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Atmospheric Chemistry and Physics Discussions

Interactive comment on "UV variability in Moscow according to long-term UV measurements and reconstruction model" *by* N. Y. Chubarova

N. Y. Chubarova

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1. Answer to the comments of the referee Richard McKenzie. First of all, I would like to thank Richard McKenzie for the detailed analysis and the comments, which help me to improve the text. I tried to take into account all the remarks and my answers are given below. Unfortunately, I was not able to show the equations.

1. The model excludes the effects of changes in the vertical profiles of temperature and ozone. It has been shown previously that these can have significant effects on UV radiation (McKenzie et al., 2003).

Yes, I did not take into account for the long-term changes in the effects of vertical profiles of temperature and ozone. There is no reliable information about the ozone profiles in the past decades over Moscow. I have added the following text to the updated version: The model of reconstruction does not include the effects of changes in the vertical profiles of temperature and ozone which were shown to have the effects on UV-B irradiance (McKenzie et al., 2003). However, due to lack of information on ozone vertical profiles in the past this factor has been neglected.

2. It is also possible that there have been long term changes in surface albedo over the long period of study.

In the approach used in this study I take into account for the year-to-year variations of days with and without snow using the standard meteorological information on spatial coverage by snow (spatial snow coverage). Using this information I can take into account for the temporal changes in days with and without snow. I have included the description of how the albedo is calculated in the updated version of the text: The surface albedo A was estimated using the following equation:

(4) where A1=0.4 is the albedo of snow, A2=0.02 is the grass albedo. The value of snow albedo is in accordance with the typical TOMS MLER values over Moscow (Chubarova et al., 2002). The wA is a weighting coefficient, which accounts for snow occurrence, estimated using a standard meteorological characteristic - spatial snow coverage.

3. It is not always clear how some of the model parameters have been deduced. For example at page 897, line 26, how did the authors deduce that the snow can increase CQ by 0.15 to 0.17?”

I have inserted the part with the description of how main characteristics of the model have been evaluated to make the text more clear (see the equations (1)-(4)). The equation (2), which describes the cloud-albedo interaction, has been tested against RT model. In addition, the sentence has been slightly changed:

According to our estimates (see the equation (2)) snow can increase the CQA=0 values on about 0.15-0.17 during winter months…

Is that consistent with the spatial snow surface albedo of 0.4 as measured by TOMS;

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and if not, why not?

Yes, the spatial surface albedo is consistent with TOMS data which is described in our earlier paper (Chubarova et al., 2002). (see the extracts from the update variant above).

4. The changes attributable to ozone changes are important - though apparently not as important as changes in cloud and aerosol. However, when discussing the effects of seasonal changes in ozone, as in Figure 1, it may be more appropriate to measure the ozone change from a constant ozone baseline (one value for the whole year) rather than from a minimum ozone for each day, as stated.

I would argue. If I take one value I would neglect the seasonal changes in ozone which are significant in high latitudes. Here, I would like to compare the effects of cloudiness and ozone on Qer, which are calculated by Qer comparison with and without the factor. We can not exclude all ozone content – this is the unrealistic situation. But when we take the minimum daily ozone content, we make the similar thing as in the analysis of cloud influence on Qer when taking minimum (zero) cloudiness, which can be zero at a given day.

5. Also, it would have be more useful to plot the ozone effect in terms of a change in optical depth for erythema (rather than just ozone amount), in a similar fashion to the plot for NO2.

Optical depth of ozone has a very strong spectral dependence compared with NO2 and aerosol optical depth. Also the effective wavelength for Qer will change during the year and this should change the choice of the effective ozone absorption wavelength and , hence, its optical depth. That is why I am inclined to leave the Figure as is.

6. It would also be helpful if the model could be written algebraically.

This part of the text has been modified in the following way: .. The UV reconstruction model used in this study is described in details in (Chubarova and Nezval; 2000,

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Chubarova et al., 2005). However, in order to better understand the obtained results, main characteristics of the model are shown below. The model is based on the assumption that the year-to-year UV variability Vi can be written as a sum of UV variations due to variations in total ozone v1, aerosol optical thickness v2, cloud optical thickness v3 and cloud amount v4 with account of surface albedo A and monthly weights Wj of solar angle h:

(1)

where index i corresponds to a year number, index j - to a month number; X is the total ozone content; а and c are an aerosol and cloud optical thickness; Pcf and Pov are the occurrences of clear sky and overcast conditions. UV variability due to cloud amount (v4) was estimated using the effective cloud amount transmission (CQA). The influence of surface albedo on this characteristic is accounted in the form of geometric progression:

(2) where C=0.9 and D=0.6 according to the model simulations. The CQA=0 is determined as a combination of relative frequency of different cloud amounts weighted on their UV transmission:

(3) Here, P(Nl) is the frequency of low layer cloud amount (Nl) with different amounts of total cloudiness for a given month, P(Nl, N=10) – is the frequency when total cloud amount is equal to N=10, always corresponding to overcast conditions but with different amount of low layer clouds; CQ,A=0(Nl) – is the UV transmission by low layer cloudiness; CQup =0.93 is a mean UV transmission by overcast upper layer cloudiness. The second term of equation (3) accounts for the UV transmittance in overcast cloud conditions, while all other situations are considered in the first term. This equation is obtained with the assumption that upper level clouds do not affect the UV transmittance except when overcast with upper level clouds. Therefore this method independently accounts for UV transmittance by optically thick low-level cloudiness and thin upper level clouds. The UV transmission of different cloud amount has been

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evaluated on the base of long-term measurements of UV irradiance of 300-380 nm (Chubarova, 1998). UV transmission is known to have some spectral features in its attenuation (see, for example, Chubarova et al., 1996, Lindfors, Kylling 2007). However, our model calculations have shown quite similar effects of clouds on UV irradiance 300-380nm and Qer due to minor difference in their effective wavelengths with few percents higher Qer cloud transmission. Whereas we are interested in relative changes of UV irradiance, we neglect this small difference.

7.The author should justify the statement on page 896 line 19 that the model uncertainty is "less than 2%". For example, were corrections applied as discussed elsewhere (Seckmeyer et al. 2006) for differences between the instrument band pass and the true erythemal weighting function, or for errors in the cosine weighting function? If not, I would expect the measurement uncertainties to exceed 10%. Even with such corrections it is extremely difficult to achieve an absolute measurement accuracy of better than 5%.

The uncertainty of 2% is the uncertainty of the application of the independent terms in the equation (1) in algebraically way. Of course, this is not the total difference between measurements and modeling which can be much higher due to the uncertainties of measurements and some non accounted parameters. To make this more clear I’ve modified the sentence: For the large range of atmospheric parameters (total ozone of 250 - 450 DU, aerosol optical thickness at 380nm (AOT380) of 0.05-0.6, cloud optical thickness of 0 – 60) this approach was shown to give uncertainty less than 2% compared with the accurate model calculations.

All comprehensive types of corrections have been applied to obtain the corrected erythemally weighted irradiance from UVB-1 YES measurements. I had worked in close connection with Colorado UVB monitoring group (D. Bigelow, J. Slusser and K. Lantz). Kathleen Lantz is a co-author of the new WMO publication (Seckmeyer et al., 2006). I have read this publication attentively and I can say that our program is organized in accordance to its main recommendation. As a result, the text has been slightly mod-

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ified: The corrections on total ozone and solar zenith angle have been applied to the initial data in order to minimize the errors for high solar zenith angles and large ozone content according to (Lantz et al., 1999, Chubarova, 2002), which is in accordance with (Seckmeyer et al., 2006).

8.In Figure 2 it is puzzling that the maximum seasonal UVI values exceed the peak daily values by a factor of two. Is this because the upper panel includes all weather, whereas the lower panel is essentially a clear sly envelope. In that case, I presume the errors bars show the year to year variability. These points should be clarified.

