

Atmos. Chem. Phys. Discuss., referee comment RC1  
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## Comment on acp-2021-565

Anonymous Referee #1

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Referee comment on "Measurement report: Summertime and wintertime VOCs in Houston: Source apportionment and spatial distribution of source origins" by Bavand Sadeghi et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-565-RC1>, 2021

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### General comments

The research about the local VOC properties is basically interesting and fits into the scope of ACP. To be published, however, a dense revision looks required.

(1) To derive some essential key points of this study looks necessary. This is a study at a certain region (Houston). In this kind of study, readers would like to see why we need to understand the VOC pattern in Houston, meaning that what is similar to / or different from VOC properties in other regions. That is a very important information for making a study about the local air quality suggesting some generalized lessons to the readers.

(2) Please add some more efforts to have a better organization. It is difficult to find a main flow of this work (A lot of findings are scattered so it is hard to catch what is important).

(3) Please include deeper discussion with previous findings. It is hard to see what is the specialty of study and how this work has the discriminated merits compared to previous VOC works.

(4) The research motivation was not clarified in the introduction.

(5) Several statements were provided without references. Please proofread whole manuscript again and reinforce the references.

(6) Results are too peripheral. I do not know how to apply/generalize these results for the advanced understanding of VOC chemistry.

(7) I agree that the results are interesting and informative for the local air quality study. But the dataset is not very something new and most analyses is so qualitative based on simple PMF and backtrajectory calculation (As authors mentioned in the manuscript, results usually supports the previous findings. New lessons or arguments cannot be found much). The final decision is not sure but therefore it seems better to submit this

manuscript to other journals if serious revision is not guaranteed (For example, Atmospheric Environment).

#### Specific comments

line 78-80: Not well connected to the previous statements (Suddenly VOC influence is connected to the VOC source)

line 94-103: It seems that there are already deep studies about VOCs at Houston area. Considering past findings, what do we need to know more? What are the missing points in the previous works. These things are not shown in this introduction part.

line 147-151: A reference is required about the shorter lifetime of atmospheric alkene/aromatics than alkanes + longer lifetime of ethane and propane.

line 149-150: => Differences of alkane concentrations

line 247-248: This should be confirmed with some data analysis in Houston. Different from the convective mixing and photochemical activity (discussed after this statement), energy demand pattern can have a larger regional difference, so it is hard to generalize simply without the specific clue.

line 300-304: Important information to compare between C2-C5 and C8-C10 isomers. But how to obtain these OH reaction rate constants is not clear here. Any related figure or table is not found in this manuscript.

line 307: 'in Figs. 4 and 5.' => 'in Figs. 4 and 5, for summertime and wintertime, respectively'

line 308: What are 3 O<sub>3</sub> species factors? (Factor 1, 3, and which? Clearly indicate the factors)

line 309-310: How to justify this statement?

line 307-314: Most interpretations are not connected to clues. At least the references should be added.

line 319-356: Please write this part again matching to the findings in Fig. 4 and 5. Different from Factor 1, 2, and 3, analysis about factor 4, 5, 6 looks only based on Fig. 3. There is no examination about the diurnal cycle. Organization of paragraphs looks problematic. This part should be significantly improved.

line 344-346: What is the clue to support this statement? I cannot figure out this, even based on Fig. 3.

line 393-395: How to justify this interpretation?

line 486-493: Overall summary is so ambiguous. Specific lessons to make summer-winter difference are necessary.

Whole results: The reason only focusing on summer and winter is not clear, while the full year measurement data exist. Is there any specific reason not to see the spring and

autumn. It is also worth to investigate other seasons (For example, Hurricane effect is also huge in autumn).