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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 #2

Received and published: 5 August 2016

Review of ACPD manuscript

The manuscript submitted to ACPD presents continuous lidar measurements of ozone and aerosol extinction at 532 and 1064 nm during 11 days over Southern Japan. The data are compared with two Japanese global models: MRI-CCM2 for ozone and MASINGAR-mk2 for aerosol. The analysis of this long lidar data set is worth publication. However the goals of the paper are not well established and as already noticed by reviewer 1 the interpretation does not provide enough details to be very useful. There are already many publications reporting high ozone episodes measured with lidar techniques and a new paper on this topic has to go beyond existing literature. I see two ways: either to make a complete use of the long record and the vertical information

available in the lidar data (e.g. comparing the 21-23 March with the 30-31 March ozone episode) or a true validation exercise of the models. In the present version neither option is really developed.

As suggested by reviewer 1, a focus on the vertical transport during the high ozone episode using planetary boundary layer (PBL) development from the Mie lidar and temporal evolution of the modelled plumes may be a good option. Comparison of the March 21-23 with the March 30-31 ozone episode could be also interesting because the aerosol properties look different and the AOD on March 30 is less than the very large AOD seen on March 22. If the model assessment is the preferred option, then proper metrics used in papers discussing model accuracy have to be applied (e.g. see AEROCOM web site). Statistical parameters to quantify the quality of the simulation (mean bias, RMSE, normalized mean bias, etc...) and a discussion of all the possible model error sources must be provided.

Therefore I propose a major update of the paper in either direction to avoid having just another report of a high ozone episode seen by a lidar.

Detailed comments

Introduction

A lot of details are given on GOSAT satellite validation which is not the scope of the paper.

Line 60 Provide references about previous work dealing with high ozone episode measured by lidar: Kuang et al. Atmos. Env. 2011, Banta et al. JGR 1998, Eisele and Trickl Appl.Optics 2005, Ancellet et al. Atmos.Res. 2005, Kourtidis et al. JGR 2002, ... I believe none of these papers have published an 11-day continuous record so the advantage of your data set could be better presented in the introduction.

Section 2

Technical details about the lidar system are already given in Uchino 2012 and 2014

so it can be shortened, and alternatively provide the measurements characteristics (vertical and temporal resolution, range for the different wavelengths, specific aerosol corrections, lowest measurement range . . .). Are the ozone concentrations really given with 7.5 m and 1 min resolution ?

Section 3

line 93 Be more precise about the criteria used to remove data affected by aerosols or clouds. Do you apply any aerosol corrections before this quality check ? If yes describe this correction. If not how large will be the bias in the ozone retrieval with a AOD larger than 1 at 532 nm as seen on March 22 ?

line 94 Define Ox. I understand why Ox is useful to discuss photochemistry in NO2 rich environment, but why not reporting surface ozone in Figure 2 ?

line 103 The daily cycle which is clearly seen in surface Ox measurements is hardly visible in the lidar record even at the lower bound near 500 m. What is the reason for this ? Is the nocturnal PBL always lower than the lidar measurement range ?

Line 106. This statement is not supported by any analysis. At least references must be given to support such a statement and all the observational evidence must be provided to validate this hypothesis.

Line 126. What the uncertainty of the DIAL ozone data ? The discrepancy between the model O3 concentrations and the lidar data is very large (> 100%) which is generally well simulated by model (mean bias usually less than 50%). This should be discuss with more details to attribute the bias either to uncertainties in the data or to model error. Could you quantify the uncertainty related to the emission inventory ?

Section 4

line 139 What is the reference altitude where molecular scattering can be assumed ? On March 22 aerosol layers are seen up to 6 km and the Fernald inversion may be biased. This point must be addressed in the paper.

Line 140 Lidar ratio may change from 70 sr for pollution aerosol to 30 sr for dust or marine aerosol. What is the reason for choosing 50 sr ? Of course it is likely to increase again the model underestimate of the extinction on March 22 if you apply 70 sr. What are the lidar ratio values assumed in the model simulation ?

Line 155 I assume the authors are using the 1064 backscatter ratio to calculate the wavelength exponent alp but the error is generally large for the inversion in the IR. What is the expected error on alp ? How does this exponent change for the different layers observed in the lidar record.

Line 169 and 171 Provide error bar on the depolarization ratio and wavelength exponent.

Line 169 Instead of looking at the variability of depolarization and wavelength exponent between the high aerosol event with the cleaner atmosphere before and after, it is probably more relevant to compare the different aerosol plumes between each other (e.g. March 22 with March 30)

Line 173 Provide references when interpreting the variability of Dp and Alp.

Section 4

Line 204 Is 225 m the lowest lidar measurement ? See my previous comments. It is important to be clear about this when discussing exchanges between the PBL and the layers aloft.

Line 214 There are many possible error sources in aerosol models. A better discussion is needed here including the existing literature about aerosol model errors and specific error quantification of MASINGAR-mk2 which have been already published. It seems that dust concentration is overestimated while pollution aerosol concentration is underestimated when looking at the March 22 and 30 extinction..

Section 5

Line 240-241 I fully agree with this statement but it is not really developped in the paper.

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