The Figure 2 has been modified. Yes, Fig 2a shows the average UVI values (now with error bars) for typical (cloudy) conditions. I am very grateful for this comment because I was able to find a small bug during the reconstruction of the Figure. And the corresponding text has also been slightly changed. Since in Moscow cloudy weather is dominating, the average values are much lower than the maxima. At the same time, the maxima can be observed not in clear sky conditions, but in conditions with open sun and broken cloudiness. That is why Fig. 2b does not represent clear sky envelope;, but the UVI maxima.

9. In my opinion, the key result is Figure 3, in which the various contributions to changes are compared (Fig 3a), and in which the overall model result is compared with measurements over the period from 1999 to 2007. As the author states, the tendency towards smaller cloud transmission that occurred between about 1980 and 2003 did not continue over the 3-4 years since 2003, and that covers a large fraction of the period for which corroborative data were available. The paper would therefore be greatly improved if previously-published UV results from the same group could have been overlaid. Because they have a lower sensitivity to ozone changes, it would perhaps be appropriate to use three panels in that case. The new panel could compare the results from that older instrument over a longer period, with an appropriately weighted version of the model parameters identified in Fig 3a. When redrawing this Figure, please also take care to ensure that the years in the lower panel line up with those in the upper

panel. The four points in Fig 3a should line up with the point for the corresponding year in Fig 3b.

I agree. Our previous analysis described the period only up to 2003. Here, I have significantly changed the text and have added the additional Figure 3c. One can see, that the UV measurements, which are not sensitive to ozone, have much less growth during the last 3-4 years (and even a tendency to decrease) due to the drop of effective cloud amount transmission (see the blue curve in Fig.3a). Several modified parts of the section 4 are the following: Fig.3 a,b presents Qer variations due to different atmospheric parameters for 1968-2006 period as well as reconstructed and observed long-term Qer variability. In addition, Fig 3c shows the interannual variations of measured and reconstructed UV irradiance 300-380nm (Q380), which has negligible dependence on ozone.

The substantial growth of effective cloud amount transmission at the end of the century has not continued during the last few years since 2003 but still there is statistically significant increase in Qer due to CQA of about +2.1% per decade since 1980.

..Fig 3c shows the similar character of UV irradiance 300-380nm interannual changes but with less pronounced variations (within 10%) than those obtained for Qer. There is also the absence of further Q380 increase since 2003 due to a tendency of reduction in effective cloud amount transmission from its level on the frontier of the centuries (see the blue curve in Fig.3a). At the same time, the Q380 level during the last years is still about 10% higher than it was observed at the beginning of 1980s. However, this growth is significantly less than that estimated for Qer. due to the additional influence of ozone decrease on erythemally-weighted irradiance during the last years.

10. In the discussion of health effects (page 899, line 15), I would suggest that the authors clearly attribute the statements about vitamin D sufficiency to Holick et al. Their statement that no vitamin D is made in the Boston winter is inconsistent with the action spectrum for vitamin D production is (e.g., see (McKenzie, 2007, McKenzie et

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al., 2007 submitted)). As discussed in the latter paper, the relationship between UVEry and UVV itD becomes non-linear for low values of UVI, and depends on the ozone amount and the solar zenith angle. Consequently, it is not really valid to use a constant threshold as has been implied by the horizontal green lines in Figure 2. However, it is probably sufficient here to emphasise that the threshold is only approximate.

I agree that the thresholds for Vitamin D are approximate and I have added this in the text: Using this simple approximate threshold we show the inability to get vitamin D;.

I clearly understand that the uncertainty is high when we use Qer instead of real QvitD irradiance. I hope to continue the studies in this direction. But at present, the people in Moscow need this information (although it is very rough). That’ is why I decided to use this approximate thresholds.

Answers to minor points I have changed the text according to all the suggestions shown below.

