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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Performance of MAX-DOAS measurements of aerosols at Tsukuba, Japan: a comparison with lidar and sky radiometer measurements" *by* H. Irie et al.

H. Irie et al.

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Reply to anonymous referee 1

We thank the reviewer very much for reading our paper carefully and giving us valuable comments. Detailed responses to the comments are given below.

Comment 1: Since the authors present a very new technique which is not yet approved to deliver reliable results sensitivity studies are necessary. The authors have already performed such studies on several parameters. But to my mind, it is indispensable for publication of this paper that sensitivity studies on the a priori values as well as on the a priori errors are presented.

Reply: As the reviewer suggests, we have made sensitive studies of the a priori values and their errors. The results are now summarized in Table 1 of the revised manuscript.

Comment 2: There is no information how much of the retrieval is actually derived from the measurements and how much is the a priori input for the lowest two air mass layers.

Reply: The area (Rodgers, 2000), which provides a rough measure of the fraction of the retrieval that comes from the measurements, was calculated as the sum of all elements in the averaging kernel profile weighted by the a priori error (Liu et al., 2005). The areas were 1.0, 0.2, 0.1 and 0.1 for τ, F1, F2, and F3, respectively. This indicates that the retrieval has been done by scaling the given a priori profile shape first, followed by changing the profile shape. This is now stated in section 2.1 of the revised manuscript.

Comment 3: Furthermore, error bars in Figures 4,6,7,8 and 9 as well as enlarged Figures (except for Fig. 1 and 3) would help the reader to evaluate the retrieval. Maybe, in terms of Fig. 6 and 8, additional plots with the comparison of the 4 chosen days could be useful. Moreover, a direct comparison of Fig. 2 and 4 would be highly appreciated.

Reply: We have enlarged the figures and provided error bars, as the reviewer suggests. We have added plots (Figs. 8 and 11 of the revised manuscript) showing the comparisons of the 3 chosen days (November 7-9). November 6 has been omitted, because of no coincidences with lidar or sky radiometer measurements. We attempted to add a plot of the direct comparison, but it looked very busy, as ten different colors or lines were needed. Instead, the residuals between the measured and modeled ΔSCD values have been added in Fig. 4 (Fig. 5 of the revised manuscript) to show the differences seen from the direct comparisons.

Comment 4: P. 9774, l. 2: There is a space character too much.

Reply: Deleted.

Comment 5: P. 9774, l. 16/17: The definition of partial optical depths by F1, F2 and F3 seems to be more complicated than necessary. Can the authors transform them

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*(to e.g. G1*tau for layer 1, G2*tau for layer 2, G3*tau for layer 3 and G4*tau above) at least for the illustration of the averaging kernels in Fig. 3? The Figure would be more comprehensible.*

Reply: The parameterization used here has an advantage that the retrieval can be made without a priori knowledge of the absolute value of the aerosol extinction, while Frieβ et al. (2006) have argued that using inappropriate a priori constraints on the absolute values of the aerosol extinction can easily cause unrealistic or strongly biased results. Instead of the absolute values, our algorithm used a priori information of the profile shape, which is parameterized by the F values, because the (relative) variability of the profile shape, in terms of 1-km averages, was much less than that of the absolute value for the measurement site and period presented here, as seen from the lidar data (Fig. 5 (Fig. 6 of the revised manuscript)). This is now stated in section 2.1 of the revised manuscript. As suggested by the reviewer, however, we attempted to transform these parameters, but found it very difficult, because changing one of the parameters alters the aerosol extinction (and G*tau) values at all altitudes. Instead, the figure (Fig. 3 of the revised manuscript), showing how the profile is represented by the parameters and how it is dependent on each parameter, has been added to assist the readers' understanding of the parameterization used here.

Comment 6: P. 9777, l. 18ff: The comparison of the aerosol extinction from MAX-DOAS, which is derived by the Lidar data as a priori values, with the Lidar data itself might lead to a bias in the evaluation. Please insert a comment on that.

Reply: These influences are now summarized in Table 1 and commented in section 2.1.

Comment 7: P. 9778, l. 7/8 and p. 9779, l. 8-10: The differences of the integration times of the MAX-DOAS measurements and the Lidar/sky radiometer could easily be overcome by averaging the data of the Lidar/sky radiometer according to the respective MAX-DOAS measurements. Is there a reason why this has not been done?

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Reply: I agree with the reviewer, if lidar/sky radiometer measurements could be regarded as continuous measurements. Strictly speaking, however, lidar measurements (and sky radiometer measurements) were not continuous. In the case of the lidar measurements, for example, a cycle of a 5-min measurement followed by a 10-min rest was repeated only twice for a 30-min integration time of the MAX-DOAS measurements. Therefore, we still think that the difference of the integration times can be a source of the differences between MAX-DOAS and lidar (sky radiometer) data.

References

Liu, X., Chance, K., Sioris, C.E., Spurr, R.J.D., Kurosu, T.P., and Martin, R.V.: Ozone profile and tropospheric ozone retrievals from the Global Ozone Monitoring Experiment: Algorithm description and validation, J. Geophys. Res., 110, D20307, doi:10.1029/2005JD006240, 2005.

Rodgers, C.D.: Inverse methods for atmospheric sounding: Theory and practice, Ser. Atmos. Oceanic Planet. Phys., 2, edited by F.W. Taylor, World Sci., Hackensack, N.J., 2000.

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