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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observation of ozone enhancement in the lower troposphere over East Asia from a space-borne ultraviolet spectrometer" *by* S. Hayashida et al.

Anonymous Referee #2

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This manuscript examines the OMI-derived lower tropospheric ozone over East Asia by comparing with MOZAIC/IAGOS airborne measurements and by using CO and hotspot measurements as evidence of air pollution. The authors found that the derived ozone quantities in the lowest layers certainly have pollution signals by extracting the daily variations from monthly averages not attributable to the a priori variations. The magnitude of the signal was smaller than the observations but was consistent when Averaging Kernels were taken into account. The elevated ozone concentrations over Shandong Province and surrounding areas were consistent with past studies and attributed to the post-harvest open crop residues burning (OCRB). Basically the topic is

within the scope of this journal and the presentation is clear. Lower tropospheric ozone data in this region, associated with sharp increases in the emission rates of ozone precursors, are lacking and therefore highly demanded to study the trans-boundary and hemispheric air pollution. The described OMI data, after careful evaluation made in this study, would provide key information for such future studies. To improve the manuscript, I would suggest the following major revisions:

1. More explanation should be given why layered information of the tropospheric ozone can be retrieved in this study using the UV spectrum only, as other past studies failed. The principle is to include at least two spectral regions having sensitivities at different altitude levels? The authors should also address the issue of the effect of co-existing aerosol particles on the radiative transfer in the UV spectral region.

2. Same profile as OMI is assumed for the 21th and above layers in the derivation of MOZAIC-based values convolved with the AKs. The common contribution from these high altitude layers could be the source of correlation in Figure 5 and therefore the authors need to test if such contribution is small, in order to retain the conclusion.

3. The authors should not overstate the importance of OCRB in June over the Central Eastern China region. For example Yamaji et al. (2010) mentioned that even without the residues burning, the monthly average ozone level is high (73.9 ppbv), being fueled by normal anthropogenic emissions, and (only) about 7 ppbv increase from that level is attributable to the OCRB. They discussed that the OCRB is certainly important for the reproduction of the observed monthly ozone level (81.3 ppbv) in the model. Also, I feel that evidence is not enough to attribute the high ozone episode on June 22, 2005 to the influence of OCRB. In this manuscript only monthly accumulation of hotspots is discussed; the burning period should be normally much shorter than a month. I am afraid that the rate of hotspot detection is much less in the latter half of June.

4. In sections 4.1 and 4.2, the authors frequently repeated the results already displayed in the previous section. I would suggest that Results and Discussion sections are

merged to form a single section 3.

Other technical comments are provided below:

1. Page 2015, line 7. Not only infrared but also UV absorption leads to global warming.

2. Page 2019, line 22. of which 3-7 layers are

3. Page 2023, section 3.1.2 Are the MOZAIC data over the airport represent the 52 x 48 km footprint size of the satellite observation?

4. Page 2024, line 3. How CO was measured?

5. Page 2026, Equation (1). A minus character in the term [X_t,j – X_a,i] is missing.

6. Page 2027, section 3.2. How much was the a priori profiles variable longitudinally for a given latitudinal band?

7. Page 2032, line 16. What are the altitude ranges of the layers 19 and 20?

8. Page 2032, section 4.2. The authors need to state which relationship is expected between the position of subtropical jet and descent of stratospheric ozone.

9. Page 2034, line 10. The authors propose using a scale factor 0.17. Considering this factor how much of ozone variation (in DU or in ppb) in the lower troposphere can be detected by OMI?

10. Page 2035, lines 25-27. Please reword "The validation has not been evaluated . . ."

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 2013, 2015.