Page 894, line 3. According to "a" reconstruction model... Done Page 894, line 11. Over "the longer" 1968-2006 period... Done Page 894, line 21. and "trace" gas... Done Page 897, line 2. bias "less than 0.5, but only" if AOT550 is calc... Done Page 897, line 3. using "Mie" theory... Done Page 897, line 10. Were extinctions by other potentially important trace gases (e.g. SO2) considered?

No. It was previously shown that in Moscow the SO2 concentrations are very small and their effects do not reach 1% for Qer. There is a corresponding reference to (Chubarova, 2006) in the paper.

Page 897, line 16. to explain "the" main features... Done Page 897, line 23. .. latitudes", a strong" seasonal cycle... Done Page 897, line 27. .. CQ values "by" about... "at this site." [How was this determined?].

It was determined from the equations (2) and (3), which have been included in the updated text (see the extract from the paper above (the answer to the remark number

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6).

Page 899, line 5. .. indices can "reach" middle... Done Page 899, line 9. .. sun disk was "unobscured by cloud"... Done Page 899, line 15. .. Furthermore "they state" ... Done Page 900, line 2. .. plays "a" noticeable role ... Done Page 900, line 9. .. at the end of the century "has not continued in the last 3-4 years". Done but instead of ..3-4 years..; I have added ..since 2003..;.

Page 900, line 12. .Without having read the paper cited, or having first hand experience of the measurement site, I still suspect that the statement about the typicality of aerosol effects is too strong. It seems unlikely that the Moscow site would be completely uninfluenced by local aerosol sources.

Yes, of course, the aerosol loading in Moscow is systematically higher than in clear areas with the mean difference of about 0.05-0.06 in the UV spectral region according to my estimates obtained from our simultaneous AERONET measurements in Moscow and at close rural upwind site. But the long-term tendency can be the same both in rural and industrial areas. As I understand, the main reasons for this tendency are the following: the change of fuel from coal to gas at the end of 1980s at the whole territory of the USSR and the stagnation of industry in Russia.

Page 900, line 22. .. No variations in "astronomical" parameters have been discussed. I suggest deleting the word. Done Page 901, line 1. .."small but quite pronounced" is contradictory. Perhaps better as "small, but still detectable"? Done Page 901, line 6. .. "reach" middle and high .... Done Page 901, line 11. .. unfavourable conditions for "human" health; Done

2. Answer to the comments of the anonymous reviewer.

I would also like to thank the anonymous reviewer for his helpful comments. Here, I combined the remarks and my answers to them. Unfortunately, I was not able to show the equations.

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The author uses two different concepts to describe the cloud effects in various places in the text and in Figure 3; namely the effective cloud amount transmission and the cloud optical thickness. I wonder if these two parameters can be safely considered independent of each other. This should be made clearer in the text.

To clarify the reconstruction model approach used in this text I have added the description of main postulates of the reconstruction model. In order to describe the influence of clouds I use two parameters (but not two concepts!). The first one, effective cloud amount transmission, which is determined on the base of pre-calculated dependence of UV transmittance on cloud amount and frequency of different cloud situations. As a result, this characteristic does not take into account variations in optical thickness. The second one, cloud optical thickness, which is determined in overcast (cloud amount equals to 10) conditions. A method to determine the cloud optical thickness from measurements of shortwave (300-4500 nm) irradiance is described in [Tarasova and Chubarova, 1994]. For these overcast conditions (most important case) we account for the changes in cloud optical thickness. The text, which describes the reconstruction model has been changed in the following way:

..The UV reconstruction model used in this study is described in details in (Chubarova and Nezval; 2000, Chubarova et al., 2005). However, for better understanding of the results, the main characteristics of the model are shown below. The model is based on the assumption that the year-to-year UV variability Vi can be written as a sum of UV variations due to variations in total ozone v1, aerosol optical thickness v2, cloud optical thickness v3, and cloud amount v4 with account of surface albedo A and monthly weights Wj of solar angle h:

(1)

where index i corresponds to a year number, index j - to a month number; X is the total ozone content; а and c are an aerosol and cloud optical thickness; Pcf and Pov are the occurrences of clear sky and overcast cloudy conditions.

