Atmospheric Chemistry and Physics Discussions

Interactive comment on "Towards closure between measured and modelled UV under clear skies at four diverse sites" *by* J. Badosa et al.

B. Mayer (Referee)

bernhard.mayer@dlr.de

Received and published: 5 March 2007

Review of "Towards closure between measured and modelled UV under clear skies at four diverse sites" by Badosa et al.

The manuscript presents a detailed comparison between observations and simulations of ultraviolet irradiance under cloudless conditions at four sites with low pollution. The study is one of the most comprehensive investigations of that kind and the comparison of the different climates at the four sites gives interesting insights into the accuracy of radiative transfer simulations. A detailed analysis of the relevance of various input parameters is presented. The manuscript is well-written and I suggest publication in ACP after consideration of my minor comments below.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

Page 1508, Abstract: I suggest to add a sentence that the aerosol load was very small at the four sites. Much larger differences are to be expected for polluted conditions.

Page 1510: The conclusion that "absolute errors of radiative transfer models could be larger" is a bit misleading. The radiative transfer in the ultraviolet spectral range is well-understood, but the input parameters are usually not known accurately enough, in particular in the case of clouds and aerosols.

Page 1511, line 21: A good example for a comparison between observations and simulations for a polluted site is given by Kylling et al., Effect of aerosols on solar UV irradiances during the Photochemical Activity and Solar Ultraviolet Radiation campaign, JGR 103 (20), 26051-26060, 1998. I suggest to add the reference.

Page 1511, line 24: All of the mentioned points have been addressed by one of the referenced publications. The sentence "The study differs from previous ones in the following senses" is a bit misleading as it may suggest that each of the following points has been addressed for the first time by the new manuscript. I suggest to simply remove this sentence.

Page 1513, line 8: I would lay the emphasis on the Dobson and TOMS ozone observations and not start with the Stamnes data. As you discuss later in the paper, the Stamnes (1991) method is not an independent observation of ozone as the ozone derived from global irradiance is then again used to simulate the global irradiance.

Page 1513, line 20: Which instrument was used to measure AOD?

Page 1513, line 25: If the instrument is a DTM300, is was manufactured by Bentham, not by NIWA. NIWA certainly adapted the monochromator but they did not really manufacture it?

Page 1513, line 9: Some of the uncertainties (including the largest ones) are not random but systematic, e.g. the calibration uncertainty of NIST and the cosine response.

Page 1517, line 5: Setting the altitude of the site to 2.4km at Mauna Loa and consider-

ing an extra layer of 1km air is one of many ways to address the mountain. However, there is of course some arbitraryness here. This is already kind of "model tuning" to make the simulations agree with observations for which reason the model is no longer fully independent. Please mention this!

Page 1517, line 6: Replace "multiscattering with the air layer underneath" by "multiple scattering between the atmospheric layers underneath and above"

Page 1517, line 15: Why was alpha assumed constant instead of using the observed values?

Page 1517, line 23: I wouldn't call these "cases" but "scenarios"

Page 1518, line 3: A single scattering albedo of 0.9 is already quite absorbing. It would have been interesting to include a single scattering albedo of 1.0 in the calculations to get the upper bound of the irradiance for the observed AOD.

Page 1518, line 15: I am not completely sure, but TOMS ozone might be referenced to sea level, not to the average altitude of the TOMS pixel. TOMS does not "see" down to the surface anyway and as far as I know, a climatological value is added, so that the total column is the column down to sea level. If this is the case then it is not surprising that a difference of 9.5 DU is found between the observations from Mauna Loa and the TOMS value.

Page 1519, line 23: A site with an average AOD of less than 0.1 around 400 nm cannot be called polluted.

Page 1520, line 7: Replace "interestingly" with "accidentally" or "incidentally"

Page 1521, line 8: It is interesting to see that the change from the USSA to the actual ozone profile causes a systematic change in the UV radiation in Figure 4 at all locations except Boulder. That probably means that the profiles at these locations are SYSTEMATICALLY different from the USSA. Also, do you have an explanation why the temperature profile affects the irradiance in a completely different way than the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

ozone profile. Both profiles directly affect the absorption coefficient but obviously in a completely different way.

Page 1521, line 24: Replace "multi scattering" with "multiple scattering"

Page 1522, line 23: Please mention that much larger differences are expected for more polluted sites

Page 1523, line 1: I disagree with the conclusion that there is no circularity when using Stamnes ozone as input to the model. You demonstrate that there is good agreement with Dobson measurements which shows that there is reasonable agreement of the "dependent" observations with independent ones and that the effect of using the "dependent" observations is small. And you should be careful with the interpretations because in Figure 7 the systematic difference between the "dependent" observations and the independent ones is already half of the diurnal variation, the study of which is the justification for using the Stamnes instead of the Dobson ozone. Please discuss more carefully!

Page 1524, line 17: There is a variety of unnecessary abbreviations in the manuscript, including "SD" for standard deviation, "D1-D14" for "difference", and "R1-R14" for "ratio". I suggest to avoid those.

Page 1526, line 20: Is there any information about the type of aerosol? A single scattering albedo of 0.7 is quite low!

Page 1527, line 3: Boulder doesn't really become Melbourne-like if you remove the day with snow cover. Boulder is still centered around the 1-line while Melbourne is systematically lower. However, the example illustrates that the results are not very robust: removing data from one single day changes the average significantly.

Page 1527, line 10: What is the physics behind the change of the transmission of PTFE with temperature?

Page 1527, line 28: "responded more slowly" is a bit misleading. What you probably

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

mean is that the diffuser temperature needs some time to adapt to the ambient temperature and for quick changes in the ambient temperature the diffuser temperature cannot follow.

Page 1528, line 23: The suggestion that "cumuliform clouds were more effective in reflecting UVI than stratiform clouds" is a bit surprising. Cumuliform clouds certainly have a somewhat higher reflectivity than stratiform clouds but stratiform clouds usually have a much higher cloud cover.

Page 1528, line 18: Could you provide the effective cloud albedo corresponding to "the 9% increase in the R2 ratio"?

Page 1530, line 3: Replace "5% for the 77% to 98%" by "5% for 77% to 98%"

Page 1530, line 16: From your calculations it seems that a "better knowledge of aerosol properties in the UVB region" can actually not explain the differences between simulations and observations at Melbourne

Page 1530, line 27: You did not have any observations of the single scattering albedo. Hence I suggest to replace "Uncertainty in the value of the SSA" by "Missing knowledge of the SSA"

Page 1530, line 31: Yes, in case of clouds much larger differences are to be expected between observations and simulations. However, this is again not the fault of the radiative transfer model, as we have detailed 3D models nowadays which allow to simulate radiation for nearly arbitrary clouds. It is again the missing observations of the input parameters which are particularely hard to obtain in case of clouds.

Table 2: The font size is too small. Maybe the table can be printed in Landscape?

Table 3: You probably mean "SZA < 65", not "SZA $<< 65$"

Table 6: You should not call Melbourne and Boulder "polluted"

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1507, 2007.

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU