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Atmospheric Measurement Techniques Discussions

Interactive comment on "On the improvement of NO_2 satellite retrievals – aerosol impact on the airmass factors" *by* J. Leitão et al.

Anonymous Referee #2

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The manuscript "On the improvement of NO2 satellite retrievals - aerosol impact on the airmass factors" by J. Leitao et al. analyses aerosol effects on NO2 AMFs in depth. It thus contributes a valuable, extensive study of high relevance for the quantification of NO2 columns from satellite measurements. The topic matches the scope of AMT well. But before publication, several issues have to be solved, as listed below. Some of them might require major revisions.

General comments:

1) The paper analyses the impact of several aerosol parameters on AMFs, i.e., a multi-dimensional parameter space, which is of course challenging to break down to structured results and conclusion. Nevertheless, the overall structure of the paper might be

improved. In detail, I suggest to introduce some additional sub-sections, especially in section 2.3 (one sub-sections per parameter, in analogy to section 3). Also the different approaches (A to H versus I to P) could be separated by new sub-sections.

2) In this study, the sensitivity of satellite observations for tropospheric columns is expressed in terms of airmass factors (AMFs). Given that AMFs are the central quantity, their definition is far too vague. In particular, no clear distinction between the height-dependent sensitivity (Averaging Kernel or box-AMF) and the total AMF is made; the former does not depend on the (relative) trace gas profile, but the latter does! Both quantities are inaccurately mixed in this study using the same term (Fig. 1 shows box-AMFs!). I thus recommend to add an extra paragraph (like "The concept of AMFs"), preferably at the beginning of the method section, to clarify this matter, and give also some equations.

3) Only one (quite low) value of 0.03 was considered for the surface albedo. This is not sufficient: even if this value would be representative for urban areas, NO2 plumes might also be transported out of the cities; in addition, there are sources at non-urban sites, e.g. power plants, or biomass burning. The authors have to repeat their study for at least one higher surface albedo value, and report on the resulting effects.

4) Aerosols also have effects on cloud retrievals. This aspect is shortly mentioned in the conclusions, but should be discussed before in some more detail, at least qualitatively. To what extent do cloud correction schemes also correct for aerosol effects, and under which circumstances clouds and aerosols have to be treated differently?

5) The impact of aerosols is the main topic of this study, but figs. 5-11 show AMFs as funtion of the SZA. One can not see the general effect of AOD on AMF easily. I strongly recommend to re-organize the figures in such a way that the aerosol property under investigation is presented on the x-axis (either replacing or supplementing the existing figures). On the other hand, some differentiations of the plots might be left out (e.g., the discrimination of DD and BB in fig. 8, which has no effect) and just be mentioned

in the text.

Minor comments:

1) The introduction is extensive and might be shortened. Subsections 1.1 and 1.2 are partly rather "methods" than "introduction".

2) p3222 line 3: rather "substantially" than "partly"

3) p3222 line 17: delete "a"

4) p3223 line 8: "interesting information": please be more specific.

5) p3226 line 20: how far is this "a first approx."?

6) p3231 line 3: "0.6, 1.0 or 2.0 km height".

7) p3231 line 25: "datasets of"

8) p3233 line 12: In heavily polluted scenes, AOD may be considerably higher than 0.9.

9) p3233 lines 17-21: In line 17, SSA is varied between 0.8 and 0.95, but in line 20, it is varied between 0.93 and 1. More confusing, the latter range (where the minimum is almost equal to the maximum of the former range) is used to estimate the "maximum effect".

10) p3235 lines 9-11: If the profiles are not representative, but extreme scenarios instead, they might indeed have impact on the conclusions!

11) Results: The labels used for the scenarios (table 1) should be added in the description of the scenarios in the plain text (for instance p3237, lines 6-9).

12) p3237 line 11: correct "a too low the BL".

13) p3238 lines 10-12: Is this also true for low SSA?

14) How far is an aerosol layer above the NO2 layer different from a cloud? (see (4) of

the general comments)

15) Section 3.4: Again, the values for SSA are confusing. Why not have just 3 values (0.8, 0.9, 1) in the RTM runs to get the general picture?

16) p3243 lines 1-3: Even without aerosols, an underestimation of the BL leads to overestimation of NO2 the column!

17) p3243 lines 4-5: Is this also true for low SSA?

18) p3243 line 26: "moderate": please be more specific.

19) Given the importance of simultaneous profile information of both aerosols and NO2, it might be worth adding a reference to the potential of MAX-DOAS measurements.

20) The conclusions miss some important aspects out: What is the conclusion for operational NO2 retrievals? Can you give an estimate of the errors made by neglecting aerosol effects (or considering them being corrected by the cloud retrieval) in current NO2 retrievals (at least a sign)? Do you recommend to ignore aerosol effects until better aerosol data is available? Which kind of aerosol information (AOD, BL, ...) is needed (with which accurracy) to significantly improve the NO2 retrieval, and is there a chance to have it in near future?

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 3221, 2009.