

The manuscript by Sun et al. presents and analyses a 5-year time series of new HCHO measurements obtained from ground-based FTIR spectra recorded between 2015 and 2019 at Hefei, eastern China. Seasonal and interannual variabilities of HCHO were analysed and hydroxyl (OH) radical production rates from HCHO photolysis at the measurement site were also evaluated. A statistical model is adjusted in order to reproduce the HCHO abundance and variability based on ground level in situ measurements of CO and O_x (O₃ + NO₂) taken in the vicinity of the FTIR site. CO and O_x are used as tracers for emitted and photochemical HCHO, respectively, in order to estimate the contribution of direct emissions and oxidation of gas precursors to the HCHO abundance and variability. Finally, contributions of emission sources from various categories and geographical regions in China to the observed HCHO summertime enhancements were determined by using a series of GEOS-Chem sensitivity simulations.

I read the revised version, and went back and looked over the original version. The authors have put in a nice effort into revising their manuscript and mainly responding to the critiques of the original manuscript (especially by reviewer #1). The questionable investigation of diurnal modulation of HCHO at Hefei with GEOS-Chem simulations is removed. The discussion on the retrievals, kernels, uncertainties is well done. The revised version exploits the resulting new dataset extensively and presents sufficient perspectives in relation with the literature. The FTIR column-averaged dry air mole fractions of HCHO has been compared with the ground level measurements to ensure that the HCHO column measurements at Hefei can be used as representative of near-surface conditions. The interpretation of the GEOS-Chem results now has a much clearer message and storyline. The observed increasing change rate of HCHO from 2015 to 2019 over Hefei was attributed to the increase in photochemical HCHO resulting from increasing change rates of both CH₄ and NMVOCs oxidations, which overwhelmed the decrease in emitted HCHO. This study provides a valuable evaluation of recent VOCs emissions and regional photochemical capacity in China.

I think that this is now publishable in the ACP, after a few mostly minor revisions. Generally, the English should be improved throughout the manuscript, especially in the newly inserted sections or sentences, e.g., p17-L42, “drive” should be “driving”, p18-L29, “Conclusions” is better than “Concluding remarks”. I am not a native speaker, therefore I did not attempt to correct all these flaws throughout the whole paper. I assume that much of this will be done in the copy-editing phase. For the reference section, inconsistent order of first names and family names between different references are found, and some with DOI link, while some are not or with wrong DOI. The authors should refer to the guideline for reference in the ACP homepage. Nevertheless, this is interesting data from a highly polluted region that should be in the literature. Hence, I feel this should be published.