



Interactive comment on “Assessments of urban aerosol pollution in Moscow and its radiative effects” by N. Ye. Chubarova et al.

Anonymous Referee #1

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This manuscript by Chubarova et al, entitled "Assessments of urban aerosol pollution in Moscow and its radiative effects" presents analysis of AERONET retrievals of two sites. Unfortunately, it is not possible to make a full evaluation of this study based on the current form of the manuscript. Therefore, my purpose below is to mention the major comments only.

The description of methodology is too cursory. More details should be provided about the statistical approach, method and assumptions used etc. Now far too little information is given, which is moreover left vague to some extent. Some examples below.

Regarding the section 2)

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1) Explain the statistical test you used. How did you consider the uncertainty in AERONET-retrieved AOD? It is 0.01-0.02 (Eck et al. 1999). You say that the difference of 0.02 is statistically significant. Please give the details behind this conclusion, considering the AERONET uncertainty which is about the same size than this difference. The AERONET-uncertainty should be considered and discussed throughout, regarding AOD differences, Angstrom Exponent, SSA.

2) The descriptions were occasionally difficult to follow. Just as an example, in lines 15-15 of the block 5473. You say that the difference in M1 can be under-estimated due to the different number of observations. Earlier you say that the difference is based on pairs of quasi-simultaneous measurements. If you have pairs, why do you have different number of observations in the analysis. And even if you do have, for some reason, why it should result in underestimation, in other words why the effect would be systematic?

Regarding the section 3)

1) You go directly to the differences, it would be informative to have first seasonality of AOD (for one site or both) to get an idea about the AOD variability too, not only about the differences. Also, is there seasonality in the prevailing wind directions?

2) There are two type of speculative conclusions in the Results -section: 1) some of them could be further assessed, 2) some of them should be better explained/justified. About the first category, you mention that SCIAMACHY resolution may play a role. It would be very easy to take OMI, with better resolution, and focus on Moscow, to get evidence if this is the case. About the second type of conclusions (just one example), discussion in lines 20-28 in the block 5475 about the dominance of natural processes, only based on the correlation in Angstrom exponent, is not clear and with obvious evidence.

3) Figure 10 is explained in Conclusions, why not in Results? Also, it is not clear how exactly the "temporal lag" was accounted for. What data and how used. Here, as in

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many places more generally, the reader was not provided with the sufficient details.

The lack of sufficient details is the major weakness in the current form the manuscript. Once those will be given and the descriptions and conclusions will be clarified, it will be possible to evaluate the manuscript.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 5469, 2010.

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