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Comment on acp-2022-354

Anonymous Referee #2

Referee comment on "Understanding the variations and sources of CO, C₂H₂, C₂H₆, H₂CO and HCN columns based on three years of new ground-based FTIR measurements at Xianghe, China" by Minqiang Zhou et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-354-RC2>, 2022

- This study was based on the FTIR measurement at Xianghe, China. Using the measurement data, the variation and correlation of CO and some hydrocarbons were examined. The methodology was well prepared and also clearly suggested in this manuscript. All results looks moderately fine.

- But this manuscript does not include the new finding and discriminated feature compared to previous works. In other words, the research motivation looks very weak. The only special motivation of this work is that this study is the first FTIR measurement at Xianghe, China. Probably this can be a good motivation, because there were not much FTIR measurements and related analyses in China, I think. However, this 'first measurement' was not resulted in fresh ideas about the hydrocarbon pattern in China. All results and discussions were too plain. What can we learn more for the hydrocarbon pattern in China using this first FTIR measurement at Xianghe, China?

- In detail, the methodology part looks fine, but this is the thing that we already know. This information (retrieval channel, absorption lines, averaging kernels, etc.) has been much discussed in other previous works. Do we have something new in this manuscript, which was not treated in the previous work?

- In results, the analyses was composed of simple time-series, monthly mean pattern, correlation between CO and other hydrocarbons, back-trajectory pattern, and emission estimation. The performance is not bad, but again, this is just a 'revival' of previous FTIR works. We already have a number of previous studies showing these information in recent 10-20 years (e.g., Zhao et al., 2002; Vigouroux et al., 2012; Viatte et al., 2013; Viatte et al., 2014; Lutsch et al., 2016, etc.). To have another publication, the new finding should be included in this manuscript, which was not discussed in these previous works. Therefore, the 'unique' pattern of CO and hydrocarbon should be provided with this 'first' FTIR measurement at Xianghe, China. That is the expected point, but it is not included in

this manuscript.

- Nonetheless, I still would like to put the weight on the meaning of 'first' FTIR measurement at Xianghe, China. This may not be the first FTIR measurement in whole China, but based on my knowledge, the 'CO and hydrocarbon' analysis based on Bruker FTIR was very rare in China. It actually relates to the unique meaning of this work. Considering this point, I strongly suggest authors to withdraw this manuscript from ACP, and to submit this manuscript again to AMT (or transfer from ACP to AMT review process, if possible). I think that the AMT looks much better journal to deal with this manuscript.

- Before the re-submission or transfer, please improve the quality of discussion. Now, most of statements in the result chapter is just a simple reading of graphs, or too typical not including any meaning (e.g., page 18, line 4-6, "Our results show that ... atmospheric vertical stability" => This kind of statement does not have any real meaning. Anybody can say this with any result), or too limited to the small case that is not well generalized (e.g., page 19, line 21-23, " we understand that the drop ... fire emission in the Siberia" => This is just for July and August 2021. Can we generalize this different influence in other years?). Deeper analyses looks very required.