

Interactive  
Comment

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

**R. McKenzie (Referee)**

r.mckenzie@niwa.co.nz

Received and published: 29 January 2008

### General Comments

This short study is based on a very interesting poster that was presented at the PMOD "One century of UV radiation research" meeting in Davos, in September, 2007. The paper was not published in the conference proceedings, so it is pleasing to see that the work is now available to a wider audience.

The paper investigates long term changes in erythemally-weighted UV in the Moscow region from 1968 to 2007 using radiative transfer calculations. It quantifies the contribution to these changes from various atmospheric effects, and it verifies the resultant model to represent those changes by comparing its results against measurements

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



taken with an RB meter over the period since 1998. In addition, the paper describes the effects of seasonal and diurnal changes in UV on health for the population in that region. The paper provides and interprets original data from a data-sparse region, and weighs the relative importance of several topical effects, including ozone depletion, global dimming and brightening, and local pollution. I recommend publication in ACP after attending to the issues raised below.

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). It is also possible that there have been long term changes in surface albedo over the long period of study. These points should at least be mentioned, and addressed in more detail if possible.

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? Is that consistent with the spatial snow surface albedo of 0.4 as measured by TOMS; and if not, why not? 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. Also, it would have been 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 NO<sub>2</sub>. It would also be helpful if the model could be written algebraically.

The author should justify the statement on page 896 line 19 that the model uncertainty is "less than 2%". I have my doubts about this, and wonder how that figure was determined when the precise values of some of the important input parameters (e.g., single scatter albedo of aerosols) are not known. I wonder if the intention was rather to state something like "the measured results could be reproduced to x% using realistic input parameters for the model". Even this result would be rather surprising given the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



uncertainties in measuring UV. For example, were corrections applied as discussed elsewhere (Seckmeyer et al. 2006) for differences between the instrument band pass and the true erythral 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%.

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 sky envelope. In that case, I presume the error bars show the year to year variability. These points should be clarified.

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.

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 al.,

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



2007 submitted)). As discussed in the latter paper, the relationship between  $UV_{Ery}$  and  $UV_{VitD}$  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.

While the paper is clearly written and understandable, there are a few cases where English grammar could be improved. I have highlighted some of these below.

#### Minor Points

Page 894, line 3. According to "a" reconstruction model...

Page 894, line 11. Over "the longer" 1968-2006 period...

Page 894, line 21. and "trace" gas...

Page 897, line 2. bias "less than 0.5, but only" if AOT550 is calc...

Page 897, line 3. using "Mie" theory...

Page 897, line 10. Were extinctions by other potentially important trace gases (e.g.  $SO_2$ ) considered?

Page 897, line 16. to explain "the" main features...

Page 897, line 23. .. latitudes", a strong" seasonal cycle...

Page 897, line 27. .. CQ values "by" about... "at this site." [How was this determined?].

Page 899, line 5. .. indices can "reach" middle...

Page 899, line 9. .. sun disk was "unobscured by cloud"...

Page 899, line 15. .. Furthermore "they state" ...

Page 900, line 2. .. plays "a" noticeable role ...

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Page 900, line 9. .. at the end of the century "has not continued in the last 34 years".

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.

Page 900, line 22. .. No variations in "astronomical" parameters have been discussed. I suggest deleting the word.

Page 901, line 1. .."small but quite pronounced" is contradictory. Perhaps better as "small, but still detectable"?

Page 901, line 6. .. "reach" middle and high ....

Page 901, line 11. .. unfavourable conditions for "human" health<sup>8230</sup>;

## References

McKenzie, R.; Smale, D.; Bodeker, G.; Claude, H. (2003). Ozone profile differences between Europe and New Zealand: Effects on surface UV irradiance and its estimation from satellite sensors. *Journal of Geophysical Research* 108(D6): 4179 doi:10.1029/2002JD002770.

McKenzie, R.L. (2007). UV Radiation Climatology and Trends (Invited presentation). PMOD UV Meeting in Davos celebrating "One Hundred years of UV Radiation Research".

McKenzie, R.L.; Liley, J.B.; Björn, L.O. (2007 submitted). UV Radiation: Balancing Risks and Benefits. Photochemistry and Photobiology ms number: PHP-2007-12-RA-0313.

Seckmeyer, G.; Bais, A.; Bernhard, G.; Blumthaler, M.; Booth, C.R.; Lantz, K.; McKenzie, R.L. (2006). Instruments to measure solar ultraviolet irradiance. Part 2: Broadband instruments measuring erythemally weighted solar irradiance. WMO, Global Atmo-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



spheric Watch No. WMO TD No. 1289. 51 pp.

---

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 893, 2008.

ACPD

8, S59–S64, 2008

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