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UV variability due to cloud amount (v4) was estimated using the effective cloud amount transmission (CQA). The influence of surface albedo on this characteristic is accounted in the form of geometric progression:

(2) where C=0.9 and D=0.6 according to the model simulations. The CQA=0 is determined as a combination of relative frequency of different cloud amounts weighted on their UV transmission: (3) Here, P(Nl) is the frequency of low layer cloud amount (Nl) with different amounts of total cloudiness for a given month, P(Nl, N=10) – is the frequency when total cloud amount is equal to N=10, always corresponding to overcast conditions but with different amount of low layer clouds; CQ,A=0(Nl) – is the UV transmission by low layer cloudiness; CQup =0.93 is a mean UV transmission by overcast upper layer cloudiness. The second term of equation (3) accounts for the UV transmittance in overcast cloud conditions, while all other situations are considered in the first term. This equation is obtained with the assumption that upper level clouds do not affect the UV transmittance except when overcast with upper level clouds. Therefore this method independently accounts for UV transmittance by optically thick low-level cloudiness and thin upper level clouds. The UV transmission of different cloud amount has been evaluated on the base of long-term measurements of UV irradiance of 300-380 nm (Chubarova, 1998). UV transmission is known to have some spectral features in its attenuation (see, for example, Chubarova et al., 1996, Lindfors, Kylling 2007). However, our model calculations have shown quite similar effects of clouds on UV irradiance 300-380nm and Qer due to minor difference in their effective wavelengths with few percents higher cloud transmission for Qer. Whereas we are interested in relative changes of UV irradiance, we neglect this small difference in cloud transmission. The surface albedo A was estimated using the following equation:

(4) where A1=0.4 is the albedo of snow, A2=0.02 is the grass albedo. The value of snow albedo is in accordance with the typical TOMS MLER values over Moscow (Chubarova et al., 2002). The wA is a weighting coefficient, which accounts for snow occurrence, estimated using a standard meteorological characteristic - spatial snow coverage.

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Specific comments

896, 10: This sentence does not give enough information for the method of calculating the effective cloud transmission. I suggest either to provide some more details or to remove this sentence and give only the reference.

Yes, I have added the piece of the text shown above.

896, 20: What exactly represents the quoted uncertainty of 2%?

The uncertainty of 2% is the uncertainty of the application of the independent terms in the equation (1) in algebraically way. Of course, this is not the total difference between measurements and modeling which can be much higher due to the uncertainties of measurements and some non accounted parameters. To make this more clear I have modified the sentence:

..For the large range of atmospheric parameters (total ozone of 250 – 450 DU, aerosol optical thickness at 380nm (AOT380) of 0.05-0.6, cloud optical thickness of 0 - 60) this approach was shown to give uncertainty less than 2% compared with the accurate model calculations.

897, 1-3: Please include some discussion on using a different Angstrom parameter from the measured one in order to remove the \bias in AOT550.

Another Angstrom parameter has been used only(!) for extracting more precise AOT550 from direct shortwave irradiance measurements and water vapour content according to [Tarasova and Yarkho, 1991]. Angstrom parameter is considered to be a fixed parameter in the equations obtained in this paper. Therefore, by changing Angstrom parameter one can obtain slightly different AOT550. In our study we have shown that the best agreement with AERONET data is obtained when we specify the Angstrom parameter as alfa=1. It seems that this is not very important information for understanding the whole text and I would like not to add this discussion in the text. But the sentence itself has been slightly changed:

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The test has confirmed the absence of the bias for AOT550 less 0.5 but only if AOT550 is calculated with Angstrom parameter of =1 instead of the observed =1.4…

899, 24: The agreement between measured and reconstructed data is not very good. Differences of at least 5% are evident. This should be discussed in the text. Similarly the quality of the reconstruction model cannot be regarded as *high*.

