Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-520-RC1, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Lidar detection of high concentrations of ozone and aerosol transported from Northeast Asia over Saga, Japan" *by* Osamu Uchino et al.

Anonymous Referee #1

Received and published: 26 July 2016

The paper by Uchino et al. presents lidar measurements of high ozone and aerosol concentrations over southern Japan and a comparison with models. Measurements and the model outputs are interesting and a complete analysis of these datasets is worth scientific publication. However, the current analysis of these datasets does not seem to fully exploit the information they provide and many statements are not clearly justified (even though the observational and modelling datasets to do it are potentially available already). I recommend the following major revisions:

1) Evidence on the origins (location and type of source) and transport pathways (particularly in the vertical) of the ozone and aerosol plumes are not clearly provided. Chemistry-transport model outputs are not exploited for this, as currently, simulations

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are only used for comparison with lidars. I recommend fully developing this aspect, with analysis of the model outputs at the scale of East Asia and in the vertical. This is very important to provide information on the horizontal and vertical pathways of the pollutant plumes and their origin (e.g. type of aerosols).

2) Analysis of lidar data: the vertical structure of ozone and aerosol layers are not completely analysed, as the heights of the atmospheric boundary layer (mixing and residual layers) are not depicted. As shown in numerous papers, the detection of these layers may be easily done with the aerosol lidar. Such analysis should be added in the paper and precisely explain in a chronological order the mechanisms involved (i.e. vertical mixing, arrival of pollutants, etc).

3) Technical characteristic of the datasets: in the presentation of the results, too much technical information of the datasets (instrument characteristics, model configuration, variables describing the datasets, etc) is given. The geophysical interpretation of the measurements is relatively scarce. For sake of clearness for the readers, I recommend that most of technical remarks on the datasets are given a previous section dedicated to datasets characteristics and then only the geophysical interpretation is given when describing the figures.

4) The extinction-to-backscatter ratio : the climatological values that are used are expected to be suited to a particular type of aerosol (it changes a lot depending on the origin and size). What kind of aerosol is it for 50 sr at 532 nm? Measurements suggest the presence of large non-spherical particles (likely desert dust). How is this taken into account? I recommend using different extinction-to-backscatter ratios for each kind of particles (dust, sulphate, etc).

I recommend the following minor revisions:

1) English language should be revised in the whole the paper.

2) Colour scales in figure 2 should be changed in order to highlight changes from

background.

3) Line 93: what is the meaning of "The errors were equated to 100% times the lidar signal-to-noise ratios". Please clarify.

4) Lines 106-108 : The justification of the following statement is not clear : "These observational results show the descent of regions of high ozone concentrations and suggest that air with high ozone concentrations was transported to the surface by vertical mixing when the planetary boundary layer developed during the daytime". What is the height of the boundary layer? This height should be clearly shown in the figures. When was it mixed? Which elements suggest a "descent" or downward mixing of the high ozone plume? This should be thoroughly explained and justify. The model may be used to describe this mixing mechanism. When it is mentioned “daytime” vertical mixing, it is in which day? In which location vertical occurs? The current explanation is very scarce and unclear. This should be thoroughly explained and justify.

5) Line 120: The sentence "The most reasonable results were obtained when the following changes were made (Fig. 2b)" does not seem to match the Figure as it does not explain the changes in the model, but it presents the final results.

6) Line 130: there is also a difference in the time of the arrival of the plume. Please, report.

7) The term "bad weather" is not objective. I suggest to use "rainy" conditions or similar.

8) The wavelength exponent, Alp : the name to this variable does not seem very conventional. For optical depth, the term "Angström exponent" is usually used in literature. The name Âń wavelength exponent Âż does not seem very explicit. Why the symbol is "Alp" ? Is this term often used by the scientific community?

9) Line 167 : The term "always existed" is not clear. Does it mean that it is observed in the timeseries shown in Figure 3 ? Please, clarify.

10) What is the effect of aerosols in the ozone DIAL measurements ? What are the

possible biases that they might induce ? Please provide an estimation of this bias for the cases of very high aerosol concentrations shown in Fig 3 on 22 March 2015.

11) Line 170 : What is the scientific evidence that suggest that small particles in the event of 22 March 2015 are "sulphate" particles ? This should be clearly justified.

12) Lines 174-181: Surface measurement of PM2.5 should be presented in the figure. A literal description seems insufficient. Please, add the time series of theses measurements.

13) Section 4.1: the model should be use to identify the type and origin of the aerosols.

14) Line 208: the term "AODs were almost the same" is only approximate. Objective terms should be used. Mean bias and RMS differences should also be given.

15) Trajectory analysis: Timing in the comparisons does not seem to be precise enough. The ozone and aerosol plume arriving to Saga lasts for a few hours only. This cannot be justified with high ozone and aerosol concentration during a large period of time (e.g. March 2015). These airmasses arriving to Saga on 22 March were likely located near the Beijing area on which date precisely? On that date, where ozone and aerosol concentrations high?

16) Lines 237-238: The justification of this statement “Based on these lidar data and the in-situ measurement data at Takagimachi in Saga city, an air mass with high ozone and aerosol concentrations could have been transported from the free troposphere” is not clear to me. The high concentrations are observed in the lidar time series below 1.5 km. There is not indication that transport of pollutants only occurs in the free troposphere, given that the top of the boundary layer is usually near 1.5 km. What evidence is given that transport does not occur in the boundary layer or both in the free troposphere and the boundary layer? What is the height of the mixing boundary layer (during, the day, the night, in the region). The altitude of the back-trajectories should also be given.

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