds have blank response less than 0.1 ppb.

Comment 10: what's SEAS site?

Response:

Thank you for your question. We added the illustration in the revised manuscript.

"The meteorological variables including temperature, RH and wind speed were simultaneously acquired from a weather station about 10 km northwest of the Shanghai Academy of Environmental Sciences."

Comment 11: How can one site have spatial heterogeneity? Do you mean each pair of sites?

Response:

Thank you for your question. We revised the sentence in the new manuscript.

"The spatial heterogeneity of VOC concentration between two different sites was determined by the coefficient of divergence (COD) (Wongphatarakul et al., 1998; Sawvel et al., 2015)."

Comment 12: Line 202: I think the authors need to rewrite the description about g_{ik}, f_{kj}, and e_{ij}.

Response:

Thank you for your comment. We rewrite the description in the revised manuscript.

"g_{ik} represents the species contribution of the kth source to the ith sample, f_{kj} is the jth species fraction from the kth source, e_{ij} is the residual result for jth species in ith sample."

Comment 13: Line 205-212: The authors should explain how the Q value works in PMF model, for example, how it can help determining the PMF solution.

Response:

Thank you for your comment. We added the description in the revised manuscript.

"where MDL is the minimum detection limit, EF is the error fraction and can be set to 0.05-0.2 (Song et al., 2007). It was 0.1 in this study. In this study, four to eleven factors were utilized to determine the option solution. Q_{true}/Q_{robust} and and $Q_{true}/Q_{expected}$ are important parameters for characterizing the rationality of the PMF results (Brown et al., 2015). Seven factors were regarded as the optimal solution, comparing the ratios of Q_{true}/Q_{robust}, $Q_{true}/Q_{expected}$ and the PMF results. The Q_{true}/Q_{robust} values were set to 1.0 at the three sampling sites. The $Q_{true}/Q_{expected}$ values were 1.3, 1.1, and 1.0 at the JS, PD and QP sites, respectively."

Comment 14: How is weight function applied in PSCF?

Response:

Thank you for your comment. We added the description in the revised manuscript. In this study, the pollution trajectory was defined as the trajectories corresponding to the total VOC (TVOC) concentration that exceeded the 75th percentile concentration of TVOCs. The m_{ij} is the number of endpoints of the pollution trajectory passing through the grid (i, j), and n_{ij} is the number of endpoints of all the trajectories falling within the grid (i, j).The weight function W_{ij} was used to increase the accuracy of the model.

"Therefore, the $PSCF_{ij}$ can be calculated using the Eq. (7) as follows:

$$PSCF_{ij} = \frac{m_{ij}}{n_{ij}} \times W_{ij} = \frac{m_{ij}}{n_{ij}} \times \begin{cases} 1.00 & 80 < n_{ij} \\ 0.70 & 20 < n_{ij} \le 80 \\ 0.42 & 10 < n_{ij} \le 20 \\ 0.05 & n_{ij} \le 10 \end{cases} \qquad (7)”$$

Comment 15: Please confirm exactly how many VOC species were observed, 60 or 43? It will be better to list the observed VOC species in a table.

Response:

Thank you for your comments. We have revised the statement in the new manuscript. We list the observed VOC species in the Tab. S2.

> *“During the observation campaign, 43 VOC species including 16 alkanes, 11 alkenes, 16 aromatics and 1 alkyne were measured and the contributions of total VOCs (TVOCs) > 1 % were marked.”*

Comment 16: Line 277-284: How does the comparison meaningful, without clarifying the observed VOC species?

Response:

Thank you for your comment. We rewrote the description in the revised manuscript.

> *“Compared with the relevant measurements performed previously in Shanghai at the same sampling sites, this study generally presented lower VOC concentrations (Cai et al., 2010b; Zhang et al., 2018; Zhang et al., 2020a). In detail, at the JS site, the VOC concentration was approximately 4 times lower than the measurement of Zhang et al., (2018) (94.14 ppb). At the PD and QP sites, the results in this study were slightly lower than those reported by Cai et al. (2010b) (24.3 ppb) and Zhang et al. (2020a) (15.41 ppb). A variety of control strategies, such as prohibiting of fireworks in the open air, improving VOC detection standards and strengthening control technology were implemented, thus resulting in the low VOC concentrations herein. Particularly, the policy of “one factory, one strategy”, targeted at mitigating VOC emissions, was published by Shanghai government in 2018.”*

Comment 17: Lin310-312: “This phenomenon was because there similar VOC emission intensity …..”. The discussion about different COD is too simple.

Response:

Thank you for your comment. We added the illustration in the revised manuscript.