In this context I agree that it can not be regarded as high. However, I assume that the difference is caused not only by the quality of reconstruction model but by the quality of the empirical data as well. It is very hard to reach the uncertainty better than 5% for broadband UV measurements [Seckmeyer et al., 2006]. I would like to emphasize that our previous comparison between reconstruction model and satellite UV data (for example, v.8 TOMS UV retrievals with account for absorbing aerosol) has revealed much better agreement. The correlation coefficient increases from 0.82 (between surface UV measurements and reconstruction model) to 0.94 (between TOMS UV series and reconstruction model) (see Chubarova et al., 2005). The text has been changed in a following way: ..There is quite satisfactory agreement between measured and model values that has confirmed a good quality of the reconstruction model. Some difference between the observed and reconstructed UV irradiance can be also caused by the uncertainty of broadband UV measurements, which is about 5%...

904, figure 1: The NO2 optical thickness is very small and hardly visible in the upper graph. On suggestion would be to multiply OTNO2 by 10 in order to make it more evident in the graph.

I have slightly changed the graph (the scale of left axis) to make the OTNO2 more pronounced. I would like to leave the real scale in OT_NO2 in order to make the difference between OTNO2 and AOT340 more pronounced.

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895, 15: radiative —> radiation Done 896, 21: delete *estimates of the* and 896, 23: replace *were also fulfilled* with *were estimated* Both comments concern the same sentence, which has moved to another place and has been rewritten:

..Fig 1b presents seasonal variation of mean Qer losses due to different atmospheric parameters, which have been estimated using TUV model, except effective cloud amount transmission which has been calculated using the equations (2) and (3). 898, 8: cycle —> variation Done. 898, 23: significant —> significance Done. 899, 3: compare with the —> compared to this part has been rewritten. 899, 27: dominated comprising ´s10% —> dominant explaining about 10% of the variations Done 900, 2: plays —> play Done 900, 9: is getting down —> decreases Changed in another way: The substantial growth of effective cloud amount transmission at the end of the century has not continued.

900, 13: *work* on the Qer increase which comprises of —> result in Qer increase, which is estimated to

Done 900, 27: for —> by Done 901, 11: people health during —> people’s health during the Changed to “human health”

3. Answer to the comment of Anders Lindfors.

Also I have added the text, which is the response to the comment made by Anders Lindfors. This is a description why we neglect the spectral features of the cloud influence.

..The UV transmission of different cloud amount has been evaluated on the base of long-term measurements of UV irradiance of 300-380 nm (Chubarova, 1998). UV transmission is known to have some spectral features in its attenuation (see, for example, Chubarova et al., 1996, Lindfors, Kylling 2007). However, our model calculations have shown quite similar effects of clouds on UV irradiance 300-380nm and Qer due to minor difference in their effective wavelengths with few percents higher Qer cloud

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transmission. Whereas we are interested in relative changes of UV irradiance, we neglect this small difference..

References: Chubarova, N., Nezval Ye. I., Verdebout J., Krotkov N., and Herman J.: Long-term UV irradiance changes over Moscow and comparisons with UV estimates from TOMS and METEOSAT, “Ultraviolet Ground- and Space-based Measurements, Models, and Effects” edited by G. Bernhard, J.R. Slusser, J.R. Herman, and W. Gao, SPIE, 63-73, 2005. Seckmeyer, G., Bais, A., Bernhard, G., Blumthaler, M., Booth, C.R., Lantz, K., McKenzie, R.L.: Instruments to measure solar ultraviolet irradiance. Part 2: Broadband instruments measuring erythemally weighted solar irradiance. WMO, Global Atmospheric Watch No. WMO TD No. 1289. 51 pp., 2006. Tarasova, T. and Chubarova N.: On the calculation of optical thickness of extended low and middle clouds using measurements of solar radiation in three solar spectrum ranges on the Earth s surface, Izvestiya Atmospheric and Oceanic Physics, (English translation), 30 , 253-257, 1994. Tarasova, T.A. and Yarkho E.V.: Determination of aerosol optical thickness using measurements of direct integral solar radiation. Soviet Meteorology and Hydrology, (English translation), 12, 66-71, 1991.

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