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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.

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The authors agree with the reviewer that there are many assumptions on the way from the measured satellite signal to PMVC product. However, one must remember that a considerable time is needed to verify assumptions in satellite retrievals and improve them. For instance, in cloud remote sensing an assumption of a single plane parallel cloud is used up to now. Clearly, his is not a realistic assumption for most of cases.

With this in mind we proposed the new technique, which must be checked and improved in further research. We assumed (as pointed out by a reviewer as well): monomodal size distribution, fixed refractive index, fixed single scattering albedo.

The general grounds for these assumptions are as follows: 1) As shown by Hansen

and Travis (1974), the monomodal and bi-modal size distributions have very similar optical properties, if the effective radius and variance of both distributions are fixed. Therefore, the bi-modal or mono-modal size distributions can not be distinguished from a satellite (at least as far as MERIS measurements are of concern). 2) There is no way to assess the aerosol single scattering albedo from MERIS observations. Therefore, we assumed the typical value for the continental aerosol. Because the aerosol optical thickness is low, the assumption will not influence the retrieval results in a great extent. The same applies to the refractive index. The corresponding sensitivity study can be easily performed but this is not a task of this work.

The reviewer says that the correlation between the satellite-retrieved and ground-based Angstrom coefficient is not convincing. We agree with this point and, therefore, we delete the correlation plot from this paper. More points on this plot covering the Angstrom coefficient values from 0 to at least 2.5 are needed to have right to speak about correlations. However, our retrievals of the Angstrom coefficient are not bad and errors are below 10% for most of cases. This shows the power of our approach (at least for the case studied).

The focus of this paper is on the determination on PMVC. So the study on PM10 is out of focus and we do not like to extend the paper in this direction. Also we do not like to remove Fig.10 because it shows the skill of the method. Reviewer says that 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). We would like to underline that cases with are all belong to traffic sites not resolved by this algorithm. The note is added to the paper as well.

We agree that with an advanced validation the value of the study could be increased significantly. However, we feel that this effort is well out of scope of this paper, which is aimed to the general presentation of the method, which can be also used and checked by other research teams worldwide. Also for validation the selection of very diverse

situations must be used. Please, note that the covered area corresponds to the whole country. So the numbers of points and the range of spatial variability is not small.

With respect to the specific comments, they all have been accounted for in the revised paper. We have extended both the abstract and introduction. Also figure / table captions were extended. The method part related to assuming desert dust particles is not used further in the paper. However, we think that the analytical derivation of the parameter is of interest on general grounds. So we prefer to keep these sentences in the paper. In the discussion of difficulties for the validation argument 1 was deleted from the revised paper as advised by the reviewer. In fig. 11 the histograms show an overall underestimation of the satellite results and this was stated in the revised text.

With respect to technical corrections, they have been accounted for. However, we really like to keep Fig.1 with the phase function used because this function is of paramount importance for the quality of retrievals.

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