> *"Note that the distinct spatial heterogeneity of VOCs was also observed with the highest value of the coefficient of divergence (COD = 0.36) between the JS and QP sites, followed by the PD and QP sites (COD = 0.33), with that between the JS and PD sites (COD = 0.20) being the lowest. Hence, the spatial heterogeneity of VOCs between the JS and PD sites was narrow, while the QP site was largely different from other two sites. This result was ascribed to the fact that there were similar pollutant concentrations, meteorological factors, emission intensities and atmospheric conditions at the JS and PD sites, while these indexes in the QP site were rather different from those of the other sites.*

Comment 18: Line 317-318: "Statistically, VOCs were found to be positively correlated with $PM_{2.5}$ due to the fact that VOCs were a significant precursor of $PM_{2.5}$." I do not think this explain is correct.

Response:

Thank you for your comment. We rewrote the description in the revised manuscript.

Lines 251-252:

> *"VOCs was found to be positively correlated with $PM_{2.5}$, and the pearson correlation coefficients ($R_{Pearson}$) were 0.58, 0.71 and 0.25 at the JS, PD and QP sites, respectively."*

Comment 19: Line 324-331: What's the view point the authors want to discuss? Determining the controlling factor of O_3 formation merely based on the ratio of VOCs/NOx ratio is too simple. Moreover, in Line 329 "a higher proportion of OH radical reacted with NO_2 to suppress the O_3 formation", how the authors get this conclusion from the VOCs/NO_x ratio?

Response:

We want to highlight the progress of O_3 formation and why the VOCs was negatively correlated with the O_3. We rewrote the description in the revised manuscript.

> *"However, the VOC concentrations were negatively correlated with O_3. The termination and titration ($NO + O_3 \rightarrow NO_2 + O_2$) were more efficient and lots of*

factors, rather than the emission of precursors, impacted on the surface O_3. Li et al. (2019b) emphasized that the absolute concentration of precursor was not the only factor during the O_3 formation in Zhengzhou, China."

Comment 20: The authors have highlighted some differences among three sites. Such as in Line 356-357, "the contribution of toluene at JS site was markedly increased (~ 3times) relative to the other two sites." However, such discussion about the difference of VOC markers is insufficient in this section. The authors should discuss more about such difference and make connection of the observed difference with the difference of land use type or emission sources.

Response:

Thank you for your comment. We added the discussion about the effects of land-use types on VOC concentrations, sources and O_3 and SOA formation potentials in the Sec. 4.

For example, lines 527-528:

> *"The mean VOC concentrations at the JS (21.88 ± 12.58 ppb) and PD (21.36 ± 8.58 ppb) sites were 1.83 and 1.79 times higher than that at the QP site (11.93 ± 6.33 ppb), implicating the impact of land-use type."*

Lines 534:

> *"Besides, the land-use types could not only affect the VOC concentrations but also compositions."*

Lines 553-555:

> *"The VOC diurnal variations were analyzed with respect to each of the land-use types. The result showed that the VOC concentration in the early morning (5:00 LT) at the JS site was 17.06 % and 52.91 % higher than those at the other two sites, which was attributed mostly to the land-use types."*

Lines 560-561:

> *"In addition to the diurnal variations, the "weekend effects" of VOCs also appeared to be variable among the different land-use types."*

Lines 570:

"VOC sources in this study were sensitive to the local emission with the different land-use types."

Lines 612:

"The OFP values were closely related with the land-use types."

Lines 623-626:

"The discrepancies of SOAFP values among the sampling sites were observed since they could be significantly influenced by the land-use types. At the JS site, the SOAFP value (1.00 ± 2.03 μg m^{-3}) was 2.17 and 2.44 times higher than those at the PD (0.46 ± 0.88 μg m^{-3}) and QP (0.41 ± 0.58 μg m^{-3}) sites, which was consistent with variations of VOC concentrations especially aromatics and connected with the land-use types (Zhang et al., 2017; Jookjantra et al., 2022)."

Comment 21: Line 369-406: the authors discuss too much about the diurnal variation pattern, which is similar with the reported diurnal characteristics in many other studies. There are some interesting parts such as at Line 390-391: "The VOC concentrations on the weekends were 3.31, 10.19 and 1.19% lower than those on the weekdays," and at Line 394-395: "It should be noted that there were narrow discrepancies of VOC concentrations at the site between the weekdays and weekends ….". The difference at weekends and workdays can be attributed to the influence of the local emission sources. The author should rearrange this section focusing on the discussion about the difference of weekend/ holiday effects.

Response:

Thank you for your comment. We rewrote the description in the revised manuscript, specified in newly lines 301-320.

Comment 22: Line 415-416: how is industrialization and urbanization results in the stagnant weather condition?

Response:

Thank you for your question. We revised this sentence as:

"Such scenario could be attributed to the locations of JS and PD sites which implicated stagnant weather conditions and high anthropogenic emissions, therefore inducing the severe haze pollution."

