Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-1176-RC3, 2018

Atmospheric Chemistry and Physics Discussions

Open Access

Interactive comment on "Ozone seasonal evolution and photochemical production regime in polluted troposphere in eastern China derived from high resolution FTS observations" *by* Youwen Sun et al.

Anonymous Referee #3

Received and published: 8 April 2018

1 Overall remarks

The paper reports on about three years of tropospheric ozone and formaldehyde measurements from a new FTIR instrument in Heifei, China. The data are compared to a number of correlative data, including tropospheric NO2 from the OMI satellite instrument, and results from chemical transport models.

The authors give a very long and detailed description of their instrument and retrieval

Printer-friendly version

Discussion paper

technique. They then analyse their observations using the correlative data mentioned above. Overall, their results, such as annual cycle, correlations, and trajectory analyses are plausible. However, the authors tend to discount differences and poor correlations, and to ignore the very coarse altitude resolution of their tropospheric ozone data, which average over a very wide altitude range, and have relatively little sensitivity to the planetary boundary layer, where a substantial part of the smog related ozone photo-chemistry takes place.

Largely I concur with the comments by the other two reviewers. The paper does not present major new insights. However, it is important to report on new instruments and on tropospheric chemistry findings in China. Therefore, and also considering that this is a special issue for the last Quadrennial Ozone Symposium, I recommend publication after a few major deficits have been addressed.

2 Suggested Changes

The description of the FTIR technique and FTIR profile retrieval in lines 148 to 248, as well as the averaging kernel smoothing used for comparison (lines 249 to 279) is pretty much standard. This could all be omitted, or moved to an appendix. A short paragraph and a few references in the main text are enough.

lines 339 to 342, lines 367 to 370: I think these simple attributions to "model input files" are not valid. The wide averaging kernels and low sensitivity of the FTIR tropospheric ozone columns to boundary layer ozone, as well as the limited horizontal resolution of the model data could play a very large role here. Please reword or omit these parts.

Appendix A: Basically this is textbook / Rogers (2000), right? So this could/ should be omitted.

Fig. 6: I am not sure how meaningful this comparison of ozone profiles is. Both

have very poor altitude resolution, and profile shape is determined to a very large degree by a priori assumptions. Comparison with a real tropospheric ozone profile from ozone-sondes or lidar would be much more meaningful. Maybe drop this Figure and its discussion? Similar considerations apply to Figs. 8 and 10.

In most respects, I concur with the detailed recommendations by the other two reviewers. However, after shortening, and addressing the major comments, I think this manuscript is publishable in ACP.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-1176, 2017.