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Atmospheric Measurement Techniques Discussions

Interactive comment on "Determination of particulate matter vertical columns using satellite observations" *by* A. A. Kokhanovsky et al.

Anonymous Referee #2

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1) General comments

The paper presents a new approach to estimate particulate matter column concentrations from multi-spectral satellite data as comprehensive as possible. The new element is twofold in deriving a column quantity and in using the Angstrom coefficient derived from satellite multi-spectral information to estimate the effective particle radius. However, this study makes many assumptions (e.g. mono-modal size distribution, fixed refractive index, fixed single scattering albedo) and is validated only for one case study (1 day over Germany) where a really homogeneous aerosol type prevails. As the correlation between the satellite-retrieved and ground-based Angstrom coefficient is not convincing (the noise range is close to the range of variability of the ground-based dataset for that day) proof of the method requires its application and validation to at

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least few further case studies with larger variability of aerosol type (between days or over the study area).

Finally, the paper makes an attempt at converting column particle mass concentrations into ground-based PM10 values by using ECMWF boundary layer height (which frequently agrees quite well with CALIPSO), which would in fact be highly valuable as cross-border transfer standard. However, the validation in its present form does not make any distinction between rural/background sites and traffic site locations (in figure 10 only a hint is given with the note “traffic”, but it is not clear to which / how many stations this applies). Using EMEP background stations or marking background and traffic stations in figure 10 would show how good the retrieval is under the conditions for this one day. If all stations used were inner city locations the dataset is not useful for validating 5x5 km2 satellite results and alternative (EMEP) measurements should be sought. With an advanced validation (further days 7 more variable aerosol type and better characterizing each PM10 station) the value of the study could be increased significantly.

2) Specific comments

Both the abstract and introduction are very short. Also figure / table captions are very short.

Several parts in the introduction (p. 1028) and discussing the satellite advantages (p. 1037 f.) are very general in nature and contain no specific new arguments.

Discussion elements are spread over several sections of the paper (e.g. p. 1033 / l. 3-16) rather than condensed in the discussion section, where the reader would find it easier to get a comprehensive assessment of the value of the results.

The state of the art is extremely short and no information on achieved accuracies of other studies and their temporal / spatial reach is provided – I recommend to extend this part and include it into the introduction thus clearly separating it from the method

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description.

The method part related to assuming desert dust particles is not used further in the paper and therefore should be deleted.

In the discussion of difficulties for the validation arguments 1 and 3 are almost identical – should be shortened.

In fig. 5 correlation coefficient and regression function should be provided.

In fig. 9 background and roadside / inner city locations should be marked with different colours.

In fig. 11 the histograms show an overall underestimation of the satellite results, which should be stated in the text.

3) Technical corrections

English grammar needs thorough proof reading as there are several incomplete or wrong sentences.

A motivation should be stated, why the satellite results are retrieved on 5x5 km2 grid instead of using the full 1.1 km2 resolution.

Fig. 1 shows graphically the phase function used (which is quoted from another study) – I do not think it is needed.

A vague statement “some other factors” on p. 1037 / l. 13 is not helpful – explain or delete.

The statement that you use 1x1 km2 satellite results (p. 1038 / l. 10) is incorrect – you work with 5x5 km2.

The statement on trend analysis (p. 1038 / l. 12-14) is much too general and optimistic – although satellites have the potential for systematic retrieval, trend analysis requires thorough calibration control and for long-term monitoring inter-satellite cross

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calibration, which is very demanding – this must be added to this statement.

The statement on PM2.5 retrieval at the end of the discussion is unclear – why not use a similar approach rather than applying local correlations PM10 – PM2.5? The difference in AOD is defined inconsistently (AERONET-SATELLITE in tab. 1 – SATELLITE-AERONET in figures).

In fig. 6 pixels with large error due to incorrect cloud detection are clearly visible at edges and near the Alps (red) – this should be stated. Also the subsequent error in PMVC is visible in fig. 9 and should be stated.

In fig. 9 the letters showing major city locations are not visible very

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Interactive comment on Atmos. Meas. Tech. Discuss., 2, 1027, 2009.