Comment 23: Line 430: How about the “clean-haze” discrepancy of other VOC species?

Response:

Thank you for your question. At the JS site, the “clean-haze” discrepancies were 1.38, 1.51, 1.63 and 1.02 of the alkanes, alkenes, aromatics and alkyne, respectively. At the PD site, the “clean-haze” discrepancies were 1.39, 1.58, 1.83 and 1.63 of the alkanes, alkenes, aromatics and alkyne, respectively. At the QP site, the “clean-haze” discrepancies were 1.37, 1.24, 1.35 and 1.32 of the alkanes, alkenes, aromatics and alkyne, respectively.

Comment 24: Line 36: “...both of which formation are ...” better to use two sentences here.

Response:

Thank you for your comment. We rewrote the sentence in the revised manuscript.

> *“Serious air pollution in China is currently characterized by the high levels of ozone (O_3) and fine particulate matters (PM) especially $PM_{2.5}$ (PM with an aerodynamic diameter less than 2.5 μm). The atmospheric volatile organic compounds (VOCs) greatly influence the O_3 and $PM_{2.5}$ formations, and function as the important precursors (Carter, 1994; Liu et al., 2008; Yuan et al., 2013; Lu et al., 2018; Ma et al., 2019; Yu et al., 2021).”*

Comment 25: Line 180-181: “The samples were condensed low-carbon (C_2-C_6) compounds and high-carbon (C_6-C_{12}) compounds ...”. Rearrange the sentence.

Response:

Thank you for your comment. We rewrote the sentence in the revised manuscript.

> “*The samples were condensed for low-carbon (C_2-C_5) compounds at 15℃ and high-carbon (C_6-C_{12}) compounds at 30℃.*”

Comment 26: Line 185: what is trace instruments?

Response:

Thank you for your question. It should be the “trace gas instruments”. We rewrote the description in the revised manuscript.

"The O_3, NO-NO_2-NO_x were characterized by trace gas instruments (49i ozone analyzer and 42i nitrogen oxide analyzer, produced by Thermo Environmental Instruments Inc., USA) with the detection limits of 0.50 and 0.40 ppb, respectively."

Comment 27: Line 211: "greatest solutions" should be optimal solutions.

Response:

Thank you for your comment. We rewrote the sentence in the revised manuscript.

"Seven factors were regarded as the optimal solution, comparing the ratios of Q_{true}/Q_{robust}, $Q_{true}/Q_{expected}$ and PMF results."

Comment 28: Line 218: Should be "This study was determined by the 24-h back trajectory

Response:

Thank you for your comment. We rewrote the sentence in the revised manuscript.

"This study was determined by the 24-h back trajectories (one hour interval) at the height of 500 m via the MeteoInfoMap software."

Comment 29: Line 266-267: "The temperatures were averaged to be ....

Response:

Thank you for your comment. We rewrote the sentence in the revised manuscript.

"The average temperatures were 8.69 ± 3.24, 9.02 ± 3.24 and 7.73 ± 2.92 ℃, and the mean RHs were 83.77 ± 11.38, 75.37 ± 13.29 and 71.80 ± 9.28 % at the JS, PD and QP sites, respectively."

Comment 30: Line 300: "a large number of organizations...". Improper use of organizations here.

Response:

Thank you for your comment. This sentence has been deleted.

Comment 31: Line 304: "The reduced VOC concentrations coincide...". This sentence is confusing.

Response:

Thank you for your comment. The original line 304 has been deleted.

Comment 32: Line 356-357: "the contribution of toluene at JS site was markedly increased (~ 3times) relative to the other two sites." Should be higher instead of increased.

Response:

Thank you for your comment. We rewrote the sentence in the revised manuscript.

Lines 533-534:

> *"Specifically, the JS site exhibited higher fractions of aromatics and alkenes, particularly toluene and propylene, than those at the PD and QP sites."*

Comment 33: Line 375: Should be "the VOC concentrations also tended to increase"

Response:

Thank you for your comment. We rewrote the sentence in the revised manuscript.

> *"During the rush-hour traffic at 18:00 to 21:00 LT, the VOC concentrations also tended to increase, and the evening peak values were 18.46, 20.82 and 10.22 ppb at the JS, PD and QP sites, respectively."*

Comment 34: Line 424: should be "At the QP site".

Response:

Thank you for your comment. We rewrote the sentence in the revised manuscript.

> *"At the QP site, alkanes (2, 2, 4-trimethylpentane, n-hexane, n-heptane) presented significant 'clean-haze' discrepancy (~ 36.58 % uplift), implying the great influences of vehicle exhaust and fuel evaporation."*

Lastly, we would again express our appreciation to the reviewers and editor for their warmhearted help. Thank you very much!

References

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