

## ***Interactive comment on “Aerosol seasonal variations over urban sites in Ukraine and Belarus according to AERONET and POLDER measurements” by G. Milinevsky et al.***

**Anonymous Referee #2**

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The paper deals with the new data obtained within the frame of AERONET network and satellite POLDER instrument over several urban sites in Ukraine and Belarus located in the Eastern Europe. This is the area, which does not have accurate aerosol data measurements and any new statistic and its analysis is very valuable. However, there are several major and a lot of minor comments which are necessary to take into account before publishing the manuscript. I hope my recommendations will significantly improve the final version of the text. Major remarks: 1. The structure of the paper is not good. Seasonal features are described several times, there are many other duplications in the text. I would recommend to re-arrange the text combining the data description, meth-

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ods, and the results in the separate sections. In addition, all the data, which describe seasonal changes, should be considered together and then the authors could add the material about the interesting urban experiment and its results. 2. I would recommend to add the POLDER measurements as a base for the analysis over the considered area or at least over the urban centers(not only over few of them). It would be important to see the comparison of the POLDER AOT retrievals with ground-based measurements in a special method description section. After this analysis, if the validation shows good results, the authors could describe the ground-based and satellite retrievals together (for example, considering their seasonal changes). 3. I agree with the first reviewer that the analysis of the data is not enough, it includes mainly the description of the Figures. This should be significantly improved. 4. I am not a specialist in English but even I can see a lot of bad English style expressions, the absence of articles in many cases and even mistakes in grammar. This should be also improved. Minor remarks: 1. In the Introduction it should be clearly stated what are the principally new ideas and intent of the authors in this study. What is the difference with their previous paper in the ACP, 2013. I would recommend, in addition, to give more attention to the results of POLDER data. 2. Since the authors stress on studying the urban effects, the additional references on aerosol urban effects might be added, the results should be compared and analyzed in the text (see, for example, Zavadska et al. *Atm. Env.* 2012, Chubarova et al. *AMT*, 2011, others). 5. The description of aerosol ground-based measurements can be shortened. When describing Angstrom exponent the authors should mention the conditions on Junge distribution which is not often observed in reality and is necessary for reliable evaluating of Angstrom exponent. Please, make a special section on the data and method description in which all different methods are described. The sources of the aerosol can be described separately. The authors should be very cautious and describe thoroughly all specific conditions when they use the level 1.5 data. I would recommend to describe the reasons in each time since the absence of level 2 data could mean the poor final calibration of the data. But I understand that utilizing level 1.5 in single scattering albedo is sometime necessary. The authors sometimes

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tried to describe these aspects but irregularly and in different places of the text. Please, combine this analysis in one place. 6. Page 10736 lines 1-5. Too much information and duplications in the description of Angstrom exponent. 7. Page 10736 line 10-13. Duplication. 8. Page 10736 line 16-17 As far as I know the uncertainty of AOT latest measurements is about 0.01 in visible and in near-infrared channels and 0.02 in UV. Please, check and make a reference. 9. Page 10737 line 7-8. Another values for the AOT uncertainties? 10. Page 10737 line 17. POLDER data should be compared with the AERONET. The uncertainties of POLDER measurements should be stated. 11. Page 10737, line 27. Fine mode aerosol can be also produced by biomass burning for example. There are a lot of publications on this account. This should be clarified in the text. The procedure of satellite data cloud screening is very important for evaluating good aerosol data. This should be described and analyzed in the text. 12. Page 10738, section 4. In the analysis of seasonal changes I would recommend to combine POLDER and AERONET data. 13. Page 10738, section 4. AOT at 440 is significantly contaminated by NO<sub>2</sub> in the urban areas. The correction in AERONET algorithm is not full (see, for example, the discussion in Chubarova et al., 2011). The wavelength 440nm is near the maximum of the NO<sub>2</sub> absorption. So I would recommend to use another wavelength in the analysis, the same or close to the POLDER wavelengths, for example. This would make the analysis of the satellite and ground-based AOT data more clear and comparable. 14. In the analysis of seasonal changes, please, add the comparisons with the available climatologies over this area (see, for example, Kinne et al., "A new global aerosol climatology for climate studies", 2013, Chubarova et al., 2009). 15. Page 10739 line 3. Missed reference to Fig.1 a?. 16. Page 10739, line 7. The use of Figures should be in a sequence form. Please, change numeration of Fig.4. Figure 4 does not prove the statement of prevailing the coarse mode aerosol in the fall and winter. Please, check. The prevalence of fine mode aerosol might not be the urban effect. The authors should make the analysis in the non-urban conditions in the region for this statement as well. 17. Page 10739, line 13. You mentioned prevailing the coarse mode particles from January to May 2012. Please, analyze this

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effect in more details, use the vertical profiles and any additional information (for example, available synoptic meteorological data to remove possible cirrus contamination) to prove your statement. 18. Page 10739 line 22. Please, put this part in a separate section with more details on the meteorological conditions with the description of the pollution sources, etc. Please, make a comparison with the other results. I would recommend to add much more material and discussions in this section. 19. Page 10741, line 1. There is no strong seasonal dependence in real part of refractive index. Please check. 20. Page 10741, line 4. Please, make a table with the statistics of cases and mean, median, st deviation, etc. AOT values for different sites. 21. Page 10741, line 8. It is strange to see biomass burning as a source of aerosol in winter. Please clarify. 22. Page 10741. Section 5 "Comparison. . . Please combine this part with the previous seasonal changes analysis. 23. Page 10743. Section 6. I would recommend to make the analysis only on POLDER data over the whole territory with adding winter data from AERONET. Otherwise the title of the section is not right and there is too small material. At least the data on many Ukrainian and Belarus urban sites should be added. Please, re-arrange the structure. 24. Page 10744, line 9. The effect of the Saharan dust should be studied in more details using the profile information or other data.

I would recommend the paper to be published after making the corrections.

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Interactive comment on Atmos. Meas. Tech. Discuss., 6, 10731, 2013.

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