



Interactive comment on “Inversion of tropospheric profiles of aerosol extinction and HCHO and NO₂ mixing ratios from MAX-DOAS observations in Milano during the summer of 2003 and comparison with independent data sets” by T. Wagner et al.

Anonymous Referee #1

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General comments

This paper presents an inversion scheme retrieving profiles of aerosol extinction and HCHO and NO₂ mixing ratios from MAX-DOAS. The inversion scheme is applied to MAX-DOAS observations (with a unique setup using three telescopes) during the FORMAT campaign. The results are compared with independent measurements and inter-

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compared for the different telescopes to assess the ability of the inversion method. Moreover, by additionally developing a cloud classification algorithm, potential effects of clouds on the profile inversion are investigated. An assessment for the ability of the profile inversion performed in this way is unique and the subject of this paper is appropriate for AMT. However, I raise some points that require additional consideration. For example, I am afraid that the presented inversion scheme may be too incomplete to give a systematic interpretation for the inversion results. I recommend this paper will be published after adequately addressing my concerns, which are described in detail below.

Specific comments

As mentioned at several places in the manuscript, we need to pay much attention to the stability of the retrieval. Because of this, I am afraid that the presented inversion scheme is too incomplete to give a systematic interpretation for the inversion results. For example, the results may change considerably in the retrievals that use different initial values. Since the instability comes from the fact that the problem the inversion method tries to solve is ill-posed, would it be a reasonable way to add the results of AOD and VCD, which are retrieved under stable conditions with prescribed values of both layer height and shape parameter? I think that this study clearly indicate the need for the optimal estimation method, and recommend stating this in the manuscript, although I realize the authors use a different strategy without a priori profile information.

I understand from the presented radiative transfer simulation that the diffusing screen effect can lead to an underestimation of AOD. In reality, however, cloud particles can change the path length in a way similar to aerosol particles, so that a clear separation of AOD from the cloud optical depth (COD) is very difficult with the inversion using O₄. In this case, it is expected that the retrieved AOD should contain a contribution of COD, leading to overestimation of AOD. Is it reasonable to add this additional important influence of clouds? Why don't the AOD retrieved in this study show such overestimation?

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It seems to me that section 3.4 is hard to understand. Please consider to simplify it.

As mentioned in section 3.4, the retrieved aerosol layer height requires a correction factor as high as 2. I am afraid that this correction is crucial and it is unfair to say that the inversion method presented in this study can retrieve a vertical profile.

Technical corrections

Please use a word, FORMAT, FORMAT 2003, or FORMAT-II campaign, consistently throughout the manuscript.

p.3898, line 25: "formaldehyde" should be "HCHO". "nitrogen dioxide" should be "NO2".

p.3899, line 15: "accuracy" should be "accuracy".

p.3899, line 15: Why is the accuracy for ultra-light research aircraft measurements better than that for ground-based measurements?

p.3899, line 27: Why does a full sequence take 10 min, while a single measurement of 90 sec is made five times (the number of elevation angles) in the sequence?

p.3900, line 8: Please explain why Fraunhofer reference spectrum is better to contain small atmospheric absorptions.

p.3901, line 9: While the O4 VCD varies with temperature and pressure, a constant O4 VCD has been assumed. How much is the actual diurnal variation of O4 VCD due to changes in temperature and pressure? Does it influence the accuracy in determination of AMF?

p.3903, line 4: It is stated that the trace gas concentration or aerosol extinction is constant below the layer height. However, this is not the case if the shape parameter is larger than 1. Please correct this.

p.3904, line 24: "of " should be "or".

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p.3905, line 1-5: I do not understand the sentences "Exponential profiles ..."

p.3905, line 12-13: Why do both possibilities fail if smooth vertical gradients have to be described?

p.3907, Eqs.(8) and (9): It seems to me that the equations should be "AOD/L(2-S) or VCD/L(2-S)". Please check them.

p.3912, line 17: Please revise the sentence "Fortunately ..." to be more quantitative one.

p.3914, line 23: It is unclear to me what the results of the radiative transfer simulations presented in Fig.5 indicate here.

p.3915, line 4: I also think that aerosol retrievals with $S = 1.1$ is not a good choice, because it requires a correction factor as high as 2.

p.3915, line 8-10: It is stated here that the results of the trace gas inversions are more realistic and consistent, if $S < 1$ for the aerosol profile inversion is chosen. However, on p.3914, line 14-17, the authors mention that stable results for AOD have not been obtained for all days with $S < 1$. I think that using different $S(aer)$ values for aerosol and trace gas inversions to achieve good agreement for both cases would be unfair.

p.3915, line 20: It would be better to say that "... the dAMF(alpha) values for NO2 and HCHO do not depend on the absolute value of VCD, ...".

p.3915, line 27: Why the absolute value of dAMF can be obtained from the comparison between normalized (i.e., relative) quantities dSCD and dAMF?

p.3918, line 15-17: For the light path along the instrument line of sight to increase, the cloud bottom should be higher than the altitudes, where the last scattering by air molecules occurs under clear sky conditions. This condition should be mentioned here.

p.3919, line 7: "... the dAMF(alpha) are ..." should be "... the dAMF(alpha) values are ..."

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p.3919, line 12-25: The profile inversion is used to investigate the diffusing screen effect. However, I am skeptical that the used profile inversion is suitable for this purpose, because it may be unstable. Was the layer height L stable for all cases investigated here? I expect that aerosol inversion tends to retrieve AOD that contains a contribution from cloud optical depth assumed and may show overestimation. Why doesn't this tendency appear in Fig. 11?

p.3921, line 1: I understand that the colour index is less sensitive, but does it give additional information?

p.3925, line 18: What are the parameterised MAX-DOAS errors?

p.3927, line 13: Perhaps a better word is "available" than "possible".

p.3928, line 21: Please delete "a" just before "rather".

p.3931, line 10: It seems to me that the word "true" is too strong to be used as a value obtained by inversion.

p.3931, line 16: It would be better to explain "systematic dependence" quantitatively, for example, using the correlation coefficient, slope, and intercept.

p.3934, line 26-29 and p. 3935, line 1, 9-12: What do the ranges of agreement (+/-) represent?

p.3935, line 9: Would it be better to mention the agreement for aerosol extinction as well?

Interactive comment on *Atmos. Meas. Tech. Discuss.*, 4, 3891, 2011.

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