**Atmospheric
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Discussions

Interactive comment on "Evaluation of MAX-DOAS aerosol retrievals by coincident observations using CRDS, lidar, and sky radiometer in Tsukuba, Japan" _by_ H. Irie et al.

H. Irie et al.

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Received and published: 14 May 2015

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.

General comments:

A general problem of the methodology used by Irie et al. is the fact that elevation angle dependent correction factors are inferred from the disagreement between modelled and retrieved O4 dSCDs. However, the observation of a larger disagreement at higher

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elevation angle is most likely not real, but due to the fact that the measurements at lower elevation angles are much more sensitive for the atmospheric state (much larger weighting functions near the surface). Thus, the retrieval algorithm will always try to bring the measurements at the lowermost elevation angles into closer agreement than at higher elevation angles, even if the discrepancy between measurement and model has other reasons.

Reply: We understand the general statements by the reviewer. In the manuscript, however, we discuss the situation that the retrieved O4 dSCDs at higher elevation angles are too large while the MAX-DOAS-retrieved AOD is underestimated. To increase the AOD, modelled O4 dSCDs should decrease, enlarging the discrepancy between retrieved and modelled O4 dSCDs. This issue cannot be explained by an influence of the sensitivity the reviewer is mentioning. Instead, we conclude that the retrieved O4 dSCDs are overestimated and the disagreement between modelled and retrieved O4 dSCDs is real. As a potential cause for the overestimation, a DOAS fitting error, which cannot be ruled out at this stage because of the lack of independent dSCD observations, is discussed in the manuscript.

Specific comments:

Introduction: Publications on the relevance of atmospheric aerosols for the climate system should be cited (e.g., IPCC report).

Reply: This citation will be added in the revised manuscript.

1019.10: Where did you obtain the pressure and temperature variations from? Do you only have surface data or information on their vertical profile?

Reply: The pressure and temperature variations obtained from the NCEP surface data are used in the forward model. This will be stated in the revised manuscript.

1019.15: The statement that the forward model should be identical to the measurement vector for a perfect inversion is incorrect. There is always measurement noise that

leads to a discrepancy between measurement and modelling.

Reply: We agree. The statement will be revised to consider the measurement noise.

1019.17: I disagree that no prior knowledge on the absolute value of the AEC is provided. If prior information on the AOD and the relative profile shape (i.e., fractional aerosol optical density in different layers) is provided, then the AEC is simply the product of both quantities and therefore specified implicitly

Reply: We agree. In the revised manuscript, we will state that "An advantage of this parameterization is that the absolute value of the AEC is unnecessary in the state vector. Instead, a priori knowledge of the profile shape (represented by the F values) is needed. The AEC is given as the product of the AOD and profile shape but the AEC retrieval is less subject to a prior knowledge of the AOD and profile shape as the resulting a priori error for AEC is large, as discussed below."

Section 2.3: The estimation of aerosol extinction profiles from lidar backscatter signals is usually problematic since the results are very sensitive to assumptions on the backscatter-to-extinction ratio. Please add a description how extinction profiles were derived from lidar data, and a discussion of their uncertainties.

Reply: We will add the description "We assumed a constant extinction-to-backscattering ratio (S) of 50 sr. According to Cattrall et al. (2005) and Tatarov et al. (2006), the S ratio can vary by more than 30% at Tsukuba, with resulting errors in AEC due to the use of a fixed S value occasionally exceeding 30%."

1024.16: It is stated that most aerosols are located at an altitude below 1 km, but from Fig. 2 it seems that the aerosol layer frequently extends to up to 2 km, in particular at the end of the period (after 16. October).

Reply: The word "1 km" will be revised to "2 km".

1026.16: Please specify what you mean with the statement that 'fO4 = 1.00 brought MAX-DOAS AEC values closer to CRDS and lidar data'. How do you quantify the level

of agreement? The R2 is larger for fO4 = 1.25, and also the slope is closer to unity than for fO4 = 1.00.

Reply: The level of agreement discussed here is based on the mean difference and its standard deviation. Discussion on the statistics will be added in the revised manuscript.

1026.12: The better agreement between model and measurement at 3 elevation angle is most likely due to the larger weighting functions at low elevation, see my general comment.

Reply: The AOD comparisons between MAX-DOAS and sky radiometer do not support the reviewer's argument. See above for more detail.

1027.1: I do not understand how the discrepancy between model and measurement can be resolved by adding more or less aerosols to the model profile. The discrepancy between model and measurement should be minimised by the retrieval algorithm and if this is not possible, then this is either due to (1) a problem with the measurements (i.e., systematic biases), or (2) an erroneous forward model or (3) inappropriate a priori constraints.

Reply: We agree with the reviewer but we want to state here the anticipated influences of "artificially" adding or lowering aerosols in the model profile on the O4 dSCD(mdl) and R ratio. For clarity, the word "artificially" will be added in the revised manuscript.

1028. 14: How exactly have the effective temperatures been derived?

Reply: In the period of measurements of this study, the mean surface temperature was 292 K with a standard deviation of 7 K. When the surface temperature varies by 7 K, the estimated effective temperature also varies by 7 K under conditions given in the caption of Table 1. However, differences between effective temperatures at different elevation angles, which are discussed in the manuscript, become much smaller. These information will be added in the revised manuscript.

1030.1: There is no connection between information content and agreement between

measurement and model, and an increase in DOFS does not mean that the measurements are better explained by the model. For this reason, I doubt that the accuracy of the correction factor can be assessed based on DOFS.

Reply: We agree. The paragraph will be omitted in the revised manuscript.

1031.11: I do not think that a poorer agreement of modelled and measured O4 dSCD at high elevation angles is due to inaccurate dSCDs at higher elevations, but due to a general disagreement, combined with a lower sensitivity to the atmospheric state (see also my general comments).

Reply: The AOD comparisons between MAX-DOAS and sky radiometer do not support the reviewer's argument. See above for more details.

Technical correction:

1020.5: 'AOD F1' -> 'AOD x F1'

Reply: This is a typeset error and to be corrected.

1025.22: 'small' -> 'smaller'

Reply: To be corrected.

Table 1: First and second row show the same numbers (or is this by coincidence?)

Reply: This is a typeset error and to be corrected.

Figure 5: What do you mean with 'each bin of the CRDS or lidar data'? Information on slope, intercept and R^2 for the rightmost panel is missing (also in Fig. 7).

Reply: This phrase will be revised to "each 0.05-km^{-1} bin of the CRDS or lidar data." Information on slope, intercept and R^2 for the rightmost panel has been omitted, because we do not expect a linear correlation due to less sensitivity of our MAX-DOAS to high-altitude aerosols (and therefore AOD) and potential influences by clouds, as discussed by Irie et al. (2008a), Irie et al. (2008a), and Irie et al. (2011).

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