

Interactive comment on “Effects of urban pollution on UV spectral irradiances” by R. L. McKenzie et al.

Anonymous Referee #1

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Referee comments to the paper MS-NR: acpd -8-7149-2008

Effects of urban pollution on UV spectral irradiances by R. L. McKenzie et al.

The paper is devoted to the comparisons of UV irradiance over an urban and clean site in order to understand the role of urban pollution in UV attenuation. The authors have made significant efforts to organize this experimental study. The results are based on about 1 year of simultaneous spectral precise measurements at the urban site in Tokyo and at the pristine site (Lauder New Zealand), where the effects of urban pollution can be neglected. In addition, the RT calculations were used for better understanding of the obtained results. Unfortunately, the supplementary information about the aerosol and cloud properties was unavailable in this study that made the analysis much more

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complicated. However, I think that this analysis is very helpful in understanding the role of different factors in solar radiation attenuation over urban area, although not all the problems were solved and even several new ones have been arisen. The paper is well organized and the summary is quite complete. However, there are several remarks, which should be clarified before accepting it for the publication in ACP.

General comments:

Two points should be discussed in more details:

1. The possible difference (or its absence) in cloud optical thickness and cloud amount over Tokyo and New Zealand in different seasons.
2. The absorbing properties of aerosol over Tokyo and spectral features of absorbing aerosol optical thickness. Even in model calculations the authors used only one value of single scattering albedo for the whole period of observations which is constant over the UV spectral interval.

I would recommend the authors to use for this analysis additional literature sources and to discuss the uncertainty due to these factors.

Specific comments:

1. p.7151, line 26 .What was the time interval for the midday conditions?
2. p.7152, line 27-28. From what sources the total ozone amount was taken? It would be helpful if the description of the seasonal features of main atmospheric parameters is included in the additional paragraph. I propose to add the solar angle changes in Fig.1. It will be also helpful to add some climatic features of cloud and aerosol properties from literature sources.
3. p. 7153, line 4. The difference can be also observed due to the difference in cloud properties over Tokyo and Lauder. It should be discussed in the text.
4. p.7154, line 9. In Fig.2a it is better to show solar time on the axis.

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5. p.7155 line 1. Table 3 is not very informative. All helpful information has been already included in the text. I propose to remove the Table 3.
6. p.7155, line 5. Too large solar angle intervals of 5 degrees can lead to an additional bias. I would suggest the authors to make the additional model correction within the bins or to discuss this point in the text.
7. p.7155, line 11. The different slope in UV-A and visible region can be attributed not only to the NO₂ content but to the aerosol. Aerosol optical thickness has a distinct spectral dependence and, therefore, this can lead to the noticeable spectral dependence shown in Fig.3. This should be clarified in the text.
8. p.7155, line 23. I propose to emphasize here or earlier (at line 19) that this spectral dependence takes place in situation with higher ozone over Tokyo.
9. p.7155, line 25. Does this unexpected increase lie out the error bars? It should be clarified.
10. p.7156, line1. It can be also the effect of higher cloud optical thickness and cloud amount. During summer conditions Japan is located in the area of heavy monsoon, which is characterized by significant increase of cloudiness.
11. p.7156, line 5. Is the difference statistically significant?
12. p. 7156, line 7. Fig.4 is not clear. I would recommend the authors to show the iso-lines of several large SZA and use black and white scheme instead of the photo to have better contrast. I would also propose to make several calculations of the obscuration effect using at least isotropic radiance distribution and to add it in the analysis.
13. p.7157, line 13. Do I understand correctly that the calculations were made using the exact SZA of each scan measurement?
14. p.7158, line 26. I propose to show the error bars on the curves of Fig.7.
15. p.7159. Section 6. It would be helpful to have the direct comparisons between the

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two methods using the additional figure.

16. p.7160, line 23. The year should be 1984 as I understand from the reference list.

17. p.7161, line 5. I would recommend including the value of the mean tropospheric NO₂ content in the caption of Fig.5.

18. p.7163, line 9. From what source the value of the asymmetry factor was taken? It should be a reference here.

19. p.7163. line 14. I propose to show the value of total ozone content in the caption of Fig.12.

20. p.7164, line 7. It is not clear, how it is possible to calculate the combined effect of cloud and aerosol without including the cloudiness in the model? I assume that the authors thought to exclude the effect of cloudiness by having a ratio but this will happen only in the case of the same cloud properties at both sites. This point should be discussed in more details.

21. p. 7164, line 16. Why the asymmetry factor here is 0.67 and earlier it was 0.61? Is it an erratum? What is the value of Angstrom parameter?

22. p. 7164, line 24. It is not always clear whether the aod equals to 0.5 or 0.2 in summer and at what wavelength.

23. p. 7165, line 21. Fig.13. I assume that the measurements will be within the model calculated values if we account for the uncertainty of measurements at the shortest wavelengths. I propose to add the marks of different aods on the model curves. There is an erratum in the caption of Fig. 13: it should be ..around.. instead of ..about.. It is not clear if the NO₂ content has been accounted for in the model calculations.

24. p. 7165, line 24. I agree concerning the effects of surface albedo. But I am not sure that this is right if speaking about Angstrom parameter exponent. If you specify aod=0.5 at 1 mkm (like in summer conditions), the transition to 308nm, for example,

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would lead to the difference of 0.34 in aods calculated with the Angstrom parameter exponents of 1 and of 0.8. And this difference gives about 5-10% change in the UV flux at 308 nm. At the same time, Angstrom parameter exponent can be much higher (about 1.4-1.6) over the urban area due to increasing of the fine mode aerosol particle distribution. This should be at least discussed in the text.

25. p.7167, line 22. This is true only if the sun is obscured by clouds. If not, the signal can be quite stable even in cloud conditions. And, vice versa, in conditions with high aerosol loading the signal can be very unstable.

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