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Atmospheric Measurement Techniques
Discussions
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Interactive comment on "Long-term variability of aerosol optical thickness in Eastern Europe over 2001–2014 according to the measurements at the Moscow MSU MO AERONET site with additional cloud and NO₂ correction" *by* N. Y. Chubarova et al.

Anonymous Referee #2

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General comments.

This paper is basically divided into two parts. The first part introduces interesting analysis with which the authors refine the AERONET Level 2.0 spectral data AOT for Moscow_MSU_MO site through a cloud screening based on visual cloud observations, eliminating hourly observations in which cloud cover by cirrus is greater than 6 tenths from March to October (8 months), and hourly observations with overcast skies by cir-

rus (10 tenths) in winter months. An additional NO2 correction considering in-situ NO2 observations at Moscow is performed since the NO2 climatology used by AERONET (based in SCHIAMACHY data base for Moscow AERONET site) significantly underestimates the NO2 content in this AERONET site. This AERONET station is the only one available in a wide region of Eastern Europe so that the results are also relevant for its geographical representation.

In the second part of the paper, chubarova et al. use the revised dataset of monthly mean aerosol optical thickness and Angstrom exponent in Moscow AERONET site for studying and characterizing aerosol seasonal changes as well as AOT long-term variability over the 2001–2014 period. This second part has a more scientific content as it provides an assessment of the corrected AOT values trends at Moscow in the period 2001-2014 crossing the results with data on emissions of various components (WebDab-EMEP database) that could potentially affect AOT in this site.

Since Atmos. Meas. Tech. Journal is dedicated to the publication and discussion of advances in measurement techniques, all technical and methodological aspects should be described with minimal detail. In this framework there is a clear imbalance in the treatment given to the two issues of technical and methodological character which are discussed in the first part of the paper.

The main concern is the methodology used for a second cloud screening since the largest correction of AOT climatology for Moscow clearly comes by removing AERONET data coincident with the presence of cirrus clouds (see Figure 5). I interpreted the screening cloud using cloud cover, it only applies to the AERONET observations in which there are only cirrus clouds, otherwise the approach used would be wrong because in this case valid AERONET observations would be removed of arbitrary manner which would affect the aerosol climatology of unknown way. It is well known that the AERONET algorithm may fail with homogenous and persistent cirrus blocking the sun. In other cases the cloud screening is quite effective. Unfortunately the description of the cloud screening methodology is quite short and does not provide the necessary details to understand how it has been designed and performed (see specific comments).

Although the correction of NO2 is a minor correction compared with that performed with a second cloud screening, it is described in more detail. In this last case, and from a methodological point of view, authors should better assess the new findings in relation to those reported in the paper of Chubarova and Dubovik (The sensitivity of aerosol properties retrievals from AERONET measurements to NO2 concentration over industrial region on the example of Moscow) published in 2004. Concerning the NO2 climatology it is clear that AOT corrections are needed in megacities and major urban sites where NO2 values are significantly higher than those of the SCIAMACHY data base, although we must emphasize that this correction is, in most cases, lower than the AOT measurement uncertainty (0.02). However, the methodology used in this study does not seem the most appropriate since it is limited to replacing the AERONET SCIAMACHY-based NO2 climatology by an "improved" NO2 climatology based on observations, when in the second part of the paper an AOT trend analysis is approached. Why not address the correction of the AOT series with observed daily/monthly/annual NO2 data series? NO2 varies significantly in very short periods, and clear negative trends in large urban areas have been reported (i.e. Schneider et al., (2015) and Hilboll et al. (2013), the latest referenced in the paper).

The joint analysis of AOT data series and SOx, CO and NOx emissions is performed with annual mean values. This is a simple approach that can be misleading, especially when the authors previously identified that the statistically significant trends of mean and daily maxima AOT500 values are observed in April, May and September. Why not pay attention to explain trends in these months taking into account that atmospheric processes driving AOD are not the same throughout the year? Related to this, somewhat surprisingly, the authors claim that no significant correlation was statistically obtained in AOT relationship with different meteorological parameters and CAPE. Probably there is no correlation working with annual averages, but it is diffi-

cult to admit that in certain months/seasons, meteorological conditions do not affect AOT values. It is clear that the synoptic patterns may be subject to important interannual variations in certain months/seasons affecting local/regional meteorology around Moscow (i.e. precipitation, affecting wet deposition, wind and mesoscale convective processes, affecting dust resuspension. . .) and aerosol long-range transport from very different regions.

Surely, the negative emissions of aerosol precursors play an important role in the AOT negative trend found in Moscow, as stated by the authors, but they should further investigate the role played by air masses transport and, for that, the study should be performed for each month/season, at least in months when a clear negative trend in AOT is observed (April, May and September).

I recommend this paper to be considered for publication in AMT after these major comments have been adequately addressed, and have been properly answered the specific comments below.

Specific comments

Page 7845; Line 7: Some basic references should be provided for each satellite platform. Acronyms should be described.

Page 7845; Line 10: Some basic references should be provided for each aerosol metwork. Acronyms should be described.

Page 7845; Line 14: ".. is equipped by Cimel sun/sky..." should be "... is equipped with Cimel sun/sky..."

Page 7846; Lines 26-27: Notice that the uncertainty of 0.01 corresponds to a Master instrument. The uncertainty of a filed instrument, as that of Moscow MSU MO AERONET site, is 0.02 in the visible range (Eck et al., 1999).

Page 7847; Section 3.1.1. This issue was already addressed by Chubarova et al (2011) using similar criteria and even then with little details. Authors should explain whether the cloud screening was performed only for cirrus or for all type of clouds. They must clarify whether they applied the cloud screening using quasi-simultaneous Cimel-Cloudiness observations at the same site, and then corrected daily averaged AOT values are obtained. They must also address the fact that although there is presence of cirrus, the Cimel direct sun measurements may not be affected by them (cirrus are not blocking the sun) causing valid Cimel data removing. How can this affect the corrected AOT database? Moreover, authors should explain the limitations of visual observations, as these observations are subjective, and performed by several observers may have very different observation skills.

Page 7850, Line 13: The following paper: Chubarova, N. E., Larin, I. K., and Lezina, E. A.: Experimental studies and modeling of nitrogen dioxide variations in the lowest troposphere layer in Moscow, Newsletter Moscow State University, series 5, Geography, 5, 11–18, 2010, in which is based the evaluation of NO2 content, is not easy to be accessed. It might be included as supplement information. What is the NO2 uncertainty from in-situ observations + modeling?

Page 7850; Line 13-Page 7851; Line 2; Detail, please uncertainties in NO2 estimation related with Boundary Layer height assumptions.

Page 7851; Lines 20-26; Errors of the means should be added in estimated NO2 optical thickness. OT(NO2) values are lower (no higher) than the uncertainty for AOT in wavelengths > 440 nm (0.02).

Page 7857; Lines 22-23; Authors should discuss the fact that no trend is observed in PM2.5 concentration when they attribute the negative trend of AOT500 to negative emissions of aerosol precursors (basically of anthropogenic origin), since PM2.5 mainly accounts for anthropogenic aerosols.

Page 7858; Lines 2-4; Although the assessment of AOT trends has been addressed in General Comments, authors should avoid simplistic and misleading arguments, like this one. In 2009 emissions of SOx and NOx, similar to 2006, were registered but,

instead, AOT values were significantly lower than in 2006. Why?

Figure 1 Caption; Units of water vapour in Figure 1a is cm?

Figure 1 Caption; Notice that AOT uncertainty marked (0.01) corresponds to a Master AERONET instrument not to an AERONET field photometer (0.02 in this case).

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 7843, 2015.