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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spatial distribution and temporal trend of ozone pollution in China observed with the OMI satellite instrument, 2005–2017" *by* Lu Shen et al.

Anonymous Referee #3

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This paper explored the capability of OMI ozone columns to represent the surface O3. I feel the satellite data is over-interpreted based on the evidence provided in the paper. However, I do believe it will be big news if substantial improvements are made to prove that the conclusion is solid.

General comments: 1. The sensitivity of OMI O3 to the lower troposphere is very low. I feel that is the reason why no quantitative comparison to surface observations has so far been done. I'm wondering is there any improvements that have been made to make the quantitative comparison robust? Why does not the quantitative comparison work for other regions, but work for China? 2. The robustness of the residual. How large is

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Discussion paper

the temporal and spatial variations of the background? Is it likely that such variations bring significant uncertainties to the subtraction? 3. The correlation between MEE and OMI. The correlation seems to be related with the dependence of O_3 on latitude. I suggest additional analysis here to prove that is not the case.

Specific comments: 1. “We exclude outliers with over 35 Dobson Units (DU) at 850-400 hPa (>99th percentile in eastern China) and exclude July 2011 when the retrievals are anomalously high.” Please give the reference to the exclusion. Otherwise, please quantify the influence of the exclusion. 2. “We see that high-ozone episodes in the 950-850 hPa sonde data are systematically associated with high OMI values, though the converse does not always hold.” Additional explanation for the reason is expected.

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