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.

Major comment

Major 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.

Major 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.

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

Specified comments

1. The term" secondary formation potentials" in the title is confusing, it will be better to use "ozone and SOA formation potential".
2. 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 alkene-related reactions". It is confusing at this place to see VOCs-S_{O3} without illustrations of the methodologies. It will be better to just state the main collusion here.
3. 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?
4. Line 35- 139 I do not think it is necessary to put some many basic knowledge in the introduction part.
5. 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).
6. Line 177: what's TD300?
7. 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%?
8. Line 188: what's SEAS site?
9. Line 190: how can one site have spatial heterogeneity? Do you mean each pair of sites?
10. Line 202: I think the authors need to rewrite the description about g_{ik}, f_{kj}, and e_{ij}.
11. 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.

12. Line 224-225: How is weight function applied in PSCF?
13. Line 265-266: 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.
14. Line 277-284: How does the comparison meaningful, without clarifying the observed VOC species?
15. Lin310-312: "This phenomenon was because there similar VOC emission intensity …..". The discussion about different COD is too simple.
16. 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.
17. 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/NOx ratio?
18. 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.
19. 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.

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

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

Grammatical errors and confusing expressions

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

Line 180-181: “The samples were condensed low-carbon (C2-C6) compounds and high-carbon (C6-C12) compounds …”. Rearrange the sentence.

Line 185: what is trace instruments?

Line 211: “greatest solutions” should be optimal solutions.

Line 218: Should be “This study was determined by the 24-h back trajectory”.

Line 266-267: “The temperatures were averaged to be …..”

Line 300: “a large number of organizations….”. Improper use of organizations here.

Line 304: “The reduced VOC concentrations coincide ….”. This sentence is confusing.

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.

Line 375: Should be “the VOC concentrations also tended to increase”

Line 424: should be “At the QP site”.

I have not listed all the grammatical errors and inaccurate expressions. Please check the whole manuscript and make corrections.