

Atmos. Chem. Phys. Discuss., referee comment RC1
<https://doi.org/10.5194/acp-2021-1016-RC1>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on acp-2021-1016

Anonymous Referee #1

Referee comment on "Measurement report: Intra- and interannual variability and source apportionment of volatile organic compounds during 2018–2020 in Zhengzhou, central China" by Shijie Yu et al., Atmos. Chem. Phys. Discuss.,
<https://doi.org/10.5194/acp-2021-1016-RC1>, 2022

General comment

The paper reports a discussion of a measurement dataset of VOCs collected in Zhengzhou (China) between 2018 and 2020. Discussion on trends, potential sources is included in the paper. The approach is not particularly new, however, the dataset and the analysis is quite complete and I believe that the paper could be interesting for the scientific community and suitable for the Journal. However, a few aspects are not completely clear or discussed in sufficient details so that a revision would likely improve the paper, see my specific comments.

Specific comments

Lines 64–66. Here it would be better to use some references, especially for CMB applied to gaseous VOCs. I am quite aware of use of CMB receptor model for particulate matter and several source profiles are available in the scientific literature but, likely, much less information is available for source profile of VOCs.

Section 2.2 is quite stingy of details and should be enriched. I would suggest to mention the work of Belis et al. (Atmospheric Environment X, 5, 2020, 100053) regarding performances of receptor models and mention if specific constraints were used in the PMF run and how measurement uncertainties were taken into account and what is the total variable used.

In supplementary material, and related to the previous point. It is mentioned principal component analysis on this dataset but there is not trace of it in the paper. In addition, it should be explained how the number of factors was chosen because Figure 1 with a constantly decreasing Q/Q_e does not seem to allow this identification by itself.

Lines 215-218. It should be mentioned is the differences in these yearly averages are statistically significant considering the large standard deviations (are STD reported as errors?) indicated.

Lines 333-335. To better explain this reasoning, it should be mentioned that CO and NO₂ are mainly gases from combustions sources strongly influenced by urban activities such as traffic and domestic heating. Instead, SO₂ is generally mainly due to industrial sources or combustion of heavy oils such as fuels used in ships.

Section 3.3.1. This part could be made more strong if related to the diagnostic ratios. For example, the B/T ratio in the different profiles are similar to those found in literature for the specific sources as discussed previously. Actually, the figure 4 is very small and I do not see clearly. I also suggest to increase the size of this figure.

Section 3.3.2. At the end it is not clear if the trends are present and statistically significant. Actually in Figure 5 it seems that trends are not so relevant in relative terms.

Line 485. Why here it is mentioned ppbv rather than s^{-1} ?

Figures 2 and 3. I suggest to change the vertical scale to maximize the visibility of the data. For example, B is always less than 3 in Figure 3, so why to choose a scale at 6 that compress everything? In addition, in the data in Figure 3 it is missing the results for 20 and 21 (i.e. 8 and 9 pm). The same problems are also present in Fig. 7, Fig. S3.

Tables S2, S3, and S4. Better to indicate the measurement units and also explain what is Pr.

Figure S2. Please correct Mixing on the y-axis label.

Line 56. Better contributors to.

Line 73. Better hot topic.

Line 415. Probably it is night time.

Line 477. Remove the t in excess.